CHAPTER FIVE

TECHNOLOGICAL TRIUMPH: SOCIAL TURMOIL, 1964-1969

THE VIETNAM AGONY

When Vice President Lyndon B. Johnson assumed the presidency after John F. Kennedy's assassination, he inherited Vietnam. In 1963, the South Vietnamese were assisted by 16 000 Americans acting as "military advisors" or conducting "combat support" missions. Americans were already dying; seventy-seven died in that year.[1] However disturbing the situation, Johnson felt that he could not disengage. As he was later to say, "if I left that war and let the Communists take over South Vietnam, then I would be seen as a coward and my nation would be seen as an appeaser and we would both find it impossible to accomplish anything for anybody anywhere on the entire globe."[2] While the president did not want to be called a coward untrue to Kennedy's commitment, he also feared deeply that Vietnam might escalate into a third world war.[3] Johnson thus began a forced-step by forced-step, start-and-stop escalation of the war.

Johnson took the first major step on August 2 and 4, 1964, in the Gulf of Tonkin. There, in international waters, three North Vietnamese patrol torpedo boats fired on the American destroyer *Maddox*. The president ordered reprisal bombing of North Vietnamese patrol boat bases and oil depots. At Johnson's request, the Congress passed a Joint Resolution on August 10 that approved and supported "the determination of the President, as Commander in Chief, to take all necessary measures to repel any armed attack against the forces of the United States and to prevent further aggression."[4] The resolution, a tacit declaration of war, was passed with a House vote of 414-0 and a Senate vote of 88-2. The Congress was solidly behind the president. Early in 1965, communist insurgents in South Vietnam—known to themselves as the National Liberation Front but derisively referred to as the Viet Cong—attacked a U.S. Air Force barracks at Pleiku, killing 8 American soldiers and wounding 126 others. Johnson resumed bombing and committed U.S. ground troops to fight. Thus began a cycle of battles, bombing halts, ignored proposals for peace conferences, and resumptions of bombing. The targets and scale of the bombings were personally chosen by the president, who was inhibited by the possibility of provoking Chinese or Soviet retaliation. Troop strength was increased periodically. By the end of 1965 American

[1] James MacGregor Burns, *The Crosswinds of Freedom: From Roosevelt to Reagan—America in the Last Half Century* (New York: Vintage Books, 1990): 390-391.

[2] Johnson quoted in Doris Kearns, *Lyndon Johnson and the American Dream* (New York: Harper & Row, 1976): 252.

[3] Kearns, *Lyndon Johnson*: 253.

[4] Joint Resolution *To promote the maintenance of international peace and security in southeast Asia, U.S. Statutes at Large*, 78(1964): 384.

troop strength was 180 000. It rose to its maximum of almost 550 000 troops by the end of 1967.

In April 1965 the undercurrent of protest that had been present since the beginning of American involvement in Vietnam became a rushing torrent. Tens of thousands of young people, along with intellectuals, teachers and entertainers, came to the Nation's capital to picket the White House and march to the Washington Monument for songs and speeches protesting the war. Similar marches by large and small groups were occurring ever more frequently throughout the Nation. Coupled with these anti-war protests were student revolts against university administrations. Anti-war demonstrations, sit-ins, and teach-ins took place at many of the Nation's finest educational institutions. Unrelated to the Vietnam War but even more destructive were riots that took place in the black sections of the Nation's largest cities. In the hot July of 1964 major riots broke out in Harlem and Brooklyn and then spread to Rochester, Jersey City, Paterson, Chicago, Cleveland, and Philadelphia. In August 1965 the predominantly black Watts section of Los Angeles erupted into six days of rioting and arson. In July 1967 riots in Detroit killed forty-three people; a situation so serious that President Johnson was forced to call out the 18th Airborne Corps. In the words of Paul Johnson, "large-scale riots by blacks in the inner cities became a recurrent feature of the Sixties, in sinister counterpoint and sometimes in deliberate harmony with student violence on the campuses."[5] The self-immolations at the Pentagon and outside the United Nations building, and the tragic events on the campus of Kent State University, where rookie soldiers fatally shot students who had vandalized the ROTC building, epitomized the chaos. Together, the triple stridencies of war protest, rebellion against racial discrimination, and student unrest—all catalyzed and heightened if not caused by aversion to the war—sounded a shrill note in the second half of the decade.

The revolt against the war was not confined to anti-war activists. Noted columnist Walter Lippman turned against the Johnson policy. Martin Luther King broke with the president, calling his own Government "the greatest purveyor of violence in the world today."[6] Antagonism spread to the halls of Government. Senator Richard Russell of Georgia and lawyer/statesman Clark Clifford, both formerly close to the president, now broke with Johnson and his policies. In Congress, Senator Fulbright, who had guided the Gulf of Tonkin Resolution through the Senate, became a strong opponent of the war.

In February 1968, in the midst of this turmoil, the Viet Cong and the North Vietnamese Army attacked. In an operation called the Tet Offensive because it came during the normally festive and peaceful Lunar New Year celebration, or "Tet," more than one hundred cities in South Vietnam, including Saigon, Hue, and Khe Sanh, were attacked. The attackers achieved early success, but after a month of bitter fighting, the United States and South Vietnamese defenders regained all lost ground and inflicted

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[5] Paul Johnson, *Modern Times: The World From the Twenties to the Nineties* (New York: HarperPerennial, 1992): 646.

[6] King quoted in Barbara Tuchman, *The March of Folly: From Troy to Vietnam* (London: Cardinal, 1990): 426.

huge losses on the enemy forces. It was a serious loss for the Viet Cong, and they were thought to be essentially finished as a fighting force. National Security Advisor Walter W. Rostow said, "The other side is near collapse."[7] But the mere fact that the North Vietnamese forces could mount such an attack gave them a psychological victory. The prevalent media perception was aptly expressed by Walter Cronkite, who, having gone to Vietnam to see the results of Tet, was said to have declared, "What the hell is going on? I thought we were winning the war!"[8]

The Tet offensive occurred at the beginning of an election year, and less than a month later the New Hampshire primaries were held. The vigorously anti-war Senator Eugene McCarthy entered the presidential race and in the primary garnered 42 percent of the vote. This was an amazing achievement against a sitting president, particularly one as politically skilled as Johnson. The president won the majority of the votes, but McCarthy won the headlines. It was Johnson's own Tet.[9] Sensing Johnson's weakness, Robert F. Kennedy entered the race.

On March 31, 1968, President Johnson took to the airwaves to announce a unilateral halt to naval and air bombardment above the 20th parallel and to call for peace talks. Historian James MacGregor Burns, aptly describes what happened next:

> Then, as his listeners stared speechless at their television screens, the President said at the end of his talk, 'I shall not seek, and I will not accept, the nomination of my party for another term as your President.' He had had enough, he told Doris Kearns later. He was being stampeded from all directions—'rioting blacks, demonstrating students, marching welfare mothers, squawking professors, and hysterical reporters.' Then the thing he had feared most—Bobby Kennedy back in the fray, embodying the Kennedy heritage.[10]

Yet only three days after Johnson's speech, to the surprise of everyone, Hanoi announced that it was ready to meet with U.S. representatives to begin preliminary negotiations. Talks did begin, but they were about "where to hold the talks, about protocol, about participation by South Vietnam and the NLF [Viet Cong], about seating and even the shape of the table."[11] The United States maintained its bombing halt above the 20th parallel, but the fighting continued, killing 14 000 Americans in 1968.[12]

As if to underscore the mindless suicidal tenor of the times, two calamitous assassinations occurred in quick succession. Less than a month after Johnson's speech, Martin Luther King was shot by escaped convict James Earl Ray. Riots exploded in cities and towns across the Nation. Heaviest hit was Washington D.C., which experienced 700 fires and 10 deaths. Then, in early June, after speaking out against violence,

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[7] James Trager, *The People's Chronology: A Year-by-Year Record of Human Events From Prehistory to the Present* (New York: Holt, Reinhart & Winston, 1979): 1119.

[8] Burns, *The Crosswinds of Freedom*: 411.

[9] Ibid., 412.

[10] Ibid., 413.

[11] Tuchman, *The March of Folly*: 448.

[12] Ibid.

Robert Kennedy was shot while leaving a Los Angeles hotel. As he took what was presumed to be the safer route through the kitchen, he was killed by a bullet from the pistol of a disaffected Jordanian-American, Sirhan Sirhan.

Later that summer at a stormy Democratic nominating convention, young war protestors fought pitched battles with the Chicago police. These demonstrators were perhaps more intent on disrupting the proceedings than on forcing the nomination of their chosen candidate, George McGovern. Despite their agitation, the moderately liberal Vice President Hubert Humphrey was nominated. The Republican candidate was Richard Nixon. Aided by the third-party candidacy of Alabama Governor George Wallace, who siphoned off almost 14 percent of the vote, Nixon won the election with 31.8 million votes as compared to Humphrey's 31.3 million. It was a meager victory, but Nixon was the new president. Vietnam was now his problem.

A Spate of Legislation

In comparing his feelings toward the Great Society with those for his inherited Vietnam problem, President Johnson told biographer Doris Kearns, "If I left the woman I really loved—the Great Society—in order to get involved with that bitch of a war on the other side of the world, then I would lose everything at home. All my programs. All my hopes to feed the hungry and shelter the homeless. All my dreams to provide education and medical care to the browns and the blacks and the lame and the poor."[13] Although he did become more and more deeply involved with "that bitch," Johnson did not entirely desert his beloved. During his administration, laws were passed that permanently changed the social and political landscape. Some of those laws also had a direct impact on the National Bureau of Standards.

In his first year, Johnson saw the Food Stamp Act of 1964 passed and he skillfully guided the epochal Civil Rights Act of 1964 through Congress followed by the equally important Voting Rights Act of 1965. In the same year, Congress passed a momentous group of Johnson's proposals—the Social Security Amendments of 1965 (Medicare), the Elementary and Secondary Education Act of 1965, the Higher Education Act of 1965, and the Housing and Urban Development Act of 1965—and also created the National Foundation on the Arts and the Humanities. The Child Nutrition Act of 1966 followed and later in 1968 the Handicapped Children's Early Education Assistance Act.

Throughout the Johnson administration environmental concerns were constantly addressed by periodic amendments and revisions of the "Clean Water" and "Clean Air" acts. The most important of these came in the 1965 with the Motor Vehicle Air Pollution Control Act, which gave the Government the authority to control emissions from motor vehicles.

In this spate of legislation there now arose a new theme that was to have important consequences for the Bureau's program. The theme was consumer protection. Of course, the Federal Government had a long history of protecting the economic and

[13] Johnson quoted in Kearns, *Lyndon Johnson*: 251.

physical safety of consumers. In 1872, Congress made it a crime to use the mails to defraud the public, and in 1887 the Interstate Commerce Commission was formed to regulate interstate carriers: railroads, barges, buses, and trucks. Most notable among this class of laws was the Pure Food Act passed in 1906 to regulate food, drugs, medicines and liquor. In support of this act, specific language in the Department of Agriculture's appropriations law allowed the Bureau of Animal Industry to regulate the cattle and other livestock industries. Catalyzed by the publication of Upton Sinclair's dramatic novel *The Jungle,* with its exposure of horribly unsanitary conditions in the Chicago stockyards, these laws were the first to protect consumer health.

However, between those early years and the New Deal years, there was little progress in consumer protection. In 1911, the Supreme Court ruled that the 1906 Pure Food Act did not prohibit false therapeutic claims. In 1912 Congress responded by passing the Sherley Amendment, which specifically forbade such deceptions. Other notable events during this period were the establishment in 1914 of the Federal Trade Commission (FTC), formed to prevent unfair or fraudulent trade practices, and the Food, Drug, and Insecticide Administration in 1927. Later known simply as the Food and Drug Administration (FDA), this agency took over the administration of the Pure Food Act from the Department of Agriculture's Bureau of Chemistry. During the New Deal years, under the advocacy of Assistant Secretary of Agriculture Rexford G. Tugwell, consumer protection in the sphere of food and drugs again became an issue. The main point of contention between manufacturers and advocates of consumer protection was the prohibition of false advertising. Consumer advocates wanted advertising held to the same standards as package labels. However, their legislative proposals went nowhere until jurisdiction over advertising claims was transferred from the FDA to the more cooperative FTC, and until 107 persons died tragically from taking a solution of sulfanilamide in the poisonous diethylene glycol, advertised as an "elixir." These two events set the stage for the passage of the Federal Food, Drug, and Cosmetic Act in 1938.[14]

Although there were two consumer protection laws passed in the early fifties—the Flammable Fabrics Act in 1953, and the refrigerator safety devices legislation in 1956 requiring safety devices on household refrigerators—new consumer-protection legislation did not arise as an important issue until the early sixties. Then, partly as the result of a more complex marketplace with new, unknown, and dangerous products, and partly, one can hypothesize, as the result of the emergence of an affluent society whose members had the leisure to be concerned with such issues, consumer protection became good politics. Thus, in 1962, President Kennedy sent to Congress a consumer message enunciating four "consumer rights": the right to safety, the right to choose, the right to be informed, and the right to be heard. This initiated a custom of yearly presidential consumer messages that lasted well into the Nixon years. Kennedy also formed the Consumer Advisory Council as part of the Council of Economic Advisors, composed of persons outside the Government. President Johnson made this council

[14] Mark V. Nadel, *The Politics of Consumer Protection* (Indianapolis: Bobbs-Merrill, 1971): 16-19.

part of the President's Committee on Consumer Interests, composed of high-level Government officials and designed to coordinate consumer issues in the Government. It was headed by a special assistant to the president, a newly created post first occupied by Esther Peterson. Consumer interests were now represented at the highest levels of the Executive Branch.

Consumer advocacy was not limited to the president. In the Congress, a number of members—all northern Democrats—also became advocates. Senators Philip Hart, Warren Magnuson, Joseph Montoya, Gaylord Nelson, Walter Mondale, Paul Douglas, William Proxmire, and Abraham Ribicoff, and House members Thomas Foley, Benjamin Rosenthal, Richard Ottinger, Harley Staggers, Leonor Sullivan, and Neil Smith all became champions of consumer causes.[15]

In 1964 legislation was passed that required vehicles purchased by the Government to pass certain passenger safety standards. However, in order to make consumer protection an important congressional issue, a "breakthrough" was needed. In a manner reminiscent of the events that occurred sixty years earlier with the publication of *The Jungle,* Ralph Nader catalyzed the Nation's interest in automobile safety with the publication of *Unsafe At Any Speed* in late 1965. Nader's book identified known design flaws in automobiles as the cause of traffic accidents. When it was learned that General Motors was investigating Nader's personal life, the book became a best seller. Both houses of Congress, which had been holding hearings on automobile safety, quickly joined forces and passed the important National Traffic and Motor Vehicle Safety Act of 1966.[16] The act gave the secretary of commerce authority to set minimum safety standards for automobiles, including standards for brake fluid and tires. On the same day, the Highway Safety Act of 1966 was also passed.

As if flood gates had been opened, a rash of new consumer safety legislation was passed in the next few years:

- The Department of Transportation Act in 1966 established the department, took over all transportation safety regulatory authority, and formed the National Transportation Safety Board, which was given authority to investigate all transportation accidents and report recommendations to the secretary of transportation. Its members would become fixtures in the investigation of aircraft accidents.

- The Clinical Laboratories Improvement Act of 1967 required that laboratories be licensed and operated in accordance with standards designed to assure consistent performance of accuracte laboratory procedures and services.

- The Natural Gas Pipeline Safety Act of 1968 provided for Federal safety standards for the transportation of natural and other gas by pipeline and for pipeline facilities.

- The Fire Research and Safety Act of 1968 amended the Organic Act of the National Bureau of Standards and authorized a fire research and safety program.

- The Child Protection Act of 1966 banned hazardous toys and articles intended for children, and other articles so hazardous as to be dangerous in the household.

[15] Ibid., 108-109.

[16] Ibid., 137-143; *National Traffic and Motor Vehicle Safety Act of 1966, U.S. Statutes at Large* 80, (1966): 718.

Ralph Nader, consumer advocate, critic of automobile safety standards, and author of *Unsafe At Any Speed,* sat motionless in Washington traffic in 1970. (AP-Wide World Photos)

- The Child Protection and Toy Safety Act of 1969 expanded the 1966 law and included mechanical, electrical, and thermal hazards.
- A Joint Resolution on November 20, 1967, established the National Commission on Product Safety.
- The Flammable Fabrics Act, amendment of 1967 amended and extended the Flammable Fabrics Act of 1953.
- The Radiation Control for Health and Safety Act of 1968 reduced the exposure of the public to all unnecessary hazardous radiation from electronic prodicts.

These laws showed a widening Government concern for consumer protection. Whereas previous legislation had been primarily concerned with foods, drugs, and cosmetics, the new laws were concerned with a wide variety of products, from toys and apparel to motor vehicles. And old areas were not overlooked. In 1967, the Wholesome Meat Act was passed, followed in 1968 with the Wholesome Poultry Products Act. Both stiffened existing laws.

Finally, the Government interest was expanded beyond safety to economic protection with the passage in 1966 of the Fair Packaging and Labeling Act and in 1968 of the Consumer Credit Protection Act, which required the disclosure by the lender of all costs associated with repayment of loans. While the total effect of these consumer laws did not match the momentous impact of the medicare and civil rights acts, they formed a notable part of the Great Society and initiated new duties for NBS.

Technological Triumph and Drama

While thousands watched in person and millions more watched via television, on Wednesday, July 16, 1969, at 9:32 a.m. EDT, Apollo 11 blasted off from Cape Kennedy. Carrying astronauts Neil A. Armstrong, Edwin E. Aldrin, Jr., and Michael Collins, Apollo 11 was launched in an attempt to achieve President Kennedy's goal of "landing a man on the moon and returning him safely to earth." After a flawless three-day journey, the command ship and its attached lunar module went into orbit around the moon. Eleven orbits and some twenty-two hours later, Armstrong and Aldrin crawled into the spidery lunar module. On the twelfth orbit they separated the lunar module from the command/service modules. The astronauts made another orbit in the lunar module and then began their descent to the Sea of Tranquility. At 4:17 p.m. EDT on Sunday, July 20, 1969, Armstrong announced to mission control, "Houston, Tranquility Base here. The Eagle has landed." Six hours later, while millions throughout the world stared captivated at the somewhat fuzzy television images, Armstrong descended the lunar module's ladder, stepped onto the powdery surface of the moon, and uttered the now-famous words, "That's one small step for man, one giant leap for mankind." Twenty-one and a half hours later, after emplacement of a U.S. flag and scientific equipment, the astronauts blasted off the Moon. Docking with the orbiting command/service modules was readily accomplished, and two and a half hours later a course was set for home. Splashdown took place on July 24 at 12:50 p.m. EDT—eight days and three and a half hours after launching. It seemed too easy.

The Rise of Relevance

When President Johnson took office, science was in its glory years. Funds for research were plentiful and science stood high in the eyes of policy makers and the general public. Between 1956 and 1966, helped of course by the sputniks and the "space race," Federal Government funds for research and development, grew from $17.3 billion to $40 billion in constant 1982 dollars. This whopping increase meant an annual average growth rate over those years of 9.8 percent. While not increasing as rapidly, industrial support for R&D over the same period nearly doubled—from $11.9 billion to $21 billion, for an annual growth rate of 6.5 percent. The total funding, which included university and non-profit funds, reflected these figures, increasing from $29.8 billion to $62.6 billion, or an annual growth rate of 8.6 percent. The figures for basic research are even more dramatic. For the same period, Federal Government support of basic research showed an average growth rate of 18 percent. Industrial support of basic research grew by an annual average of 5.5 percent. It was the continuation of a trend started at the end of World War II. Everything seemed rosy for science and technology.

However, the trend did not last long. In the next ten years things changed drastically. Between 1966 and 1976, Federal Government R&D expenditures (in constant dollars) *decreased* from $40 billion to $31.8 billion, yielding a growth rate over those years of *negative* 2.5 percent. While not quite so drastic, total U.S. funding also dropped. Industrial funding, while not dropping, slowed significantly to an annual growth rate of 3.3 percent. Funding specifically earmarked for basic research mirrored these results.[17] It was as if the brakes had been suddenly applied to a speeding truck. What happened?

Public support for science dwindled during the decade 1966-1976 for a complex set of reasons. A review of the decade, however, suggests that three forces dominated the crunch on Federal support of science and technology. One issue was the enormous financial drain of the Vietnam War. A second was a gradual fading in the public perception of science as a panacea for social ills. A third force could be characterized as "show me that your science has a rapid payoff."

Since the end of World War II, before things changed in 1966-1967, science had been in ascendancy. It was generally viewed as a benevolent activity, very helpful if not crucial to the social and economic well-being of the Nation. Furthermore, according to a commonly accepted belief the key to the productivity of science was basic research. Basic research, it was thought, would provide the "advances that would sustain the pace of inventions and applications."[18] But there could be a long lead time between basic advances and the resulting benefits, and few institutions could make such a long-term investment. The one organization with sufficient resources and time was the Federal Government. Hence the Government needed to be the principal supporter of basic research. And the Government had its own needs for science, particularly in the military and in space, so it would also carry applied research and development. These basic research, applied research, and development activities were viewed as more or less automatically leading to new products and better lives for us all.

Who was to decide what basic research to carry out? The prevalent view was expressed in 1959 by a White House panel on high energy physics:

> It is not possible to assign relative priorities to various fields of science. Each science, at any given time, faces a critical set of problems that require solutions for continued growth. Sometimes these solutions can be acquired at little cost; sometimes larger expenditures of funds are needed. Hence the cost may not reflect the relative value but rather the need. Each area must be funded according to these needs.[19]

And what is to determine the size of the research budget? Speaking before Congress in 1964, Lee A. DuBridge stated, "It is adequate when, and only when, every competent research scholar in our universities is finding adequate support for the research program he is able to carry out."[20]

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[17] *National Patterns of R&D Resources: 1987* (Washington, D.C.: National Science Foundation, 1988). The changes are more startling than described above. Funding generally rose until 1967, then dropped to a minimum in 1975. The figures for those years makes the changes seem more dramatic than for the equal ten-year periods chosen, but do not affect any conclusions.

[18] Bruce L. R. Smith, *American Science Policy Since World War II* (Washington, D.C.: The Brookings Institution, 1990): 36.

[19] D. S. Greenberg, "Money for Research: LBJ's Advisers Urge Scientists to Seek Public Support," *Science* 156 (May 19, 1967): 920.

[20] DuBridge quoted in Greenberg, "Money for Research": 920-921.

When Johnson assumed office in 1963, this "bottoms up" model in which new advances welled up out of the laboratories of researchers pursuing their own scientific ends, controlled only by peer pressure and peer review, seemed to have worked quite well. The Nation was the unquestioned technological leader of the world; its industrial productivity far surpassed that of any other country.

With the expansion of the Vietnam War came austerity and divisions within the society at large that carried severe consequences for the scientific community. The cost of the war was an enormous drain on the Treasury. Faculty and student protests against ROTC units and military research on campuses led to a split between liberals and conservatives in the scientific community. In-house dissent was matched by the layman's disenchantment with science. Books like Rachael Carson's *Silent Spring* and Ralph Nader's *Unsafe At Any Speed* convinced many that science did not automatically lead to beneficial results.

These changes had a profound effect on President Johnson. He was reputed to have "cursed the 'draft dodgers' who hide in graduate school while seeking advanced science degrees . . . and [to have] 'hit the roof' when George B. Kistiakowsky, who served as science advisor to former President Eisenhower, severed his long-standing advisory ties with the Defense Department in protest over Vietman [*sic*]."[21] A politician to the core, the president could not understand the attitude of scientists who took Government funds for research and at the same time criticized the policies of his administration. He had problems with which he needed help. He had a war to fight and a Great Society to build in the midst of burning cities, protest demonstrations, and campus unrest. Why could not science be directed to solve these problems? He made his feelings clear when, on October 15, 1966, speaking at the dedication of the Institute of Medicine, he stated, "A great deal of research has been done. . . . But I think the time has now come to zero in on the targets to get our knowledge fully applied. . . . Presidents, in my judgement, need to show more interest in what the specific results of medical research are during their lifetimes, and during their administrations."[22]

The new position of science was forcefully articulated by Donald F. Hornig, who had been appointed science advisor by Kennedy and served throughout the whole Johnson Administration. Speaking before the American Physical Society on April 26, 1967, he said:

> The scientific community is going to have to learn to articulate its hopes, to describe the opportunities which are before us for practical advance, to express the excitement of the new intellectual thrusts—but to do these in terms which the American people, who are expected to pay the bill, will generally understand and have faith in. There is no alternative.[23]

[21] Philip M. Boffey, "The Hornig Years: Did LBJ Neglect His Science Adviser?" *Science* 163 (January 31, 1969): 454.

[22] Johnson quoted in Smith, *American Science Policy*: 75.

[23] "Hornig on Research Policy: Public Understanding Essential to Scientific Progress," *Science* 156 (May 5, 1967): 629.

Donald F. Hornig was appointed science advisor to President Kennedy and remained in that position throughout the Johnson Administration. In this photograph (1966), Hornig (left) reported to President Johnson on his recent visit to Europe. The Europeans had acknowledged a "technological gap" between that continent and the United States. (UPI/Corbis-Bettmann)

While implying no decreased interest in basic research, Hornig continued, "We are determined that the knowledge and understanding we have gained from science will be put to use to meet the needs of our people and the world as expeditiously as possible. . . . To this end the Federal Government supplies research and development funds where the results are technically feasible and economically or socially worthwhile."[24]

Two years later, DuBridge, appointed science advisor by President Nixon, had much the same message:

> The day is past when scientists and other scholars can sit quietly in their ivory towers unaware of and unconcerned with the world outside their laboratories. Science is now a part of society, is a part of politics, is a part of the social and economic system. Scientists must carefully ponder the relevance of their work to the problems of human beings, and they must ponder the ways in which this relevance can be clearly explained to the public at large. . . . The members of Congress are apparently not convinced that the continued growth and virility of basic science in this country is essential to the national interest, and to the national welfare."[25]

[24] Ibid.

[25] DuBridge quoted in Robert Reinhold, "Scientists Urged to Stay Relevant," *New York Times*, November 2, 1969.

It was not only the administration that determined the new course for science funding. The Congress took action as well. In complementary bills, Congressman Emilio Q. Daddario and Senator Edward M. Kennedy introduced legislation to revise the charter of the National Science Foundation. The two bills were combined and signed into law by President Johnson on July 8, 1968. The most important new authority given to the NSF was to conduct applied as well as basic research. The crucial phrases read, "the Foundation is authorized to initiate and support scientific research, including applied research, at academic and other nonprofit institutions. When so directed by the President, the Foundation is further authorized to support, through other appropriate organizations, applied scientific research relevant to national problems involving the public interest."[26] In response to this, the Foundation formed the Interdisciplinary Research Relevant to the Problems of our Society Program in 1969. A new era of scientific relevance had begun.

In the midst of these tribulations, new and important advances were made in all the fields of basic science and in technology as well. In astronomy, for example, Arno A. Penzias and Robert W. Wilson discovered background radiation that had filled the universe since about 300 000 years after the "big bang," when matter and radiation were decoupled. Now cool, the radiation corresponded to a temperature of about 3 K, as predicted by theory. Penzias and Wilson's discovery occurred in 1964. Two years later, Raymond Davis, Jr., opened a "neutrino observatory" deep in South Dakota's Homestake gold mine. With a telescope that consisted of a 100 000 gallon tank of perchloroethylene, the instrument was designed to detect neutrinos by their conversion of chlorine-37 to argon-37.

In biology, Charles T. Caskey, Richard E. Marshall, and Marshall Nirenberg showed in 1967 that "identical forms of messenger RNA are used to produce the same amino acids in bacteria, guinea pigs, and toads, suggesting that the genetic code is a universal system used by all life forms."[27]

Two massive installations were established to study particle physics. On the West Coast, the Stanford Linear Accelerator Center went into operation in 1965, and in Batavia, Illinois, the Fermi National Accelerator Laboratory was established in 1969. In theoretical particle physics, Steven Weinberg, Abdus Salam, and Sheldon Glashow independently showed how two forces, the weak and the electromagnetic, could be unified. Called the electroweak theory, it offered insight into the fundamental laws of nature.

The premier technological accomplishments came as part of the space race. Step by carefully planned step, the United States worked its way to Apollo 11 and the moon landing. While never landing a man on the moon, the Soviet Union preceded the United States in some of the forerunner experiments. Both nations sent several space probes to Venus, and the United States sent two Mariner probes close to Mars. All were examples of technological virtuosity.

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[26] *National Science Foundation Act of 1950, amendments, U.S. Statutes at Large,* 82 (1968): 361.

[27] Alexander Hellemans and Bryan Bunch, *The Timetables of Science* (New York: Simon and Schuster, 1988): 554.

Yet while these space spectaculars were dramatic and newsworthy, other space advances were of more immediate use to the public. In 1965, an Early Bird satellite went into synchronous orbit around the earth, and real time communication between all points on earth was at hand. Then an Environmental Science Services Administration (ESSA) weather satellite went into polar orbit so that all parts of the globe could be observed. Weather patterns and their movements were to become a constant feature of television news broadcasts and their prediction of severe weather conditions were to add considerably to public safety.

In 1969 a development occurred that strikingly extended the capabilities of the normal research laboratory. In that year the scanning electron microscope became a practical laboratory instrument. A new dimension was added to the views of the microscopic world.

TOWARD A NEW LEADERSHIP

As its institute structure was put in place in early 1964, NBS was in the middle of a period in which its direct appropriations grew substantially. The Bureau had also significially increased the amount of basic research it was performing and, most importantly, had enriched its staff with new, vigorous scientist-leaders. Now it could look forward to busy years for the remainder of the decade with an impressive array of tasks before it. It had to learn how to manage a new organization and to develop it into a smoothly functioning entity. It had to complete the construction of its new home in Gaithersburg and the relocation of all of its Washington laboratories. Since the formation of the institutes, it had already acquired a new set of functions and would acquire more. In the introduction to the 1965 Annual Report, Director Astin labelled the Bureau "An evolving institution." He wrote:

> The exponential growth of U.S. scientific and technological activity has increased the Bureau's workload in measurement and related fields many fold. At the same time, several new responsibilities have recently been assigned to it.
>
> Among these are:
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> - To serve as the focal point within the Federal Government for stimulating the application of science and technology to the economy. . . .
> - To set up and operate the National Standard Reference Data System. . . .
> - To establish and expand a Clearinghouse for Federal Scientific and Technical Information. . . .
> - To set up and operate a central technical analysis service to conduct cost-benefit studies for our own, and other Commerce bureaus and Federal agencies on request.
> - To establish a central and major Government resource in the automatic data processing field. . . . [28]

[28] Annual Report for 1965: 2.

In combination with its traditional activities, these new functions—all dictated within the Executive Branch and some by the Bureau itself—would have filled the Bureau's plate to overflowing. But then, from the Legislative Branch came a whole new set of mandates—mostly concerned with public safety—that would sorely strap the Bureau's abilities to carry out their demands. These were the National Traffic and Motor Vehicle Safety Act of 1966, the Fair Packaging and Labeling Act, the Fire Research and Safety Act of 1968 (a law sought by the Bureau), the Flammable Fabrics Act, amendments, the Standard Reference Data Act (also sought by the Bureau), the Metric System Study, the Federal Property and Administration Services Act of 1949, amendment (the Brooks Act) in 1965 to include automatic data processing equipment, and the Radiation Control for Health and Safety Act of 1968. The Bureau, which since 1901 had basically lived with one piece of authorizing legislation, was now involved in carrying out the dictates of eight new laws.

During the period covered in this chapter, the Bureau decreased in size. In 1965 the Central Radio Propagation Laboratory, a part of the Bureau for twenty years, was transferred to the Environmental Science Services Administration, although the CRPL continued to occupy its quarters in Boulder. By the end of the period, all of the old-guard leaders, who had been with NBS since before World War II and had guided it through those trying war and post-war years, had retired from the Bureau. They were replaced by a new, equally dynamic set of younger leaders. It was truly a time of evolution for the institution.

Budget, Personnel, and Management Matters

In the years 1966 to 1976, Federal research and development budgets showed a reversal from the healthy increases experienced in the immediate post-sputnik years. A peak in overall Federal R&D expenditures occurred in 1966-1967, and funding (in constant dollars) then decreased until 1975. This was not the case for the Congressional appropriation for NBS. While not increasing at the high rates of the early sputnik years, its appropriation continued to increase in the period 1964 to 1970. However, there is more to this increase than meets the eye.

Total Congressional funds to NBS, in constant 1972 dollars, increased for the period, except in 1966. The decrease in 1966 was not a per capita decrease because it was occasioned by the divestiture of the CRPL. Figures for an analysis of the Bureau's support are shown in Appendix A, Table 3.[29] The table gives four columns of figures for the years 1961-1970. The first column is the Research and Technical Services (RTS) appropriation in current dollars. This is the appropriation allowed by the Congress for the Bureau's base program. It does not include such items as the special foreign currency program, the short-lived Civilian Industrial Technology (CIT) program, or other special programs.

[29] These figures are taken from the Annual Reports.

It also does not include income produced by the sale of Standard Reference Materials (SRMs) or from calibration charges. The second column lists the same figures in constant 1972 dollars.[30] The third and fourth columns give other appropriated and non-appropriated funds available to the Bureau and do include special foreign currency, CIT, Federal agency and non-federal funded research and development, calibration, and SRM income, etc., in current and constant 1972 dollars, respectively.

All the columns show a precipitous drop between 1965 and 1966, but this decrease is illusory and has only to do with the divestiture of the CRPL. While total Federal funds for research were showing a decrease, the Bureau was receiving an increase in real inflation-adjusted dollars, at least for inflation as measured by the GNP.

That the story is made somewhat more complex by the employment figures for the period as shown in Appendix A, Table 4.[31] The five columns shown are: Full-Time Permanent (FTP); Other Staff which includes post doctoral associates, summer help, part-time employees, temporary employees, and consultants; Total Paid Staff, which includes FTP and Other Staff; Research Associates and Guest Workers; and Total NBS Staff. In 1965 total employment reached 4793, approximately the same as the Bureau's previous high achieved during the Korean War before the divestiture of the ordnance work. The present high also occurred just before a divestiture, but in this case, the total personnel decrease was slightly over 650 as compared to the 2000 of the ordnance divestiture.

Most interesting is the number of FTP employees—the core of the Bureau's staff. After showing a rise of 525 between 1962 and 1965, followed by a drop of 433 upon the divestiture, there was a steady decline of about 200 employees over the next five years, while other paid and non-paid workers showed small increases. That this should happen when the Bureau appropriations were increasing at slightly over the inflation rate implies that the inflation rate for science was greater than in the general economy. However, it was during this period that, in order to decrease spending in the Government, controls were placed periodically on hiring, total permanent employment, average grades, and expenditures such as travel. These controls affected mainly the size of the full-time permanent staff and came at a time when the Bureau was receiving a host of new responsibilities. It was not a good trend for long-range institutional health.[32]

* * *

[30] Data for the conversion to constant 1972 dollars are from the *Economic Report of the President Transmitted to the Congress,* February 1998.

[31] These figures are taken from the Annual Reports.

[32] Three documents among many others illustrate the nature of these controls: (1) Memorandum, A. V. Astin to Institute Directors, Associate Director for Administration, and Manager, Boulder Labs, "Moratorium on appointments and promotions to the GS-14 and higher grade levels," December 19, 1964. (NIST RHA; Director's Office; Box 381; Folder 11/1/64–12/31/64); (2) Memorandum, I. C. Schoonover to Institute Directors, Associate Directors, Division and Office Chiefs, "Freeze on Employment," October 4, 1966. (NIST RHA; Director's Office; Box 380; Folder Chrono File, Sept. & Oct. 1966) This memo begins: "As most of you know the President has directed agencies to hold employment in full-time permanent positions for the remainder of fiscal year 1967 to a level at or below that prevailing as of July 31, 1966." (3) Memorandum, A. V. Astin to J. H. Hollomon, "Employment Versus Ceilings," January 15, 1968. (NIST RHA; Director's Office; Box 386; Folder Chrono File 1/1/68–2/28/68)

When the institutes were formed in 1964, Irl C. Schoonover became deputy director of the Bureau, Robert D. Huntoon became director of the Institute for Basic Standards (IBS), Donald A. Schon became director of the Institute for Applied Technology (IAT), and C. Gordon Little remained head of CRPL. The Institute for Materials Research (IMR), however, had no director, although Schoonover acted as one. But Bureau management had a plan and a candidate. The plan was to bring in a senior person, well-established and well-known in the materials community, as director for a stay of a few years to get the new institute established and recognized. The candidate was Gordon K. Teal, at the time assistant vice president and international technical director of the semiconductor electronics giant Texas Instruments. Teal had been with Texas Instruments for eleven years and had built up a strong research capability, producing the first commercial silicon transistor and a chemical reduction process for the production of ultra-pure silicon. He had come to Texas Instruments from Bell Telephone Laboratories where he had produced the first high-quality single crystal germanium for use in transistors and, with Morgan Sparks and William Shockley, developed the junction transistor.[33] Astin and Patrick E. Haggerty, president of Texas Instruments, exchanged correspondence, and on December 12, 1964, Teal was appointed director of IMR.[34] Teal stayed three years and, during his tenure, built up a cohesive and renowned group. He also brought back Howard E. Sorrows from Texas Instruments as his special assistant. Sorrows, who began his career at NBS in 1941, would have great influence on Bureau management.

Bringing the new Bureau organization under control was to prove more taxing than the hiring of Gordon Teal had been but was to bring to the surface a cadre of new, young leaders. In due course, they would take over the leadership of the Bureau. A long series of personnel moves began when, in the spring of 1966, Schon announced his intention to resign his position as director of IAT effective July 1, to head a new non-profit organization that would study innovation.[35] John P. Eberhard, who had been IAT deputy director, was elevated to director, and Lawrence M. Kushner, another of the young Bureau leaders, left his position as chief of the Metallurgy Division to become Eberhard's deputy. With the departure of Teal at the end of 1967, another duo of young leaders arose. On January 2, 1968, Huntoon was reassigned from his position as director of IBS to chief of the Office of Program Development and Evaluation.[36] Ernest Ambler, of parity fame, was appointed director of IBS, and polymer scientist

[33] National Bureau of Standards press release, December 12, 1964. (NIST History Project File; Chapter 5; Folder Leaders)

[34] Letter, A. V. Astin to P. E. Haggerty, July 22, 1964. (NIST RHA; Director's Office; Box 381; Folder 5/64–8/64)

[35] Memorandum, A. V. Astin to J. H. Hollomon, "Director of the Institute for Applied Technology," March 30, 1966. (NIST RHA; Director's Office; Box 380; Folder Chrono 3-1-66 to 4-30-66)

[36] Memorandum, A. V. Astin to J. F. Kincaid, "Reassignment of Dr. Robert D. Huntoon from the position of Director, Institute for Basic Standards, GS-18, to the position of Chief, Office for Program Development and Evaluation, GS-18," January 2, 1968. (NIST RHA; Director's Office; Box 386; Folder Chrono 1-1-68 to 2-28-68)

John D. Hoffman was appointed director of IMR. Then, to complete the moves, Eberhard announced his resignation effective May 1, 1968, at which time Kushner took over his position.[37]

Preceding both these moves was an important one indeed. In 1967 J. Herbert Hollomon resigned his position as assistant secretary of commerce for science and technology to assume the post of president of the University of Oklahoma. He had stayed for more than five years and had brought significant changes to the Bureau. Hollomon was succeeded by the more relaxed John F. Kincaid, vice president for research and development of the International Minerals and Chemicals Corporation.

Assistant Secretary of Commerce for Science and Technology J. Herbert Hollomon, and wife, Margaret Hollomon (left), chatted with Representative Carl Albert (right) and Commerce Secretary Alexander B. Trowbridge at a reception honoring the departing Hollomons. (copyright Washington Post; reprinted by permission of D.C. Public Library)

[37] Memorandum, A. V. Astin to J. F. Kincaid, "Appointment to Directorships of the Institute for Basic Standards, the Institute for Materials Research, and the Institute for Applied Technology," January 19, 1968. (NIST RHA; Director's Officel; Box 386; Folder Chrono File 1-1-68 to 2-28-68)

Then came the departure of two of the Bureau's grand old leaders. The senior scientist, Robert D. Huntoon, who had come to the Bureau in 1941, retired on July 30, 1968, and Irl C. Schoonover, who had come in 1933 and was now deputy director, retired in January 1969. Kushner was appointed deputy director of the Bureau, and Howard Sorrows was made acting director of IAT. At the end of the moves, all the major management positions at the Bureau, except that of the director and that of associate director for administration, Robert S. Walleigh's position, had been assumed by new, young leaders who had spent all or most of their careers at the Bureau. Kushner was deputy director of the Bureau; Ambler was director of IBS; Hoffman was director of IMR; and Sorrows was acting director of IAT. The stage was set for the final act: Astin's retirement on August 31, 1969. For this, Lewis M. Branscomb waited off-stage in Boulder.

For an organization in which tenure in senior positions had been measured in decades rather than years, these moves seemed tumultuous. In reality, they were caused by the resignation of two IAT directors and the aging of two senior leaders, Schoonover and Huntoon. Nor were these personnel moves the only ones. Of particular importance was the Boulder situation. All the Boulder divisions except those in the CRPL were placed in IBS, and Bascom W. Birmingham was named deputy director of IBS for Boulder. He was also named executive officer for the Boulder Laboratories, with the authority to "plan and supervise the administrative programs required to support the Bureau's scientific and technical program at Boulder" and to "act as the personal representative of the Director in all matters at Boulder requiring a spokesman."[38]

Not only was the period characterized by personnel moves. New types of organizational units—offices and centers—were formed as the need arose, and later, a new form of management called matrix management was initiated. Despite these changes, division chiefs were still the interface between upper management and laboratory scientists, and it was difficult for them to keep up with the changes.

* * *

The enactment of the Civil Rights Act of 1964 threw the efforts to ensure equal opportunity throughout the whole Government into high gear, and the Bureau was no laggard. However, the first few years after the passage of the act were more a time of organizing for what would be a long campaign rather than actual accomplishment of major changes. The Bureau did not act autonomously; it had to coordinate its activities with those of the Department of Commerce (DOC) and the White House, and had to be consistent with the law. Thus, in late 1965, Astin, in response to Secretary Connor's request for the Bureau's plan for expanding EEO, announced his intention to form a two-man Office of Equal Opportunity. This office would report to Astin's assistant, George E. Auman, who would be named equal opportunity coordinator.[39] In the same memorandum, Astin announced a plan to appoint a qualified minority person as chief

[38] "Birmingham Appointed Executive Officer at Boulder," *Technical News Bulletin* 52 (March 1968): 53.

[39] Memorandum, A. V. Astin to J. T. Connor, "NBS plan for expanding equal employment opportunities," December 15, 1965. (NIST RHA; Director's Office; Box 381; Folder Nov. 1–Dec. 31, '65)

of one of the administrative divisions and pointed out that the situation with the technical divisions was far more complex because of the scarcity of trained African-American scientists and engineers. However, to "ensure affirmative action," he promised to evaluate the "potential value of increased training" for each minority person in grades GS-5 and above.

Until October 1967, Auman carried all the EEO functions. At that time, new appointments were made. In particular, Donald G. Fletcher was appointed deputy equal employment opportunity officer, a post required by Executive Order.[40] Now things began to get more systematic. In early 1968, Astin asked the Bureau's personnel officer to form an advisory committee composed of minorities and women which would meet with him quarterly to review progress in hiring.[41] This was the first Bureau EEO Committee. Shortly thereafter, in a memorandum to all employees, Astin established a contact person for anyone who had "experienced or observed acts of discrimination," and the procedures to be followed for responding to discrimination occurring outside the Bureau.[42] In early 1969, the EEO Committee, which had advised the personnel officer, was changed. The new nine-member committee was placed under the chairmanship of chemist Avery T. Horton. The committee advised top management directly "concerning programs which must be undertaken to make equal employment a fulfillment rather than a promise." At the same time an affirmative action plan covering "recruitment, training, information dissemination, skills utilization surveys, and all aspects of the incentive awards program" was adopted. Progress in execution would be monitored by the EEO Committee.[43] The Bureau finally had an EEO structure in place, but progress was slow.

Not only was the Bureau concerned with EEO within its confines, it also had to be concerned with possible discrimination in the surrounding community. In 1966, Astin and James A. Shannon, director of the National Institutes of Health, sent a firmly worded letter to Kathryn E. Biggs, President of the Montgomery County Council, to make her aware of their "concern over the need for more aggressive and positive action to make suitable housing available to all members of our staffs. . . . " The letter

[40] Memorandum, R. S. Walleigh to D. R. Baldwin, "Designations of Responsibility for Equal Opportunity Programs," October 12, 1967. (NIST RHA; Director's Office; Box 386; Folder Chrono Sept. 1 to Oct. 31, 1967)

[41] Memorandum, A. V. Astin to Deputy Director, Associate Director for Administration, Institute Directors, Director Center for Radiation Research, Division Chiefs, "Reaffirmation of Equal Employment Opportunity Policy and Practices," May 16, 1968. (NIST RHA; Director's Office; Box 386; Folder Chrono File May 1 Through June 30, 1968)

[42] For incidents within the Bureau, the contacts were Donald G. Fletcher and Karl E. Bell, both blacks, and Robert F. Bain. For incidents outside the Bureau, a written complaint was to be lodged with the executive secretary of the County Commission on Human Relations. Memorandum, A. V. Astin to All Employees, "Equal Opportunity," July 12, 1968. (NIST RHA; Director's Office; Box 386; Folder Chrono File 7/1/68 thru 8/31/68)

[43] "NBS Moves to Insure Equal Opportunity For All Employees," *NBS Standard* 14 (June 1969): 1.

continued,

> It is essential that we be able to assure prospective employees that they and their families will have full and equal opportunity to participate in the economic, social, educational and cultural life of our community, State and Nation. It is equally imperative that such assurance be afforded our present employees, so that we may retain both quantity and quality of personnel. Their employability must not be hindered by customs, traditions, and practices which would deny them the opportunity to enjoy the natural benefits of their earnings. . . . [44]

By mid-1968 Montgomery County, Maryland, had a Public Accommodations Ordinance applicable to all places of public accommodation to ensure their use without discrimination.

THE PLANNING-PROGRAMMING-BUDGETING SYSTEM

At a news conference on August 2, 1965, President Johnson told of a new administrative system about to be installed in the Federal Government.

> This morning," he said, "I have just concluded a breakfast meeting with the Cabinet and with the heads of Federal agencies and I am asking each of them to immediately begin to introduce a very new and very revolutionary system of planning and programming the budgeting throughout the vast Federal Government, so that through the tools of modern management the full promise of a finer life can be brought to every American at the lowest possible cost.[45]

What the President was talking about was installing in the Federal Government a Planning-Programming-Budgeting (PPB) system. Generally thought to have been born in the Department of Defense in the early sixties, PPB had, in fact, a much longer history.[46] It was an activity intended to coordinate the planning of agency programs with the budgeting necessary to achieve the objectives of those programs. Installed in every agency, it was expected to give the President, through his Bureau of the Budget (BOB), greater input into the budgeting process. It was to have a profound effect on Government management systems.

Two months later, BOB issued to all agencies Bulletin 66-3, which explained what had to be done to install the new system.[47] It set target dates, culminating on May 1, 1966, when the system would be essentially installed. Managers in the Federal Government hurriedly began attempting to implement the system.

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[44] A draft of this letter dated 9-1-66 is attached to: Memorandum, A. V. Astin to J. H. Hollomon, "Issuance of Joint NIH-NBS Statement on Open Housing," September 1, 1966. (NIST RHA; Director's Office; Box 380; Folder Chrono File Sept. & Oct. 1966)

[45] Johnson quoted in "The Origin and History of Program Budgeting," in *Program Budgeting: Program Analysis and the Federal Budget,* David Novick, ed. (Cambridge: Harvard University Press, 1967): xv.

[46] Novick, *The Origin and History of Program Budgeting,* xvi-xxi.

[47] Executive Office of the President, Bureau of the Budget, *Bulletin No. 66-3,* October 12, 1965. Revisions were made in 1967 (Bulletins 68-2, 68-3) and in 1968 (Bulletin 68-9).

Essential to the system was a program structure which reflected each agency's objectives. Included in this structure was an analysis of alternative objectives and alternative programs which could meet those objectives. An analysis would compare the costs and the benefits of the alternative programs.

It was clear to the Bureau that this new system required a major staff effort. In 1966, the Bureau opened an Office of Program Development and Evaluation, but it remained vacant for some time. Most of the work was apparently carried out by the institute offices, with Astin's long-time associate George Auman the focal point in the director's office. In 1967, Huntoon took over the Office of Program Development and Evaluation in an acting capacity, joined in 1968 by Merril B. Wallenstein and Robert E. Ferguson. By 1969 Ferguson became Astin's special assistant for program planning. A program office of this kind would continue to exist far into the future—even after it was no longer directly associated with PPB—and would have a major impact on the Bureau's management operations.

The development of a program structure to describe the Bureau's work was not accomplished immediately. All-day meetings were held among Astin and his top leaders. Proposed program categories and program issues were communicated to Assistant Secretary Hollomon.[48] By April 1, the deadline for submitting the program financial plan, the Bureau's programs had been segregated into three PPB categories: "advancement of industry and commerce" with three subcategories, "basic measurement system" with four subcategories, and "general administration and special services," with two subcategories.

In June 1966, Astin sent Hollomon a memorandum to be used in briefing the secretary on the FY 1968 budget—the first budget developed using the PPB system.[49] The Bureau's base RTS appropriation was initially broken up among the three PPB categories given above, but eventually a program structure containing just two categories—the physical measurement system and general administration and special services—was agreed upon and used for the preparation of a PPB analysis.[50] It proved difficult to apply the principles of the PPB system to NBS. Despite considerable effort, the system seemed overly complicated and not very helpful to management.

The PPB system as a Government-wide activity disappeared in the early 70s, but it left an important legacy. The agencies of the Government had set in place a structure for the justification of new and existing programs, and this was not easily abandoned. In science, it was now the age of relevance, and agency programs had to be justified on the basis of their value to the agency's customers, using such techniques as cost-benefit analysis for these justifications. The Bureau itself had now set up a Program Office to assist the director in deciding which of the programs sponsored by the institutes, centers, and divisions should be pursued. Gone were the days when a

[48] A. V. Astin to J. H. Hollomon, "Revision of NBS Planning-Programming-Budgeting Categories," January 21, 1966; A. V. Astin to R. D. Huntoon, Teal, Schon, "Definition of NBS Program Category Structure," February 24, 1966. (NIST RHA; Director's Office; Box 380; Folder Chrono 1-1-66 to 2-28-66)

[49] A. V. Astin to J. H. Hollomon, "Briefing Memorandum," June 7, 1966. (NIST RHA; Director's Office; Box 380; Folder Chrono May 1 to June 30, 1966)

[50] A. V. Astin to J. H. Hollomon, "Program Memorandum and Program Financial Plan for Commerce Program Category V, The Physical Measurement System," June 12, 1967. (NIST RHA; Director's Office; Box 386; Folder Chrono May 1, 1967–June 30, 1967)

division chief in conference with the director decided what to do in the forthcoming year—science and technology had grown too big and too costly for such a manner of operating. While the PPB system proved unworkable, it was perhaps inevitable that some type of program analysis should have arisen.

The Gaithersburg Relocation

By the end of World War II, the Bureau facilities at the Van Ness site were on the verge of becoming totally inadequate. Already considered for major rehabilitation in the mid-thirties,[51] the physical plant had suffered greatly from overuse and lack of

Left: Demolition of the Industrial Building at the old NBS campus. (copyright Washington Post; reprinted by permission of D.C. Public Library)

Below: All that remains of the Bureau in Washington, D.C. is the Newton apple tree, a direct descendent of the tree said to have inspired Newton's first throughts on the law of universal gravitation. On March 15, 1957, the NBS tree was appropriately planted beside the East Building where the force of gravity was soon re-measured with modern precision.

[51] 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*, 84th Cong., 2d sess., National Bureau of Standards, 20 March 1956: 139. This testimony is an almost verbatim repeat of: Memorandum, A.V. Astin to George T. Moore, "Policy Considerations, Relocation of the National Bureau of Standards," July 14, 1955. (NARA; RG 127; Astin Files; Box 35; Folder Gaithersburg Site and Relocation)

proper maintenance during the war. Indeed, William I. Ellenberger, newly appointed Plant Division chief shortly after the war, declared the facilities a "sordid mess." Not only did the buildings suffer from years of lack of proper care, but records of what had been built and the location of power, steam, water, and electrical lines had been lost.[52] In the early fifties, $2 million were spent on "extraordinary maintenance to rehabilitate the electrical system," and by 1955 it was already showing inadequacies.[53] By 1956 the Bureau was completing a $1.3 million program to rehabilitate the mechanical systems, but it was anticipated that because of their age the systems would require additional large expenditures to keep them in "minimum operating conditions."[54] Moreover, in its more than fifty years, the Bureau had grown manyfold, but the physical plant to accommodate all of its added responsibilities had grown haphazardly. In 1955, there were eighty-nine buildings at the Van Ness site, of which fifty-three were temporary structures. The average age of the permanent buildings was thirty years, and the temporaries had already largely exceeded their life expectancies. With this diversity of buildings, closely related research units were widely separated. The average division was scattered in eight buildings. Lacking the stimulation brought about by close association of related groups, the conglomerate was inefficient and managing it was a constant headache. Worse, what in 1901 had been a rural location had by the fifties become an urban one. A residential area of the city now surrounded the Bureau. Noise, electrical disturbances, air pollution, and the dangers in this setting associated with the potentially hazardous nature of some its own operations constrained the Bureau's freedom. A new rural setting was needed.[55] It is small wonder that in 1953 the second recommendation of the first Kelly Committee was the modernization of the physical plant.[56]

Added to these considerations was the newly formulated policy of dispersal. With the Van Ness site only 3.6 air miles from the White House, NBS was extremely vulnerable to atomic attack. In addition to the potential for loss of life, there was the possible loss of the national standards. Astin estimated that it would require more than ten years to replace the Bureau's laboratories and technical personnel if they were to be lost.[57] Thus in mid-1955 when James Worthy, assistant secretary of commerce for administration and a strong Bureau supporter in the AD-X2 affair, told Astin that the Bureau had been chosen as a possible agency for relocation and asked if he were interested, Astin answered "yes" with alacrity. Worthy asked Astin to obtain an estimate on how much the relocation would cost. There were only about two weeks left for the

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[52] MFP, 503.

[53] Appropriations Hearings for 1957: 134.

[54] Ibid.

[55] Ibid., 135.

[56] *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: 19.

[57] Appropriations Hearings for 1957: 135.

preparation of the President's budget for FY 1957, so the Bureau asked the General Services Administration (GSA) for a rough estimate of the cost of such a relocation.[58] In retrospect, asking this of the GSA was a mistake that was to cause considerable discomfiture for the Bureau.[59] The GSA had plenty of experience with office buildings but little or none with laboratories. Their first estimate was for $40 million, a figure which proved to be woefully and embarrassingly low.

Before any move could be contemplated the Bureau had to decide where it wanted to move. Bureau management was very conscious of the fact that the move of the CRPL to Boulder caused a loss of approximately 50 percent of the relevant senior staff and they wanted to make this new move as convenient for the employees as possible.[60] To decide what to do, a study was made of the residential distribution of the work force. The study found that the center of gravity of the whole staff was the Van Ness site itself, but for the professionals the center was farther out Connecticut Avenue, at Chevy Chase Circle on the Maryland-District of Columbia boundary. It was therefore decided that the move would be northwest into Maryland, as close as possible to the District of Columbia consistent with a rural location and with the dictates of dispersal. A distance of twenty miles from the center of Washington was deemed sufficient for purposes of dispersal. A move into the Washington-Baltimore corridor might have provided a rural setting, but this was ruled out by the administration as being a prime target area.[61]

It was estimated that a site of 500 acres was necessary to maintain isolation and allow for long-term expansion. Fortunately, as part of its own relocation to Germantown, Maryland, the Atomic Energy Commission (AEC) had recently had a number of sites investigated by the Public Buildings Service (PBS). For this agency, the Corps of Engineers had prepared a Site Investigation Report. Astin requested and received a copy of this report, and the Bureau investigated several of the listed sites. Besides the location and the size, other considerations were topography, accessibility by road and railroad (a requirement later removed), and cost. Aerial and topographic maps were studied by senior Bureau staff and several possibilities selected.[62]

The relocation plan was placed before the Congress on March 20, 1956, at the House Appropriations Committee Hearings for the FY 1957 budget. This was just nine months after Assistant Secretary Worthy had broached the subject with Astin. The figures on the cost of the relocation and the submission, prepared in July of the previous year, were greatly underestimated.

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[58] A. V. Astin Oral History, July 12, 1983, p. 35.

[59] R. S. Walleigh, who took over direction of the relocation, was to state, "I think the biggest mistake we ever made in connection with the Gaithersburg planning was bringing GSA into it at all and if I had it to do over again I certainly would rather have kept them out of it." Astin Oral History: 44.

[60] Ibid., 39.

[61] Ibid., 40-41.

[62] "Summary of Files on Gaithersburg," 2.00: 1-2. (NIST Historical File). Prepared by the NBS Management Planning Division in May 1958, this document gives a summary of many of the files on Gaithersburg up to 1958. Not all of the original files cited in the summary were investigated for this history.

Now a reason for quick action arose. When the Senate reported on the FY 1957 appropriations bill, Senator Carl T. Hayden, chairman of the Appropriations Committee, wrote to Secretary Weeks on May 23, 1956, asking that a specific site be identified.[63] Thus, on May 24, 1956, Associate Director for Administration Robert S. Walleigh took Astin to see what he considered to be the best of the favored candidates. It was near the small, sleepy town of Gaithersburg, bounded by U.S. 240 (soon to become Interstate 70S and later renamed Interstate 270), State Highway 124, and Muddy Branch Road. It contained an estimated 575 acres of mostly open, quite level ground, and the area was relatively unpopulated. Proximity to tracks of the B&O railroad provided for the possibility of a spur (subsequently dropped because of cost) and also for commuter access via a station in Gaithersburg (again, never used). Astin chose the site, the selection was approved by Secretary Weeks, and the appropriations committees of both houses were notified.[64] The Bureau had taken the first tangible step toward finding a new home. It was to be a long road.

At that time, the Bureau was requesting $2.75 million for preliminaries including soil tests, the site survey, purchase of the land, preparation of plans and specifications, general expenses, and contingencies. As of yet, no money was being requested for construction.[65] The committee was willing to help but was concerned with the cost. Perhaps with tongue in cheek, Chairman Prince H. Preston said to Astin at the beginning of the session, "Doctor, you had better put on your top hat and your patent leather shoes now, and give us a good sales talk on this one."[66] Later, after some figures had been presented, the following exchange took place:

> MR. PRESTON: Naturally, our first reaction would be we feel it is a matter of national pride in having a splendid scientific laboratory set up for the Bureau of Standards, but at the same time, $50 million is a large sum.

At this point, Congressman Albert Thomas of Texas uttered the prophetic and remarkably accurate words: "It will probably be 85 to 100 before you get through." Astin then returned to the Bureau's plans, "Mr. Chairman, I felt in terms of our responsibility, we should make some long-range plans for our program. . . . If they are shocking, then we are shocked, too."[67]

What the GSA had developed in response to Astin's original request was a plan which provided 1 million square feet of space (as compared to the Van Ness site's 840 000 square feet) in a six-story-plus-penthouse square building—the least expensive

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[63] Ibid.

[64] Ibid., 3.

[65] Appropriations Hearings for 1957: 134-136.

[66] Ibid., 137.

[67] Ibid., 141. The reader will recall that this was a little more than a year and a half before the sputniks.

form of construction.[68] In addition to the cost of this building, there was $750 000 for the land and further costs for a powerhouse, radioactivity building, nine miscellaneous buildings, underground vaults, parking, a cafeteria, protection equipment, roads, walks, a fence, a guardhouse, and street lights. Costs associated with design and construction supervision brought GSA's total to a round $40 million.[69]

But these could not be the only costs associated with the relocation. There would be costs for equipage and moving. The GSA could not estimate these; that was a Bureau responsibility. Astin allowed, "We don't have any estimates on those. Those would come out of the study, but I don't know whether my budget officer would want to make a wild guess. . . . " Budget Officer Wilbur W. Bolton did not hesitate. "Without regard to special facilities that might be added to our building, our experience has been that equipage and moving costs may run 50 percent of the structure cost."[70] What the Bureau was asking for was $2.75 million to begin the process of construction of facilities whose cost, arrived at partly by a suspect estimate and partly by a wild guess, was about $60 million. The House voted no funds.

The Senate was more lenient. It voted to grant $930 000 and the House concurred. This amount was transferred to the GSA "[f]or acquisition of necessary land and to initiate the design of the facilities to be constructed thereon for the National Bureau of Standards outside of the District of Columbia to remain available until expended. . . . "[71] The Bureau relocation effort was haltingly underway, but at least it was started.

With the appropriation in hand, the Bureau's management went into high gear. Nicholas E. Golovin, Astin's associate director for planning, was placed in charge of the relocation effort. Golovin worked closely with Robert Walleigh, who took over the assignment in May 1958. Two main problems were identified: the acquisition of the site, and the design of the laboratories. Two committees were formed. A Gaithersburg Planning Group with eight persons under Hylton Graham, chief of the Plant Division, provided liaison between the technical staff and the architects, and a Laboratory Planning Committee composed of outstanding younger scientists provided direct input from the scientific staff.[72]

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[68] On July 7, 1955, the Bureau advised Fred S. Poorman of the GSA "that a wing-type structure similar to the Boulder Laboratory is desirable. Poorman seemed inclined towards a block-type structure with few windows, a high lighting level and air conditioning." "Summary of Files on Gaithersburg," 3.21: 3-4.

[69] Appropriations Hearings for 1957: 136.

[70] Ibid., 139.

[71] *Department of Commerce and Related Agencies Appropriation Act, 1957, U.S. Statutes at Large,* 70 (1956): 321.

[72] Lewis M. Branscomb was chairman of the committee until he resigned in June 1959. John D. Hoffman replaced him. The other members of the committee were Herbert P. Broida, Alan D. Franklin, F. Ralph Kotter, Lawrence M. Kushner, and Leo A. Wall. (Memorandum, L. M. Branscomb to A. V. Astin, "Laboratory Planning Committee," June 19, 1959. Memorandum, A. V. Astin to J. D. Hoffman, "Chairmanship of the NBS Laboratory Planning Committee," August 31, 1959. (NARA; RG 167; Astin File; Box 35; Folder Gaithersburg Site and Relocation, 1955-61))

Robert S. Walleigh began his career at NBS in 1943 as an electrical engineer in the Ordnance Development Division. Walleigh's talent for management was recognized, and after promotion to assistant chief for administration of the Ordnance Division, in 1955 Walleigh was appointed associate director for administration of the Bureau as a whole. In the latter position, Walleigh expertly directed the planning, construction, and move of the Bureau's faoilities from the District of Columbia to Gaithersburg, Maryland.

Having selected the site and received the appropriation, NBS left to the Public Buildings Service the actual mechanics of acquiring the land from its owners. Condemnation was the procedure followed to obtain the property. It was Astin's desire to file a declaration of taking as soon after July 1, 1956, as possible.[73] A survey was begun on July 18, and the completed boundary and topographic surveys were completed on September 13, 1956.[74] The declaration of taking was accomplished, and though the final price was not agreed upon, $325 000 was deposited with the court. In January 1957, a number of appraisals had been made, and the land was valued at an average of $850 per acre.[75] With money appropriated piecemeal by Congress, the final cost of the land was $574 000, although this included twelve more acres needed to provide extra space around the reactor building, as required by the AEC. The final size of the site was 560 acres.[76]

[73] "Summary on Files of Gaithersburg," 2.00: 4.

[74] Ibid., 6.

[75] Ibid., 8, 10.

[76] House Committee on Appropriations, Subcommittee on Departments of State, Justice, Commerce, the Judiciary, and Related Agencies Appropriations, *Departments of State, Justice, Commerce, the Judiciary, and relatd Agencies Appropriations for 1967: Hearings Before a Subcommittee of the Committee on Appropriations,* 89th Cong., 2d sess., National Bureau of Standards, 14 March 1966: 711. The size was provided by the Plant Division in June 1992.

Of great immediate interest to the Bureau staff was the style of the whole emplacement and the facilities to be provided in the laboratories. In late 1956, the architectural firm of Voorhees, Walker, Smith, and Smith, a firm with experience in the design of laboratories, was retained for this preliminary phase. Input from the staff came from three sources: an attitude survey conducted in late 1956, the Laboratory Planning Committee and, of course, from the division chiefs.

The attitude survey showed that a campus-like atmosphere with more than one building was important in attracting scientists and engineers.[77] In fact, in a letter to the mayor of Gaithersburg on October 16, 1956, Walleigh wrote,

> The Bureau wishes to develop on its new site a university-campus-type atmosphere similar to the one which has been achieved on the present site. It has been found that such surroundings are an asset in attracting and retaining scientists and in producing the environment which stimulates scientific productivity.[78]

A little more than a year later, the Laboratory Planning Committee wrote Golovin with the same sentiments, but the committee was not sure that such an atmosphere required a multi-building structure.

While it was a little early to decide completely the matter of services, the GSA recorded the following NBS requirements for all general-purpose labs: compressed air, vacuum, natural gas, oxygen, 110 volt and 208 volt alternating current, direct current, and other special voltages and currents in some cases.[79] Chemistry labs would in addition receive distilled water. And, as an important corollary to these services, it was urged by the Laboratory Planning Committee that the construction should be such that laboratories could be easily expanded at minimum cost. Other requirements communicated to the architects were that the property should be completely fenced, that a railroad spur to the site should be planned, and that while blast protection was not necessary, there should be personnel bomb shelter areas.

The architects visited Boulder since they had been told that it had the general character that the Bureau would want in the new site. Finally in December 1956 Astin and Golovin, meeting with the architects, agreed:

1. That probably a number of structures would be preferable to one or two large buildings.
2. That the general motif would be austere but austerity would not be carried to the point where additional costs would be involved.
3. The general atmosphere of the facilities would be that of a college campus and perhaps the general style of structure would be wing-type with adjacent parking areas. There will be many entrances rather than one or two.[80]

The last was a remarkably apt description of the site that resulted.

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[77] "Summary of Files on Gaithersburg," 3.21: 1.

[78] Ibid.

[79] The general services finally distributed were cold water, hot water, laboratory waste drain, chilled water (55 °F), burner gas, compressed air, vacuum (20 in to 22 in mercury), steam (15 lbs), 120 V AC, 208 V AC single and 3 phase, and standard frequencies.

[80] "Summary of Files on Gaithersburg," 3.21: 5-6.

In January 1957, Astin, Golovin, Walleigh, and the architects visited laboratories at DuPont, Bell Telephone, Argonne, Midwest Research Institute, and Lincoln Laboratories. This trip seemed to have reinforced their concept, which was reflected in the interim plan that was presented to the House Appropriations Committee in the hearings for the FY 1958 budget. Gone was the monolithic rectangular parallelepiped, now replaced by four wing-like structures, one housing administration and the other three providing laboratories for chemistry, physics, and engineering. The construction would be of the modern modular type with movable partitions that would allow flexibility and convertibility in space configuration.

At these hearings, the Bureau requested $2 million "to undertake the design and specifications for most of the construction program."[81] Astin gave a good sales talk, stating, "I feel strongly that the fulfillment of these plans will mark a major turning point in the history of the National Bureau of Standards." He followed with a remarkable statement which articulated the aim of his administration as well as his feelings about what had happened to the Bureau during and after World War II: "I believe the laboratories now contemplated, when completed, will help to raise the National Bureau of Standards to the stature which it had in world science before World War II."[82]

Astin, however, had a serious problem. The rough estimate made earlier had increased alarmingly. What was once $63.5[83] million had jumped to $85.81 million. Most of the increase was in the buildings, which had jumped $17.7 million—from $33.627 to $51.325 million. Interestingly, Bolton's "wild guess" of $20 million for equipage had increased by only $0.4 million. When increases for site development, utilities, a railroad spur, and a number of smaller increases were added, the total increase was $21.68 million or just about one-third of the original estimate.[84] It turned out that the bulk of the increase was in the ratio of usable or assignable area of the buildings to the total area. The GSA had used a ratio of 70 percent which was typical for the kind of buildings (offices, courthouses) that it was familiar with. Visits to other laboratories and questionnaires sent by the architect to twenty-seven firms specializing in designing laboratories showed that for this type of facility, a ratio of 50 percent to 55 percent was more the norm because of the space needed to provide services to the laboratory areas. With an increase of 70 000 square feet of assignable area in Bureau requirements, this lower ratio added 555 000 square feet of space.[85]

[81] House Committee on Appropriations, Subcommittee on Department of Commerce and Related Agencies, *Department of Commerce and Related Agencies Appropriations for 1958: Hearings Before a Subcommittee of the Committee on Appropriations,* 85th Cong., 1st sess., National Bureau of Standards, 12 March 1957: 199.

[82] Ibid.

[83] The original $60 million estimate ($40 million for site acquisition and construction plus $20 million for equipage and relocation) settled down to $63.5 million which included funds for reinstalling equipment and site development.

[84] Appropriations Hearings for 1958: 208.

[85] Ibid., 206, 208-210.

The committee was not pleased. Fred S. Poorman of the PBS was firmly admonished, and Chairman Preston did not spare Astin: "I am fearful, doctor, since you got the green light on the purchase of the land you may have modified your plans and made them more elaborate than you had in mind when you appeared before us last year." Assured that the only major increase was the 70 000 feet of space, Preston nevertheless continued, "I am not sure if we had known it was going to cost $85 million we would have approved the $930 000 last year. This is an amazing increase."[86] It is possible that without Astin's reputation for rectitude developed during the AD-X2 affair and his subsequent appearances before the Appropriations Committee, the committee might have considered the low estimate to be a subterfuge to get money appropriated and the project underway. The Congress did not appropriate the requested $2 million, but at the same time, it did not veto the project. It merely postponed it.

Now began a fallow period in the appropriations area.[87] Redesign of the basic laboratory and other savings, such as cancellation of the railroad spur, reduced the estimate to $82 million, and the Bureau asked that the FY 1959 budget include a request for design and construction funds. The president, however, did not request any such funds. Nevertheless a supplemental appropriation request for $3 million was allowed, and the funds were appropriated. The Bureau could now begin serious design work.

During 1958, with the flight of the sputniks, two new pieces of equipment—a linear accelerator and a 1 000 000 pound dead weight machine—had become paramount. The Bureau requested that construction funds be sought in the 1960 budget, particularly for the critical Radiation Physics and Engineering Mechanics Laboratories. This request was disapproved because of the president's "no new construction starts" policy. Nevertheless, work did not completely stop. An architectural design contract was negotiated with Voorhees, et al., and detailed architectural work was begun.

Finally, five years after the relocation project was initiated, construction funds were included in the FY 1961 budget request. Ironically, the first funds were not for the general purpose laboratories, but for the Radiation Physics Laboratory and the Engineering Mechanics Laboratory, facilities which had not even been considered at the beginning of the relocation project. Along with funds for these facilities were requests for the boiler plant, initial site development, and utilities. The total request was for $23.5 million, with $9.27 million for the Radiation Physics Laboratory and

[86] Ibid., 202.

[87] The chronology given here is from: Memorandum, A. V. Astin to the Secretary of Commerce [F. H. Mueller], "Planned relocation of NBS laboratories at Gaithersburg, Maryland," Feb. 2, 1961. (NARA, RG 167, Astin File, Box 35, Folder Gaithersburg Site and Relocation 1955-1961.) The chronology is printed verbatim in House Committee on Appropriations, Subcommittee on General Government Matters, Department of Commerce, and Related Agencies, *General Government Matters, Department of Commerce, and Related Agencies Appropriations for 1962: Hearings Before a Subcommittee of the Committee on Appropriations*, 87th Cong., 1st sess., National Bureau of Standards, 3 May 1961: 879.

$6.49 million for the Engineering Mechanics Laboratory. The remainder was for the more general work.[88]

Part of the FY 1961 budget request also included funds for a nuclear reactor. This request was for $9.1 million,[89] but it was part of the Plant and Facilities request, not the Gaithersburg relocation. This caused no little amount of confusion. The reasoning was that while the radiation and mechanics laboratories were replacements of facilities already existing at the Van Ness site, the reactor was a completely new facility with no counterpart at the old site. Undeniably, the Bureau maneuvered to keep down the cost of the Gaithersburg move.

In the meantime, design of the new facilities proceeded apace. In June 1960, just six months after the FY 1961 hearings, the architects produced a model of the proposed facilities. On June 1 the model was presented to the associate directors and the following day to the press. Pictures appeared in the local papers on June 3.[90]

By now the total estimated cost of the Gaithersburg relocation was $94 million. The new estimate included funds for the radiation and mechanics laboratories, some new funds for a cyclotron laboratory, and funds for a fallout shelter. The increase in costs due to inflation was not an inconsiderable part of the price rise. Indeed, economies had reduced the original $85 million to $81.58 million by 1959, but inflation had increased the estimate to $87.14. The $85 million estimate proved remarkably accurate. Only the addition of $9 million for the nuclear reactor increased it.

Congressman Elford A. Cederberg was not upset by the $94 million figure. "If you can do that you are doing reasonably well," he said. "I doubt if you can stay within that $94 million figure when you start to consider it cost almost $68 million for one office building here on the Hill. If you can do it I think you are doing well."[91]

Chastened by the success of the Soviet space effort, the Congress appropriated the full $23.5 million requested for FY 1961. Groundbreaking for the Engineering Mechanics Building took place on June 14, 1961. Secretary of Commerce Luther Hodges wielded the same gold-plated shovel that had been used in the groundbreaking ceremony for the Chemistry Building in 1915 at the old site. The log jam was broken, and both appropriations and construction progressed steadily.

The final design was very different from the original. If one counts buildings connected by corridors as wings, then in the administration/general-purpose-laboratory complex there were nine wings. By the time the new facilities were dedicated in

[88] House Committee on Appropriations, Subcommittee on Department of Commerce and Related Agencies, *Department of Commerce and Related Agencies Appropriations for 1961: Hearings Before a Subcommittee of the Committee on Appropriations*, 86th Cong., 2d sess., National Bureau of Standards, 14 January 1960: 286.

[89] The final cost was $8.85 million for the reactor and $490 000 for an isotope separator. Letter and attachments, A. V. Astin to J. H. Hollomon, February 8, 1965. (NIST RHA; Director's Office; Box 381; Folder 1/1/65–4/30/65)

[90] Robert S. Walleigh, "The Gaithersburg Site," in *NBS/NIST, A Historical Perspective, A Symposium in Celebration of NIST's Ninetieth Anniversary, March 4, 1991*, Karma A. Beal, ed., Natl. Inst. Stand. Technol. (U.S.) Special Publication 825; April 1992: 52.

[91] Appropriations Hearings for 1961: 304.

November 1966, the main administration/laboratory complex and three special-purpose laboratories—Engineering Mechanics, Radiation Physics, and the Reactor—were completed. Five more—Industrial, Non-Magnetic, Sound, Concreting Materials, and Hazards—were under construction.[92]

Besides laboratories, there were a number of service buildings all completed by 1964. A power plant with an adjacent electrical substation installed by the Potomac Electric Power Company and a special gas inlet station provided the power and heat services. A supply and plant building provided facilities for purchasing and maintenance, and the Bureau's motor vehicles and equipment were maintained in a garage service building. One of the wings of the administration-laboratory complex contained the instrument shops.

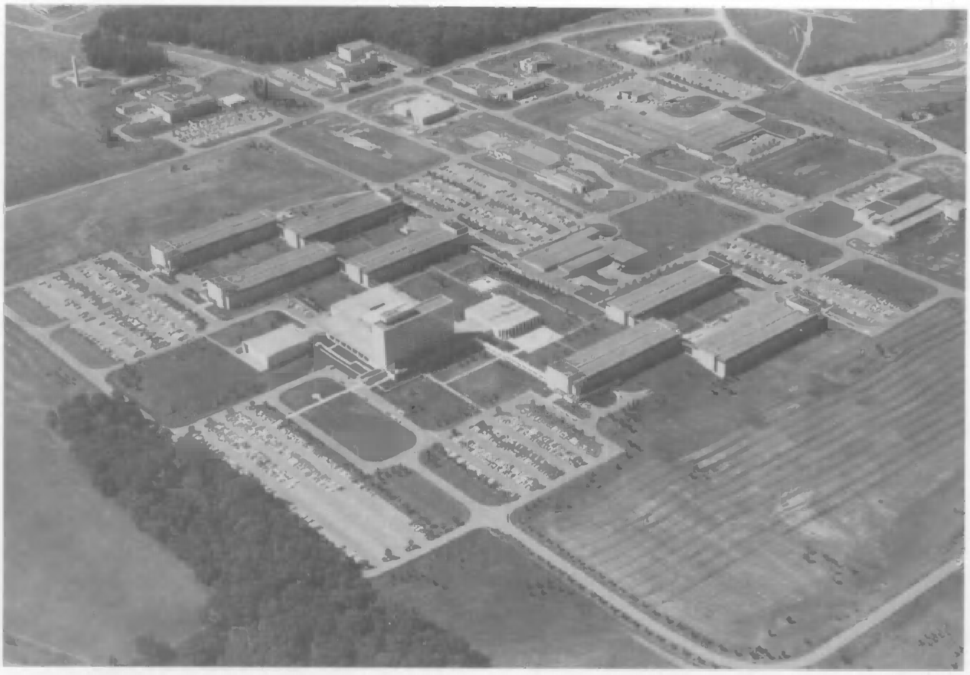
The focal point of the whole site was an eleven-story high-rise office building containing all the activities, administrative and otherwise, that required only office-type space. The director's office was located at the southwest corner of the eleventh floor, and the view from there was little short of spectacular. Attached to the high-rise building were the library, the cafeteria, a meeting complex containing four small auditoria-meeting rooms, the 289-seat Green Auditorium, the large 756-seat Red Auditorium, and the instrument shops. These wings were arranged to provide a

Eleven stories high, the Administration Building towers above the other buildings in the laboratory complex. The building houses the director and his staff as well as other activities that do not require laboratory space. The Red Auditorium to the left, and the Technology Building to the right, flank the Administration Building.

[92] Appendix J lists the Gaithersburg site buildings with the dates of construction, occupancy, and square footage for each building.

In 1948 a sundial of unusual design and high degree of accuracy was erected on the terrace east of the Chemistry Building by members of the NBS staff in honor of Lyman J. Briggs. In January 1966, R. Newton Mayall, designer of the sundial, advised the Bureau that moving the sundial to Gaithersburg would introduce an error of 0.002 feet, "a negligible amount except for the purist. The dial should be raised at an angle of 0.003 feet in 1 foot to correct for latitude. Again a negligible amount, except for the purist." The sundial was moved to its new location in the Administration Building courtyard.

The original gates of the Bureau's main entrance to the former site—Connecticut Avenue and Upton Street —with their background of trees and azaleas, became a symbol of the Bureau. The gates were relocated to the Gaithersburg campus on the occasion of the Bureau's 75th anniversary in 1976.



Aerial view of the Bureau's Gaithersburg campus taken August 12, 1969. The arrangement of buildings in a spacious, park-like setting had antecedents in forms such as the corporate research park, the American college campus, and the Bureau's former site in Washington, D.C.

Interior of the NBS Library showing the helical white terrazzo staircase to the mezzanine.

courtyard, delimited by the huge ground-level windows of the cafeteria on one side, and glass corridors around the remaining three sides.

The general-purpose laboratories were also wings of the Administration Building complex but further removed from it. A long, multi-level corridor on a north-south axis formed the main spine of the complex and connected the Metrology, Physics, Chemistry, and Materials Buildings with the western edge of the Administration Building. These buildings, each 96 ft. × 300 ft., alternately branched from this spine. A similar corridor on the west side of the laboratory buildings ran parallel to the main corridor and connected three more general-purpose laboratories (Polymers; Instrumentation, later renamed Technology; and Building Research) and the instrument shops with the west end of the Materials Building. The linear system made all buildings in the main complex easily accessible from all the others without creating a need to go outdoors. At the outside end of each of the attached buildings was a parking lot. All the other buildings on the site were similarly supplied with parking.

The general-purpose laboratories were the heart of the system. All rose three stories above the ground. Three (Technology, Physics, and Metrology) also had an underground level. Along the north and south sides of the buildings were windows. The rooms along these sides were basically designed to be offices, although they could be arranged for some light experimental work. Thus, each of the above-ground offices had a large window. All construction was modular, 11 ft. × 16 ft. for the basic office module and 11 ft. × 24 ft. for the laboratory module.[93] The office and laboratory modules could be lengthened in increments of 11 feet, and in addition, if a very large laboratory was required, the wall between the back-to-back laboratory modules could be removed to create a 48 foot wide space. A cross-section of the building showed an office module (16 feet), a corridor (8 feet), a laboratory module (24 feet), another laboratory module (24 feet), a corridor (8 feet), and another office module (16 feet). The design had the flexibility requested by the staff and the advantage that each scientist could have an office just across the corridor from his laboratory.

Because of its complexity the whole construction sequence was divided into four phases:[94]

Phase I: Engineering Mechanics, Power Plant, and initial site work.
Phase II: Radiation Physics, Administration, and service buildings.
Phase III: Seven general purpose laboratories,
Phase IV: Sound, Hazards, Industrial, and Concreting Materials.

A fifth phase was added for the Fluid Mechanics and Non-Magnetic Buildings, and a gate house. The schedule was followed very well and work was completed by 1970. The Reactor was not included in the phases listed above since it was not officially part of the Gaithersburg relocation.

[93] The 11 foot dimension was a compromise between the architects, who wanted 10 feet, and the Laboratory Planning Committee, which wanted 15 feet.

[94] House Committee on Appropriations, Subcommittee on Departments of State, Justice, and Commerce, The Judiciary, and Related Agencies, *Departments of State, Justice, and Commerce, The Judiciary, and Related Agencies Appropriations for 1964: Hearings Before a Subcommittee of the Committee on Appropriations,* 88th Cong., 1st sess., National Bureau of Standards, 28 February 1963: 1098.

On March 27, 1962, the first permanent employees moved to the site, forming a skeleton crew from the Plant Division. By October 1963, the steam and chilled water generation plant was continuously manned. In April 1963 work was begun on the Reactor Building and, a month later, the first scientific staff moved to Gaithersburg to supervise its construction. The building was completed in August 1965, but the reactor (NBSR) itself was finished in late 1967. This supervisory group of staff was followed in October by the Office of Weights and Measures and the Engineering Mechanics Section staff. Almost two years later, on September 13, 1965, the Administration Building was occupied,[95] and a month after that, the Radiation Physics Building staff and equipment completed a difficult move to their new building.

Logistically, the most difficult moves were those to the seven general purpose buildings. The complicated assignment of space to the various divisions was carried out by the Gaithersburg Planning Group. After conferences with the division chiefs, the planning group assigned space for the divisions' laboratory and office needs and relayed the requirements to the architects.[96] Then, on floor plans of the offices and laboratories assigned to the divisions, the location of each item of equipment and furniture was marked. The items themselves were tagged and coded to correspond to the locations on the plans. Books, files, and small pieces of equipment were placed in cartons and similarly marked. When the time arrived for a division's move, the division staff stayed home for as many days as necessary to complete the move. When staff reported to their new quarters in Gaithersburg, they found their desks, bookcases, and equipment where they had requested them to be placed. The system worked remarkably well.

[95] The American flag last flew at the Van Ness site on September 10, 1965. The same flag flew once at Gaithersburg two days later, and then was presented to Astin as a memento. Since Astin's office after that date was located at Gaithersburg, he wanted a flag to be raised at the site, but there was no flagpole. The GSA had not planned on constructing one until the completion of the site. Using the GSA plans, the Bureau let its own contract for the flagpole. The $44 768 contract called for a 90-foot stainless steel flagpole, the removal of a small building, the leveling and sodding of the site in front of the administration building, the provision of a granite walkway, and a circular granite base for the pole incised with George Washington's words, "Let us raise a standard to which the wise and honest can repair." Alas, at the next House Appropriations Committee hearing on April 19, 1967, Astin, not having been thoroughly briefed, did not know the details of the contract, and was under the impression that it included only a 90-foot pole. Thus, when Congressman Andrews of West Virginia declared that the cost of the flagpole was $500 a foot, Astin had no comeback and was embarrassed. Although the cost of the pole itself was $100 per foot, the $500-a-foot number stuck and Astin was constantly reminded of this by the acidulous committee chairman John Rooney. Added to the original woeful Gaithersburg cost underestimate, the incident was only partly humorous. (*NBS/NIST, A Historical Perspective*: 53)

[96] There was a definite hierarchy in space allocation. Institute directors and division chiefs were provided a double office module adjacent to a double module for secretarial and administrative staff, and beyond this a single module for an assistant division chief or lower aide, or a double module for the deputy institute director. Section chiefs had a single module adjacent to a secretary in a single module. Access to these officials was past a secretary. Scientists of grade GS-15 or above had private offices, but lower grades shared office space. There were also distinctions in furniture. Institute directors, their deputies, and division chiefs were allowed a couch and handsome wooden desks, credenzas, bookcases, and comfortable upholstered chairs. Section chiefs were permitted older wooden desks and other furniture, but grey steel was more the norm. But while these furnishing rules were stipulated, they were not followed slavishly. Window plants became common and often elaborate.

The move into the general-purpose laboratories began in March 1966 and was finished by the end of the summer. During the same period the library was moved, completing the bulk of the relocation. Still left to finish, however, were the Sound, Hazards, Industrial, Concreting Materials, Non-Magnetic, and Fluid Mechanics Buildings. All were completed in 1968 except the Fluid Mechanics Building which was finished in 1969.

Plans for the dedication of the Gaithersburg installation began in 1964.[97] By 1965, it was proposed that President Johnson be asked to dedicate the new facilities, and the Visiting Committee was asked to plan a special symposium. A letter inviting the president for the date of June 14, 1965, was prepared for the secretary's signature, but this plan fell through.[98] In a second attempt, the president was invited and the date planned for November 15, 1966. According to the agenda, Secretary John T. Connor would preside, and the formal ceremonies would be followed by a special luncheon. The next two days would feature a symposium on "Technology and World Trade," followed by a day of open house for the general public.[99] The dedication was held on the stipulated date in the courtyard facing the library. Speaking to some 3000 distinguished guests, Connor called the facilities "a blue-chip investment . . . which will pay dividends to American science, industry, and commerce." The president was unable to attend, but he did send a message, stating, "This eminent institution now has the resources for even greater service to America and the world."[100] The two-day symposium, attended by over 500 international dignitaries and leaders in industry, education, and commerce, went on as scheduled. On the fourth day of the festivities, 20 000 guests toured the facilities and visited the newly opened laboratories. The Bureau had formally dedicated a new home.

The total cost of the Gaithersburg relocation is somewhat difficult to estimate, for it is hard to know when to stop counting. The costs were reviewed at the FY 1967 House Appropriations Hearings. At the time of the dedication, funds had been appropriated for all four phases, but the fluid mechanics facility was excluded because the construction bids were higher than expected. The appropriated funds totaled $105.94 million. The Bureau asked for a final $1.2 million to cover moving and occupancy expenses, bringing the Gaithersburg relocation cost to $107.14 million. However, when $8.85 million for the reactor and $490 000[101] for the isotope separator were added, the total became $116.48 million. It was almost twice the $63.5 million estimated at the first hearing, but the Bureau now had a fine and completely adequate new home.

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[97] Memorandum, A. V. Astin to J. H. Hollomon, "Dedication Ceremony, Gaithersburg," July 14, 1964. (NIST RHA; Director's Office; Box 381; Folder 5/64–8/64)

[98] Memorandum, Assistant Secretary Hollomon to the Secretary, "Dedication of New Facilities for the National Bureau of Standards," prepared January 25, 1965; Letter, Secretary of Commerce to the President, prepared January 25, 1965. (NIST RHA; Director's Office; Box 381; Folder 1/1/65–4/30/65)

[99] Memorandum, I. C. Schoonover to J. H. Hollomon, "Dedication of NBS Gaithersburg," July 13, 1966; Letter, John T. Connor to the President, July 18, 1966. (NIST RHA; Director's Office; Box 381; Folder Chrono File, 1/1/65–4/30/65)

[100] "NBS Gaithersburg Dedicated Nov. 15; Symposium, Open House Held," *NBS Standard* 11(9) (December 1966): 1.

[101] Appropriations Hearings for 1967: 712-716.

It is the rare visitor who, being driven around the Gaithersburg site, does not remark on its beauty.[102] Indeed, with its 400 acres of well-mowed lawns; 67 acres of woods in two lots; two 4 acre ponds occupied by mallards, black ducks, and large flocks of Canada geese; and the hundreds of strategically placed and well-maintained trees and shrubs, the site has more the aspect of a park than a workplace. The numbers of trees and shrubs planted are impressive: approximately 1800 large deciduous trees of 38 varieties, 926 small and flowering trees of 32 varieties, 1548 coniferous trees of 9 varieties, and hundreds of shrubs. The azaleas and rhododendrons of the pre-1990s have been replaced with deer-resistant varieties of shrubs, plants, and ground covers. Included in the collection and planted as a grove between the two ponds, in 37 varieties, are the 53 officially designated trees of the states, the District of Columbia, Puerto Rico, and the Virgin Islands. Elsewhere on the grounds are individual specimen trees, such as a spectacular weeping beech which, along with other trees and shrubs, and flowers is planted in the courtyard near the cafeteria. Not counted in the collection, but planted in the large courtyard next to the library, is the Newton apple tree. This tree is reputed to be a direct descendant (via the British East Malling Research Station and the U.S. Beltsville Agricultural Research Station) of the tree that revolutionized physics by dropping one of its apples alongside the young Sir Isaac. Beneath it is a plaque bearing the inscription, SCIENCE HAS ITS TRADITIONS AS WELL AS ITS FRONTIERS.[103] Artfully planted to soften the otherwise austere facade of the buildings, the trees and shrubbery provide pleasing color from spring through fall.

Upon entering the Administration Building, the feeling of spacious aesthetic design is not lost. The large reception area is floored in black terrazzo, and its walls are white or black marble. When entering, one sees on the far wall an inscription taken from a May 14, 1900, House Committee report on the bill to establish the Bureau. In gold letters, incised into black marble, the quotation states:

> IT IS THEREFORE THE UNANIMOUS OPINION OF YOUR COMMITTEE THAT NO MORE ESSENTIAL AID COULD BE GIVEN TO MANUFACTURING, COMMERCE, THE MAKERS OF SCIENTIFIC APPARATUS, THE SCIENTIFIC WORK OF THE GOVERNMENT, OF SCHOOLS, COLLEGES AND UNIVERSITIES, THAN BY THE ESTABLISHMENT OF THE INSTITUTION PROPOSED BY THIS BILL.

It forms an impressive greeting.

Upholstered furniture is scattered throughout the reception area, and ample corridors lead to the auditoria, cafeteria, and library. With an immense glass wall on its north side, the cafeteria is bright and spacious, a great improvement over the one at the Van Ness site. Much use is made of wood paneling in the auditoria and on the rest of the ground floor of the Administration Building complex. Glass walls are abundant and provide a sense of spaciousness. The north wall of the library's main reading room is all glass, but the most striking architectural feature of the library is a helical white

[102] All the numbers given in this paragraph were provided by the Plant Division in 1992.

[103] One of its siblings, still fronted by a similar plaque, is the only remnant of the Bureau at the Van Ness site.

terrazzo staircase leading up to a mezzanine of stacks which look down on the reading room beneath them. Again, a sense of spaciousness is achieved. An employees' lounge was placed across the hall from the cafeteria and the Green Auditorium, and a slightly longer walk from the larger Red Auditorium. The lounge is used as a gathering place during official functions, such as meetings and symposia, and paintings of all the former directors hang in it. A corridor leads from the lounge to wood-panelled private dining rooms and to the senior lunch club. No longer serving a fixed menu boarding house style, the Gaithersburg club operates buffet style with an ample variety of food.

Left: A workman welds the framework of the apparatus that will carry the test exposure wall from the old site to Gaithersburg.

Below: The wall, built in 1948 to study the action of various weathering agents on structural materials, was moved intact on May 18, 1977. The wall contains 2059 samples of stone in the front face, and 293 in the back and ends; of these, 2032 are domestic stones supplied by 47 states, and 320 are foreign samples supplied by 16 countries. The wall is approximately 37 ft. 9 in. long, 12 ft. 10 in. high, 2 ft. thick at the bottom and 1 ft. at the top, and weighs 39.6 tons.

Environmental Science Services Administration

In September 1963, meteorologist Robert M. White came to the Department of Commerce as chief of the Weather Bureau from his position as president of the Research Center of the Travelers Insurance Company. Along with his duties as Weather Bureau chief, he became deeply concerned with "the problem of how we have organized our geophysical or environmental scientific, engineering, and service activities in this country, particularly in the Federal Government. Have we organized ourselves so that we can attack the problems of man's natural environment effectively? And will our present organizational forms prove adequate to the tasks of the years that lie ahead?"[104] With the assistance of meteorologist Edward S. Epstein, then a consultant to the assistant secretary of commerce for science and technology, White prepared a report for the assistant secretary, giving his views on the organizational problems in environmental science and the role of the Department of Commerce in that field. He presented arguments that since "there is an essential unity linking the environmental sciences. . . . [m]any benefits will accrue to the Federal Government and to the Nation at large by establishing a single organizational entity to conduct research and provide services dealing with man's total environment." Moreover, Commerce, with its Weather Bureau, its Coast and Geodetic Survey, and its CRPL at the Bureau, uniquely among Government agencies possessed the "experienced nucleus of scientific and technological capability including the necessary service apparatus covering the full spectrum of environmental sciences." He proposed that all the relevant units of the department be pulled together into a National Environmental Services Administration.[105]

White sent the proposal to Astin and Admiral H. Arnold Karo, director of the Coast and Geodetic Survey, for their comments. After receiving enthusiastic assurances of support from both, White sent the proposal to Assistant Secretary Hollomon with the recommendation that the secretary appoint a distinguished committee "for their comments and recommendation."[106] Hollomon, however, continued the process a little differently. He formed a three-person committee of Astin, Karo, and White, with White as Chair, to "review . . . the Environmental Scientific Activities of the Department of Commerce."[107] In the meantime, the assistant secretary kept the White House—particularly Science Advisor Donald Hornig—apprised of what the DOC was doing. On

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[104] Speech, Robert M. White, "The Organization of the Environmental Sciences in the Federal Government," April 23, 1964. (NIST RHA; Director's Office; Box 382; Folder ESSA 1964-1965.) White delivered this speech before a joint banquet of the American Meteorological Society and the American Geophysical Union.

[105] Memorandum, Chief, Weather Bureau to Assistant Secretary for Science and Technology, Department of Commerce, "Environmental Services in the Federal Government," January 24, 1964. (NIST RHA; Director's Office; Box 382; Folder ESSA 1964-1965)

[106] Ibid. The reader will recall that the Environmental Protection Agency was established in 1970.

[107] Memorandum, J. H. Hollomon to Director, National Bureau of Standards; Director, Coast and Geodetic Survey; Chief, Weather Bureau, "Review of the Environmental Scientific Activities of the Department of Commerce," May 18, 1964. (NIST RHA; Director's Office; Box 382; Folder ESSA 1964-1965)

January 15, 1965, the committee issued a report which had been reviewed by a distinguished advisory group. It showed no basic difference from the original White report except for a change in the name of the proposed agency to National Environmental Science Service and small changes in its structure.[108]

On July 13, 1965, Reorganization Plan No. 2 of 1965 became effective, and a new agency called the Environmental Science Services Administration, or ESSA, came into being.[109] On October 11, all 592 CRPL staff members were transferred to the new agency but remained in Boulder.[110] ESSA became a joint tenant with NBS in what became the U.S. Department of Commerce Boulder Laboratories. Within ESSA, the CRPL was renamed the Institute for Telecommunication Sciences and Aeronomy. None of the staff concerned with radio standards—who had never been part of CRPL—were transferred. However, fifteen members of the Bureau's Sound Section formed a Geoacoustics Group in the new institute, and they did move to a new location. The Bureau had lost, at least administratively, one of its elite units and valued members of another. Astin's reaction to this divestiture is not recorded. It can, however, be assumed that he probably felt some relief at the loss of the foremost of his special central responsibilities for which he had had difficulty in obtaining authorization.

A Number of New Responsibilities

New legislation gave the Bureau a number of additional responsibilities during the period. Most of these laws arose from heightened public concern for consumer product safety, but some of them responded to other considerations. A list of the laws that involved NBS is given in Appendix C. Though they were the most numerous, the safety-related laws were not the only ones that provided the Bureau with new responsibilities. There was the automatic data processing equipment legislation (the "Brooks Act"), the Metric System Study legislation, the Fair Packaging and Labeling Act, and the Standard Reference Data Act, which gave the Bureau's existing program a sound legal basis.

Mention should also be made of the 1967 Joint Resolution to Establish a National Commission on Product Safety, and the 26-page omnibus Consumer Product Safety Act of 1972. The first act mandated the formation of a temporary commission to conduct a study of product safety and write a report to the president and the Congress. The second act formed the Consumer Product Safety Commission. A far different body from the first commission, it was given complete regulatory responsibility for consumer product safety as spelled out in the act. The law reassigned responsibility for administering the Flammable Fabrics Act and the old refrigerator safety devices legislation,

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[108] Chief, U.S. Weather Bureau; Director, National Bureau of Standards; Director, U.S. Coast and Geodetic Survey, "Report of the Committee for Review of the Environmental Science and Service Activities of the Department of Commerce," January 15, 1965. (NIST RHA; Director's Office; Box 382; Folder ESSA 1964-1965)

[109] Appropriations Hearings for 1967: 490.

[110] Handwritten notes, "ESSA File," undated. (NIST RHA; Director's Office; Box 382; Folder ESSA 1964-1965)

thereby removing the burden of these two laws from the secretary of commerce and hence from NBS. Moreover, the act stipulated that, to the extent possible, the commission should use the "resources and facilities of the National Bureau of Standards, on a reimbursable basis." It was an important law for the Bureau.

The principal problem with these new responsibilities was finding the resources for them. With the Bureau appropriations rising at a mere 2 percent above the GNP inflation rate, expansion funds were scarce. Funds and people had to be taken from existing programs and placed on the new ones, a recourse to "reprogramming" that could only be partly successful. Astin, seeking money for NSRDS and ADP at the FY 1967 House Appropriations Hearings, complained of "enlarged responsibilities" that "have been added to the Bureau without a corresponding assignment of the resources to carry out those responsibilities. . . . "[111] At the 1968 hearings, he asserted, "[Belt tightening] does not provide enough. We have made available through this reprogramming process about $1 million. We need to carry out these responsibilities that have been assigned to us in excess of $7 million."[112] At the 1969 hearings, Astin protested:

> Let me emphasize that in the last few years especially, we have done everything possible to reprogram or to curtail programs—and this includes people—so as to transfer the available money to the highest priority ones. But reprogramming has been made very difficult because of the new responsibilities assigned to the Bureau in the past few years by both the administration and the Congress; namely standard reference data, fair packaging and labeling, automatic data processing, flammable fabrics, and fire research. However, requested increases for these new responsibilities were not fully granted. At the same time we have tried to keep abreast of the rapid technological advances requiring basic standards and data services. This reprogramming has seriously hurt our longer established programs. We have little or no flexibility left.[113]

The passage during 1969 of the Metric System Study would compound the problems pointed to by Astin. The glory days were over.

Automobile Safety

NBS had worked in the automotive field for many years with both Congress and groups representing various aspects of automotive development or safety coming to the Bureau for help with such problems as tire quality. Two public laws that predated the current period involved the Bureau in work on brake fluids and seat belts.

[111] Appropriations Hearings for 1967: 649.

[112] House Committee on Appropriations, Subcommittee on Departments of State, Justice, and Commerce, The Judiciary, and Related Agencies, *Departments of State, Justice, and Commerce, The Judiciary, and Related Agencies Appropriations for 1968: Hearings Before a Subcommittee of the Committee on Appropriations*, 90th Cong., 1st sess., National Bureau of Standards, 19 April 1967: 715.

[113] House Committee on Apropriations, Subcommittee on Departments of State, Justice, and Commerce, the Judiciary, and Related Agencies Appropriations, *Departments of State, Justice, and Commerce, the Judiciary, and Related Agencies Appropriations for 1969: Hearings Before a Subcommittee of the Committee on Appropriations*, 90th Cong., 2d sess., National Bureau of Standards, 28 March 1968: 1193.

Public Law 87-637 and Public Law 88-201 directed the secretary of commerce to prepare specifications for brake fluids and seat belts, respectively. In each case, the secretary of commerce turned to NBS for technical assistance. Working with outside groups, principally the Society of Automotive Engineers, the Bureau adopted and/or modified the existing standards. The first brake fluid standard was published on December 4, 1962, just three months after the enactment of the law, thereby complying with the 90-day deadline set by the legislation.[114] The standard was modified on July 31, 1963.[115] The properties standardized for the various types of brake fluids available were boiling point, flash point, viscosity, pH value, stability at high temperatures, corrosion, fluidity and appearance at low temperatures, evaporation, water tolerance, compatibility, resistance to oxidation, and the effect on rubber. Brake fluid was also required to pass a simulated service test.

The seat belt standard followed a similar course, but plans were made for it even before the law was passed.[116] Again, the plans were basically to adopt SAE standards and modify them as required. The law, enacted on December 13, 1963, required that standards be promulgated before one year had passed. On September 9, 1964, Secretary Hodges issued a "Notice of Proposed Rule Making for a Seat Belt Standard," where he announced, "Notice is hereby given that the standards for seat belts for use in motor vehicles as set forth in tentative form below are proposed to be prescribed and published as required under Public Law 88-201, approved December 13, 1963, on or before December 12, 1964."[117] On that date, having received comments, the secretary pointed out that the standards were essentially the same as existing SAE standards and that their purpose was "to provide the public with safe seat belts so that passenger injuries in motor vehicle accidents can be kept to a minimum" as the legislation required.[118]

The standards themselves consisted of a set of requirements and associated test procedures for all the components of seat belts for adults and children: the webbing (or strap), the hardware, and the assemblies. In both these cases, after the issuance of the first two versions of the standard, the secretary delegated to Astin the authority to "perform the functions vested in the Secretary of Commerce" under the appropriate law.[119] From then on, the notices in the *Federal Register* pertaining to the two laws were signed by Astin. While his authority was subject to policies and directives from both the secretary and the assistant secretary for science and technology, it appears that for the first time in its history, the Bureau's director was responsible for changes in mandatory standards.

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[114] *Federal Register* 27 (December 4, 1962): 11941-11943.

[115] *Federal Register* 28 (July 31, 1963): 7773-7775.

[116] Memorandum, A. V. Astin to J. H. Hollomon, "Plans for Developing the Seat Belt Standard Required by Public Law 88-201," January 9, 1964. (NIST RHA; Director's Office; Box 381; Folder Chrono 1/64–4/64)

[117] *Federal Register* 29 (September 9, 1964): 12736.

[118] Public Law 88-201 quoted in *Federal Register* 29 (December 11, 1964): 16973.

[119] *Federal Register* 29 (February 28, 1964): 2779-2780 and *Federal Register* 30 (April 24, 1965): 5802. Quote is contained in the second of these.

Far more important for the Bureau than this delegation of authority was the question of policing the marketplace. Both laws were silent on this issue; no responsibility was given to anyone, but neither was this activity forbidden. In a memorandum to Robert E. Giles, general counsel of DOC, Astin took up the issue. He wrote, "With respect to brake fluids, it was agreed that the National Bureau of Standards would (a) test brake fluids which were sent to it on complaint, and (b) see to it that samples were collected under standard procedures on some regular basis and then tested by NBS." He then stated that the Bureau would try to get the General Services Administration or the Federal Trade Commission to do the actual collection of brake fluid samples. The procedure for seat belts would be similar. Astin then summarized:

> The seat belt and brake fluids cases pose an issue of policy for the Department of Commerce. Both laws leave ambiguous the question of the agency responsible for inspection procedures. It is my view that the Department of Commerce should not attempt to put itself into the position of inspecting and policing industry, both because the Department is not equipped to perform these regulatory functions and because these functions are in conflict with the services to industry and the general relationship to industry that we now have and that we are attempting to promote. . . . Future laws such as the Automotive Tire Safety proposal should contain a section in which responsibility for inspection is made to reside with a regulatory agency such as FTC.[120]

With respect to these laws, the issues eventually resolved themselves. The National Traffic and Motor Vehicle Safety Act of 1966 (Public Law 89-563) specifically repealed the brake fluid and seat belt laws since the broader law made the old laws redundant. Under this law, the secretary of commerce was given the responsibility to "establish by order appropriate Federal motor vehicle safety standards. Each such . . . standard shall be practicable, shall meet the need for motor vehicle safety, and shall be stated in objective terms." Introduction into commerce of any vehicle not conforming to these standards was forbidden and punishable by fine or imprisonment. The law provided for a National Motor Vehicle Safety Advisory Council to guide the secretary.

Important for the Bureau, the secretary was directed to "conduct research, testing, development, and training" and was given the authority to make grants for this purpose. Title II of the law took up the difficult problem of tire safety, and Title III authorized the secretary to "make a complete investigation and study of the need for a facility or facilities to conduct research, development and testing in traffic safety. . . . " Finally, a 1960 act providing for a register in the Department of Commerce listing the names of persons who had their motor vehicle operator's licenses revoked was amended in Title IV to include in a National driver register each individual whose license had been denied, terminated, or temporarily withdrawn.

[120] Memorandum, A. V. Astin to R. E. Giles, "Inspection Procedures—Seat Belts and Other Safety Standards," September 24, 1964. (NIST RHA; Director's Office; Box 381; Folder 9/1/64–10/31/64)

Director Allen Astin had been prepared for the passage of Public Law 89-563. In September 1965 he had written to Assistant Secretary for Administration David R. Baldwin, "in anticipation of the passage of the Traffic Safety Act of 1966, we request approval to establish a Center for Vehicle Safety Standards, to report to the Institute for Applied Technology." A shortened version of its functions was, "conducts research, development, testing and evaluation directed at reducing the occurrence of automotive accidents and the deaths and injuries which result."[121]

With the actual passage of the National Traffic and Motor Vehicle Safety Act of 1966, definition of the Bureau's role became necessary. An internal memorandum of understanding between the Office of the Undersecretary for Transportation and the Bureau was drafted,[122] but before it could be agreed to, the Department of Transportation (DOT) was formed. All DOC activities in transportation were moved to the new department. In particular, the DOT act required the secretary of transportation to form a National Highway Safety Bureau (NHSB) to carry out the provisions of the 1966 act. The Bureau would have to consult with this new agency if it was to have a part in implementing the law.

It found its role quickly. William Haddon, Jr., M.D., who had moved from Commerce to Transportation to head the NHSB,[123] informed Astin that "certain tasks in the field of vehicle safety would be assigned to NBS." As a result, Astin again wrote to Baldwin asking that a new unit which he now called the Office of Auto Safety Research be formed in IAT.[124] This time Astin's request was granted, and the new unit was formed. Its name became the Office of Vehicle Systems Research (OVSR). In March 1967, Secretary of Commerce Alexander B. Trowbridge and Secretary of Transportation Alan S. Boyd signed an interagency agreement. The Bureau had a new organizational unit, which was completely supported by the Department of Transportation and which provided that agency continued technical support.

Organized and operated under the direction of Paul J. Brown, the office had programs in three areas: tires, occupant restraint systems, and braking systems. The main aim of the tire program was to develop a uniform quality grading system, which by law was to be in operation by 1968. This system was "one of the biggest challenges to the Safety Laboratory."[125] Rating tires on the basis of treadwear, traction, and temperature resistance, the system was opposed by the tire industry. The industry

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[121] Memorandum, A. V. Astin to D. R. Baldwin, "Amendment to Department Order No. 90-B as Amended," July 19, 1966. (NIST RHA; Director's Office; Box 380; Folder Chrono File; July & Aug. 1966)

[122] Memorandum, A. V. Astin to J. H. Hollomon, "Implementation of the National Traffic & Motor Vehicle Safety Act of 1966," September 16, 1966. (NIST RHA; Director's Office; Box 380; Folder Chrono File Sept & Oct)

[123] Called the National Traffic Safety Agency while it was in the Department of Commerce..

[124] Memorandum, A. V. Astin to D. R. Baldwin, "Amendment to Department Order No. 90-B as Amended," January 17, 1967. (NIST RHA; Director's Office; Box 386; Folder Chrono 1-1-67–2-28-67)

[125] Paul J. Brown, "Automotive Safety Laboratory," in *NBS/NIST, A Historical Perspective: A Symposium in Celebration of NIST's Ninetieth Anniversary, March 4, 1991*, Karma Beal, ed., Natl. Inst. Stand. Technol. (U.S.) Special Publication 825; April 1992: 65-67.

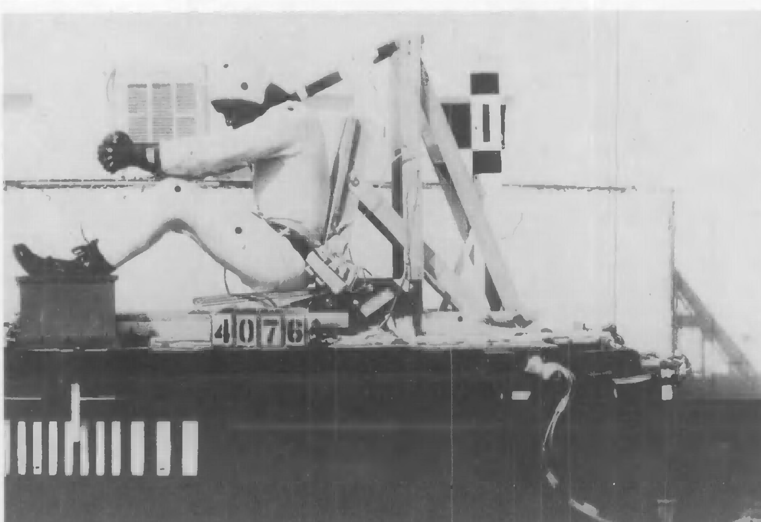
fought the rule-making all the way to the Supreme Court, where the Government's position was upheld. In an about face, "one of the tire companies that strongly opposed the rulemaking is now citing the grading of its tires under the Government system in its advertising."[126]

The study of occupant restraint systems led to some dramatic movies. The main thrust of the work was to improve the dynamic performance of anthropomorphic dummies used in the study of humans under crash conditions. Working with a decelerator at Holloman Air Force Base, tests that simulated a 17 mph auto crash into a

Robert Christian held "Sandy Bagg," as Earl Cooke belted Sandy in place on the sled of the NBS dynamic testing machine. Sandy was one of the contenders for the "standard dummy" for dynamic testing of automobile occupant restraint systems.

[126] Ibid., 66.

Earl Cooke belted in an anthropomorphic dummy.



An airman volunteer underwent tests in a Daisy Decelerator, a sled-and-track device at Holloman Air Force Base, New Mexico. In 1967, NBS carried out tests on twenty-three human subjects in which physiological responses, sled velocity and deceleration, and displacement and loading of belt and shoulder harnesses were recorded.

barrier were held with human volunteers. The subjects first used lap belts alone, and then, in later tests, shoulder harnesses were added. High-speed motion pictures of these tests were compared with similar tests that had used dummies. The goal was to improve the fidelity of the latter tests. Once their fidelity had been established, these tests were cited as "justification for mandating shoulder harnesses in motor vehicles."[127]

Braking systems were studied with an inertia-disk dynamometer. The disks could represent anything from a small car to a 40-ton GVW vehicle. Studying such braking properties as repeated stops, fading, and brake wear, correlations were obtained with instrumented vehicles in road tests.

NBS contiuned its cooperative work on automotive safty throughout the 1964 to 1969 period.

Automatic Data Processing (ADP)

On October 30, 1965, the 89th Congress amended the Federal Property and Administrative Services Act of 1949. More specifically, the new legislation, Public Law 89-306, added to Title I of the 1949 act a new section entitled "Automatic Data Processing Equipment."

Public Law 89-306 was introduced by Congressman Jack Brooks of Texas and has come to be famous as the "Brooks Act" in spite of its unprepossing brevity (slightly more than two pages of text) and tone. In the addition, the administrator of general services was "directed to coordinate and provide for the economic and efficient purchase, lease, and maintenance of automatic data processing equipment by Federal agencies."

Section III (f) of the Brooks Act authorized the secretary of commerece to provide scientific and technological advice on ADP and to recommend to the President "uniform Federal ADP standards." The secretary also was authorized to undertake necessary research as required by his responsibilities under the act. These responsibilities of the secretary immediately became responsibilities of the National Bureau of Standards. The "Brooks Bill" (H. R. 4845) introduced by Congressman Brooks in February 1965 was the culmination of years of activity within the Executive Branch concerning ADP.

In the late fifties and early sixties, with the rapidly expanding use of computers by the Federal Government, management and coordination of ADP activities was a serious concern. Therefore, in September 1958, the Bureau of the Budget, the logical agency to worry about such Government-wide management questions, began a study "[t]o identify and clarify the Government-wide functions performed, or to be performed, in the utilization of Automatic Data Processing (ADP) equipment and to propose assignments of these functions to specific agencies."[128] The report listed fourteen separate

[127] Ibid.

[128] "Report of Findings and Recommendations Resulting from the Automatic Data Processing (ADP) Responsibilities Study, September 1958, June 1959," reprinted in Subcommittee of the House Committee on Government Operations, *Hearings Before A Subcommittee of the Committee on Government Operations on H.R. 4845*, 89th Cong., 1st sess., 30, 31 March and 7 April 1965: 571.

actions that the BOB, with the help of other agencies, should undertake, such as "fostering, promoting, and coordinating the interagency sharing of ADP equipment," and "fostering and promoting desirable standardization in ADP systems which are common to all agencies."[129]

All the while, ADP activities continued to grow. In 1963, it was reckoned that the Federal Government used 1,767 computers, around 10 percent of all the computers in the Nation. Thus, upon a request from Congress, in 1963 President Kennedy directed BOB to conduct a comprehensive review of the subject and prepare a report. By 1965, expenditures associated with the procurement and operation of ADP equipment amounted to $3 billion per year, which at that time represented 3 percent of the total Federal budget.[130]

Completed on March 4, 1965, the Bureau of the Budget report, which was presented to President Johnson, repeated "[t]o a significant degree" the recommendations of the 1959 report.[131] Two of the twelve recommendations made in the BOB report specifically mentioned NBS. Recommendation 8 advised the President to "[g]ive increased attention to the coordination and evaluation of research and development programs in the field of computer sciences. Expand the resources of the National Bureau of Standards to advance the development of computer technology and systems oriented primarily toward Government needs." Recommendation 12 (b) asked the President to "strengthen the authorities for the development, testing, and implementation of standards; the performance of research in computer sciences and the provision of advisory services by the National Bureau of Standards; and the establishment of a revolving fund to finance arrangements for the joint utilization of computer facilities."[132]

The Bureau was again specifically mentioned in a section of the report on the need for expanded research on special activities. The following suggestion appeared:

> The National Bureau of Standards has pioneered in the development and use of computers since 1946. It currently emphasizes research and development on common use aspects of computers and, on a reimbursable basis, it assists other Federal agencies in systems research. . . . The Department of Commerce should determine the extent to which the resources of the National Bureau of Standards need to be expanded to serve as a research center on computer science and technology, primarily oriented toward Government applications, and to serve as an advisory service and consulting center for all Government agencies."[133]

[129] Ibid., 575.

[130] Hearings on H.R. 4845: 9. The $3 billion figure is from the testimony of Joseph Campbell, Comptroller General of the United States. This figure includes the use of computers by Government contractors.

[131] *Report to the President on the Management of Automatic Data Processing in the Federal Government,* prepared by the Bureau of the Budget and submitted by John L. McClellan, Chairman, Committee on Government Operations, United States Senate, March 4, 1965. Prepared under the general chairmanship of BOB Director Kermit Gordon, the report was often called the "Gordon Report." *Automatic Data Processing Equipment, Report of the Committee on Government Operations on H.R. 4845*: 14.

[132] *Report to the President on the Management of Automatic Data Processing:* 7.

[133] Ibid., 55.

The report also proposed legislation. Indeed, recommendation 12 (a) urged that the president "[p]ropose the enactment of legislation by the Congress which would . . . constitute an expression of congressional policy and interest with respect to effective and economical use of automatic data processing equipment."[134]

The BOB study was made under the direction of Carl Clewlow, who was on leave of absence from Arthur Young and Company. Howard Gammon, Astin's special assistant for ADP, was a member of the five-person staff. Because of the presence of Gammon on the Clewlow study staff (and of the Bureau's Samuel N. Alexander on the BOB's Advisory Council on ADP), the Bureau was well aware, even before publication of the report, that new responsibilities were likely to be given to it. Thus, three months before the report to the President was issued, acting upon a request from BOB, Astin wrote a short report to Hollomon: "Augmentation of Computer Related Activities in Partial Implementation of the 'Gordon Report.'"[135] In it, Astin asked for an augmentation of the Bureau's budget request for FY 1966 by $700 000. He noted that "[i]n the course of the study on which the Gordon Report was based, there was general agreement that the increased effort by NBS should be undertaken in three areas:

1. ADP standards development.
2. Assistance to other areas.
3. Research in the computer sciences.[136]

Astin also noted that "[a]dditional activities to fully implement the recommendations of the Gordon Report will require additional legislative authorization." Along with new responsibilities, legislative authorization would be forthcoming.

Delegation of the Bureau's new activities was not slow in coming. On March 6, 1965, BOB issued Circular A-71, "Responsibilities for the Administration and Management of Automatic Data Processing Activities." While laying responsibility on all agencies, the BOB singled out itself, the General Services Administration, the Department of Commerce, and the Civil Service Commission for special responsibilities. The DOC was directed to help achieve increased cost effectiveness in the selection, acquisition, and utilization of ADP equipment. It was given four specific functions. In shortened form, they were:

1. Provide consultant and advisory services.
2. Undertake research on computer sciences and techniques as related to Government applications.
3. Provide day-to-day guidance of an executive branch program for development and testing of voluntary commercial standards for ADP.
4. Improve compatibility in Federal Government ADP equipment.

[134] Ibid., 7.

[135] Memorandum, Director National Bureau of Standards to J. H. Hollomon, "Proposed Augmentation of NBS 1966 RTS Budget Submission," December 11, 1964. (NIST RHA; Director's Office; Box 381; Folder 11/1/64–12/31/64)

[136] This broad scope of research is not reflected in either A-71 or the Brooks Act. Both of those limit the research to Government-related applications.

Given the contributions already made by NBS to ADP standards, it was natural for the secretary of commerce to turn to the Bureau to perform the new functions delineated in BoB Circular A-71. The implementation of these new responsibilities by NBS, however, would require more resources as well as organizational change. Thus, just over a month after the issuance of Circular A-71, Astin proposed the formation of a new organizational unit that he called the Computer Science and Technology Center. Incorporating the Information Technology Division and "those portions of the Applied Mathematics Division which are primarily in support of the functions assigned by A-71," the center would initially comprise three divisions and would report to the director. Left open was the possibility of moving the remainder of Applied Mathematics into the center and "perhaps redesignat[ing] the Center as the Institute for Mathematical and Computer Sciences."[137]

The proposal was adopted but not exactly in its original form. The new unit was called the Center for Computer Sciences and Technology (CCST) and was placed in the Institute for Applied Technology. It consisted of the Information Technology Division and the Computation Laboratory of the Applied Mathematics Division, which was carrying out the Bureau's responsibilities under BOB Bulletin 64-9.[138] The plan was carried through expeditiously. Personnel from the Applied Mathematics Division transferred on September 15, 1965.[139] Norman J. Ream, a computer standards expert from industry, was hired to direct the new unit.

CCST remained in IAT until 1969 when it became a separate unit, reporting to the director. As a "Center" it joined the Center for Radiation Research which, in 1968, had combined the Reactor Radiations Division and the Radiation Physics Division into the Reactor Radiation Division. Beginning in 1969, both centers reported to the director. From 1901 to 1964, the Bureau had operated with an unchanging organizational structure based on divisions and sections. In the 1964 reorganization, the structure was changed by imposing an institute level. Now, just five years later, a new type of unit somewhere between an institute and a division was formed. The changing face of science, which provided new obligations for the Bureau, required a rapidly changing organizational structure that was previously unknown at NBS.

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[137] Memorandum, Dr. A. V. Astin to J. H. Hollomon, "ADP Report to the President," April 15, 1965. (NIST RHA; Director's Office; Box 381; Folder 1/1/65–4/30/65)

[138] Memorandum, A. V. Astin to D. R. Baldwin, "Plan to Create a Center for Computer Sciences and Technology in the Institute for Applied Technology: Proposed Revision of D.O. 90 (January 15, 1965)," August 20, 1965. (NIST RHA; Director's Office; Box 381; Folder 8/1/65–10/30/65); Executive Office of the President, Bureau of the Budget, *Bulletin* No. 64-9, January 2, 1964.

[139] Memorandum, A. V. Astin to G. R. Porter, "Transfer of Employees in the Computation Laboratory From the Applied Mathematics Division to the Information Technology Division," September 14, 1965. (NIST RHA; Director's Office; Box 381; Folder 8/1/65–10/30/65)

As noted at the outset of this section, the legislation recommended in the BOB report to the president was actualized in the form of a bill, H.R. 4845, introduced by Congressman Jack Brooks of Texas, a member of the House Committee on Government Operations. Cast as an amendment to the Federal Property and Administrative Services Act of 1949, it specified a new section entitled "Automatic Data Processing Equipment" for Title I of that act. Public Law 89-306, the "Brooks Act," primarily concerned itself with GSA and DOC. To the former it gave the authority and direction to "coordinate and provide for the economic and efficient purchase, lease, and maintenance of automatic data equipment by Federal agencies" and went on to spell this out in some detail. The authorization for the secretary of commerce was short:

> The Secretary of Commerce is authorized (1) to provide agencies, and the Administrator of General Services in the exercise of the authority delegated in this section, with scientific and technological advisory services relating to automatic data processing and related systems, and (2) to make appropriate recommendations to the President relating to the establishment of uniform Federal automatic data processing standards. The Secretary of Commerce is authorized to undertake the necessary research in the sciences and technologies of automatic data processing computer and related systems, as may be required under provisions of this subsection.[140]

The law was basically an authorization to carry out the requirements of Circular A-71.

To implement these new activities at NBS required either new appropriations or reprogramming. In fiscal years 1965 and 1966, about $2 million of reprogramming was carried out; that was felt to be the limit.[141] For FY 1966, the Bureau asked for $548 000 for ADP standards, bringing the total base for ADP in FY 1966 to $1.33 million. This was the starting point for implementation of A-71 and the Brooks Act.

At the House Appropriations Committee Hearings for FY 1967, the Bureau presented a five-year estimate of funding requirements for these new legislated responsibilities. According to the Bureau's estimate, appropriations would have to reach $5.49 million in 1970 and $7 million in FY 1971.[142] For 1970, the actual appropriation was $1.85 million, an increase of 39 percent over the 1966 base but hardly what the Bureau felt was necessary for a first-class program. Nevertheless, NBS once again had provided expert assistance to a Federal need.

Fire Research and Safety

When a special NAS-NRC panel recommended that a fire group be formed in the Federal Government, the Federal Council for Science and Technology (FCST) designated the Bureau as "a central agency for fire research" and DOC made plans to form a National Center of Fire Technology, reporting to Hollomon. This recommendation

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[140] *An Act To provide for the economic and efficient purchase, lease, maintenance, operation, and utilization of automatic data processing equipment, by Federal departments and agencies, U.S. Statutes at Large,* 79 (1965): 1127-1128.

[141] Appropriations Hearings for 1967: 663.

[142] Ibid., 681.

and designation was not surprising since NBS had conducted research on fire problems for many years. At the House Appropriations Committee Hearings for FY 1964, the Bureau asked for $1.2 million to begin a new fire research program. The objectives of the proposed program were to educate the public on fire dangers, assist academic institutions in providing better education to engineers in fire prevention, support research institutions, and provide better support for the existing NBS program.[143]

Although supported by the International Association of Fire Chiefs, the proposed new program was strongly opposed by industry, particularly the insurance industry, as well as by the National Fire Protection Association, the leading private organization dedicated to fire protection. It was charged by these groups that the program was unnecessary.[144] As a result of this opposition, Congress appropriated no funds. Indeed, the next year, when the Bureau asked for only a small increase in its $215 000 existing program, Chairman Rooney had to be reassured several times that nothing in the whole budget request had anything to do with the previous year's fire proposal.[145]

But this reversal did not stop Astin's attempts to respond to the increasing need for fire research.[146] Astin began by convening a meeting of representatives of all Federal agencies concerned with fire research problems to "explore ways of encouraging greater support for fire research work." He had in mind the formation of an Interdepartmental Committee on Fire Research. Such a committee would establish a coordinated policy on fire research and see that the policy was implemented in each participating department or agency. The FCST requested that the secretary of commerce establish such an entity. The secretary complied, and an interagency committee was established on August 5, 1966.[147] The committee heightened awareness in the Federal Government concerning fire research and safety problems but does not appear to have done much beyond this.

More significant progress in obtaining a full-scale fire program came from the legislative side. In early 1966, James V. Ryan, assistant chief of the Bureau's Fire Research Section, was detailed to Hollomon's office as part of the department's Commerce Science Fellowship Program. His assignment was to develop the rationale for a fire safety program and its content. Citing the enormity of the fire safety problem, which in 1965 cost the Nation 12 100 fatalities and at least $1.6 billion in material losses, Ryan found six problem areas: insufficient and inadequate data on the

[143] Appropriations Hearings for 1964: 978-980.

[144] Ibid., 955, 957-959.

[145] On reading the record, one can conclude that the fact that Hollomon's name was associated with the request was partly responsible for Rooney's opposition.

[146] Letter, A. V. Astin to W. A. Schmidt, Feb. 17, 1965. (NIST RHA; Director's Office; Box 381; Folder 1/1/65–4/30/65); Memorandum, J. H. Hollomon to Donald Hornig, "Interdepartmental Committee for Fire Research," Aug. 31, 1965. (NIST RHA; Director's Office; Box 381; Folder 8/1/65–10/30/65)

[147] Memorandum, A. V. Astin to J. H. Hollomon, "Establishment of Interdepartmental Committee on Fire Research," Aug. 5, 1966. (NIST RHA; Director's Office; Box 380; Folder Chrono File July & Aug. 1966)

nature and magnitude of the problem, lack of knowledge of basic mechanisms of flammability and fire countermeasures, lack of knowledge and awareness of fire safety in the general public, insufficient training of fire fighting personnel, lack of national standards on fire safety in building construction, and problems of coordination and mutual assistance for coterminous fire departments. To correct these problems, a six-point program was developed, consisting of:

1. Collection, analysis, and dissemination by a national organization of fire data on a uniform, national basis.
2. Research to improve understanding of fire prevention and control.
3. Improved and expanded education of fire professionals.
4. Development and encouragement of nationwide use of nationwide, uniform fire safety standards.
5. Establishment of minimum mandatory standards for performance and compatibility of fire fighting equipment.
6. Expansion of research in such areas as treatment of burn injuries, and economic recovery from fires for business and commercial areas.

This report was used by the Department of Commerce to prepare a suggested law, the Fire Research and Safety Act of 1967.[148]

On February 16, 1967, in his consumer message, President Johnson asked the Congress to pass ten bills into law. Among these were three that had important consequenses for the Bureau. One concerned the formation of a National Commission on Product Safety, another provided amendments to the Flammable Fabrics Act, and the third was the Fire Research and Safety Bill of 1967. About the latter, Johnson said that it should be one early step in a major national effort to reduce the shameful loss of life and property resulting from fires.

Things happened rapidly. The bills were introduced in both the Senate and House. The National Commission on Product Safety was created by a joint resolution on November 20, 1967 (Public Law 90-146), and the Flammable Fabrics amendments were passed on December 14, 1967 (Public Law 90-189). The fire legislation, however, took a little longer. It was finally enacted on March 1, 1968, as the Fire Research and Safety Act of 1968 (Public Law 90-259). Except for the fact that it authorized $5 million, rather than $10 million, for a two-year program, Title I of the act as passed was identical to the 1967 bill. It was in the form of an amendment of the Bureau's enabling legislation, and it called upon the secretary of commerce to "provide a national fire research and safety program including the gathering of comprehensive fire data; a comprehensive fire research program; fire safety education and training programs; and demonstrations of new approaches and improvements in fire prevention

[148] Memorandum, J. H. Hollomon to J. A. Califano, Jr., November 17, 1966. In the memo Hollomon specifically cites non-uniform adoption of building construction standards and the compatibility of fire fighting equipment, as serious problems. The Ryan report was sent along with the memorandum. (J. V. Ryan, private communication.)

and control, and reduction of death, personal injury, and property damage." According to the act, it was "the sense of Congress that the secretary should establish a fire research and safety center for administering this title and carrying out its purposes, including appropriate fire safety liaison and coordination."[149] The Bureau had been given new authorities, and it had also been told how to change its organization to manage them. It had obtained a legal basis for one if its special central responsibilities. Title II of the act established a twenty-member National Commission on Fire Prevention and Control to study the fire problem and report in two years with recommendations on how the Nation could reduce the destruction of life and property caused by fire.

Implementation of the legislation proceeded on both the appropriation and organizational fronts. Signed into law too late to be included in the FY 1969 House appropriation request, it was appended to the later Senate request. The Senate recommended $500 000 for Title I and $160 000 for Title II, but in the Senate-House conference, the funds were dropped.[150] However, by means of reprogramming, funds were obtained to set up an Office of Fire Research and Safety in IAT under John A. Rockett. It was purely a program planning office; the technical work on fire research continued in the Fire Research Section of the Building Research Division. Only in 1972 was fire work at the Bureau given division status under Joseph E. Clark. While not yet a center, the Fire Technology Division had achieved independence and had at last obtained legal underpinning.

Systems Analysis and the Technical Analysis Division

With the development of computers and the mathematical modeling of physical and social systems in the fifties and early sixties, the disciplines of operations research and systems analysis flourished. The Bureau's Applied Mathematics Division had a sizable program in operations research.[151] Upon the reorganization into institutes, the Bureau's activities in this area expanded. In FY 1965, the Bureau received a new responsibility to conduct a cost-benefit analysis service for the DOC bureaus and for other Federal agencies, and in 1967 it undertook responsibility "for developing data on decision making on . . . systems problems involving a combination of technology, economics, logistics and sociology."[152]

By the time the first of these announcements was published, action in this sphere had already occurred at DOC. In March 1964, all the bureaus of the department "concurred in the Inter-Bureau Agreement" to establish a Technical Analysis Group which would analyze the effect of science and technology on the programs of the

[149] *Fire Research and Safety Act of 1968, U.S. Statutes at Large*, 82 (1968): 34-39.

[150] John A. Rockett, private communication.

[151] "Data Processing and Operations Analysis on a Scientific Computer," *Technical News Bulletin* 44 (January 1960): 14-18.

[152] Annual Report for 1967: 5.

science-oriented agencies in the Department. This group would "conduct technical-economic analyses and develop analytic processes" which could facilitate policy making. The group, however, would not engage in policy decisions. The responsibility for establishing the group was given to the Bureau. Funds would be provided jointly by the Patent Office, the Weather Bureau, the Coast and Geodetic Survey, and NBS, all of which reported to Hollomon. The group was to be located in the Institute for Applied Technology, and was meant to be concerned solely with DOC programs.[153]

It did not take long for the Bureau to implement this new activity. A Technical Analysis Division (TAD) was created in IAT. Astin wrote, "It has long been clear that such an activity—an internal 'IDA' [Institute for Defense Analysis] for the Department—would be of very great value."[154] W. Edward Cushen, trained in logic and metaphysics and experienced in operations research, was hired to head the new organization.

The first project the TAD worked on followed very naturally from the role envisioned for it. It was a study of transportation in the Northeast Corridor, i.e., the region stretching from Washington, D.C., to Boston, Massachusetts. Entered into cooperatively with DOC's Office of Transportation, the project aimed at providing a computer simulation of transportation in the corridor to permit a systems analysis evaluation of the effect of the introduction of new technologies, such as high-speed rail, automated highways, and vertical take-off aircraft. The aim of the model was to "determine the flow characteristics of the transportation system." The division also developed a computer model for cost-benefit analysis to aid in "decisions concerning [the] relative benefit and cost of transportation technologies for the corridor."[155] This, its most famous project, was to continue for almost a decade, with support shifting from the DOC to the Department of Transportation when the latter agency was formed in 1967.[156]

Other early projects were also undertaken with support from DOC agencies: modeling of patent activities to predict backlogs, examiner workload, and monthly output in 1965, a study of earthquake protection with the Coast and Geodetic Survey in 1966, and the study of the World-Wide Seismology Net operated by the Environmental Science Services Administration in the same year.[157] Later, the work of the division expanded beyond the confines of the DOC to encompass system analysis for other

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[153] Memorandum, J. H. Hollomon to A. V. Astin, "Technical-Economic Analysis," July 30, 1964. (NIST RHA; Director's Office; Box 381; Folder 9/1/64–10/31/64)

[154] Memorandum, A. V. Astin to W. F. Rapp, "The Technical Analysis Division in the Institute for Applied Technology in the National Bureau of Standards," Oct. 28, 1964. (NIST RHA; Director's Office; Box 381; Folder 9/1/64–10/31/64)

[155] Annual Report for 1965: 103.

[156] "Computer Model Simulates Northeast Corridor," *Technical News Bulletin* 53 (January 1969): 8-9, 20. "Transportation in the Northeast Corridor," *Technical News Bulletin* 56 (August 1972): 186-187.

[157] Annual Report for 1966: 94-95.

Federal agencies and even local governments. In 1967, TAD noted that it was the largest systems analysis group "[w]ithin the civilian agencies of the Government" and that it assisted "other agencies in the solution of their specific systems analysis problems." It also conducted "research on cost benefit analyses for Government programs."[158] This represented a significant expansion of its original mandate, and work for other agencies flourished. Projects were undertaken for the Agency for International Development, the Department of the Interior, the Post Office Department, the Department of Housing and Urban Development, the Atomic Energy Commission, the Coast Guard, and even Montgomery County, Maryland—working with that locality "to determine an optimal school districting plan."[159] In 1970, the NAS-NAC-NRC evaluation panel, a very strong supporter of the TAD, wrote:

> TAD has helped: the Interstate Commerce Commission evaluate its plan for an adequate national freight-car supply; the Maritime Administration determine a preferred deployment of inland cargo consolidation centers; the Post Office with its mail-handling and processing systems; the Weather Bureau by evaluating the performance of its Miami hurricane warning center; the Atomic Energy Commission with its problem of controlling the supplies of nuclear material.[160]

Soon the work for other agencies overshadowed the work for DOC agencies, and the division grew to a size of almost 150 persons. Many of these, however, were not in the full-time permanent category.

But Bureau support for the division lagged. Astin was strapped for funds to carry out mandated responsibilities and did not support the division, despite his praise for it. After praising the work of the division at the Bureau oversight hearings in 1971, the now director emeritus noted, "But this activity receives only 10 percent of its funding through direct appropriation, far too small a percentage to provide for the planning and techniques development that are necessary to achieve the potential benefits."[161]

The lack of RTS support became a bone of contention with the Evaluation Panel. In its 1970 review, the panel noted "despite continuing emphasis on this matter by the Panel, the Bureau's support to TAD in the form of RTS funds remains far below the desirable level." It recommended that RTS funds be increased to at least one third of the total budget.[162] Then, in the following year, the panel almost rebelled. When

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[158] Annual Report for 1967: 99.

[159] Annual Report for 1968: 134-135; Annual Report for 1969: 156.

[160] National Academy of Sciences, National Academy of Engineering, National Research Council, "Annual Report of the Panels for the Evaluation of the Research, Development and Technical Service Activities of the National Bureau of Standards 1970-1971": 368.

[161] Subcommittee on Science, Research, and Development of the House Committee on Science and Astronautics, *National Bureau of Standards Oversight Hearings: Hearings Before the Subcommittee on Science, Research, and Development of the Committee on Science and Astronautics*, 92d. Cong., 1st sess., 22 September 1971: 222.

[162] NAS, NAE, NRC, "Annual Report of the Panels for the Evaluation": 372.

the Bureau did not increase RTS support for TAD, the panel noted that "this absence of response to the most mature and significant communication the Panel had ever addressed to the Bureau left the Panel in something of a quandary."[163] One of its members proposed that the Bureau take one of three specified courses of action:

1. Reduce the scope of TAD to service NBS alone.
2. Arrive at a clear understanding that the TAD exists for the benefit of all Government agencies, and that the Bureau simply become its manager.
3. Remove it from NBS and attach it to another agency, or make it into a private corporation or institute.

The source of the problems experienced by TAD in the Bureau is not hard to understand. The TAD had only a tenuous relation at best to the Bureau's measurement standards mission. As long as it was small and as long as it was providing a service to NBS and other DOC agencies, this could be tolerated. However when it became primarily a contractor for other Government agencies, then TAD was open to the criticism that its work could be done as well or better by the private sector. In fact, these were exactly the conclusions that DOC came to. In August 1974, DOC abolished the program, citing the fact that TAD's work appeared to compete with services offered by the private sector, that 60 percent of its work did not relate to the NBS mission, and that all the DOC agencies operate under tight personnel ceilings. Bureau management went to great pains to relocate TAD personnel either in or outside of the Bureau. By March 1975, most of them had been relocated.[164] The official abolishment date was July 22, 1975.[165] It was an unfortunate situation that such a competent organization had been located in the wrong place.

The National Measurement System

The use of systems analysis at the Bureau was not limited to the sophisticated computations of TAD and the Applied Mathematics Division. Ever concerned with the role of the Bureau in the society at large and doubtless spurred by his appointment as director of the Institute for Basic Standards, Robert D. Huntoon applied the concepts of system analysis to what he called the "national measurement system." In his analysis, Huntoon saw all the Nation's measurement activities as forming a social system similar to the communications, transportation, defense, education, medical, and legal systems.[166] The analysis was undertaken "partly because of a growing realization of the

[163] National Academy of Sciences, National Academy of Engineering, National Research Council, "National Bureau of Standards—An Evaluation—1971–1972": 118.

[164] Richard W. Roberts, State of the Bureau–Gaithersburg, March 11, 1975.

[165] U.S. Department of Commerce. National Bureau of Standards. "Organizational and Staffing Changes as Provided in DOO 30-2B," *Administrative Bulletin*, 75-49, September 10, 1975.

[166] Annual Report for 1967: 7.

all-pervasive nature and great economic importance of the nation's measurement activities, and partly because of the challenge to NBS in putting its splendid new facilities to optimum use for the benefit of the nation."[167]

Huntoon demonstrated that measurement was big business. He estimated that each day something on the order of two billion measurements were made.[168] Rather more meaningful was the calculation that in 1963, five economic sectors, contributing $396 billion to the $591 billion gross national product, spent $13.9 billion and 1.3 million man years in measurement. He also estimated that the Nation had investments of $25 billion in measurement instruments and $20 billion in data and that these were increasing at yearly rates of $4.5 billion and $3 billion, respectively.

Huntoon's analysis of the national measurement system began by recognizing that, as in many other social systems, it consisted of two subsystems, an "intellectual system" and an "operational system." The intellectual system consisted "of the set of rules and conventions that govern the operation of the system. . . . [It] is universally applicable, much like the laws of physics. . . . An example of an intellectual system is the International System of Units (abbreviated SI for Système International)—an intellectual concept, a set of rules regarding units. This system in [sic] universal; not only is it international, but it could be used on other planets if we ever succeed in communicating with them."[169]

The operational subsystem, on the other hand, consisted of the people and organizations which were actually involved in measurements and insured "proper linkage of the U.S. system to the international measurement system." The operational system also had to "analyze and work on the pool of unmet needs" and "maintain and disseminate information on the reservoir of capability" that a user might call upon. This second subsystem consisted of three networks: an instrument network, a data network, and a techniques network. The instrument network provided "calibrated traceable instrumentation, consistent and compatible with the national standards." Since the national standards were part of the intellectual system, the instrument network was directly tied to that system. The data network provided critically evaluated data on the properties of materials so that more often than not, the system user did not need to make a measurement. The National Standard Reference Data System was clearly central to this network. The techniques network disseminated knowledge on how to make meaningful measurements.[170]

The overriding rationale for the whole system was to provide assurance that all measurements, wherever made, were compatible. Compatibility provided a firm quantitative basis for the interchange of goods and services in commerce, of machine parts and devices in industry, and of scientific and technical information. The system also

[167] Robert D. Huntoon, "Concept of a National Measurement System," *Science* 158 (October 6, 1967): 67.

[168] This estimate included such passive activities as reading a clock or a speedometer.

[169] Robert D. Huntoon, "The Measurement System of the United States," in *Proceedings of the 1966 Standards Laboratory Conference,* Natl. Bur. Stand. (U.S.) Miscellaneous Publication 291; July 1967: 89.

[170] Annual Report for 1967: 11.

provided a quantitative basis for a safe course of action. For example, an aircraft pilot made essential decisions during flight based on the readings of "measurement output dials." These readings had to be compatible with similar readings by other pilots and air controllers if the flight was to be safe and on schedule.[171]

Compatibility was assured by having all measurements traceable to the units for the basic quantities, as embodied at present in the SI.[172] Because the Bureau developed and maintained its own versions of these basic units, it had the central role in the national measurement system. NBS also provided calibration services and standard reference materials for the instrument network, operated the NSRDS which generated and evaluated data for the data network, and developed measurement methods for the techniques network.

But the Bureau did not work alone in implementing the national measurement system. Even in providing calibration services, it had—and needed—help. The Bureau was instrumental in the development of a chain of standards laboratories and the organization of the yearly meetings of the National Conference of Standards Laboratories. In turn these became a new part of the national measurement system. Moreover, laboratories in industry, government, and academia, at one time or another, inadvertently or by design, provided information to the instrument, data, and techniques networks, and there were special laboratories devoted solely to one or more of these networks. Scientific societies, via their publications, were disseminators of instrument design information, data, and techniques. Of particular importance were the standardizing societies like the American Society for Testing and Materials, American Institute of Mining, Metallurgical, and Petroleum Engineers, and the Society of Automotive Engineers, which provided forums in which test methods or special purpose measurement methods were developed. Indeed, the concept of the national measurement system was so all-encompassing that all scientific and engineering laboratories were both providers to and users of the system.

For the Bureau, the national measurement system was a very natural way of defining its role in the economic and scientific life of the Nation. Thus, the concept was widely promulgated in publications, symposia, and conferences. Internally, the national measurement system concept was most useful in analyzing the programs of the Institute for Basic Standards, since it had responsibility for the basic national standards, and the NSRDS was one of its units. In 1974, there were eighteen "microstudies" or miniature planning-programming-budgeting-type issue studies of the national measurement system in relation to each institute program area.

[171] Huntoon, "The Measurement System of the United States": 91.

[172] Ibid. Huntoon recognized four such quantities—mass, length, time, and temperature—along with their base units. He points out that the SI recognizes two other base units, the ampere and the candela.

THE CLEARINGHOUSE FOR FEDERAL SCIENTIFIC AND TECHNICAL INFORMATION

We saw how the Bureau, largely on its own initiative but strongly supported by the Federal Council for Science and Technology (FCST), created the National Standard Reference Data System, a new program for the collection, production, critical evaluation, and dissemination of scientific data. In February 1964, as part of its reorganization into institutes, the Bureau inherited another program for the dissemination of scientific and technical information.

That the Federal Government should be involved in such an information activity had been accepted for a long time. In 1945, by Executive Orders 9568 and 9604, President Truman established an interdepartmental board called the Publication Board to collect and declassify World War II technical data—including German and Japanese data—and make it available to industry. The following year the Department of Commerce established the Office of Technical Services (OTS) to consolidate the activities of the Publication Board and other organizations. Then the 81st Congress became involved. In 1950, it enacted Public Law 776, which directed the secretary of commerce to "establish and maintain within the Department of Commerce a clearinghouse for the collection and dissemination of scientific, technical, and engineering information." Such information was to be collected, coordinated, and otherwise analyzed "from whatever sources, foreign and domestic, that may be available." It was to be made available in various forms to the whole Nation.[173] These mandated activities became the Technical Documentation Center in the OTS.

In 1964, the Bureau inherited the OTS and placed it in the Institute for Applied Technology. Then in February, following endorsement by the FCST, a clearinghouse of a somewhat different character from the one described in Public Law 776 was established. This one was called the Clearinghouse for Federal Scientific and Technical Information. It would be concerned solely with Federal information, but it would be the "national center for the dissemination of Government-generated information in the physical sciences, engineering, and related technology." It was "established as the single point of contact in the Executive Branch for supplying the industrial and technical community with unclassified information about Government-sponsored research and development in defense, space, atomic energy, and other national programs." The clearinghouse made accessible inexpensive research information that could "aid in the development of a new product, solve a processing problem, or increase productivity through technical improvement." [174]

[173] *An Act To provide for the dissemination of technological, scientific, and engineering information, U.S. Statutes at Large,* 64 (1950): 823.

[174] Letter, A. V. Astin to J. L. McClellan, January 25, 1965. (NIST RHA; Director's Office; Box 381; Folder 1/1/65—4/30/65). Senator McClellan was a sponsor of Public Law 776.

Using rented space in Springfield, Virginia, the clearinghouse was in operation by July 1, 1964[175] and was dedicated in January 1965.[176] It had a broad program. During its first year, employment increased from 236 to 316, and its activities expanded. In that year the clearinghouse sold approximately 1.5 million copies of documents at the cost of reproduction and handling. It collected 60 000 documents, expanding its activities with the Atomic Energy Commission, NASA, and other agencies. The clearinghouse consummated an interagency agreement with the Department of Defense whereby it would do the processing on that department's research and development reports and distribute them to contractors as well as to the general public. It also provided more than a dozen more general services, including journals such as *Government-Wide Index to Federal Research & Development Reports, U.S. Government Research and Development Reports,* and *Technical Translations.* There were even activities designed to improve the efficiency of clearinghouse operations. For example, a demand-prediction model was developed whereby a report could be printed in quantity prior to any requests being received. This precluded the need for expensive individual handling.[177]

Astin considered the clearinghouse to be one of his special central responsibilities, along with the Central Radio Propagation Laboratory and automatic data processing. He sought to obtain increases in appropriations for it, although not as ardently as he did for the NSRDS. A large part of the clearinghouse was self-supporting through sales, but there were some services which were not. The Government-wide index, referral services, reports on research in process, and the development of focused and targeted industrial dissemination needed appropriations. Increases were not major, but they did come. In 1964, the appropriation was $940 000.[178] By 1969 it had increased to $1.28 million.[179]

Until 1969 the clearinghouse was administratively located in IAT where it clearly served the function of assisting industry in the application of new research findings to the development of technology. In 1969, the Bureau's information programs were collected in a new organization called the Office of Information Programs. In the new post of associate director for information programs, Edward L. Brady directed the organization. Along with the clearinghouse, the office contained the Office of Standard Reference Data, the Office of Technical Information and Publications, the Library, the Office of Public Information, and the Office of International Relations. This placement under an associate director who reported to the director attested to the importance Astin placed on information dissemination.

[175] Memorandum, A. V. Astin to W. F. Rapp, "Clearinghouse for Federal Scientific and Technical Information," October 28, 1964. (NIST RHA, Director's Office, Box 381, Folder 9/1/64—10/31/64)

[176] "Dedication of Clearinghouse for Federal Scientific & Technical Information," *Technical News Bulletin* 49 (April 1965): 60.

[177] Annual Report for 1965: 96-100.

[178] House Committee on Appropriations, Subcommittee on Departments of State, Justice, Commerce, the Judiciary, and Related Agencies Appropriations, *Departments of State, Justice, Commerce, the Judiciary, and Related Agencies Appropriations for 1966: Hearings Before a Subcommittee of the Committee on Appropriations,* 89th Cong., 1st sess., National Bureau of Standards, 16 March 1965: 538.

[179] House Committee on Appropriations, Subcommittee on Departments of State, Justice, Commerce, the Judiciary, and Related Agencies Appropriations, *Departments of State, Justice, Commerce, the Judiciary, and Related Agencies Appropriations for 1970: Hearings Before a Subcommittee of the Committee on Appropriations,* 91st Cong., 1st sess., National Bureau of Standards, 13 March 1969: 960.

However, this arrangement did not last long. Since the clearinghouse was not a technical or research operation, there was no advantage to its being located in the Bureau. On September 2, 1970, it became a separate agency within the Department of Commerce known as the National Technical Information Service, one of the Department's primary operating units.

INVENTION AND INNOVATION

In 1940, at the suggestion of scientists and engineers, the secretary of commerce formed the National Inventors Council (NIC) to—in the words of Jacob Rabinow—"get the lay inventors (that is, the non-professionals, and perhaps the professionals who are not part of large organizations) to submit inventions to the Government to help the war effort." The NIC also provided advice to the secretary with regard to the field of invention. Serving without pay, the Council received over 500 000 submissions during the war years, 106 of which actually went into production.[180] Then, in 1964, the Bureau formed a small group concerned with invention and innovation in the Office of the Director of the Institute for Applied Technology. Placed under Daniel V. De Simone, an engineer/lawyer who had been a consultant to J. Herbert Hollomon and the Office of Technical Services and was an expert on innovation, a primary function of the group was to reconstitute and serve as secretariat for NIC. In the same year, in his Economic Report to the Congress, President Johnson directed DOC "to explore new ways for speeding the development and spread of new technology." To help carry out the president's directive, the secretary of commerce formed a Panel on Invention and Innovation, and "[b]ecause one of the ways a government can accomplish this end [the spread of technology] is to improve the climate for technological change," the secretary asked the panel "to explore the opportunities for improving such climate-setting policy areas as anti-trust, taxation, and regulation of industry."[181] De Simone's group in IAT provided a secretariat for the panel. In February 1965, the NBS group became an office. So was born the Office of Invention and Innovation (OII).

The aim of the Office was to "help develop an environment more conducive to technological change." It did this in three separate ways: by "providing a more rational basis for the formulation of climate-setting Federal policies," by offering programs to help inventors, and by education.[182] In 1965, OII reconstituted NIC, which adopted a new charter and appointed De Simone as its executive director. Composed of fourteen outstanding inventors from the private sector and fourteen observers from various Government agencies, NIC was now "concerned with the processes of invention, the work of inventors, and ways to provide more effective assistance to them through state,

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[180] Jacob Rabinow, "Diamond Ordnance Fuze Laboratory and National Inventors Council," in *NBS/NIST, A Historical Perspective; A Symposium in Celebration of NIST's Ninetieth Anniversary, March 4, 1991,* Karma Beal, ed., Natl. Inst. Stands. Technol. (U.S.) Special Publication 825; (April 1992): 59.

[181] The fifteen-member panel was under the chairmanship of Robert A. Charpie, President of Union Carbide Electronics, and De Simone was the Executive Secretary. The panel wrote a report entitled, *Technological Innovation: Its Environment and Management,* Daniel V. De Simone, ed. (Washington, D.C.: GPO, 1967): 1.

[182] Annual Report for 1966: 93.

regional, and Federal invention programs." It served "as a forum for the views of inventors on issues concerning creativity and the conception, development, and application of inventions to the needs of society."[183]

In 1965, OII organized and then published the proceedings of a conference on creative engineering education, one of the types of activities it took on as an arm of NIC. The conference was inspired by NIC, and support for it also came from the National Academy of Engineering and the National Science Foundation.[184] The NIC also issued reports. For example, a report for the President's Commission on the Patent System gave the inventor's view of that system.[185]

Another of the main activities of the office was helping the states to assist inventors. The office encouraged states to hold expositions and congresses at which inventors and industry could meet and explore the value of specific inventions. With direct advice and assistance from OII, twelve states held such congresses in 1965, thirteen did in 1966, and twenty-one did in 1967. The office also provided assistance to the Organization of American States in designing a "strategy for the technological development of Latin America."[186]

Perhaps the most constant concern of OII was education, an area of great interest to the inventor's council. OII and the council's concern was that the necessary factual content of traditional engineering education should not stifle original or unconventional approaches to solving problems. The OII cooperated with universities, holding seminars and symposia with the aim of stimulating changes in the engineering curricula to emphasize the process of invention and innovation.[187]

In 1967, the Panel on Invention and Innovation issued its report. Concerned with taxation, finance, and competition, the panel found "no need to recommend any major changes in the present laws governing these three areas." It did, however, make seventeen specific recommendations, ranging from the handling of financial losses incurred by small technology-based companies to the application and clarification of anti-trust laws. It also recommended that DOC serve as the Federal spokesman for technology-based enterprises. Clearly, OII was an effort in that direction.[188]

All this time, in keeping with its original role, NIC continued to receive inventions, and the Bureau staff analyzed each one. Most of the 625 000 submissions received by the NIC during its lifetime were of no value. There was, for example, the occasional perpetual motion machine. But if an invention seemed to have merit, the inventors were directed to possible avenues for development.[189]

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[183] Memorandum, A/S for Science and Technology to the Secretary, "Renewal of Charter for the National Inventors Council as Required by Departmental Order No. 114 (Revised)," May 12, 1969. (NIST RHA; Director's Office; Box 388; Folder May–June 1969)

[184] *Education for Innovation*, Daniel V. De Simone, ed. (Oxford: Pergamon Press, 1968): vii-ix.

[185] Annual Report for 1967: 107.

[186] Annual Report for 1969: 164.

[187] For an in-depth analysis of the education problem see Daniel V. De Simone, "Education for Innovation," *IEEE Spectrum* 5 (January 1968): 83-89.

[188] Ad Hoc Panel on Invention and Innovation, *Technological Innovation*: 34-57.

[189] Rabinow, "Diamond Ordnance Fuze Laboratory and National Inventors Council": 59.

OII grew from a small group to an organization of twenty persons in 1969, but then dropped to half that size as the office undertook Department-wide PPB functions.[190] In 1974, Assistant Secretary for Science and Technology Betsy Ancker-Johnson abolished NIC, feeling that its work was largely completed and that she could obtain sufficient advice from other sources.[191] The dissolution of NIC removed one of the prime reasons for the existence of OII. But almost immediately a reason arose for the enlargement of one type of the activities carried on by OII. On the last day of 1974, the president signed into law the Federal Nonnuclear Energy Research and Development Act of 1974. In a very short section entitled "Energy-Related Inventions," the Bureau was assigned a new function, or perhaps it can be said that an old activity was given a legal basis. The section read:

> The National Bureau of Standards shall give particular attention to the evaluation of all promising energy-related inventions, particularly those submitted by individual inventors and small companies for the purpose of obtaining direct grants from the Administrator [of the Energy Research and Development Administration, now the Department of Energy]. The National Bureau of Standards is authorized to promulgate regulations in the furtherance of this section.[192]

On March 30, 1975, the Office of Energy-Related Inventions was established in the Office of the Director of IAT, with George P. Lewett as director. The original OII continued operating, but not for long.[193] On July 22, 1975, the day that the Technical Analysis Division was abolished, the OII was abolished as well. The sole responsibility of the Bureau with regard to inventions was now the evaluation of those related to energy. The program continued in collaboration with the Department of Energy and was known as the Energy-Related Inventions Program in the Office of Technology Innovation. From 1975 to the program's demise in 1998, over 33 000 inventions were evaluated with more than 700 considered sufficiently promising to be recommended to the Department of Energy for commercialization support. Of these, more than 130 achieved commercial success with gross market sales of more than $1 billion.[194]

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[190] Appropriations Hearings for 1970: 961-962; Annual Report for 1969: 163.

[191] Rabinow, "Diamond Ordnance Fuze Laboratory and National Inventors Council": 59.

[192] *Federal Nonnuclear Energy Research and Development Act of 1974, U.S. Statutes at Large,* 88 (1974): 1894.

[193] Department of Commerce, National Bureau of Standards, "Organizational Changes—Divisions 129 and 400," *Administrative Bulletin* 75-12, March 27, 1975.

[194] Computations of social rates of return (SRR) from data collected over the period 1980-1994 by Oak Ridge National Laboratory show that ERIP's SRR compares favorably with those of other NIST programs. Data and citations used in computation of SRR are found in R. B. Braid, Jr., M. A. Brown, C. R. Wilson, C. A. Franchuk, and C. G. Rizy, *The Energy-Related Inventions Program: Continuing Benefits to the Inventor Community,* Oak Ridge National Laboratory Report ORNL/CON-492 (October 1996). Table of SRR for twelve NIST technology areas is found in Gregory Tassey, *Rates of Return from Investments in Technology Infrastructure,* Natl. Inst. Stand. Technol. (U.S.) Planning Report 96-3; June 1996: 21.

In its budget submissions for FY 1970, the Bureau placed both the Clearinghouse for Federal Scientific and Technical Information and the Office of Invention and Innovation in its special central responsibilities category of obligations that had no direct relationship to its measurement standards mission. While many inventions were byproducts of its measurement mission, most occurred as part of its regular research or as part of its work for other agencies. The aim of trying to increase innovation by stimulating and easing the processes of invention did not seem to follow from the measurement standards mission.[195] However, the aim of improving technology development and its diffusion would become an increasingly important part of the Bureau's activities, eventually transforming it into a wholly different institution.

A Director Retires

In 1967, Allen Astin turned 63 and was looking forward to retiring at age 65. By then, he would have served as director for seventeen eventful years, from the traumatic AD-X2 events of his first years to the rewarding ones during the building and occupancy of the Gaithersburg facility. The institution he would be leaving was far different from the one he had inherited. No longer did 85 percent of its income derive from work for other agencies; now it was a more reasonable 40 percent. Astin had seen the Bureau through the most sweeping organizational change in its history and had given it a new definition of its mission. Whole organizational units had been spun off, but the institution was not materially smaller. Most important, all major organizational units were headed by new young leaders whom, as director, Astin had led throughout their years at the Bureau. It was an opportune time to yield the reins to a younger person.

Astin had chosen that person. He was Lewis M. Branscomb, founder of JILA and chief of the Laboratory Astrophysics Division. Branscomb had already had an illustrious career. Born in Asheville, North Carolina, on August 17, 1926, he attended Duke University, from which he received an A.B. degree summa cum laude in 1945. He served as an officer in the Naval Reserve with one year of duty in the Philippines. After the War, Branscomb entered Harvard University, earning an M.S. in 1947 and a Ph.D. in physics in 1949. After two years as a Junior Fellow in the Harvard Society of Fellows, the young scientist came to the Bureau as one of Condon's new minds. Branscomb was eventually appointed chief of the Atomic Physics Section of the Atomic and Radiation Physics Division, and upon the split of that entity, became chief of the Atomic Physics Division. He was the founder of JILA and, as Astin contemplated his retirement, was serving as chief of the Laboratory Astrophysics Division in Boulder.

[195] The aim of the program was not actually to develop technology. This would have put it into competition with the private sector. The perception that this competition existed was the reason for the difficulties encountered by the Civilian Industrial Technology (CIT) program. Although not directed at particular industrial sectors, both the inventions and innovation program and the clearinghouse activity were, in fact, the types of activities envisaged for CIT.

But listing Branscomb's Bureau accomplishments does not do him justice. He was widely known and active in the scientific community. He served on the President's Science Advisory Committee. Although he served as an independent person, not an official representative of an agency, he was the first civil servant to serve on that committee. Branscomb served on the Defense Science Board in 1968 and 1969, resigning when he became director of the Bureau. He served as a special consultant to the secretary general of the Organisation for Economic Co-operation and Development and participated in the International Union for Geodesy and Geophysics, the International Union of Pure and Applied Physics, and the International Astronomical Union. He was a member of the American Academy of Arts and Sciences, served on the Board of Directors of the American Association for the Advancement of Science, and, upon becoming director of the Bureau, served on the board of the American National Standards Institute. Branscomb then became a member of the National Academy of Sciences. He had been chairman of the Division of Electron and Atomic Physics of the American Physical Society and was now the editor of *Reviews of Modern Physics.* He had been an instructor in physics at Harvard (1950-51), lecturer in physics at the University of Maryland (1952-54), visiting staff member at the University College, London (1957-58), and adjoint professor of physics at the University of Colorado (1962-69). He was the recipient of many awards, among them the Rockefeller Public Service Award, the Arthur Fleming Award of the D.C. Junior Chamber of Commerce, the DOC Gold Medal, and the Bureau's own Stratton Award. He was married to the former Anne Wells and had two children, Harvie and Katharine.

As early as September 1967, Secretary of Commerce Alexander B. Trowbridge and Assistant Secretary Kincaid, doubtless spurred by Astin, were anxious to get Branscomb nominated as director. It was known that Astin would retire upon reaching his 65th birthday in 1969, after the 1968 election with the possibility of loss by the incumbent Democratic administration. The Branscomb partisans had to take some action before the forthcoming elections. One possibility was to have Astin accept a "senior advisory role" for several months in order that Branscomb might be made director before that time.[196] Astin, however, adamantly refused. He would retire from the position of director.

Another possibility was that Branscomb could be appointed deputy director, either before or after the retirement of Irl Schoonover, which was expected in 1968.[197] In a memorandum to Trowbridge, Kincaid concluded, "I suggest that we try to interest Dr. Branscomb in the Directorship, but it looks as though we will have to put it on the basis that he will have to take his chances with respect to becoming Director in 1969, if he starts as Deputy Director in 1968." Responding to a note from Kincaid, Branscomb indicated that he did not want any part in such schemes. "I think it is not

[196] Memorandum, John F. Kincaid to The Secretary, "Directorship of NBS," September 26, 1967. (DOC; Assistant Secretary for Science and Technology; Accession 40-72A-7166; Box 8; Folder Chron File (August-September 1967) JFK)

[197] Schoonover's retirement did not actually occur until 1969.

useful to the Bureau of Standards for me to make a commitment at this time which might serve to tie the Secretary's hands should there be a change in administration or for any other reason in the departmental leadership." More correspondence followed. Yet Branscomb's position regarding the deputy director position remained unchanged. However, despite his disapproval of the particular strategy that Kincaid proposed, Branscomb was not totally opposed to the idea of assuming the directorship. Even though his daughter, who suffered from asthma, might not be able to "live in good health for continuous periods in the Washington climate," Branscomb and his wife were prepared to try to work it out. He regarded his "possible appointment as NBS Director with the utmost seriousness."[198] The secretary told Branscomb, "I share his [Kincaid's] faith in NBS, and his desire that it obtain the type of top flight leadership you can provide when the time becomes propitious."[199]

The time never did become propitious. Nixon defeated Humphrey in 1968, and the Republican Party took over the White House. Although it was customary that political affiliations did not bear greatly on decisions concerning the appointment or retention of the Bureau director, they were not completely ignored. Certainly they were not in Branscomb's case. In particular, Anne Branscomb, a lawyer by profession, had become state chairwoman of the Democratic Party in Colorado and hence a member of the Democratic National Committee.[200] Branscomb himself, while a Democrat, was basically apolitical. His wife's affiliation, however, was to prove somewhat of a stumbling block.

Astin did not let the change in administration stand in the way of having his chosen successor become the next director of the Bureau. Whether he did it himself or via the visiting committee or by some other means is not known, but somehow Branscomb's name found its way to the White House. That Branscomb had served on one of Nixon's transition teams—the Technology Transition Group—apparently had nothing to do with it. Shortly after Christmas 1968, Astin told Branscomb that he was "trying to rig this" for him to become director. Branscomb, still ambivalent about assuming the post and quite happy with being a division chief and with his JILA/University position, said, "Look, I don't want to be the Director, but if you rig it, I owe you so much, I won't turn it down. I'll do it."

But the problems caused by Anne Branscomb's affiliation still had to be overcome. As Branscomb recounted it in an interview, "they [the White House] looked me up and discovered not only was I a Democrat, my wife was on the DNC [Democrat National Committee]. That didn't go down too well. The reason it didn't go down too well is because the chairman of the Republican Party in Colorado was a man named John Flanigan. . . . His brother was Peter Flanigan, the investment banker. Peter Flanigan

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[198] Letter, L. M. Branscomb to J. F. Kincaid, January 27, 1968. (DOC; Assistant Secretary for Science and Technology; Accession 40-70A-6988; Box 35; Folder National Bureau of Standards–General 1966-68)

[199] Letter, A. B. Trowbridge to L. M. Branscomb, February 7, 1968. (DOC; Assistant Secretary for Science and Technology; Accession 40-70A-6988; Box 35; Folder National Bureau of Standards–General 1966-68)

[200] Interview with Lewis M. Branscomb, July 12, 1988: 45-51. (NIST Oral History File)

was a close Nixon confidante." John Flanigan, at the insistence of the White House, had reluctantly agreed to the appointment in the Nixon Administration of a Democrat from Colorado and was smarting as a result. He would have no more Democrats. Branscomb continued, "Whoever was trying to engineer my appointment, whether it was [newly appointed Secretary of Commerce Maurice] Stans or whoever Astin's intermediaries were, went to Peter Flanigan and persuaded Peter to persuade his brother. I later became a good friend of Peter Flanigan, and he told me that that's what happened."

With that stumbling block out of the way, Branscomb was called to meet with Secretary Stans. He remembered, "I went into his big office and sat down. Marty Stans said to me the following thing, more or less in these words. He said, 'Dr. Branscomb, you have a fine reputation and a lot of people have told me you're a great scientist and so forth, and that you are not only qualified to be the director of the Bureau of Standards, but that you are the best qualified person anywhere to be the director of the Bureau of Standards, and that I'd be making a terrible mistake if I didn't appoint you director of the Bureau of Standards.' He said, 'I'm prepared to do that, but you and I need to have an understanding.' I said, 'Sure, what's that?' He said, 'I'm sure what the Bureau of Standards does is very important. I don't really know much about what it is. I don't really expect to get very involved in what the Bureau of Standards does. If you will run the Bureau of Standards competently, keep it out of trouble, do whatever it's supposed to do and do it well, and you will recognize that the reason I'm here and the only reason I'm here as secretary of commerce, is to raise money for Nixon's re-election in 1972, and you don't get in my way in this task, we'll get along fine.' I said, 'Mr. Stans, you've got nothing to worry about. . . . We'll get along just fine.' " In fact, Branscomb recalled that "Stans, once in office, took considerable pride in the Department and vigorously defended its interests."[201]

Lewis Branscomb was nominated by President Nixon on June 17, 1969 and confirmed by the Senate on August 7, 1969, more than three weeks before Astin left office on August 31. Branscomb became the Bureau's sixth director on September 1.

Standards Matters

The standards activities of the three institutes illustrate well the differences in emphasis among them. Thus, as its name implies, the Institute for Basic Standards (IBS) had responsibility for the basic units—standards of mass, length, time, temperature, the ampere, the candela—and the quantities derived from them. Then, in the 1964 reorganization, the Office of Standard Reference Materials (OSRM) was placed in the Institute for Materials Research (IMR) which also inherited the Analytical Chemistry Division, by far the largest producer of SRMs. The Institute for Applied Technology (IAT) took over the commodity standards program and, via the Building Research Division, took on the building codes and standards activities, as well as fire standards

[201] Letter, Lewis M. Branscomb to Elio Passaglia, March 8, 1993. (NIST History Project File; Chapter 5; Folder "LMB Director")

work. With passage of the Brooks Act, IAT was given the responsibility for the development of what came to be called Federal Information Processing Standards (FIPS). This is not to say that the work was completely compartmentalized. Development and production of SRMs took place throughout the Bureau, as for example in the Polymers Division, which produced molecular-weight SRMs. And in the Cryogenics Division, even while it was part of IMR, studies on the use of the Josephson effect for the extension of the temperature scale to the millikelvin range took place.[202] It should be borne in mind further that representatives from virtually all NBS units served on one or more of the numerous committees of standardization organizations such as ASTM and ASME.

In this section on standards matters we give illustrations of noteworthy work that was carried out in these different areas, with emphasis on the basic standards activities, which was the Bureau's unique function.

Lasers and New Vistas in Metrology

The very high coherence and brightness of lasers makes them practically ideal instruments for length measurements. Thus the work on length during the period took three different directions: the use of continuous-wave lasers for measuring moderately long (50 m) lengths by interferometry with direct counting of fringes; the use of pulsed lasers for the measurement of very long distances by the use of radar-like techniques; and a study of the wavelength stability and reproducibility of lasers as possible replacement of the krypton-86 wavelength standard for the meter. Alongside these three lines of activity was a related effort to make an absolute measurement of the frequency of laser light. In due course that effort, along with the stabilization of laser wavelength, would lead to a definition of the meter based on the velocity of light.

We now take up the use of lasers for the interferometric determination of length.[203] We saw in Chapter 4 how an interference pattern using a helium-neon laser was obtained over a 100 meter path. But that laser was unstable due to several technical factors and, as a result, so was the interference pattern. For further experiments it had to be improved. This was done by keeping the laser operating in a single mode by adjustment of the power supply, by making the cavity rugged and rigid, and by tuning the cavity length piezoelectrically to keep the resonance at the center of the neon line. There was no electronic feedback stabilization.

The laser was then turned to a very practical problem. The calibration of graduated length scales (really nothing but very fancy and precise rulers) was a very tedious and lengthy process. In the proposed method, the laser would be used as the light source

[202] The Cryogenics Division, located in Boulder, was first placed in IMR, but because all other Boulder units (except for the CRPL, which was lost in the formation of ESSA in 1966) were in IBS, it also was moved to IBS for administrative simplicity.

[203] There are quite a few publications on this topic. Those that were found useful in preparing this account are: K. D. Mielenz, H. D. Cook, K. E. Gillilland, and R. B. Stephens, "Accurate Length Measurement of Meter Bar With Helium-Neon Laser," *Science* 146 (1964): 1672-1673; K. D. Mielenz, "Length Measurement and Laser Wavelength Stability," *ISA Transactions* 6 (1967): 293-297; "Line-Standard Interferometer: For Accurate Calibration of Length Scales," *Technical News Bulletin* 51 (1967): 43-45; "International Study of Laser Wavelength," *Technical News Bulletin* 52 (1968): 95-96.

for a Michelson interferometer to be used for the automatic calibration of such scales. Due to the high coherence of the laser light, it would be very easy to obtain an interference pattern over the whole one-meter length of these precision rulers. Then, counting the interference fringes as one of the mirrors in the interferometer was moved from one end to the other of the item to be calibrated, would give the total length of the item. Calibration could also be accomplished at any intermediate position.

To carry out this process, the item to be calibrated was firmly fastened to the carriage of a massive way-bed—really the frame for a linear dividing engine. One mirror of the interferometer was fixed near a stationary microscope and the other was located on a movable carriage driven by a long screw and carrying the scale. The fringes were produced at a photoelectric counter. As the carriage moved, the interference fringes passed across the photoelectric cell and were counted. There was built into the instrument an automatic pause and centering option (by feedback from the microscope) at each graduation on the ruler, and an automatic recording of the fringe count, interpolated to 0.01 fringe. An automated system was at hand. All that was necessary now was to accurately determine the wavelength of the laser.

Herbert D. Cook (left) of the NBS Electronic Instrumentation Laboratory controlled the automatic operation of a fringe-counting interferometer (not shown). The count was shown on the monitor console (center).

In the measurement of length with a laser to better than 1 part in 10 million, Klaus D. Mielenz matched the impedance of the rf-power supply to that of the laser.

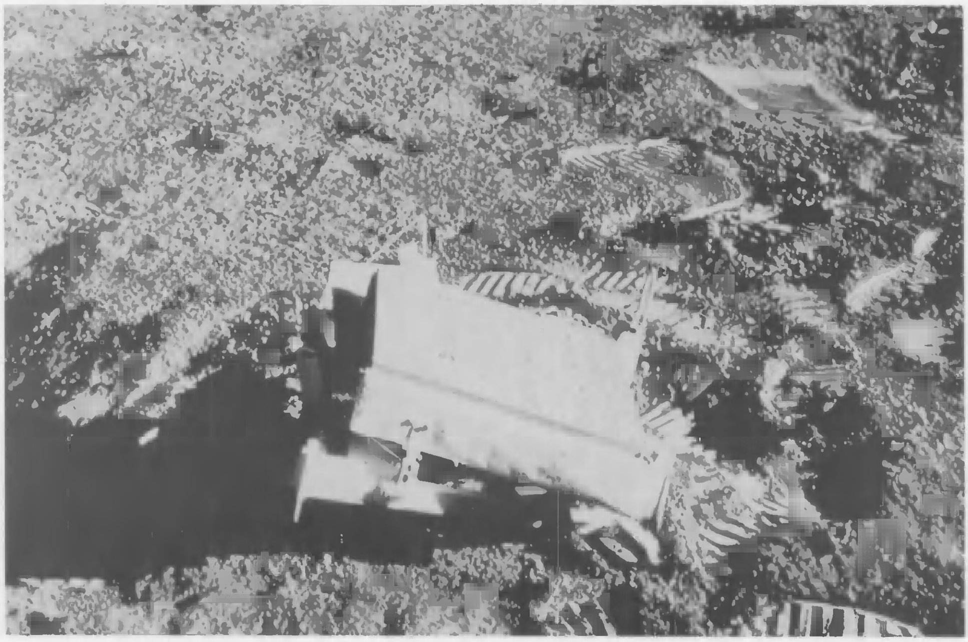
Comparison with a mercury-198 light source (a primary standard of length used in place of krypton-86) by counting fringes over a calibrated decimeter line standard gave the wavelength of the laser. The practicable length over which an interference pattern could be obtained with such a source was about three decimeters. Then, counting fringes with the laser source over the length of a standard meter bar reproduced the length of the bar to 1 part in ten million. It was excellent accuracy, and all produced automatically.

On the basis of these results the Bureau offered an improved and much less costly calibration service for graduated length scales. If the graduations were sufficiently fine, the calibration relative uncertainty offered was ± 1 μm/m (1 part per million). Improvements in the measurement process had reduced the relative uncertainty to ± 60 nm/m (6 parts in 100 million) by 1998.

To check the stability and reproducibility of the helium-neon laser, an international study involving the Bureau, the British National Physical Laboratory, and the German Physikalisch-Technische Bundesanstalt was carried out. While the study showed that the different laboratories agreed within 5 parts in 10^9 in wavelength measurements when they measured the same laser—even though they used different methods—measuring different lasers of the same type could give quite different results. The wavelength for an uncalibrated laser could not be presumed to have a relative uncertainty of less than 1 part in ten million. Regular calibration was called for, and the Bureau considered offering a laser calibration service.

* * *

When the Apollo 11 astronauts left the lunar surface they left behind a 46 cm × 46 cm aluminum panel in which 100 carefully crafted 3.8 cm in diameter fused-silica corner-cube retroreflectors were mounted. These were to be used to reflect back to its orgin a very short laser pulse sent up from Earth. The plan was to measure very precisely the travel time of the pulse and from this to calculate with great precision the earth-moon distance.[204] From the determination of this distance it would be possible to refine such lunar orbital parameters as mean distance and eccentricity, and such geophysical data as the earth's period of rotation, the motion of the pole and, after five years of observation, to determine the east-west rate of continental drift. The initial uncertainty of the lunar distance determination was expected to be ± 15 cm.



The Apollo 14 mission in 1971 left a second corner-cube retroreflector array on the lunar surface. The Apollo 11 and Apollo 14 retroreflector packages were deployed at well-separated sites near the lunar equator. These arrays were expected to yield an extended sequence of high-precision earth-moon distance measurements and thus provide a variety of information about the earth-moon system.

[204] The information presented here comes from two publications: J. E. Faller, I. Winer, W. Carrion, T. S. Johnson, P. Spadin, L. Robinson, E. J. Wampler, and D. Wieber, "Laser Beam Directed at the Lunar Retro-Reflector Array: Observations of the First Returns," *Science* 166 (1969): 99-102; and C. O. Alley, R. F. Chang, D. G. Currie, S. K. Poultney, P. L. Bender, R. H. Dicke, D. T. Wilkinson, J. E. Faller, W. M. Kaula, G. J. F. MacDonald, J. D. Mulholland, H. H. Plotkin, W. Carrion, and E. J. Wampler, "Laser Ranging Retro-Reflector: Continuing Measurements and Expected Results," *Science* 167 (1970): 458-460.

It was a conceptually simple, though in practice complicated, experiment in which the Bureau had been involved from the beginning—indeed, from the origin of the idea.[205] Briefly, in December 1962, James E. Faller, recent Princeton graduate, came to work with Peter L. Bender at JILA as a post-doctoral research associate. Faller, having used corner-cube retroreflectors in his thesis work, brought to NBS the draft of a paper on how corner-cube retroreflectors on the moon could be used to measure the distance to the moon by timing the round-trip travel time of reflected laser pulses. But laser technology in those early days was primitive, and Bender discouraged Faller from publishing his paper. However, as laser technology progressed, Faller's idea became more attractive, and a group of seven people published an article on the topic and made a proposal to NASA.[206] By then, all space for experiments on all planned Apollo flights had been committed, but some of the tasks assigned to the astronauts were thought to be too tiring. However the emplacement of the retroreflector holding plates was easy, and through this fortunate circumstance, Apollo 11 carried the retroreflector package and the astronauts emplaced it on the moon.

It did not take long for results to come. On August 1, 1969, just eleven days after the astronauts left the lunar surface, the first laser return signals were observed. To accomplish this, 7-joule pulses of light from a ruby laser were transmitted through the 120-inch telescope at the Lick Observatory. Even with a telescope of this size, the diameter of the beam at the lunar surface was still approximately 3.2 km, and the reflected return signal amounted to only slightly more than one photoelectron per pulse at the photomultiplier detector. The range accuracy on this first experiment was about 15 m. By October, regular data were coming from the McDonald Observatory, which was instrumented to be this country's lunar laser-ranging station. The initial range uncertainty was $\pm$ 30 cm. This subsequently improved to $\pm$ 15 cm and now—some years later—to $\pm$ 2 cm.

* * *

The other laser-length activity carried out during the period was not concerned with measuring length, but on the ambitious aim of replacing the krypton-86 wavelength as the international length standard with radiation from a laser.[207] From the early days of lasers it was shown that the intrinsic bandwidths of essentially all gas lasers were one Hz or less, and observable bandwidths of a few tens of Hz were obtainable, limited primarily by mechanical and thermal disturbances. Such widths correspond to an uncertainty in the frequency of about one part in 10^{13}.

[205] Interview with Peter L. Bender, May 12, 1987. (NIST Oral History File).

[206] C. O. Alley, P. L. Bender, R. H. Dicke, J. E. Faller, P. A. Franken, H. H. Plotkin, and D. T. Wilkinson, "Optical Radar Using a Corner Reflector on the Moon," *Journal of Geophysical Research* 70 (1965): 2267-2269. Faller left the Bureau in 1966 to accept a position at Wesleyan University. He remained very active in the lunar laser-ranging experiment and returned to the Bureau in 1972, rejoining the JILA staff.

[207] The physics background on why this was an attractive possibility and the problems of implementing it were reviewed by J. L. Hall, "P-1—The Laser Absolute Wavelength Standard Problem," *IEEE Journal of Quantum Electronics* QE-4 (1968): 638-641.

But this is for an operating laser. Resetting the laser, or constructing another one, could only be done with an uncertainty of one part in 10^9. This is about four decades worse than the resolution limit, and not better than the performance of the krypton-86 standard. To make good use of lasers, the resettability problem had to be solved.

As seen above, an uncalibrated helium-neon laser is likely to have an intolerable uncertainty of one part in 10^7 and, if calibration is required, such a laser cannot be a primary standard. Hence servomechanism methods of self stabilizing to the center of the emission line were tried, but the presence of Stark shifts, Zeeman shifts, Doppler shifts, and pressure broadening and shifts could cause problems for a primary standard. Various other schemes were tried to stabilize the laser, but the most promising was "based on the sharp-line absorption of laser light by suitable molecules." Molecules have rich vibrational-rotational spectra, so that the probability of finding a line within the tuning range of the laser (2 to 3 parts per million) is relatively high.

The methane absorption line at 3.39 μm was the one chosen for study by Richard L. Barger and John L. Hall at JILA for the stabilization of a 3.3 μm He-Ne laser.[208] It is a line that has a number of good features, such as being thermally well populated, and having a long natural lifetime and a high absorption. Its frequency is 100 MHz higher than the center of the 3.9 μm He-Ne laser line center, but the laser line can be pressure-shifted to be in exact coincidence with the methane absorption. Perhaps most important, when saturated by the laser field, the linewidth obtained can approach the very sharp natural linewidth without Doppler broadening.

Experiments were set up in an underground vault to minimize environmental disturbances, using a three-ton cast-iron table for further stability. Three lasers were used in the experiments. Two contained the methane absorption cell in their cavity, while the third served as a local oscillator. Using a feedback system too complex to be described here, Barger and Hall found that their two lasers containing the methane cell, independently locked to the methane frequency, differed in frequency by only 1 part in 10^{11}, about 2.5 orders of magnitude better than the krypton-86 primary standard. One of the keys to opening the door to a completely new primary standard of length was in hand.

* * *

One basic standard activity that involved lasers dealt not with length, but with time. More precisely, it was concerned with the measurement of the frequency of laser light. Again, this was not merely a display of experimental virtuosity—it had a very specific purpose. If the measurement of frequency—the most accurate measurement available to science—could be extended without too great a loss in accuracy to a region in which the wavelength of the radiation could also be compared to the krypton-86 standard, then a value of the velocity of light could be determined in which the principal uncertainty was that in the length standard. This in turn would lead to the possibility—or necessity—of redefining the meter.

[208] R. L. Barger and J. L. Hall, "Pressure Shift and Broadening of Methane Line at 3.39 μ Studied by Laser-Saturated Molecular Absorption," *Physical Review Letters* 22 (1969): 4-8.

The first determination of the absolute frequency of a laser line was made with the 311 μm and 337 μm transitions of a CN gas laser.[209] What was done was to beat the laser output against the 12th or 13th harmonic of a klystron operating near 75 GHz, the 12th harmonic for the 337 μm line and the 13th for the higher-frequency line. Heterodyning was carried out on a commercial cat's-whisker/silicon-rectifier harmonic crystal mixer. The fundamental frequency of the 75 GHz klystron was measured by comparing it to the frequency of another klystron locked to a signal generator, and the observed frequencies of the CN laser were determined as 890.7595 GHz and 964.3123 GHz with a relative error of "a few parts in 10^7."

In rapid order, the frequencies of shorter and shorter wavelength lasers were determined: 1578.279 GHz for a 190 μm D_2O laser, 1539.756 GHz for a 194 μm C_2N_2 laser, and 2527.9528 GHz for a 118.6 μm water vapor laser.[210] To go beyond this frequency a new method had to be devised. Such high harmonics of the klystron frequency had to be used that the signal-to-noise ratio suffered greatly. Something new was required. Since the absolute frequencies of some lasers were now known, they could be used in a frequency multiplier chain.[211] To this end, the harmonics of a 337 μm laser could be mixed with the laser to be measured, and a microwave signal supplied to make up the difference between the two. This technique was first tested on the known 118 μm water-vapor laser and then extended to the frequency of the 84 μm D_2O laser.[212]

Up to this point, all the cited work had been done outside the Bureau. But now work at the Bureau went ahead on two fronts. The first, under the direction of Kenneth M. Evenson of the Radio Standards Division in Boulder, followed the paths already described, while another effort under the direction of Zoltan Bay in Gaithersburg took another direction. It did not achieve the accuracy of the Evenson group effort, and did not lead to new standards, but it is included here for historical completeness.

The Evenson group reached the highest frequency measurement obtained up to that time using an experimental arrangement similar to that used by L. O. Hocker, James G. Small, and Ali Javan, but a metal-on-metal diode—a small but critical item—replaced the metal-on-silicon diode. The results were 3.821 775 ± 0.000 003 THz and 10.718 073 ± 0.000 002 THz for the 78 μm line and the 28 μm line respectively.[213]

[209] L .O. Hocker, A. Javan, D. R. Rao, L. Frenkel, and T. Sullivan, "Absolute Frequency Measurement and Spectroscopy of Gas Laser Transitions in the Far Infrared," *Applied Physics Letters* 10 (1967) 147-149.

[210] L. O. Hocker, D. R. Rao, and A. Javan, "Absolute Frequency Measurement of the 190μ and 194μ Gas Laser Transitions," *Physics Letters* 24A (1967) 690-691; L. Frenkel, T. Sullivan, M. A. Pollack, and T. J. Bridges, "Absolute Frequency Measurement of the 118.6-μm Water-Vapor Laser Transition," *Applied Physics Letters* 11 (1967): 344-345.

[211] L. O. Hocker and A. Javan, "Laser Harmonic Frequency Mixing of Two Different Far Infrared Laser Lines up to 118 μ," *Physics Letters* 26A (1968):255-256.

[212] L. O. Hocker, J. G. Small, and A. Javan, "Extension of Absolute Frequency Measurements to the 84 μ Range," *Physics Letters* 29A (1969): 321-322.

[213] K. M. Evenson, J. S. Wells, L. M. Matarrese, and L. B. Elwell, "Absolute Frequency Measurements of the 28- and 78-μm cw Water Vapor Laser Lines," *Applied Physics Letters* 16 (1970): 159-162.

Almost immediately this frequency record was broken.[214] Despite severe difficulties with the diode detectors, Evenson and his group were able to measure the frequencies of the 10.6 μm lines of the CO_2 cw laser. For this experiment, the CO_2 radiation was mixed with the just-measured 78 μm and 28 μm radiations of the water-vapor laser, and again the difference made up with a klystron. The results for the two CO_2 lines were 28.359 800 THz and 28.306 251 THz, with an uncertainty of ±0.000 025 THz. The frequency reached was still a long way from the visible region of the electromagnet spectrum, but progress had by no means slowed.

The Bay effort was based on a quite different method. In it, a crystal of potassium dihydrogen phosphate was placed in the laser cavity and used to modulate the laser beam at a microwave frequency. The sum and difference sidebands were passed into a Fabry-Perot interferometer and, using a method involving simultaneous servoing of the laser cavity length and the interferometer length for maximum output, Bay and Gabriel G. Luther were able to measure both the frequency and wavelength of the laser light. With this apparatus and the 632.8 nm He-Ne laser, they made the first measurement of the frequency of visible light.[215] But the method was incapable of the accuracy of the Evenson-Hall method, largely because of the latter's incorporation of the methane-stabilized laser.

Time Dissemination

In mid July 1963, the Bureau began disseminating time and frequency signals from its two new low-frequency stations—WWVB at 60 kHz and WWVL at 20 kHz—located on a special site at Fort Collins, Colorado. With the low-frequency stations operating, the time was propitious for the relocation to the Fort Collins site of the high-frequency WWV, broadcast from Greenbelt, Maryland, since 1931 at 2.5 MHz, 5 MHz, 10 MHz, 15 MHz, 20 MHz, and 25 MHz. It was a move that had long been planned. In 1964 Congress appropriated $970 000 for the move, and by late 1965, building construction and the purchase of new equipment were well under way. A set of eight new transmitters, four for 10 kW operation at 5 MHz, 10 MHz, and 15 MHz, and four for 2.5 kW operation at 2.5 MHz, 20 MHz, and 25 MHz, left two transmitters always in a standby mode. The broadcast powers were half the rated power of the transmitters. When the newly relocated WWV began operation at 0000 hours on December 1, 1966, the Bureau's continental time and frequency broadcast facilities were consolidated.[216]

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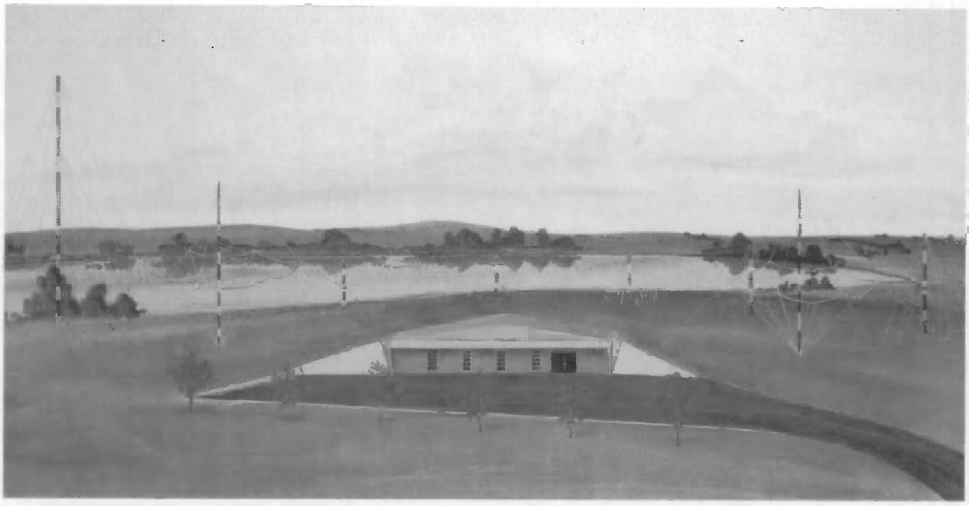
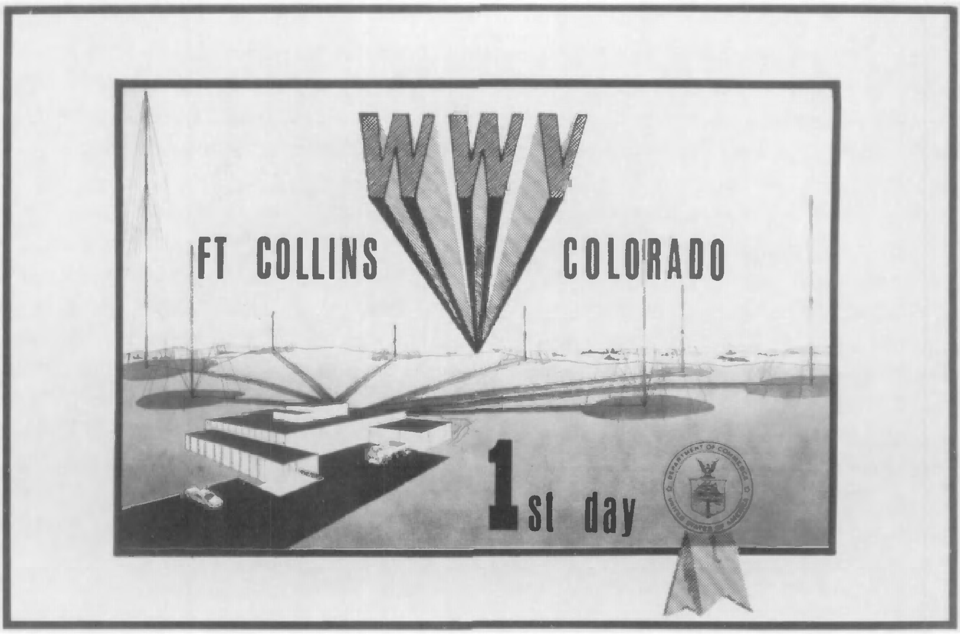
[214] K. M. Evenson, J. S. Wells, and L. M. Matarrese, "Absolute Frequency Measurements of the CO_2 cw Laser at 28 THz (10.6 μm)," *Applied Physics Letters* 16 (1970): 251-253.

[215] Z. Bay, G. G. Luther, and J. A. White, "Measurement of an Optical Frequency and the Speed of Light," *Physical Review Letters* 29 (1972): 189-192; "Experimental Measurement of Optical Frequency," *Technical News Bulletin* 57 (1973): 14, 17.

[216] Wilbert F. Snyder and Charles L. Bragaw, *Achievement in Radio: Seventy Years of Radio Science, Technology, Standards, and Measurements at the National Bureau of Standards,* Natl. Bur. Stand. (U.S.) Special Publication 555; October 1986: 282; "WWV To Be Relocated," *Technical News Bulletin* 49 (1965): 215.



Beginning on July 1, 1966, WWV, Fort Collins, Colorado, transmitted the services formerly provided from WWV, Greenbelt, Maryland. The facility was partly underground to affect as little as possible the omnidirectional characteristics of its antennas.



Amateur radio operators making contact with each other exchange "QSL" cards to verify the contact and obtain useful information on propagation conditions. On the occasion of WWV's first broadcast from its new facilities in Fort Collins, Colorado, a special first day card was issued to amateurs and shortwave listeners who reported reception.

Radio station WWVH, on the island of Maui in Hawaii. This station broadcast continuous time and frequency standards to the Pacific area with an accuracy of 1 part in 100 million. Technical services included standard radio frequencies, time announcements, standard time intervals, and standard musical pitch.

But in the Hawaiian Islands, WWVH, the Bureau's time broadcast, was in bad shape. Located on a man-made peninsula on the island of Maui, it had been operating since 1948, and now the site on which it was located was being eroded away by the sea. By 1968, the shoreline was only ten feet from the headquarters building and twelve feet from the antenna. Relocation was imperative.[217]

Sites were first investigated inland on Maui,[218] but the search was extended to all the other islands of the Hawaiian chain, and then extended to Guam, the Marshall Islands, Wake, and American Samoa. A site on the Hawaiian island of Kauai was chosen as having the best physical characteristics and availability of services. While near the ocean, it was a natural site little affected by the sea. It was owned by the Navy, so no land purchase was necessary.[219]

For FY 1969, the Bureau requested and received $700 000 to begin the relocation, and with further appropriated funds, the relocation was completed and broadcasts began July 1, 1971. The new station broadcast on five frequencies, 2.5 MHz, 5 MHz, 10 MHz, 15 MHz, and 20 MHz, and frequency control was provided by broadcasts from WWVL. The broadcasts covered Alaska in the North, New Zealand in the South, a number of the major cities of the Orient, and most of the Pacific Ocean.[220]

[217] Appropriation Hearings for 1969: 1200.

[218] Ibid., 1374-1376.

[219] Appropriations Hearings for 1970: 905.

[220] "New WWVH Facility," *Technical News Bulletin* 54 (1970): 137.

Along with this modernization of facilities came research on dissemination of time signals. Both WWV and WWVH could provide millisecond accuracy with their time signals, but for some users—television and radio stations, power companies, airlines, satellite communications, the military, universities—higher accuracy would be a boon. These latter users typically carried a portable clock from station to station to synchronize their clocks—a cumbersome, slow procedure which could only synchronize one clock at a time. Another system was highly desirable.[221] The Bureau took two paths toward the provision of clock synchronization in the microsecond range.

The primary reason that WWV or WWVH could not provide such high accuracy was a lack of knowledge regarding the path of the radiation.[222] The reason for this was that the propagation of signals from these medium-high frequency sources was via reflection from the ionosphere, the position of which varied from time to time. Hence the high accuracy of the time signal as transmitted was degraded by this lack of knowledge of the path—both its length and the refractive index in it.

Two approaches were taken to solve this problem, one using a satellite in geosynchronous orbit, and the second using the television broadcast system. In both cases the problem was the same. A signal is transmitted from a station controlled by a master clock, such as WWV and WWVH which are both controlled by the Bureau's atomic clock. The time at which this signal is received by a remote "slave" clock is unavoidably delayed, and the accuracy with which the slave clock can be synchronized with the master depends upon how well this delay is known. The delay in turn depends upon the location of the slave, the path length between the master and slave, the refractive index along the path, and the time delay inherent in equipment electronics.

The use of a geosynchronous satellite for the accurate synchronization of clocks was attractive because the bulk of the path is line-of-sight through interstellar space, and because the satellite can service any slave clock in sight of the satellite transmission.[223] In such a system, a signal from the master clock is sent to the satellite where it is received by a transponder and then re-broadcast. Comparison of the time the signal is received with the local (slave) clock gives the difference in time between the slave and master, and the synchronization is completed. The question is the accuracy with which this synchronization—really a form of calibration—can be performed, which in turn "reduces to predicting the propagation delay."[224] The elements that cause the propagation delay, with their estimated uncertainties in microseconds, as determined by Lawrence E. Gatterer, Paul W. Bottone, and Alvin H. Morgan in a series of experiments, are equipment delays on the ground and on the satellite (± 2 and ± 1,

[221] "Time Dissemination and Clock Synchronization via Television," *Technical News Bulletin* 54 (1970): 125-126.

[222] L. E. Gatterer, P. W. Bottone, and A. H. Morgan, "Worldwide Clock Synchronization Using a Synchronous Satellite," *IEEE Transactions on Instrumentation and Measurement* IM-17 (1968): 372-378.

[223] The height of the satellite is approximately 23 000 miles.

[224] Gatterer, Bottone, and Morgan, "Worldwide Clock Synchronization Using a Synchronous Satellite": 372.

respectively), location of the master and slave (± 0.7 each), satellite range (± 0.15 for both the up-link and down-link), propagation through the ionosphere for both up-link and down-link (± 6 each), troposphere delays (0.6), and noise (± 5). The total computed uncertainty was 10.4 microseconds, which means that by this system the time of the slave clock could be compared with the master with this uncertainty. It was about 100 times as good as could be obtained with WWV.

Another system for the accurate dissemination of time was to use broadcast television as the dissemination mechanism.[225] This system was attractive because the bulk of the path carried by network TV broadcast signals was via microwave repeater stations. The transmitter was line-of-sight with the location of the repeaters, and hence the path, accurately known. In this system, a master clock at the originating transmitter encoded the TV signal with the time. At the receiving TV set, this time was compared with that of the local "slave" clock and the difference fed to a decoder which, once each second, displayed on the TV screen the master time and the difference, in nanoseconds, between that time and the time kept by the slave. Measurement of the path delay between the Naval Observatory in Washington, D.C., and the Bureau's Boulder Laboratories in 1969 showed that, for all three networks, the variation over a few months averaged out to somewhat less than one microsecond. Using synchronized clocks at each of the major network broadcasting centers in New York, the technique could potentially be used over most of the United States by an already existing system.[226]

Mass and Measurement Assurance Programs

By themselves, calibrations are not sufficient to ensure sound measurements. In a typical mass calibration for example, a set of weights is sent to the Bureau and the calibration personnel determine the difference between the nominal values of the weights and their actual mass.[227] This calibrated set of weights is then typically kept by the using laboratory as a "master set," used for calibrations of its working sets, or customer sets if the laboratory is in the calibration business. The master set is perhaps returned to the Bureau periodically for recalibration. This procedure really gives only one unambiguous number—the mass of a weight when it is calibrated by the Bureau. In particular, it does not give the expected uncertainty for any mass measurements using the weights, unless the laboratory's measurement process happens to be identical to the Bureau's and is kept under control.

The reason for this is quite clear. Measurement involves much more than a set of calibrated weights, or, more generally, some other calibrated item or instrument. Measurement is first and foremost a process consisting of a set of operations, perhaps using instruments. It is influenced by such factors as operators, temperature, and other environmental factors, and other unknown influences. This process generates a number,

[225] D. D. Davis, J. L. Jespersen, and G. Kamas, "The Use of Television Signals for Time and Frequency Dissemination," *Proceedings of the IEEE* 58 (1970): 931-933.

[226] Ibid., 933.

[227] "Measurement Analysis Program in Mass," *Technical News Bulletin* 54 (1970): 202-203.

much as a manufacturing plant yields a product, with the added complexity that repeated measurements yield slightly different results. To ensure the validity of such a number, the whole process generating it must be kept under control, not simply the weights.

The whole question of the reliability of measurements was addressed by Churchill Eisenhart, who carried through the analogy between a production process and the process of making a measurement, adopting the analytical techniques of industrial quality control.[228] He described and analyzed such concepts as a process which is in a state of "statistical control," or, in lay words, one in which "the amount of scatter in the data from repeated measurements of the same item over a period of time does not change with time and if there are no sudden shifts or drift in the data."[229] Only if a measurement process is in such a state can a meaningful analysis of it be carried out. Eisenhart went on to describe his Postulate of Measurement, originally enunciated by N. Ernest Dorsey as the law of the limiting mean: "The mean of a family of measurements—of a number of measurements for a given quantity carried out by the same apparatus, procedure, and observer—approaches a definite value as the number of measurements is indefinitely increased."[230] This concept is important because the value of the limiting mean can be inferred (within calculable bounds) by statistical means from a limited set of measurements. He then discussed at length the concepts of systematic error, or "bias," or "offset"; of the true value of a quantity; of precision and accuracy; and the uncertainty of a measurement. From this analysis it became clear that not only the weights had to be calibrated to ensure reliable measurements, but also the laboratory itself.

Along with this and previous analyses came Bureau experience. Results on weights that were routinely calibrated over and over again during the course of the Bureau's calibration business, when plotted against time, showed that all the results clustered around one value. This indicated both that the measurement system was in a state of statistical control and that the results tended toward the limiting mean. From this analysis it became clear that, given enough experience, any laboratory can assess and establish its own performance.

How this could be done was developed over a period of years by Paul E. Pontius and Joseph R. Cameron, with the able assistance of Robert C. Raybold.[231] The effort was not simply driven by intellectual curiosity; there were very serious practical considerations.[232] In the early sixties, driven by military and aerospace requirements, the

[228] C. Eisenhart, "Realistic Evaluation of the Precision and Accuracy of Instrument Calibration Systems," *Journal of Research of the National Bureau of Standards* 67C (1963): 161-187.

[229] Brian Belanger, *Measurement Assurance Programs Part I: General Introduction,* Natl. Bur. Stand. (U.S.) Special Publication 676-I; May 1984: 64.

[230] N. Ernest Dorsey quoted in Eisenhart, "Realistic Evaluation": 168.

[231] P. E. Pontius, *Measurement Philosophy of the Pilot Program for Mass Calibration,* Natl. Bur. Stand. (U.S.) Technical Note 288; May 6, 1966; P. E. Pontius and J. M. Cameron, *Realistic Uncertainties and the Mass Measurement Process: An Illustrated Review,* Natl. Bur. Stand. (U.S.) Monograph 103; August 15, 1967; J. M. Cameron, M. C. Croarkin, and R. C. Raybold, *Designs for the Calibration of Standards of Mass,* Natl. Bur. Stand. (U.S.) Technical Note 952; June 1977.

[232] Much of this general background comes from an interview with Robert C. Raybold on October 9, 1992.

mass-calibration facilities of the Bureau were swamped with requests. Most military contracts carried the provision that all measurements should be traceable to the national standards. This required the continuous calibration of the contractor's weights and led to a situation at the Bureau where a dozen or so skilled technicians did nothing but calibrate weights, and another half dozen did nothing but carry out the calculations required by the calibrations. The only way to alleviate this load on the Bureau's facilities was to establish a set of laboratories which were mostly concerned with military and aerospace functions that would take over some of the calibration functions of the Bureau.[233]

Motivated also by a desire to build a mathematical model of the mass calibration process so that effects of variables such as temperature could be quantified, the Bureau set out to build what was essentially a set of satellite standardizing laboratories which it would keep under control, and whose results could be demonstrated to be traceable to the national standards. As a result, a program first called the Pilot Program in Mass, subsequently the Measurement Analysis Program, and finally the Measurement Assurance Program (MAP), was begun. The final definition of a MAP came to be:

> A MAP is a quality assurance program for a measurement process that quantifies the total uncertainty of the measurements (both random and systematic components of error) with respect to national or other designated standards and demonstrates that the total uncertainty is sufficiently small to meet the user's requirements.[234]

At first the program was concerned only with calibration laboratories, i.e., commercial calibration laboratories and those laboratories that would carry out calibrations for the operating laboratories in their institution or agency. The Bureau was, in a sense, concerned with replicating itself in a number of lower-level institutions.

To do this, it was necessary to devise a scheme by which the performance of the lower-level laboratories could be assessed. Three things had to be known: (1) whether the laboratory was under statistical control, (2) the standard deviation of its measurements, and (3) the relationship of its results to the national standards.

The key to determining the first and second of these requirements was the "check standard." This was a weight (or a number of them, usually one per decade) owned by the laboratory, that would be calibrated as an unknown during the calibration of other unknown sets of weights. The values of the check standard obtained in these various calibrations could be plotted against time. After enough background information on the check standard was obtained, a new measurement could be expected to fall within certain bounds, expressing essentially the standard deviation of that laboratory's

[233] With respect to lessening the calibration load, H. Steffen Peiser pointed out that for many uses of weights, as for example in determination of chemical composition, it is not the relationship of the weights to the standard kilogram that is important, but rather the internal consistency of the weights, and the precision of the weighings. And, as in the Measurement Assurance Program, the system has to be under control. Peiser pointed out that for many of these uses, calibration against the standard kilogram was unnecessary, but high precision was. This viewpoint was not shared by everyone.

[234] Belanger, *Measurement Assurance Programs Part I: General Introduction:* 2.

measurement process. If it did, then the process was under statistical control. If it did not, then either the measurement was an unexplained individual excursion, or a shift in the average-value baseline had occurred. Subsequent measurements would clarify the cause, and sometimes it was necessary to send the check standard to the Bureau for deeper investigation, and possibly for cleaning.

Note that, except for the last part, the Bureau's presence was not necessary in any of the process. All of it could be carried out by the laboratory itself, independently of the Bureau, and it would yield a measure of the laboratory's internal consistency. Where the Bureau did come into the process was in monitoring the process and in determining how the laboratory's results related to the national standard, thereby providing traceability. While there were various ways of assessing this, typically a weight (or a set of weights) referred to as a "transfer standard," with a value unknown to the laboratory, would be calibrated by the Bureau and sent to the laboratory for its calibration. The results and the weight would be returned to the Bureau for re-measurement and further analysis. Upon evaluation of all the results, the Bureau would then issue to the laboratory a report stating the "offset" or systematic error in the laboratory's results compared with the national standards. Combined with a measure of the laboratory's standard deviation, the total uncertainty of the laboratory's measurement process could be determined.[235] Alternatively, an extensively calibrated set of weights could be sent to the laboratory. Since these would become the laboratory's primary standards, special care was to be taken with them. In this manner the Bureau's role in the calibration process was reduced to assessing the performance of the laboratories participating in the Measurement Assurance Program, leaving the bulk of the routine calibration effort to the participating laboratory.

Participation in the MAP was, of course, voluntary, but after inauguration of the program the Bureau only did calibrations under unusual circumstances, instead sending calibration requests to a commercial laboratory participating in a MAP. And the Bureau's participation in a MAP could be deep or shallow, including in some cases personnel training, equipment investigation, suggesting weighing schemes, and carrying out the calculations necessary to determine both standard deviation and systematic error.[236] At the introduction of a new laboratory into a MAP, the Bureau generally worked with it to make sure it was under statistical control.

Mass measurements were the first to utilize the MAP, and by 1970 sixteen laboratories participated in the mass MAP. Other areas soon followed. By 1984 MAPS were available for electrical standard cells at the 1 V level, gage blocks, electrical resistors from 1 ohm to 10^9 ohms, capacitance, watthour meters, platinum resistance thermometers from −183 °C to 630 °C, and laser power and energy. Some of these areas, particularly gage blocks and temperature, involved measurements in actual production, but the

[235] Ibid., 3.

[236] Weighing is preferably done by comparing weights of approximately equal size, working with the difference in the weights. This yields a better measurement of the sensitivity. And redundant measurements are necessary in order to arrive at a standard deviation. Some of the schemes and the mathematics involved can be quite intricate. See Cameron, Croarkin, and Raybold, "Designs for the Calibration of Standards of Mass."

MAP philosophy could be adapted to those cases as well. The MAP program markedly reduced the amount of routine calibration the Bureau had to carry out while at the same time improving the Nation's measurement capabilities.

The Josephson Effect and Maintenance of the Volt

In 1894 an international electrical congress defined the unit of electrical resistance on the basis of the resistance of a column of mercury of specified dimensions, and defined the ampere on the basis of the rate of electrodeposition of silver. Since then, workers in the field of electricity had wanted to do away with these so-called international units and return to the absolute units, which are defined on the basis of mass, length, time, and the equations of physics, rather than on some arbitrary artifact.[237]

But there is a difference between how a unit is defined and realized, and how it is kept, or maintained in the standards laboratory. Thus, in the United States, the representation of the volt was maintained on the basis of the mean electromotive force (emf) of a bank of standard cells, and the ohm was maintained in the form of a bank of wire-wound standard resistors, some of which had at some time been calibrated against the mercury column international standard. Realizations of the absolute ohm were carried out using calculable inductors and capacitors, and the frequency of an alternating current. The realization of the absolute ampere basically involves the measurement of the force between current-carrying coils of very accurate construction and dimensions. Knowledge of the permeability of free space is also required, and this is defined as $4\pi \times 10^{-7}$ henries/meter. An absolute realization of the volt was not attempted until the coming of the Josephson effect.

By the beginning of World War II absolute realizations of the ampere had become sufficiently accurate to warrant their consideration as a basic standard for electricity.[238] The considerations, including reference to Ohm's law—I (amperes) × R (Ohms) = V (Volts)—led the International Bureau of Weights and Measures (BIPM) in 1946 to recommend a conversion from the "international" electrical to the "absolute" units, and on January 1, 1948, the changes were adopted. The results showed that there were significant differences between the mean international units and the absolute units as follows:

1 mean international ohm = 1.00049 absolute ohms

1 mean international volt = 1.00034 absolute volts

[237] F. B. Silsbee, *Establishment and Maintenance of the Electrical Units,* Natl. Bur. Stand. (U.S.) Circular 475; June 30, 1949; *Announcement of Changes in Electrical and Photometric Units,* Natl. Bur. Stand. (U.S.) Circular 459; May 1947; "Reference Base of the Volt to be Changed," *Technical News Bulletin* 52 (1968): 204-206.

[238] The "international" units were originally designed to be as close to the absolute units as possible at the time of their definition. It is also important to recognize the "mean international" units, which were the average of the units maintained by France, Germany, Great Britain, Japan, the Soviet Union, and the United States.

For the United States the results were:

1 international ohm (U.S.) = 1.000495 absolute ohms

1 international volt (U.S.) = 1.00033 absolute volts.

By the late sixties, the determination of the absolute ampere had progressed to the point where a re-definition of the "maintained" volt was recommended by the International Committee for Weights and Measures (CIPM) to the BIPM. Under this re-definition the volt maintained by the BIPM was decreased by 11 parts per milliom on January 1, 1969. In view of its relationship to the BIPM volt, the Bureau-maintained legal U.S. volt, as realized by the group of standard cells, was increased by 8.4 parts per million, and beginning in 1969 all Bureau calibrations were done on this new basis. The new basis was not occasioned by drift in the standard cells used to maintain the volt, but by new, more accurate realizations of the absolute ampere. It was not a large change for practical work, but a necessary one for precision measurements.[239]

The Bureau had not allowed the volt to drift willy-nilly without checking. In fact, during the six years from 1961 to 1967, the volt was under continuous surveillance. The monitoring was accomplished via the precession frequency of the proton (in water) in the field of a solenoid magnet in which the current was defined in terms of the NBS volt and the ohm. These measurements showed that the ratio of the volt to the ohm had not changed as much as 1 part per million over the six years.[240] Since other experiments showed that the ohm had changed less than this, it was concluded that the volt had also remained constant to better than 1 part per million. But this surveillance was difficult, complex, and expensive. The measurement of the dimensions of the solenoid limited the accuracy of the magnetic field to 1 part per million, and was furthermore affected by the magnetic environment. As a result, none of the other national laboratories had carried on such monitoring.[241] But triennial comparisons between the standard cells of the national laboratories with those maintained by the BIPM showed differences of the order of a few parts per million, some positive and some negative. It was hard to tell what was drifting.

In 1962, Brian D. Josephson at Cambridge University predicted the effect that would bear his name. He showed that if two superconductors were "weakly coupled," as for example two crossed superconducting film strips separated by a nanometer thick film of oxide, then a current could tunnel through the barrier. Further, impressing a voltage across the superconducting sandwich (called a "junction"), was predicted to produce radiation of a specified frequency. The ratio of the frequency to the voltage

[239] "1968 Actions–International Committee of Weights and Measures," *Technical News Bulletin* 53 (1969): 12-13.

[240] Annual Report for 1967: 22.

[241] B. N. Taylor, W. H. Parker, D. N. Langenberg, and A. Denenstein, "On the Use of the AC Josephson Effect to Maintain Standards of Electromotive Force," *Metrologia* 3 (1967): 89-98.

was equal to twice the ratio of the electronic charge to Planck's constant, or $2e/h$—a fundamental constant now known as the Josephson constant. The effect was predicted to be independent of the superconductor, the nature of the "weak coupling," the magnetic environment, and other factors. The relationship was exact.

Even more important for metrological purposes was the inverse of this phenomenon. In this case, impressing a microwave-frequency electromagnetic wave across the junction caused its current-voltage characteristic curve to show a series of steps. Increasing the current through the junction produced steps in which the voltage remained sensibly constant until the current reached a critical value at which point the voltage suddenly increased to a new level, i.e., a series of constant-voltage steps was produced. If n is the step number (an integer), V_n the voltage of the nth step, and f the frequency, then the ratio nf/V_n was predicted to be exactly $2e/h$, which, for orientation purposes, is about 484 MHz/μV.

This relationship is a metrologist's dream. The integer n is determined simply by counting, e.g., from the characteristic curve of the junction displayed on an X-Y recorder, and the frequency can be determined so accurately that any uncertainty in it is negligible. As a result, the voltage can be determined in absolute units with an uncertainty determined solely by that in the fundamental constant $2e/h$. It was another case in which there arose the possibility of a fundamental constant replacing an artifact as a basic standard.

As pointed out by Barry N. Taylor, the effect has three uses:[242]

1. To check on the constancy of reference standards of emf over a long period of time.

2. To infer the relationship between reference standards for voltage of different national laboratories.

3. To calibrate reference standards for emf in absolute units.

Note that only the last one requires an accurate knowledge of $2e/h$; the first two require only that it be a constant.

Since superconducting junctions are relatively cheap and easy to produce, are easily portable, and all the other needed pieces of equipment are readily available (except perhaps one, as we shall see) in any reasonably equipped national laboratory, the effect is particularly useful for the comparison of the voltage standards of various laboratories, and for the monitoring of one nation's standard-cell maintained volt.

The one piece of apparatus that was not so readily available was the one needed to compare the voltage across the junction with the voltage of the standard cell. The maximum voltage of a single junction is of the order of a few millivolts, while that of a standard cell is about a volt. To relate one voltage to the other with a high degree of

[242] Ibid., 90. At the time this work was done Taylor was at the RCA Laboratories in Princeton. His coauthors were at the University of Pennsylvania. Taylor joined the staff of the Electricity Division of the Bureau in June 1970.

accuracy required special equipment. To solve this problem initially, the Bureau's resident expert on such measurement matters, Forrest K. Harris, and two associates, Howland A. Fowler and P. Thomas Olsen, developed a special potentiometer which could compare a signal of 2 millivolts to 10 millivolts against the U.S. "legal" volt with an accuracy and precision of a few parts in 10^7.[243]

Using this instrument and one with an even smaller uncertainty, the Bureau began experiments using the Josephson effect to monitor any changes in the standard cells maintaining the U.S. legal volt.[244] Assuming that $2e/h$ was a constant, it was found that the mean emf of the group of standard cells decreased linearly by about 4 parts in 10^7 over the one-year period, beginning in July 1971. To compensate for this drift, the Bureau adopted the value of 483 593.420 GHz/V_{NBS} for $2e/h$, which was consistent with the existing U.S. volt, and used this value to assign a mean emf periodically to the bank of electrolytic cells. This assignment became the definition of the new as-maintained unit of emf for the Nation, and it could be maintained with a precision of about 2 parts in 10^8.[245] It was a major step in the shift to an absolute volt realized by the Josephson effect.

To take a further step, the value of $2e/h$ had to be defined internationally, and a more accurate comparison of the volt as maintained by the various national laboratories had to be carried out. In a comparison made under the auspices of the BIPM, the Bureau, with the support of the U.S. Air Force, transported standards cells in temperature-controlled containers and compared them with the cells used for maintaining the volt by the BIPM, the United Kingdom, Canada, Australia, and Germany.[246] The "main purpose was to provide a sound basis for intercomparing values of $2e/h$ obtained by the various laboratories via the Josephson effect." By the use of temperature-controlled enclosures and techniques used in the Measurement Assurance Program, it was found that, converted to a common unit, most of the values of $2e/h$ agreed with one another to 1 to 2 parts in 10^7. This was another major step toward the use of the Josephson effect for the realization of the absolute volt. The quantum Hall effect would subsequently provide a means of realizing the absolute ohm directly from a physical phenomenon. With this development, and the development of Josephson junction arrays, absolute volt and ohm standards would become commonplace.

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[243] F. K. Harris, H. A. Fowler, and P. T. Olsen, "Accurate Hamon-Pair Potentiometer for Josephson Frequency-to-Voltage Measurements," *Metrologia* 6 (1970): 134-142; Annual Report for 1970: 43-44.

[244] T. F. Finnegan and A. Denenstein, "High Accuracy Potentiometers for Use With Ten Millivolt Josephson Devices. I. Double Series-Parallel Exchange Comparator," *Review of Scientific Instruments* 44 (1973): 944-953.

[245] B. F. Field, T .F. Finnegan, and J. Toots, "Volt Maintenance at NBS via $2e/h$: A New Definition of the NBS Volt," *Metrologia* 9 (1973): 155-166; "NBS to Maintain U.S. Legal Volt Using $2e/h$ Via the AC Josephson Effect," *Technical News Bulletin* 56 (1972):159-160.

[246] W. G. Eicke, Jr., and B. N. Taylor, "Summary of International Comparisons of As-Maintained Units of Voltage and Values of $2e/h$," *IEEE Transactions on Instrumentation and Measurement* IM-21, (1972): 316-319.

Temperature

The International Practical Temperature Scale (IPTS) is the practical scale in general use.[247] It is based on certain reference temperatures or "fixed points" whose assigned temperatures are as close as possible to the true, thermodynamic values, with interpolation formulae between the fixed points. The Consultative Committee on Thermometry (CCT), in its capacity of technical advisor to the CIPM, revises the IPTS periodically—about every 20 years. The CCT meets at the BIPM at intervals of 2 or 3 years, evaluates the recent advances in thermometric metrology and recommends new or revised R&D at the world's national (or other participating) laboratories. Each delegate is an expert representing a national standards laboratory or is otherwise coopted by the CCT. The 20-year effort of research, evaluation and scale formulation culminates in a consensus recommendation to the CIPM.

In the IBS, responsibility for IPTS activities fell to the Heat Division and its relevant sections: Temperature, Radiation Thermometry (later Optical Radiation) and Cryogenic Physics. Director Astin would appoint the division chief as the NBS delegate to a CCT meeting and, on occasion, one (or even two) colleagues would go along. The division had traditionally made a major contribution to IPTS development, but so extensive was the program during this particular interval that BIPM Director Jean C. Terrien took the unprecedented step of holding the 1967 meeting of the CCT in Washington, so that the committee as a whole could examine the NBS work at first hand, prior to finalizing the formulation of IPTS-68.[248]

In a somewhat more theoretical vein, Robert A. Kamper and his associates in the Cryogenics Division developed a scheme for the measurement of very low temperatures.[249] Based on the Josephson effect, the principle of the measurement was that any noise in the voltage applied to a Josephson junction will cause a random frequency modulation of the oscillation of the junction. If the noise is Johnson noise, thermally generated in a shunt resistor through which a current is passed to bias the junction, the resistor noise voltage is easily calculated and is directly proportional to the product of the absolute temperature and the resistance. The noise voltage causes a broadening of the Josephson oscillation, and the authors showed that the resulting line width is also proportional to the absolute temperature. Thus, if the size of the resistor is known, the temperature is easily calculated since the other quantities appearing in the equations are well-known fundamental constants.

Using a dilution refrigerator, temperatures of about 0.075 K were easily measured. Later the lower limit was extended to 0.006 K by Robert Soulen of the Cryogenic Physics Section, and it was estimated that it could be extended to temperatures somewhat below 1 mK.

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[247] T. J. Quinn, *Temperature* (New York: Academic Press, 1983); Chapter 2.

[248] C. R. Barber in "The International Practical Temperature Scale of 1968," *Metrologia* 5 (1969): 35-44.

[249] A. H. Silver, J. E. Zimmerman, and R. A. Kamper, "Contribution of Thermal Noise to the Line Width of Josephson Radiation From Superconducting Point Contact," *Applied Physics Letters* 11 (1967): 209-211; R. A. Kamper and J. E. Zimmerman, "Noise Thermometry With the Josephson Effect," *Journal of Applied Physics* 43 (1971): 132-136; R. A. Kamper, J. D. Siegwarth, R. Radebaugh, and J. E. Zimmerman, "Observation of Noise Temperature in the Millikelvin Range," *Proceedings of the IEEE* 59 (1971): 1368-1369; "NBS Advances New Concept in Low-Temperature Measurement," *Technical News Bulletin* 52 (1968): 71-72.

Lewis O. Mullen (left) monitored temperature during the fabrication of a thin-film Josephson junction while Robert A. Kamper (center) and Donald B. Sullivan observed. A study by these NBS scientists led to the first observation of Josephson oscillation in the radiofrequency range using thin films.

Radio Standards

The development of radio-frequency standards for electrical measurements continued. In many cases the eventual utility of new measurement capabilities was in calibrations—for new quantities or for older quantities in new ranges of frequency, power, voltage, etc. A short list of the new calibrations offered during the period gives a feeling for the developments.

- The frequency range of the radio-frequency pulse power reference standard was increased to include frequencies for the range 300 MHz to 500 MHz, augmenting the 900 MHz to 1200 MHz range in which calibrations had been offered. The power range involved was 0.2 mW to 2 kW with an uncertainty of 3 percent. (1965)
- A calibration service for noise in the frequency range 12.4 GHz to 18.0 GHz was offered. (1965)
- A calibration service for the measurement of the reflection coefficient magnitude in a waveguide at 3.95 GHz to 5.85 GHz was developed. Interlaboratory wave guide standards with reflection coefficient magnitudes from 0.024 to 0.2 could be measured and compared. (1966)
- A pair of precision co-axial thermal noise generators were developed as primary standards for noise in the high frequency ranges. At the time both operated at 30 MHz or 60 MHz, but could be adjusted to operate at other frequencies and over a wide range of noise temperatures. (1969)
- Perhaps the instrument that caused the most excitement in the period was the Bolovac—for bolometric voltage and current—standard head. Built around the bolometric properties of a thin film in the form of a split disk, the

Myron C. Selby held the bolometric voltage and current (Bolovac) standard which he invented in 1967 to provide known voltages and currents at high and microwave frequencies to 20 000 MHz.

instrument could be used to measure voltage, current and power from 1 MHz to 20 GHz. The range of voltage and current were 0.05 V to 10 V and 5 mA to 10 A respectively. The secret of its success was that the resistance of the film was the same at all frequencies as at dc and had a frequency-independent temperature coefficient. Hence, upon simple calibration with dc, the Bolovac could be used at all frequencies, and it was easier to use and much more accurate than traditional methods. (1968)

- A calibration service for pulse rise time was begun during the period. It used a commercially available oscilloscope as the principal element of the system. The oscilloscope was evaluated in the frequency domain, and from this study it was possible to calculate its time-domain response. The service was primarily designed for tunnel diode step generators and two-port passive devices. (1970)

Toward a New Era in Radiometry

So far in this history we have discussed the work of the Bureau in five of the six basic measurement standards: mass, length, time, temperature, and the ampere. We have not discussed standards for radiometry and its sibling photometry, that are embodied in the SI unit, the candela. Because this branch of physics is somewhat esoteric, we now provide a short historical note on its development. It is an opportune time to do this, for, as we shall see, the sixties and early seventies were times in which the Bureau's activities in the field increased.

Radiometry can be defined as the detection and measurement of the energy flux carried by electromagnetic radiation, which is a physical measurement. By contrast, photometry is the study of light and its brightness as perceived by humans. As such, it is not a physical measurement, but has been called "psychophysical." Photometry is, nevertheless, very important, and the whole subject arose from studies devoted to it. Here we discuss the more general but conceptually simpler subject of radiometry.

The scientific development of radiometry began with the study of the visible effects of radiation, or photometry.[250] Using liquid-in-glass thermometers to assess the relative energy[251] content of different colors of light in the continuous solar spectrum, Friedrich W. Herschel in 1800 discovered infrared radiation and, a year later, Johann W. Ritter, studying the chemical effect of light on silver nitrate solutions, discovered ultraviolet radiation. In a sense, this was the beginning of radiometry.

The nineteenth century was a period of considerable development for radiometry, and it became one of the main thrusts of physics during that period. Aside from the pull of science, the development was spurred substantially by the development of gas

[250] Jon Geist, "Trends in the Development of Radiometry," *Optical Engineering* 15 (1976): 537-540.

[251] These early studies were carried out before the equivalence of heat and energy had been demonstrated. Thus the studies at the time were concerned with the "heat content" of radiation. We shall continue to use the modern terminology.

lighting and subsequently electric lights, and a quest for better photometric standards than the standard candles which were used earlier. Perley G. Nutting in his book *Outlines of Applied Optics* writes, "The service of radiometry to other branches of applied optics lies largely in the study of such reference standards and the determination of the constants of the radiation formulas."[252]

The quest for standards took two paths: the study of radiation sources and the study of radiation detectors. In the former direction, the concept of a blackbody was developed in mid-century, along with the realization that, at least in principle, such a body operated at a constant temperature could be used as a standard source of radiation. Of course, the crowning achievement of the work in this direction was the discovery by Max Planck in 1900 of the radiation law that bears his name, thereby setting into motion work that would lead to quantum mechanics and a revolution in physics. As is now well known, the law gives the radiated energy flux from a blackbody per unit area as a function of wavelength and temperature, and an integration over wavelength gives the total flux radiated per unit area as a constant multiplied by T^4, a relation that is embodied in the Stefan-Boltzmann radiation law. Planck's equation contains only one adjustable parameter, the constant h that bears his name, but for the purposes of radiometry, there are considered to be two constants, c_1 and c_2, which are combinations of h with the speed of light and Boltzmann's constant k. Thus, if these constants were well enough known, if the thermodynamic temperature of the blackbody were well known and could be controlled with sufficient accuracy, and if one could construct a device approximately satisfying the definition of a blackbody (a "laboratory blackbody"), one would have all the requisites for a primary basic measurement standard for both photometric and radiometric purposes. The principal quantities for which the Planck blackbody standard could in principle be used are spectral radiance (watts/m^2 · solid angle · wavelength interval), radiance (watts/m^2 · solid angle) and radiant exitance, also called irradiance, (watts/m^2). Unfortunately, it was not until much later that the constants in the Planck radiation law were well enough known to permit the calculation of these quantities with less than 1 percent uncertainty. Nevertheless, as we shall see, the laboratory blackbody became the primary standard of photometry and radiometry.

The other direction that radiation science took was that of detector development. This proved to be a fruitful area. Sensitive thermocouples and thermopiles for the measurement of radiation intensity were developed, and later the yet more sensitive bolometer. But the most important development in detector radiometry was the development and construction by Anders Jonas Ångström in 1893 of the first standard detector, an electrically calibrated bolometer. In this instrument, the steady-state temperature rise produced at the detector by a radiation source was first measured. The detector was then heated by an electric current to the same steady-state temperature. Assuming that the heat losses were the same, the power in the two cases would also be the same, and since the dissipated electrical power is very easy to measure with high accuracy, the radiant power is also measured. Such an instrument would in due course be called an "absolute radiometer" but, as we shall see, the quantitative determination of the uncertainties in its use is not a trivial task. Nevertheless, research in this new technique flourished in the early years of the century.

[252] Perley G. Nutting, *Outlines of Applied Optics* (Philadelphia: Blakiston's, 1912): 201.

The Bureau, born in the same year that Planck published his paper on his radiation law, became deeply involved in photometric standards from the beginning. It adopted the British candle as the unit of luminous intensity (visible flux per unit solid angle) but chose to maintain it by carbon-filament incandescent lamps calibrated by the Physikalisch-Technische Reichsanstalt (PTR) against their primary standard Hefner (amyl acetate) lamp, and corrected for the known relationship of that unit to the British unit.[253]

Rather more important for radiometry was the work of William W. Coblentz, who followed on the work of Ångström on electrically compensated detectors, but using a very sensitive bismuth-silver thermopile. Coblentz used the instrument to measure the radiation from a Hefner lamp, a standard sperm candle, and carbon-filament incandescent lamps. He found the last to be the best as secondary standards, writing "such a lamp has every desideratum of a standard of radiation, when calibrated against a blackbody as the primary standard of radiation."[254] In a later paper he investigated the bismuth-silver thermopile extensively, using it in thirteen different receivers. He obtained an inaccuracy of the order of 1 percent, and came to the conclusion that "one can consider the present device a primary instrument for evaluating radiant energy in absolute measure."[255]

Of course, to complete the chain he had to know the constants in the Planck and Stefan-Boltzmann laws, so he set out on perhaps his best known work: the measurement of the Stefan-Boltzmann constant. For this study he used an electrically compensated bolometer to measure the radiant flux from a blackbody. The value he obtained, using 11 differently prepared receivers in 304 measurements, was 5.72×10^{-12} watt cm^{-2} deg^{-4}. The accuracy was about 1 percent.[256] Due to an unfortunate coincidence, this figure agreed to within 0.1% with the value of the Stefan-Boltzmann constant as calculated from what was soon discovered to be an erroneous set of values for the fundamental constants. It would not be until 1970 that the error in the measured quantity would be reduced to less than 1 percent.

In the next fifty years, radiometry and photometry made some advances. The laboratory blackbody became recognized as the primary standard of optical radiation, many new types of sources were developed, and new detectors—particularly photoelectric—were developed. But as a branch of physics, radiometry lagged and assumed more of a supportive role in such emerging fields as atmospheric physics, in the development of infrared and ultraviolet spectrophotometers, and in military and space applications of radiation.

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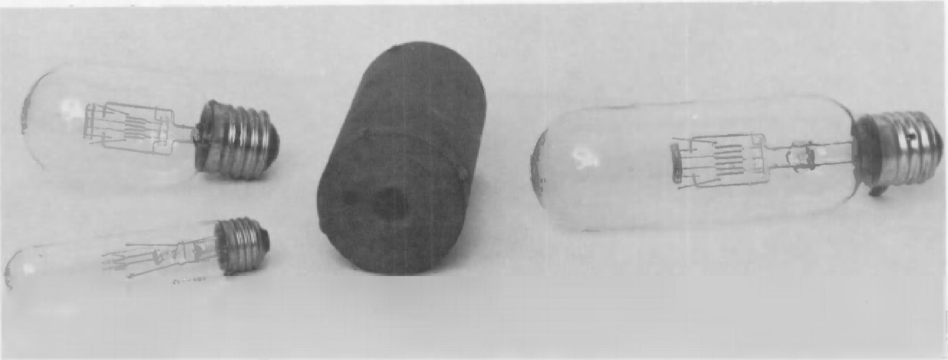
[253] Two excellent papers illustrate the Bureau's photometric work at the time. They are: E. P. Hyde, "A Comparison of the Unit of Luminous Intensity of the United States With Those of Germany, England, and France," *Bulletin of the Bureau of Standards* 3 (1907): 65-80; E. B. Rosa and G. W. Middlekauff, "Carbon Filament Lamps as Photometric Standards," *Proceedings of the American Institute of Electrical Engineers* 29 (1910): 1191-1205.

[254] W. W. Coblentz, "Measurements on Standards of Radiation in Absolute Value," *Bulletin of the Bureau of Standards* 11 (1915): 92.

[255] W. W. Coblentz and W. B. Emerson, "Studies of Instruments for Measuring Radiant Energy in Absolute Value: An Absolute Thermopile," *Bulletin of the Bureau of Standards* 12 (1915-1916): 550.

[256] W. W. Coblentz, "Present Status of the Determination of the Constant of Total Radiation From a Black Body," *Bulletin of the Bureau of Standards* 12 (1915-1916): 577.

Beginning in 1960 things began to change. In the area of sources, the Bureau introduced a tungsten-strip lamp as a new standard of spectral radiance for the wavelength range from the near ultraviolet to the infrared.[257] The announcing publication also described the construction and use of a blackbody for calibration of the lamp. Three years later another lamp was introduced, covering the same wavelength range but with higher power and radiant efficiency, to be used as a new standard for spectral irradiance.[258] This was calibrated by the tungsten-strip lamp just described, so that it too was indirectly calibrated against a blackbody. Finally, with this kind of source, in 1966 the Bureau issued another tungsten-filament lamp to be used as a standard of total irradiance.[259] The standard of total irradiance had been a 50 W carbon-filament lamp, used since Coblentz calibrated it against a laboratory blackbody in 1913. Now the need for higher accuracy and wider irradiance range required developing new standards, and tungsten-filament lamps of 100 W, 500 W, and 1000 W were chosen for the new standards. Thus, with the issuance of these three new types of lamps, new radiometric standards were made available. In addition to these new lamp standards, the reader will recall the synchrotron which is an almost ideal source of radiation in the vacuum ultraviolet. In addition to this technique were developments in the hydrogen arc which permitted the introduction of the deuterium arc as a transfer standard of spectral radiance and irradiance, which extended the range of calibrations to the vacuum ultraviolet.



Tungsten-filament lamp standards adopted by NBS in 1966. These standards increased the accuracy and range of irradiance measurements beyond those possible with the previously used carbon-filament lamps. The blackbody (center) was used to calibrate the lamps.

[257] R. Stair, R. G. Johnston, and E. W. Halbach, "Standard of Spectral Radiance for the Region of 0.25 to 2.6 Microns," *Journal of Research of the National Bureau of Standards* 64A (1960): 291-296.

[258] R. Stair, W. E. Schneider, and J. K. Jackson, "A New Standard of Spectral Irradiance," *Applied Optics* 2 (1963): 1151-1154.

[259] R. Stair, W. E. Schneider, and W. B. Fussell, "The New Tungsten Filament Lamp Standards of Total Irradiance," *Applied Optics* 6 (1967): 101-105.

Driven by the needs of the space program and laser calibration was the field of detector-based radiometry, and this led to the development of electrically calibrated radiometers for "traditional photometric and radiometric applications. . . solar radiation . . . and laser applications." In addition, a "long standing discrepancy. . . between the measured and calculated values of the Stefan-Boltzmann constant has been resolved,"[260] so that experiment and theory now agreed to within approximately 0.1 percent.[261] From the Bureau came work in two directions. First there was the work of Jon Geist which we will summarize here.[262] Second, there was the work of a group in the Electromagnetics Division in Boulder using a pyroelectric detector, to measure the power of laser beams.[263]

The work of Geist was part of a larger IBS program initiated in 1968 on optical radiation measurements. In Technical Note 594-1, Geist describes in detail the design and construction of an electrically calibrated thermopile-type radiometer, and an exhaustive analysis of the magnitudes of the various errors that could enter into the design. As we have seen, the radiometric scales obtained with such an instrument have traditionally been called "absolute" although, as pointed out by Geist, they are no more absolute than scales obtained by calibration against a blackbody, either directly or indirectly. But the latter types of measurements depend on the knowledge of the constants in the Planck and Stefan-Boltzmann radiation laws, upon the knowledge of the thermodynamic temperature (no small task at 1000 K or higher), and the experimental realization of a blackbody. The thermopile-type of measurements in principle require only the measurement of temperature rise and electric power, both of which are much simpler and far more accurate measurements.

Numerous errors can, of course, creep into the construction of an electrically calibrated instrument. To determine these errors and their effects, Geist identified two philosophies in the design and construction of measuring instruments. The first philosophy attempts to minimize all errors, while the second does not necessarily minimize the magnitude of the errors, but rather minimizes the uncertainty with which they can be measured, and then corrects for them. Geist argues that an instrument built to the second philosophy is more useful for the realization of measurement scales than one built on the first philosophy, which is more suited for the transfer of scales. As a result, the second philosophy guided the effort. In short, Geist set out to build a primary-standard radiometer.

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[260] Geist, "Trends in the Development of Radiometry": 538.

[261] W. R. Blevin and W. J. Brown, "A Precise Measurement of the Stefan-Boltzmann Constant," *Metrologia* 7 (1971): 15-29; J. M. Kendall, Sr., and C. M. Berdahl, "Two Blackbody Radiometers of High Accuracy," *Applied Optics* 9 (1970): 1082-1091.

[262] Jon Geist, *Optical Radiation Measurements: Fundamental Principles of Absolute Radiometry and the Philosophy of This NBS Program (1968 to 1971)*, Natl. Bur. Stand. (U.S.) Technical Note 594-1; June 1972.

[263] C. A. Hamilton, G. W. Day, and R. J. Phelan, Jr., *An Electrically Calibrated Pyroelectric Radiometer System*, Natl. Bur. Stand. (U.S.) Technical Note 678; March 1976.

We can only give a sketchy account of the instrument, its analysis, and its use. Basically it consisted of a receiver disk upon whose upper side the radiation flux to be measured fell. This side was coated with some type of high-radiation-absorptance coating, i.e., some type of blackening. Attached to the disk was a thermopile of circular construction consisting of copper-constantan thermocouples in a radial arrangement. Also attached to the disk was a resistance heater to measure the power necessary to raise the receiver disk to the same temperature as did the radiation. To provide a stable thermal environment, the entire assembly was placed in an isothermal chamber with an aperture whose dimensions were accurately known, and through which the radiation fell on the receiver surface. The whole assembly, including the chamber, measured 12.5 cm × 8.3 cm × 5 cm.

There are a set of "easy" errors, and a set of "hard" errors in the instrument. The measurements with "easy" errors were:

- The voltage across the thermopile
- The voltage across the heater
- The current in the heater
- The area of the aperture.

The measurements or conditions with "hard" errors were:

- Not all of the incident radiation is absorbed by the high-absorbtion coating
- Not all of the heater power is absorbed by the receiver
- The power generated in the heater leads modifies the temperature distribution in the radiometer
- The incident flux also modifies the temperature distribution in the chamber
- The temperature distribution in the receiver and high-absorptance coating is different when receiving radiation from that when being electrically heated.

All the easy errors involve only well-known customary measurements that are made routinely and accurately in any well-equipped laboratory. The "hard" errors are quite different. To determine their magnitude required first an exhaustive mathematical analysis of the physical processes involved and their sensitivity to errors. And the determination of their magnitudes and the uncertainties in the magnitudes required painstaking experimentation.

One of the radiometers, ECR-10 (for Electrically Compensated Receiver), was used as a pyrheliometer (i.e., for the measurement of solar radiation) in the Third International Pyrheliometer Comparison held at Davos, Switzerland, in September 1970. Interestingly, it was compared against Ångström 210, built on the Ångström design.[264] The results showed that the ratio of results with Ångström 210 was 1.0180 with a limit of error of ± 0.0063, or 0.63 percent. While this level of uncertainty was somewhat disappointing, it was felt that with some serious effort the error limits could be lowered to 0.1 percent.

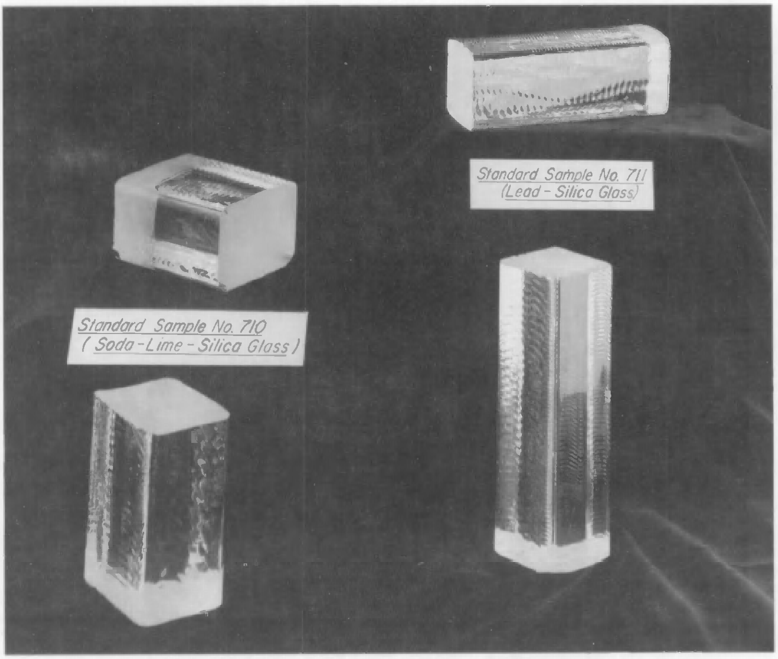
[264] As a footnote of historical interest, Jon Geist informed us that one of the pyrheliometers used in the comparisons was actually built by Ångström.

Standard Reference Materials (SRMs)

Production and sales of SRMs continued during the period. By 1968, 669 different kinds were available and the total sales numbered approximately 43 000 units. Indeed, there were requests for far more kinds of SRMs than could be developed and produced, and the newly formed Office of Standard Reference Materials had to set priorities on which of the requested materials were chosen for preparation. A few of the newer materials, culled from the Annual Reports, were:

- Lead isotopic composition standards were designed for the calibration of mass spectrometers used in the determination of the isotopic composition of lead in rocks, meteorites, and ores to determine their ages. Along the same line were natural-ratio isotopic composition standards for chlorine, bromine, copper, silver, and chromium. These were produced as part of a program on atomic weights. (1968)

- A second viscosity-of-glass standard was made available for use in the calibration of the viscosity-measuring instruments used in glass production. (1965)



NBS glass-viscosity standards are used to calibrate glass viscometers, particularly those utilized in high-speed mass production processes. Viscosity must be held to close tolerances to obtain glass products of uniform thickness, shape, and strength.

- Glass beads for neutron flux measurements were prepared from three different glasses enriched in boron-10, along with one of several activators that is potentially radioactive via neutron absorption. These materials were developed as a straightforward way to determine slow-neutron flux. Dysprosium glass and indium glass materials are alternate absolute standards, while cobalt glass is a secondary, or transfer, standard. (1965)
- A freezing-point SRM meeting all of the requirements of a fixed point on the International Practical Temperature Scale was prepared from zinc with a purity of 99.9999 percent. The zinc point replaced the sulfur point on the IPTS, and important step in the maintenance of the temperature scale. (1967)

Perhaps the most exciting development was the production of standards for use in clinical laboratories that would lead the Bureau into the whole new field of clinical standards. The clinical SRM program began by using only its own funds, but was soon given generous support from the National Institute for General Medical Sciences of the National Institutes of Health.

In the early sixties, clinical tests—mostly carried out on automated equipment—were a large and growing activity. It was estimated that 750 million to 1 billion tests were run annually, and the rate was increasing at 10 percent to 15 percent per year.[265] But all was not well with these measurements. In 1963, the College of American Pathologists (CAP) had conducted a national survey of over 1000 clinical laboratories on the analysis of cholesterol. The laboratories were sent two cholesterol solutions for analysis, and the results were disturbing. They showed variations ranging from 25 percent to 50 percent,[266] caused largely by the "systemic bias not only in a given method, but also between methods."[267] Standards were available for instrument calibration, but the CAP concluded that they varied in purity and were not suitable.[268]

As result of this survey, the CAP and the American Association of Clinical Chemistry (AACC), following a meeting of many associations interested in high-purity cholesterol,[269] approached the Bureau about producing a standard sample of cholesterol of certified purity. It was expected that the program would extend beyond cholesterol. It was a classic case of the need for a standard sample to bring a measurement system under control.

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[265] "Clinical and Biomedical Standards," *Technical News Bulletin* 53 (1969): 92-94.

[266] Annual Report for 1968: 30.

[267] More exactly, in 1949 the precision of laboratory measurements was ±23 %. By 1969, two years after the introduction of the Bureau's cholesterol SRM, the precision was ±18.5 %. By 1986, after the introduction in 1981 of a serum cholesterol SRM, the precision was ±6 %. (Figures provided by Harry S. Hertz.)

[268] "Clinical and Biomedical Standards": 92.

[269] D. S. Young and T. W. Mears, "Measurement and Standard Reference Materials in Clinical Chemistry," *Clinical Chemistry* 14 (1968): 929-943.

By December 1967, the cholesterol SRM was completed and sales began. It consisted of a material obtained from Distillation Products Industries of Rochester, New York, where the natural product was purified according to the method of Louis F. Fieser,[270] as set forth in the joint AACC and CAP specifications. The purity determination, carried out under the direction of Robert Schaffer and by far the hardest part of the project, was done by gas chromatography; thin-layer chromatography; and mass, infrared, and nuclear-magnetic-resonance spectrometry.[271] The SRM was certified as being 99.4 ± 0.3 percent pure. Later, other methods were used to assay the samples, but it was still a long job.[272]

Cholesterol was only the first of a long line of clinical SRMs. By 1969 three other materials had joined it: urea, uric acid, and creatinine. By 1979 there were thirty, including organics, metal organics, and inorganics. The Bureau had become the national center for standards for clinical laboratories.

* * *

In 1970, the Bureau announced the availability of a new class of materials. Named Research Materials (RMs), they were designed to provide research workers in materials science with materials of very high uniformity so that researchers in different laboratories could all be assured of working on material of the same composition. What made RMs different from SRMs was that the Bureau did not certify any property for the former, as it did for the latter. Unlike SRMs, Research Materials were not designed to calibrate measuring instruments, but rather to make the results of research more meaningful for having been achieved on a constant, uniform material, even though such quantities as impurity level and perfection were not necessarily well known. While the Bureau did not provide a certificate with an RM, it did provide a "Report of Investigation," generally a scientific paper, with the accuracy of its results the sole responsibility of the author.

The first RM offered was ultra-high purity aluminum. Offered in both polycrystalline rods (25.4 mm × 4.17 mm) and single-crystal cubes (approximately 1 cm on a side), the material had an extremely high resistivity ratio (resistance at 273 K divided by the resistance at 4.2 K), denoting extremely high purity. The impurity level was estimated to be 0.25 parts per million molar.

Never big sellers, by 1979, the Bureau offered eight RMs, including a homogeneous river sediment for "testing radiochemical procedures for the assay of radioactivity in sediments or soils," and albacore tuna for use in determining elements present in trace concentration.[273]

[270] L. F. Fieser, "Cholesterol and Companions VII. Steroid Dibromides," *Journal of the American Chemical Society* 75 (1953): 5421-5422.

[271] Young and Mears, "Measurement and Standard Reference Materials in Clinical Chemistry": 939.

[272] R. W. Seward and R. Mavrodineanu, *Standard Reference Materials: Summary of the Clinical Laboratory Standards Issued by the National Bureau of Standards,* Natl. Bur. Stand. (U.S.) Special Publication 260-71; November 1981.

[273] *NBS Standard Reference Materials Catalog, 1979-80 Edition,* Natl. Bur. Stand. (U.S.) Special Publication 260; 1979: 77.

Standard Reference Data

The Bureau, at the behest of the Federal Council for Science and Technology (FCST) in 1963, and in 1968 under the Standard Reference Data Act, became responsible for the operation of the National Standard Reference Data System (NSRDS). The program never became as large as optimistically projected at its inception.[274] Thus, in 1966, the Bureau projected the program to grow from $1.2 million in 1966 to $20 million in 1972, with $8 million to be spent in-house, and $12 million to be contracted out.[275] Reality was much harsher; the FY 1972 appropriations were approximately $2.1 million.[276] The program nevertheless flourished and by the time the NSRDS legislation was passed, it was producing a steady stream of data compilations. Appropriations were growing, albeit at a rate that euphemistically could only be called leisurely. But other agencies, and the Bureau's RTS appropriation coupled to reprogramming, helped. And the data centers, both inside and outside the Bureau, had other sources of funds.

By 1970 there were thirty-four established data centers associated with the NSRDS, fifteen of them at the Bureau. In addition, individual "one-shot" projects for special data compilations or critical reviews were supported. Some of the data centers outside the Bureau received no funding from the Office of Standard Reference Data (OSRD), but nevertheless were associated with the system, as had originally been envisioned by the Bureau and the FCST.

The whole technical program was divided into seven areas: (1) Thermodynamic and Transport Properties, (2) Atomic and Molecular Data, (3) Chemical Kinetics, (4) Solid State Data, (5) Nuclear Data, (6) Colloid and Surface Properties, and (7) Mechanical Properties. In addition to these areas, research on the use of computers for data processing and dissemination was carried out.

In response to needs for data for industry and for national defense—and to some extent for historical reasons—the heaviest concentrations of data were found in thermodynamic and transport properties, with eleven data centers (two from the Bureau), and in atomic and molecular data with nine centers (seven from the Bureau). The least emphasized areas, at that time, were colloid and surface properties with one center, and mechanical properties with none.

[274] *Critical Evaluation of Data in the Physical Sciences—A Status Report on the National Standard Reference Data System, June 1970,* David R. Lide, Jr., ed., Natl. Bur. Stand. (U. S.) Technical Note 553; September 1970.

[275] Appropriations Hearings for 1967: 674.

[276] House Committee on Appropriations, Subcommittee on Departments of State, Justice, and Commerce, the Judiciary, and Related Agencies, *Departments of State, Justice, and Commerce, the Judiciary, and Related Agencies Appropriations for 1972: Hearings Before a Subcommittee of the Committee on Appropriations,* 92d Cong., 1st sess., National Bureau of Standards, 20 April 1971:1150. These figures represent only those funds controlled by the OSRD. The actual expenditures for data evaluation were much higher, because of support from other agencies and from RTS funds of the NBS technical divisions.

We cannot possibly describe all the work carried out. In thermodynamics and transport properties alone, seventeen projects were actively pursued in 1970, and fifteen monographs published between 1968 and 1970. We can at least list a few of the projects and centers drawn from the areas enumerated above to give a feeling for the type of work supported by the NSRDS.

- Selected values of chemical thermodynamic properties, Donald D. Wagman and William H. Evans, Physical Chemistry Division, NBS.
- Thermal conductivity of selected materials, Y. S. Touloukian, Thermophysical Properties Research Center, Purdue University.
- Thermodynamic data on organic compounds, Bruno J. Zwolinski, Texas A&M University.
- Atomic Energy Levels Data Center, William C. Martin, Charlotte Moore Sitterly, Optical Physics Division, NBS.
- Atomic Collision Cross Section Information Center, Lee J. Kieffer, JILA, NBS/University of Colorado.
- Chemical Kinetics Data Center, David Garvin, Physical Chemistry Division, NBS.
- Critical evaluation of the gas phase reaction kinetics of the hydroxyl radical, William E. Wilson, Jr., Battelle Memorial Institute.
- Crystal Data Center, Helen M. Ondik, Inorganic Materials Division, NBS.
- Superconductive Materials Data Center, Benjamin W. Roberts, General Electric Research and Development Center.
- Diffusion in Metals Data Center, John R. Manning, Metallurgy Division, NBS.
- Photonuclear data, Everett G. Fuller, Linac Radiation Division, NBS.
- Electrochemical properties of interfaces, Johannes Lyklema, Agricultural University of Wageningen, The Netherlands.

It can be said that by 1970 the NSRDS was an established part of the Bureau's activities and the Nation's scientific infrastructure.

Weights and Measures for the States

In 1832, Secretary of the Treasury Lewis McLane, in the course of his coinage and customhouse functions, directed Ferdinand R. Hassler, his superintendent of the Coast Survey, to prepare copies of a set of standards that Hassler had recommended in reports he had prepared at the secretary's request. These were standards of length, weight, and volume derived from British standards, and in due course they were adopted by the Treasury. Congress, recognizing the value of uniformity in weights and measures, in an 1836 joint resolution directed the secretary to deliver copies of Hassler's standards "to the governor of each State in the Union . . . to the end that a uniform standard of weights and measures may be established throughout the United States." These were the first nationwide standards in the country, and were quickly adopted by the states as their legal standards, although the Congress had not legalized them. In 1864, Great Britain authorized the use of the metric system along with its own imperial system, and Congress, following suit in 1866, legalized the metric system in the United States. In that same year, the Congress, in another joint resolution, directed the secretary of the treasury to furnish each state with a set of metric weights and measures standards. By 1880 this had been largely accomplished and practically all states had weights and measures standards.[277]

But circumstances in the states had changed. As Allen Astin testified in 1965:

> "Since that time the States that have become members of the Union have acquired standards by one way or another. We provided them for the two most recent States, Alaska and Hawaii, but many of the States have lost their standards and none of them has standards adequate to meet the demands of standards metrology, the standards of measurement."[278]

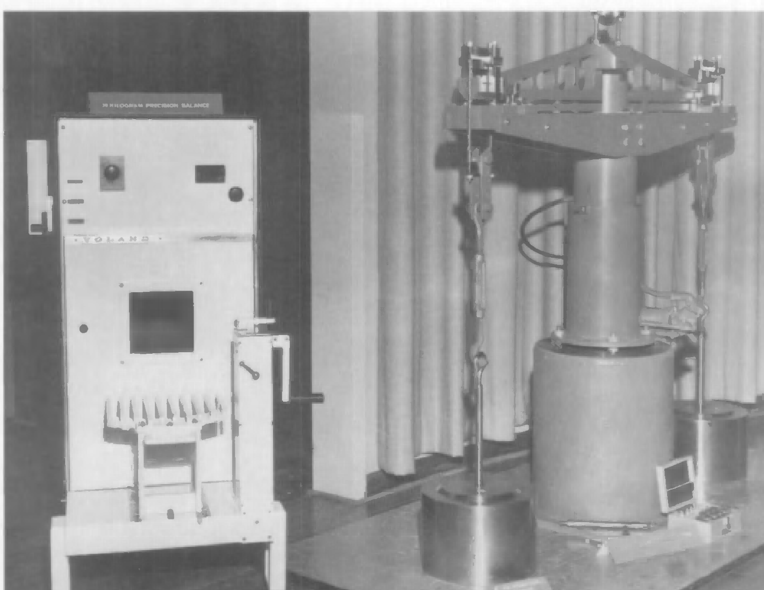
Customary standards furnished to the states under the Joint Resolution of 1836.

[277] See MFP, 24-28 for an account of this early history, and MFP, 515-526 for a biography of F. R. Hassler.

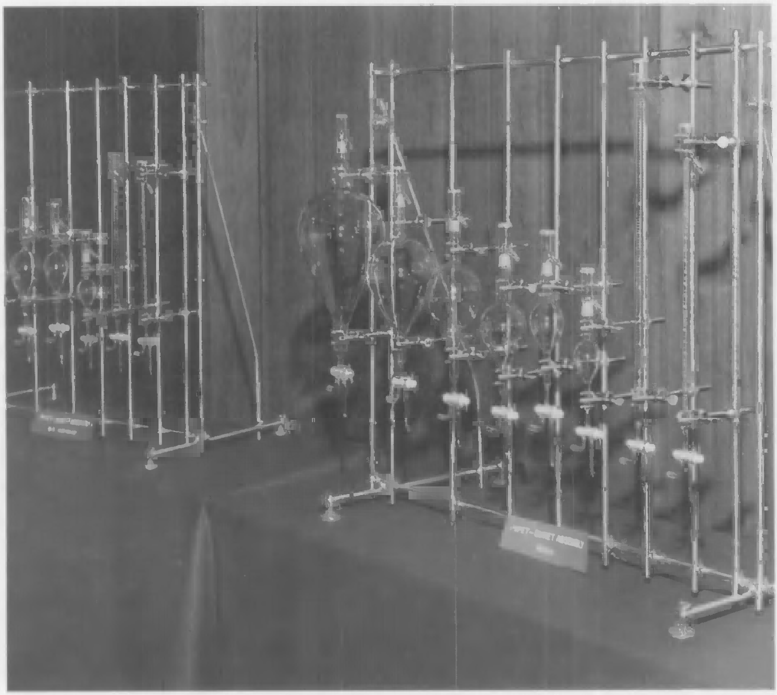
[278] Appropriations Hearings for 1966: 494-495.

A 13-year program provided working weights and measures laboratories with uniform and accurate standards in all of the 53 States and territories. The 95-piece set of weights, measures, and weighing instruments are standards in both the U.S. customary system and the metric system.

The State of Maryland received its set of weights and measures on April 3, 1970 in the Bureau's Red Audtorium.



The 30 kilogram precision balance, the 2500 pound precision balance, and the 500 pound mass standard received by the State of Maryland.



Sixteen volume standards, including 12 pipets and 4 burets, were part of the set received by the State of Maryland.

A portion of the 67 mass standards (metric and avoirdupois) presented to the State of Maryland are shown here.

He therefore requested $500 000 to begin a program of constructing and distributing standards for the states, and providing training in their use. It was expected that each set of standards, plus the associated training, would cost about $40 000, and that the $500 000 would be enough to equip about one-fourth of the states. After a modicum of discussion about why the states could not pay for the standards, and being reassured that free distribution would help attain national uniformity plus the fact that each of the states could be expected to create a new or expanded weights and measures laboratory and qualified personnel, the Congress appropriated the funds necessary to start the program.[279]

Each state was to receive a complete set of stainless steel weights in both metric and customary units, from 30 kilograms to 1 milligram, and from 50 pounds to 0.000 001 pound, plus two 500 pound stacking weights; various graduated tapes and rules and a 16-foot-long bench equipped with a precision microscope and tension weights; volumetric standards from 5 liters to 1 milliliter, and from 5 gallons to 1 minim (0.003 76 cubic inches); and five precision balances from 100 gram capacity to 5000 pound capacity. All were, of course, to be calibrated or adjusted by the Bureau before distribution.

The schedule, which called for equipping ten states per year and completion of the program in a few years, could not be maintained, nor could the $40 000 price. By 1973, 40 states had been equipped and the price had doubled, partly because the specifications had been raised. The complete equipping of the 53 states and territories was not completed until 1978. But upon its completion it could be said that for the first time in its history the Nation was equipped to provide complete uniformity in weights and measures, and all measurements in trade and commerce were at least in principle traceable to the national standards maintained by the Bureau.

Federal Information Processing Standards

It will be recalled that under the Brooks Act (PL 89-306), the Bureau, through the secretary of commerce, was given responsibility "to make appropriate recommendations to the President relating to the establishment of uniform Federal automatic data processing standards." The task of fulfilling this responsibility, along with the others spelled out in the legislation, was given to the newly formed Center for Computer Sciences and Technology. Under policy guidance from the Bureau of the Budget, the program of the center was separated into four elements: Advisory and Consulting Services, Standards, Research, and Computer Services. Standards was by far the largest program, spending approximately $4.24 million out of a total of $9.35 million in the period FY 1965 to FY 1971.[280]

[279] "Ten States to Receive New Standards," *Technical News Bulletin* 50 (1966): 181; "Three States Receive New Weights and Measures Standards," *Technical News Bulletin* 52 (1968): 30-31; "Three States Receive Weights and Measures Standards," *Dimensions* 57 (1973): 191; and "For Good Measure," *Dimensions* 62 (11) (1978): 2-7. The last article gives a good, short historical account of the history of weights and measures distribution, and of the relationship of the states, who have the responsibility to enforce weights and measures in the marketplace, with the Bureau, which has the custody of the national standards.

[280] Center for Computer Sciences and Technology, "Brooks Bill Issue Study of the National Bureau of Standards," NBS Report 10608, September 1971.

To implement its responsibilities, the Bureau divided the CCST standards area into four categories: (1) Hardware standards, including such items as character recognition, interchange codes and media, transmission, interface, and keyboards; (2) Software standards, including programming languages and operating systems; (3) Applications standards; and (4) Data standards, including representation of data elements and codes, and formats.[281] Also, because these standards had to be used in all agencies of the Federal Government, the Bureau recognized that it needed to "coordinate its activities on an interagency basis." For this purpose it formed a series of ad-hoc task groups designed to provide advice to the Bureau on specific standards, to make recommendations on specific problems, and to develop draft proposals.[282] The chairs of these task groups in turn were collected into an Advisory Committee, for which the chair was the chief of the Office of Information Processing Standards. Other members included representatives from the BOB and General Services Administration (GSA), the other main agencies besides the Bureau having functions under the Brooks Act, and other persons as necessary.

With policy guidance from the BOB, a new publication series called the Federal Information Processing Standards Publications or, more commonly, FIPS PUBS, was initiated. These publications were to be used in the promulgation of standards, for establishing new standards, and in clarifying existing ones. The Bureau was also directed to maintain a FIPS PUBS Register, which was the "official source within the Federal Government for information pertaining to the approval, implementation, and maintenance of Federal Information Processing Standards. . . ."[283]

Because the development of these standards required coordination with the myriad interested parties both in the Government and in the private sector, standards did not come immediately. On March 11, 1968—some two and one-half years after the enactment of the Brooks Act—three automatic data processing (or FIPS) standards were approved by President Johnson. The most important of the three was the first, which established the USA Standard Code for Information Interchange (USASCII), which consisted of 128 seven-bit binary numbers used to represent 32 control characters and 96 symbols—letters, numbers, punctuation marks, and the other customary symbols used in the English language. The whole was referred to as a "coded character set."

Developed in an effort begun in 1960 by the American Standards Association (ASA), now the American National Standards Institute (ANSI), USASCII was recommended by the International Standards Organization (ISO) and the International Telegraph and Telephone Consultative Committee (CCITT). All computers procured by the Federal Government after July 1, 1969, had to be capable of using USASCII. As is well-known to all users of PC word processors, the ASCII set, as it is now known, is based on the eight-bit binary, or 256 characters. Those beyond 128 are often called

[281] *General Description of the Federal Information Processing Standards Register,* Natl. Bur. Stand. (U.S.) Federal Information Processing Standards Publication 0; November 1, 1968: 3.

[282] "FIPS Coordinating and Advisory Committee," *Technical News Bulletin* 53 (1969): 185-187.

[283] *General Description of the Federal Information Processing Standards Register:* 1-2.

the "extended ASCII character set." Also approved at the same time were standards for recording the code on magnetic and paper tape. With this capability, all future computers in the Federal Government were expected to be able to communicate with one another.[284]

By 1971, six more standards had been issued, including such items as calendar dates, metropolitan statistical areas, and state name abbreviations. These were developed through the program the BOB had developed in response to its own responsibilities under the Brooks Act. Six more were approved on June 18, 1971, by the director of the Office of Management and Budget (OMB), the successor of the BOB. These six were technically based standards developed under the Bureau's program.[285] By Executive Order 11717, on May 9, 1973, the responsibilities of OMB with respect to Government-wide data processing standards were transferred to the secretary of commerce.[286]

The General Research

Superconducting Semiconductors

Approximately half the program of the Institute for Basic Standards was concerned with the measurement of the physical properties of well-defined substances.[287] Due to its close association with the temperature scale, IBS had assumed responsibility for many thermodynamic and transport properties. This kind of work was carried out in the Heat Division and the Solid State Physics Section of the Atomic Physics Division, among others. Thus it was not surprising that a joint study of superconductivity in semiconductors should take place between workers from these two organizational units.

The possibility that semiconductors might exhibit superconductivity had intrigued scientists for some time, and in two 1964 papers, Marvin L. Cohen of the University of California, Berkeley, analyzed the problem in great detail.[288] He showed that under appropriate conditions—high charge carrier density, large electron effective mass, high dielectric constant, strong interaction between electrons and lattice vibrations—semiconductors could indeed be expected to show superconductivity.

[284] "President Approves Federal ADP Standards: USACII to Extend Compatibility," *Technical News Bulletin* 52 (1968): 173.

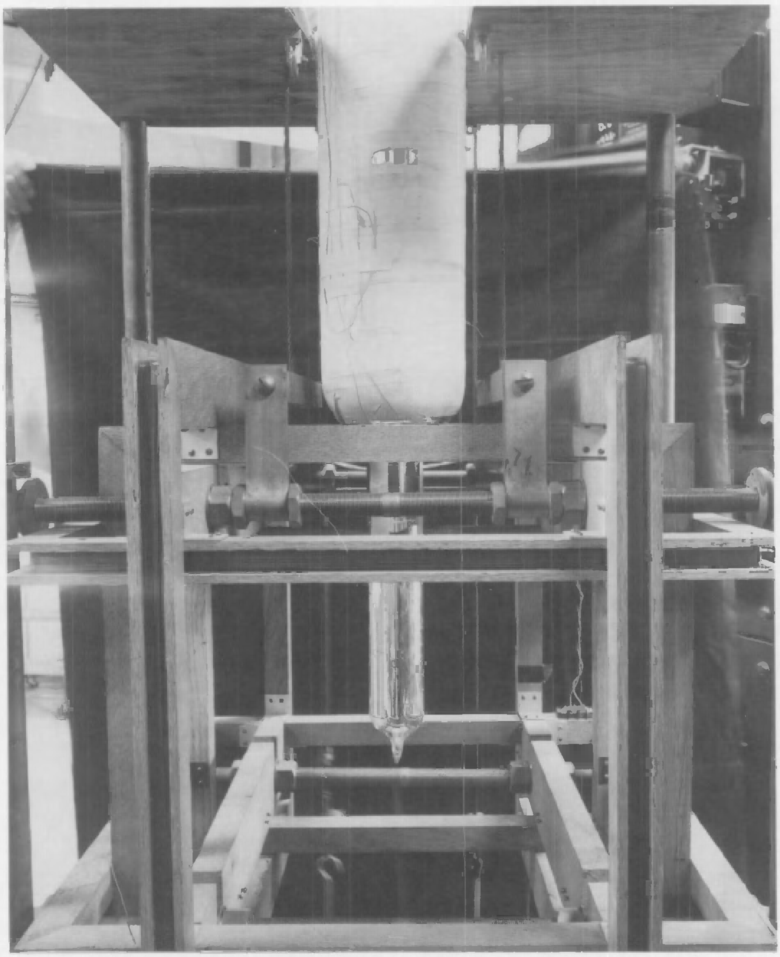
[285] Center for Computer Sciences and Technology, "Brooks Bill Issue Study of the National Bureau of Standards": VI.6-VI.9.

[286] Executive Order 11717, May 9, 1973. *Code of Federal Regulations,* Title 3, 1971-1975 Compilation: 766-768.

[287] Annual Report for 1968: 61.

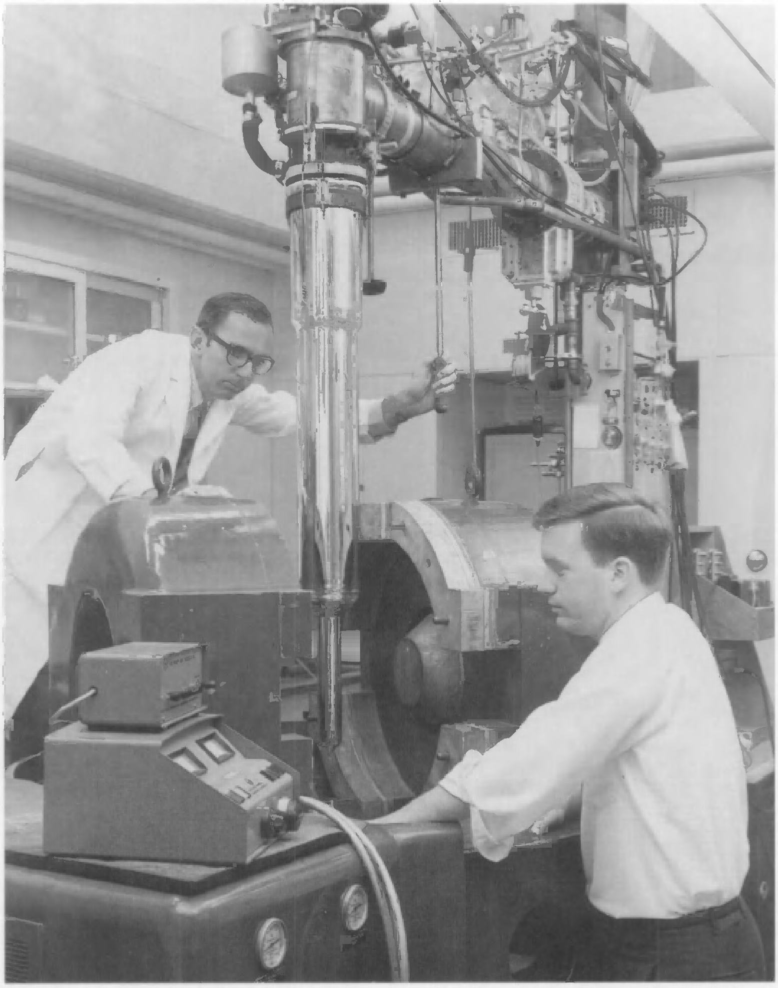
[288] M. L. Cohen, "Superconductivity in Many-Valley Semiconductors and in Semimetals," *Physical Review* 134 (1964): A511-A521; M. L. Cohen, "The Existence of a Superconducting State in Semiconductors," *Reviews of Modern Physics* 36 (1964): 240-243.

Very quickly, and almost simultaneously, groups from the Naval Research Laboratory (NRL) and the Bureau reacted. At the NRL the work was with germanium tellurides of several different compositions, and the material showed superconductivity below about 0.3 K.[289] At the Bureau, James F. Schooley of the Cryogenic Physics Section and William R. Hosler of the Solid State Physics Section, with the collaboration of Cohen, worked with the semiconductor strontium titanate, a material



Using the apparatus formerly employed in the parity experiments, a University of California/NBS group found that superconductivity could occur in oxide semiconductors. The group included Marvin L. Cohen, professor of physics at UC; his student Calvin S. Koonce; Hans P. R. Frederikse and William R. Hosler of the NBS Solid State Physics Section; and James F. Schooley, Ernest Ambler, Jack H. Colwell, and Earl R. Pfeiffer of the NBS Cryogenic Physics Section.

[289] R. A. Hein, J. W. Gibson, R. Mazelsky, R. C. Miller, and J. K. Hulm, "Superconductivity in Germanium Telluride," *Physical Review Letters* 12 (1964): 320-322.



Earl R. Pfeiffer (left) and Calvin S. Koonce positioned a Dewar inside a magnet in preparation for an experiment to determine the superconducting transition temperature of strontium titanate as a function of conduction electron concentration.

with which the Solid State Physics Section had a great deal of experience. An oxide semiconductor, it had the advantage that the number of carriers could be controlled by reduction, a relatively easy process of heating the material in either a vacuum oven or in the presence of hydrogen. Single crystals were available and remained intact during the processing.

The material did indeed show superconductivity between 0.2 K and 0.3 K. Moreover, experiments on the Meissner effect (the property of a superconductor that causes it to exclude all magnetic fields from its interior) showed that the material was a type II or "hard" superconductor.[290] Later, more extensive experiments were carried out. Samples covering carrier densities over a three-decade range showed that, as the carrier density was increased, the transition temperature rose, reached a maximum at a carrier density of 10^{21} per cubic centimeter, and then decreased again. The transition temperature covered was from 0.05 K to 0.30 K. Its behavior followed that predicted theoretically by Cohen and his student Calvin S. Koonce.[291] Strontium titanate was the first known oxide superconducting semiconductor. But it was a single oxide. Had there been leisure to try multiple oxides, the high-transition-temperature superconductor revolution that occurred in the mid-eighties might have occurred some twenty years earlier.

Electron Scattering From Nuclei

The Bureau's linear electron accelerator was accepted from the contractor in October 1965. One of the most natural things to do with the powerful 150 MeV accelerator was to study the atomic nucleus by electron scattering, and the time for such studies was propitious. In 1970 Samuel Penner, of the Linac Radiation Division, wrote:

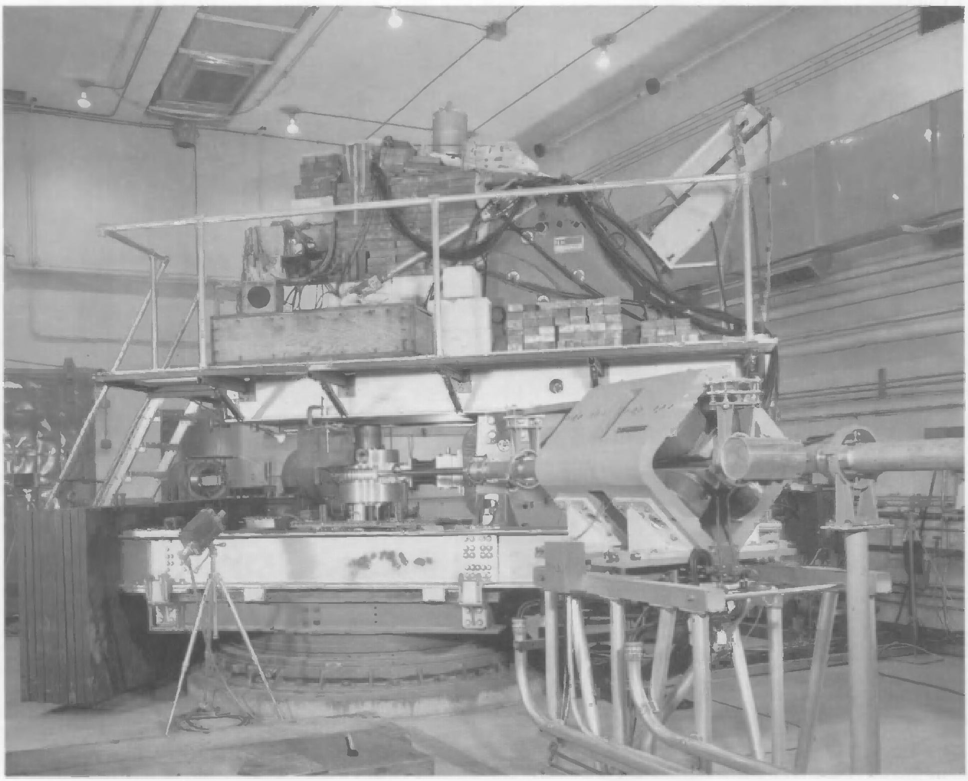
> For many years it has been said that electron scattering is a powerful tool for the study of nuclear structure, because quantum electrodynamics is a very accurate theory and because the weakness of the electromagnetic interaction allows accurate theoretical interpretation of experimental results. In spite of these advantages, electron scattering has in the past contributed little to our understanding of the nucleus, in contrast to the vast amount of information obtained by reaction studies employing nuclear particles (protons, alphas, etc.). This situation has changed greatly in recent times, and electron-scattering is at last proving its great value as an experimental technique for the study of nuclear properties.
>
> As a result of recent improvements in experimental techniques and apparatus we are now able to perform detailed and accurate experiments which yield valuable information on nuclear structure....
>
> The main reasons that we are now able to perform experiments of this quality are: (1) the development of the modern electron linear accelerator...; (2) better understanding of the principles of beam transport...; (3) improvements in the design and construction of magnetic spectrometers...; (4) the development of multi-channel "ladder" detector systems; (5) improved methods of beam current monitoring; and (6) the use of on-line computer systems.... [292]

[290] J. F. Schooley, W. R. Hosler, and M. L. Cohen, "Superconductivity in Semiconducting $SrTiO_3$," *Physical Review Letters* 12 (1964): 474-475.

[291] J. F. Schooley, W. R. Hosler, E. Ambler, J. H. Becker, M. L. Cohen, and C. S. Koonce, "Dependence of the Superconducting Transition Temperature on Carrier Concentration in Semiconducting $SrTiO_3$," *Physical Review Letters* 14 (1965): 305-307; "Superconducting Transition Temperatures of Strontium Titanate Semiconductor," *Technical News Bulletin* 52 (1968): 48-49.

[292] Samuel Penner, *Experimental Techniques for Electron Scattering Investigations,* Natl. Bur. Stand. (U.S.) Technical Note 523; April 1970: 1-2.

Electron scattering can indeed provide basic information on nuclear structure. From elastic scattering studies comes information about nuclear sizes and shapes, and from inelastic scattering comes information about the energy level structure of nuclei, including the spin, parity, and transition strength of the excited nuclear states. But before the LINAC beam could be used for any of this work, it was necessary to build a spectrometer required to study atomic spectra and obtain information about atomic states. Such a spectrometer was built by Samuel Penner, John W. Lightbody, Jr., and Sherman P. Fivozinsky of the Linac Radiation Division. In a typical experiment, the beam from the LINAC was impinged on a target of the material to be studied, and the scattered electrons passed through a solid-angle-defining slit. They then passed through a momentum-selecting bending and focusing spectrometer magnet, and were finally detected by a focal-plane array of small, solid-state detectors. Operated under computer control, the final output of such measurements was a plot of the number of scattered electrons per unit of energy per incident electron vs. the energy of the scattered electron for a given scattering angle or momentum transfer. The energy resolution of the whole instrument was 0.08 percent—sufficient for high-quality work.



The NBS electron scattering spectrometer.

In collaboration with guest workers from MIT, the University of Maryland, Catholic University, American University, the University of Massachusetts, Virginia Polytechnic Institute, and the Laboratoire de l'Accélerateure Linéaire, Orsay, France, quite a number of different nuclei were studied. In all cases, the main object was to compare experimental results with the predictions of theory. The Bureau leaders in the studies were Lightbody, Penner, and Fivozinsky. Thus, in one of the first experiments, form factors for the excitation of low-lying states of O^{16} were determined.[293] Another of the points of interest was the comparison of the giant resonance in C^{12} with that in the isotope C^{13}, with the result that "the addition of a neutron to the even-even C^{12} system results in a major restructuring of the giant resonance strength."[294] Other studies in this fruitful area were concerned with the electron scattering sum rule for C^{12} (1970); elastic scattering from Ti^{46}, Ti^{48}, Ti^{50}, (1971); electron scattering for one- and two-phonon vibrational states (1972); electron scattering from Zn isotopes (1972); quadrupole and hexadecapole deformation parameters of Sm^{152} (and other heavy deformed nuclei) by electron scattering (1972); electron scattering studies of vibrator-spectrum nuclei: Cr^{52}, Pd^{116}, Cd^{114}, Sn^{116} (1972); electron scattering from low-lying states in C^{14}, (1972); low q^2 electron scattering from the 15.109 MeV state of C^{12} and conserved-vector-current test (1973); and electron scattering from F^{19} and Ca^{40} (1973). An outpouring of basic research in nuclear physics came from the new instrument.

Making the Draft Lottery Impartial

The first draft lottery since World War II was held in December 1969, and questions were raised about its impartiality, or randomness. To ensure that a second lottery, planned for July 1, 1970, would be impartial, the Selective Service System (SSS) asked the Bureau to provide it with sets of twenty-five "random calendars" and twenty-five random permutations of the numbers 1 to 365.[295] These calendars and permutations were to be used to determine the priority order in which youths born on a given day in 1951 would be drafted. One of the random calendars would be used to determine the sequence in which dates would be entered into 365 red capsules, and one of the random permutations would determine the order in which numbers used to determine the priority, or "rank," were placed into 365 green capsules. A second permutation would determine the order in which the red and green capsules were

[293] J. C. Bergstrom, W. Bertozzi, S. Kowalski, X. K. Maruyama, J. W. Lightbody, Jr., S. P. Fivozinsky, and S. Penner, "Electroexcitation of the Low-Lying States of O^{16}," *Physical Review Letters* 24 (1970): 152-155.

[294] J. W. Lightbody, Jr., and S. Penner, "Electroexcitation of the C^{12} Giant Resonance," *Physical Review Letters* 24 (1970): 274-276; J. C. Bergstrom, H. Crannell, F. J. Kline, J. T. O'Brien, J. W. Lightbody, Jr., and S. P. Fivozinsky, "Electroexcitation of the Giant Resonance of ^{13}C, *Physical Review C* 4 (1971): 1514-1532. Giant resonances are excitations of the nucleus that occur with great strength in a narrow energy region. Usually studied by excitation with gamma radiation, they can also be studied by inelastic electron scattering. The nucleus can lose its excitation by emitting a gamma ray, or, in some cases, a particle. Three types of giant resonances are known: electric dipole, magnetic dipole, and electric quadrupole.

[295] J. R. Rosenblatt and J. J. Filliben, "Randomization and the Draft Lottery," *Science* 171 (1971): 306-308; "NBS Provides Random Tables for Use in Draft Lottery," *Technical News Bulletin* 54 (1970): 196-197.

loaded into two drums from which drawings would alternately be made. This procedure would give the order in which men, based on their birthdays, would be called up in the draft. No bias was to be allowed to enter the system.

The mathematical problem was to generate fifty random permutations of the numbers 1 to 365, twenty-five to be used for calendars, and twenty-five to be used for ranking.[296] These could have been generated on a computer, but there already existed thirty-eight tables of random permutation of numbers 1 to 500, and twenty tables for numbers 1 to 1000.[297] Aside from saving work, use of these published tables had the added advantage of permitting the work to be reproduced. Cutting off the first tables at 365 yielded thirty-eight tables. By cutting the second tables into one set for numbers 1 to 365, and a second set for numbers 501 to 865, forty permutations 1 to 365 were formed. This gave seventy-eight permutations of numbers 1 to 365.[298] These had to be cut down to fifty tables.

To do this, two sets of the seventy-eight permutation tables were prepared, one from which the calendars would come, and the other for the ranking permutation. Then numbers between 1 and 78 were selected by the throws of three dice, thrown by three members of an advisory panel for the project, and referenced to other existing permutation tables from Moses and Oakford's *Tables of Random Permutations*. These again were random permutations, but now of numbers from 1 to 78. The adopted protocol was that if the number was between 1 and 25, the priority permutation for that number would be chosen, and the calendar permutation discarded. For numbers from 26 to 50, the opposite would be done. For numbers from 51 to 78, both calendar and priority permutations with those numbers would be discarded. In this way, twenty-five calendars and twenty-five priority permutations were chosen, each one sealed in an unmarked envelope, and delivered to the SSS, the Bureau's work having been accomplished. As near as mathematics could determine, everything was totally random and impartial.

On June 29, two days before the actual drawing, an official lottery witness chose one of the sealed envelopes containing random calendars. He chose the one that contained calendar number 53, and the dates of the year were placed into 365 red capsules in the sequence of dates for this calendar. Similarly, one of the twenty-five rank, or priority, envelopes was chosen. It contained permutation number 43, and priorities 1 to 365 were placed into 365 green capsules in the order of permutation number 43. Finally, to complete the process of preparation for the drawing, another envelope from the twenty-four remaining "priority" envelopes was chosen. It contained permutation number 45, and both the calendar and priority capsules were placed in their respective drums in the order of this permutation. It was about as random a process as could be imagined.

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296 The random calendar is nothing but a random permutation with a date attached to each number in the permutation. Thus, the problems of producing a permutation or a calendar are the same.

297 Lincoln E. Moses and Robert V. Oakford, *Tables of Random Permutations* (Stanford, California: Stanford University Press, 1963).

298 The reader may be interested to know that there are approximately 2.51×10^{778} such sets. The Bureau used only seventy-eight of them.

On July 1, 1970, with dignitaries and the press present, the drawing took place.[299] The calendar drum rotated for an hour, but the priority drum malfunctioned and rotated for only a half hour. Nevertheless, subsequent analysis showed that the mixing was successful. The first date chosen was number 259, or September 16, with a rank of 139. It was looked upon as a successful job for all concerned.

Research at the Reactor

On December 7, 1967, at 3:55 in the afternoon, the Bureau's reactor, sporting its new acronym NBSR, achieved criticality, but was not ready for routine use.[300]

Staff of the Bureau's research reactor await celebration of the moment of criticality, which was achieved at 3:55 p.m. on December 7, 1967. Standing left to right in the foreground were the reactor's designers: Harry Landon, Carl Muehlhause, and Robert S. Carter. Seated in the foreground were Ray Meschke (at table) and Tawfik Raby (right). Standing left to right against the back wall were John Ring, James Knight, Nathan Bickford, and four Atomic Energy Commission officials.

[299] An apocryphal story relates that when it was announced that the procedure had been developed by the National Bureau of Standards, a large number of the press left. When asked why, one of the reporters said, "Well, if they're the guys who did this then nothing is going to go wrong, and that's not news."

[300] "NBS Reactor Achieves Criticality," *Technical News Bulletin* 52 (1968): 50.

Before such use, the reactor had to undergo extensive testing and mapping of its operating characteristics, the effect of loading and core configuration on the flux in the beam tubes had to be determined, and maps of the flux in the core had to be made so that fuel life could be estimated accurately. With these and other tests carried out, NBSR went into full 10 MW service in late summer, 1969.[301]

It had been planned that the NBS reactor would be a national facility, available to scientists from Government, industry and universities. In many cases, this would lead to cooperative work with scientists on the staff of the Bureau's Reactor Radiation Division. Thus the division had staff of two kinds: scientific researchers, and engineering and maintenance personnel. The former carried out their own research programs while the latter bore the division's responsibility to operate and maintain the reactor. The research staff actively sought out collaboration with research workers within and outside the Bureau, particularly those new to research on the reactor and not aware of its outside capabilities. These people often required instruction in working with neutron beams. Formal long-term interagency arrangements were sometimes made under which other-agency scientists were actually stationed at the reactor. One of the first such agreements was made in September 1970 with the local Naval Ordanance Laboratory (NOL), and later another was made with the Picatinny Arsenal when an army reactor in Watertown, Massachusetts, was shut down.[302] Finally, workers from other Bureau divisions used the reactor directly rather than in collaboration with reactor personnel. Prominent among these last were scientists from the nuclear physics programs of the Center for Radiation Research, and others from the Analytical Chemistry Division who routinely irradiated samples for neutron activation analysis and who, in fact, had their own laboratory in the Reactor Building. In its first year of operation, a total of twenty-five workers from other Bureau divisions spent sixteen man-years working at the reactor, either alone or cooperatively with reactor scientists. From outside the Bureau, thirty-two scientists spent twelve man-years of effort at the NBSR. It was a pace that would increase rapidly in later years.

By August 1970, experimental equipment at the reactor included four neutron and x-ray diffractometers, and a crystal time-of-flight instrument to be used in inelastic scattering studies. Under development was a triple-axis spectrometer for crystal-dynamics studies. A high-volume cold-neutron source that used D_2O, heavy ice, was under development. It was completed in 1987 and used until 1994. A new cold source using liquid hydrogen was introduced in 1995. All the diffractometers were under the control of a single mini-computer, which operated on a time-shared basis.

A great deal of crystal structure work was carried out by combining x-ray and neutron-diffraction techniques in situations where the neutron's ability to "see" light elements was essential to the structure determination. Thus, the crystal structure of a number of complex metal organics and simpler molecules like phosphonium bromide

[301] *Reactor Radiation Division: Annual Progress Report for the Period Ending October 31, 1970,* Robert S. Carter, ed., Natl. Bur. Stand. (U.S.) Technical Note 567; March 1971; "Research at NBS Reactor," *Technical News Bulletin* 54 (1970): 174-176.

[302] "Cooperative Program Engages NBS Reactor," *Technical News Bulletin* 54 (1970): 157.

were worked out. Inelastic scattering was used in the study of the diffusion and modes of vibration of hydrogen in transition-metal hydrides, and in the study of the rotator phase in *n*-alkanes, specifically *n*-nonadecane ($C_{19}H_{40}$).

Liquids and amorphous solids were not neglected. The structure factor—from which the radial distribution function could be calculated—in liquid neon was determined to compare with theoretical calculations. Similarly, liquid aluminum was studied to determine the pair distribution function, and a program to study the structure of glassy solids was initiated. A great advantage of neutrons is that the neutron possesses a magnetic moment and hence can be scattered by other magnetic moments, Thus, in collaboration with workers from NOL, studies were carried out on rare-earth iron garnets, transparent magnetic fluorides such as $RbNiF_3$ and $CsFeF_3$, and on cubic praseodymium compounds.

As already mentioned, other divisions of the Bureau were also users of the reactor facilities. One of the major users was the Activation Analysis Section of the Analytical Chemistry Division. For this section, the reactor was simply a source with which to irradiate their samples with neutrons of the proper energy, thus converting impurities or minor elements in their samples to radioactive species, so that subsequent measurements of the radioactivity would permit the determination of their concentration. The method is exceedingly sensitive. And the reactor was not the only neutron source used. Others were the 3 MeV and 14 MeV neutron generators in the CRR, and the LINAC for photon activation analysis.

Other nonreactor-division users were concerned with basic nuclear physics. Thus, there was a search for weak parity-violating interactions in the nucleon interactions—a program carried out in collaboration with scientists at Harvard University and Brookhaven National Laboratory. Other nuclear physics investigations concerned two-photon emission in (n,p) capture, decay characteristics of krypton isotopes, and nuclear orientation studies on Br^{82}.

Finally, other organizations turned to the reactor for more prosaic—but not unimportant—work. In July 1970 the Bureau entered into an agreement with the Post Office Department, Internal Revenue Service, U.S. Geological Survey, Federal Bureau of Investigation, and the Food and Drug Administration for the use of the reactor for neutron-activation analysis. The bulk of the work would be for forensic purposes. Each of the agencies was lavish in praising the NBS reactor facility and the generous and knowledgable assistance of the CRR staff.[303]

Critical Phenomena

In 1954, Melville Green, a young theorist working on the statistical mechanics of fluids, was hired by the Heat Division. In keeping with Astin's desire to emphasize theoretical work, the division in 1960 created a section primarily concerned with theory called Statistical Physics and appointed Green as its chief. One of Green's principal interests at the time was the generalization of kinetic theory to dense gases, i.e., gases at high temperature.[304]

[303] *Reactor Radiation Division: Annual Progress Report for the Period Ending October 31, 1970*: 48-55.

[304] Interviews with Johanna M. H. L. Sengers and Jan V. Sengers.

Melville S. Green came to the Bureau in 1954 and from 1960 to 1968 was the first chief of the Heat Division's Statistical Physics Section. Phenomena at the critical point was the primary interest of this section.

It was perhaps inevitable that any group concerned with phenomena—particularly in fluids—at extremes of temperature and pressure should become concerned with phenomena near and at the critical point, where properties change rapidly and anomalously. For Green and his section, phenomena at the critical point, or more briefly "critical phenomena," became the dominant interest.

The early sixties was an advantageous time to become involved with critical phenomena. Both theoretical and experimental investigations were discovering new information and converging towards new concepts. The predictions of classical "mean-field" theories for fluids (Johannes van der Waals), magnetism (Pierre Weiss), and order-disorder in alloys (William Lawrence Bragg, Evan J. Williams), which had been developed in the early half of the 20th century, and had many features in common, were shown to be quantitatively incorrect near the critical points of all these different systems. These mean-field theories, for example, predicted finite values for the heat capacity at constant volume (fluids) or at constant field (magnetic materials). Experimentally, however, it was found in both these systems that the heat capacity shows a lambda-like divergence, as had already been observed in liquid helium at the superfluid transition. Such logarithmic divergences are of great interest to theorists and would have led to excitement even without the rigorous solution by Lars Onsager in 1942 of the Ising model in two dimensions, which showed such a logarithmic divergence for the critical-point heat capacity in magnetic materials.[305]

Further, near a critical point, fluids and magnetic materials develop long-range fluctuations which lead to the phenomenon of critical opalescence, once studied by Einstein and more precisely described by Leonard S. Ornstein and Frits Zernike. Green was one of the first to warn that the classical Ornstein-Zernike theory might be incorrect, and later experiments near the consolute (critical) point of partially miscible binary liquids proved him correct.

Perhaps the most exciting aspect of the field was the close analogy displayed by so many very dissimilar systems.[306] The shape of the coexistence curve of a fluid, or of partially miscible binary liquids near a critical point is, for instance, very like that of the curve of spontaneous magnetization of an uniaxial magnetic substance near its Curie point. Both of these shapes could be expressed by so-called power laws, connecting the difference of coexisting densities of fluids, coexisting compositions in binary liquids, and magnetization in magnetic materials, with a power of the temperature difference from the critical point. The exponent turned out to be the same in all these cases, but it did not have the mean-field value. Rather, the experimental values were reasonably close to those calculated for the three-dimensional Ising model at that time.

Analogous behavior was observed for other properties, such as compressibility in the fluid and susceptibility in the magnet. The power law, or critical exponent for this property, is different from that for heat capacity, but it is the same in different systems. Although in principle many different critical exponents could be defined, they are not all independent. They have been shown to obey equalities called scaling laws, so that only two of them are independent.

With the rapid growth of theoretical understanding of critical phenomena and a flood of new experiments throughout the world producing new results, it was a time of creative excitement in the Statistical Physics Section similar to that felt in the Free Radicals Program. It was spurred by Green, and spilled over to other sections of the Heat Division, such as the Equation of State Section under Joseph Hilsenrath, and to other divisions, such as the Polymers Division, where the study of critical opalescence in polystyrene-cyclohexane solutions was begun.[307]

Green saw that this ferment, both within and outside the Bureau, made the time auspicious for a conference on critical phenomena, particularly since much of the work in the area was carried out by workers in different fields who normally did not interact.

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[305] Melville S. Green, "Introduction" in *Critical Phenomena: Proceedings of a Conference Held in Washington, D.C., April 1965*, M. S. Green and J. V. Sengers, eds. Natl. Bur. Stand. (U.S.) Miscellaneous Publication 273; December 1, 1966: ix-xi.

[306] J. V. Sengers and J. M. H. Levelt Sengers, "The Critical Region," *Chemical and Engineering News* 46, (June 10, 1968):104-118.

[307] D. McIntyre, A. Wims, and M. S. Green, "Critical Opalescence of Polystyrene Solutions," *Journal of Chemical Physics* 37 (1962): 3019-3021.

Green organized and held the Conference on Phenomena in the Neighborhood of Critical Points at the Bureau from April 5 to April 8, 1965.[308] Attended by about 170 scientists, including George E. Uhlenbeck, previous president of the American Physical Society, and Nobel laureates Peter J. Debye and Chen N. Yang, the conference had sessions on equilibrium critical phenomena in fluids, critical phenomena in ferromagnets and antiferromagnets, logarithmic singularities, elastic and inelastic scattering, and transport and relaxation phenomena. An extra session on the last day of the conference discussed new ideas stimulated by the deliberations. It is generally recognized that this event—the first conference on critical phenomena—was important in developing this field of basic science. It forcefully impressed the idea of critical-point universality on the international audience. And it gave a boost to this field which, in 1982, culminated in the Nobel Prize for Kenneth Wilson of Cornell University. In addition, it spurred new directions for study, such as wetting and interfaces, liquid-crystal phase transitions, fractals, turbulence, and chaos.

After the conference, Jan V. Sengers, co-editor of the conference proceedings and a member of Green's section, initiated his work on aspects of critical opalescence, and the dynamics of critical behavior, with members of the Polymers Division, work which continued after he left NBS to join the University of Maryland in 1968. His wife, Johanna (Anneke) Levelt Sengers, a member of the Equation of State Section but, in her own words "drawn into Green's orbit," published a seminal paper with guest scientist Matilde Vicentini-Missoni and Green on the form of the nonclassical scaling laws for the behavior of fluids near critical points.[309] Raymond Mountain, in Green's section, wrote a paper that is now a classic on the spectrum of scattered light near the fluid critical point.[310]

In 1968, Green left the Bureau for a post at Temple University. The legacy he left, however, lasts to this day. In the Heat Division practical applications of the theory of critical phenomena to fluids of industrial importance, such as steam, ethylene, and carbon dioxide, contributed to making the Bureau a world-renowned center of expertise on new applications such as phase separation, chemistry and chromatography in fluid mixtures. This practical expertise coexisted with fundamental investigations,[311] such as the proof that fluid critical exponents truly have Ising values, and that the nonclassical critical exponent for the viscosity is really the same in fluids and fluid mixtures.[312] Later work (late eighties and early nineties) in the Polymers Division on the dynamics of phase transitions in polymer mixtures is also a legacy of the work begun by Green.

[308] M. S. Green and J. V. Sengers, *Critical Phenomena.*

[309] M. S. Green, M. Vicentini-Missoni, and J. M. H. Levelt Sengers, "Scaling-Law Equation of State for Gases in the Critical Region," *Physical Review Letters* 18 (1967): 1113-1117; M. Vicentini-Missoni, J. M. H. Levelt Sengers, and M. S. Green, "Scaling Analysis of Thermodynamic Properties in the Critical Region of Fluids," *Journal of Research of the National Bureau of Standards* 73A (1969): 563-583.

[310] R. D. Mountain, "Spectral Distribution of Scattered Light in a Simple Fluid," *Reviews of Modern Physics* 38 (1966): 205-214.

[311] R. Hocken and M. R. Moldover, "Ising Critical Exponents in Real Fluids: An Experiment," *Physical Review Letters* 37 (1976): 29-32; S. C. Greer, "Coexistence Curves at Liquid-Liquid Critical Points: Ising Exponents and Extended Scaling," *Physical Review A* 14 (1976): 1770-1780.

[312] R. F. Berg and M. R. Moldover, "Critical Exponent for Viscosity," *Physical Review A* 42 (1990): 7183-7186.

Fracture Mechanics Comes to the Bureau

In December 1963 Sheldon M. Wiederhorn, a chemical engineer trained at Columbia and the University of Illinois, and who had then studied ceramics at DuPont, joined the Inorganic Solids Division. John J. Gilman had recently shown how the study of the fracture of solids could be used to determine their surface energy,[313] and what happened next is best told in Wiederhorn's own words:

> Being newly employed at the National Bureau of Standards (NBS), I was searching for an experiment on the mechanical behavior of solids that would fit into a program on ceramic science . . . [Gilman's] method of characterizing fracture behavior of single crystals appealed to me, as it was new and dealt with a fundamental property of materials. Therefore, I designed equipment to duplicate Gilman's experimental technique. The equipment was built but never used, for while awaiting its construction, I had the idea that launched me into my investigations on the fracture of glass.
>
> I realized that glass fracture could be studied if a method were devised to guide a crack along the midplane of a glass plate, so that double cantilever beam specimens of the type used by Gilman could be made. . . . In my first experiment I scratched the midplane of a microscope slide with a diamond scribe and found to my delight that the scratch could be used both to introduce a crack and to guide it once introduced. Initial experiments on precracked microscope slides indicated that, at about one-half the load for immediate failure, cracks in glass moved in a slow and controllable manner. This observation was in apparent contradiction to the Griffith criterion for fracture, which predicted a critical stress for spontaneous failure.[314]

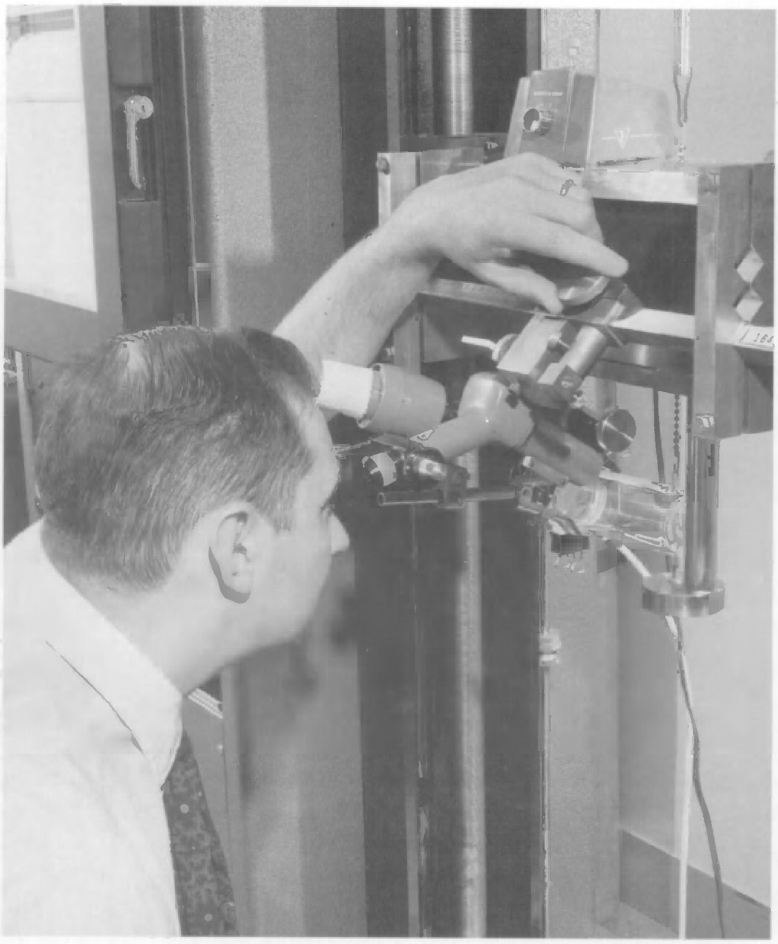
Wiederhorn did not yet know that delayed failure in glass, a portion of which he had just observed and which he would elucidate thoroughly, was well recognized and called "static fatigue."

Thus began a series of experiments that was to provide new insight into the fracture of glass and other brittle materials. Using microscope slides as specimens, and readily available laboratory equipment, he studied the motion of cracks as a function of load. Having learned from papers by Richard J. Charles and by William B. Hillig[315] that the fracture of glass was a kinetic process controlled by water in the atmosphere, he did a series of experiments at different atmospheric humidities, measuring the velocity of

[313] J. J. Gilman, "Direct Measurements of Surface Energies of Crystals," *Journal of Applied Physics* 31 (1960): 2208-2218.

[314] "This Week's Citation Classic," *Current Contents—Engineering, Technology, and Applied Sciences* 19 (January 4, 1988): 14.

[315] R. J. Charles, "A Review of Glass Strength" in *Progress in Ceramic Science*, vol. 1, J. E. Burke, ed. (New York: Pergamon Press, 1961): 1-38; W. B. Hillig, "Sources of Weakness and the Ultimate Strength of Brittle Amorphous Solids" in *Modern Aspects of the Vitreous State*, vol. 2, J. D. MacKenzie, ed. (Washington, D.C.: Butterworth, 1962): 152-198.



Sheldon M. Wiederhorn observed the motion of a crack in a soda-lime glass with the aid of a traveling microscope. The specimen, a microscope slide, could be seen within the environmental chamber with two hooks attached to one end. The hooks, in turn, were attached to the jig of a testing machine that applied a stress to the sample. At the same time, a continuous stream of the desired atmosphere entered the test chamber by way of the white tubing in the background.

cracks as a function of load. The results were startling. The behavior showed three distinct regions. At low loads the velocity increased exponentially with the load. This was followed by a region in which the velocity was independent of the load, and finally a third region where the crack grew essentially catastrophically. This type of behavior was the same at all humidities, but the effect of water vapor was dramatic, with the velocity increasing substantially as the atmospheric moisture content was increased.

The crack growth behavior had a ready explanation in terms of the reaction of glass with water. In the low-load region, the rate-limiting step is the reaction of glass with water at the crack tip. In region II, the constant-velocity region, the rate of crack advance is limited by the rate of transport of water to the crack tip, and finally, in the region where the behavior is independent of water, the intrinsic breaking strength of the glass is reached.

There were many ways to proceed from these results, for a whole new area of fracture science had been opened. First was the relatively minor matter—at least in this case—of using the overall load as the crack extension force. Modern fracture mechanics had shown that crack-tip stress was controlled by the so-called stress intensity factor, which, aside from geometric factors pertaining to the actual experimental situation, is the macroscopic stress multiplied by the square root of the crack length. In all later publications this factor was used in place of the load, and the work joined the mainstream of modern fracture mechanics. Second, the technique could be used to study other materials, and other vapors besides water. Sapphire was one of the first additional materials studied, and its behavior was rather similar to glass.[316] There was also the question of surface energies, the original impetus for the work. Such studies were carried out in sapphire, in various kinds of glasses, and in the semi-brittle material, solid sodium chloride.

There were even highly practical applications in proof-testing of brittle solids. Suppose that a structural part made of such a brittle material is to be tested. Generally, failure will arise from the presence of minute surface cracks. If the size of these were known, then the stress intensity factor at the service stress could be calculated, and, with crack-velocity data such as Wiederhorn's, the service life could be inferred. The size of these minute flaws is generally unknown, but an upper limit on the size can be determined by simply, but quickly, loading the item to a load well above the service load. If the item does not break, then the critical stress intensity factor, which is the stress intensity factor for catastrophic fracture, has not been exceeded, and from this the maximum size of the flaw can be calculated. Then, using the crack velocity data, the minimum service life can be determined. Other more sophisticated schemes can be developed.[317]

This work formed the first concerted program in fracture mechanics at the Bureau. It attracted considerable attention in the technical community, and both young and experienced scientists were attracted to joining it. The study of fracture in ceramic materials has been continuously and productively pursued at the Bureau since Wiederhorn's early work, much of it supported by the Office of Naval Research (ONR). Eventually a program in fracture that included theorists and work on the difficult problems of metal fracture was initiated, but it is fair to say that, at the Bureau, the genesis of fracture mechanics research was with Wiederhorn's work on the fracture of glass.

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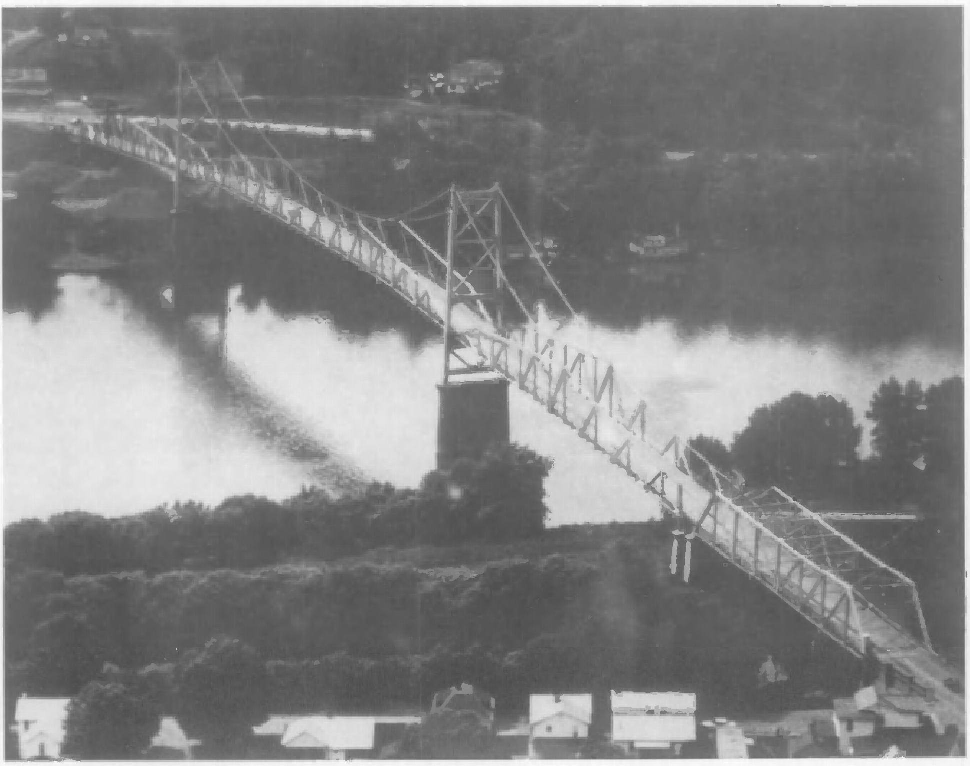
[316] S. M. Wiederhorn, "Fracture of Ceramics" in *Mechanical and Thermal Properties of Ceramics: Proceedings of a Symposium Held at Gaithersburg, Maryland, April 1-2, 1968,* J. B. Wachtman, Jr., ed., Natl. Bur. Stand. (U.S.) Special Publication 303; May 1969: 217-241.

[317] A. G. Evans and S. M. Wiederhorn, "Proof Testing of Ceramic Materials—an Analytical Basis for Failure Prediction," *International Journal of Fracture* 10 (1974): 379-392.

The Investigation of the Point Pleasant Bridge Collapse

Like the Bureau's investigation of the failure of cargo ships during World War II, the investigation of another dramatic structural failure was carried out by the Metallurgy Division.[318]

On the western boundary of West Virginia, at the town of Point Pleasant, U.S. Highway 35 was carried over the Ohio river by a suspension bridge named the Point Pleasant bridge, but more often called the "Silver Bridge" because it was painted with aluminum paint. On December 15, 1967, while the bridge was crowded with afternoon rush-hour traffic, it suddenly collapsed, spilling people and vehicles into the river and onto its banks. Forty-six persons lost their lives. The newly-formed National Transportation Safety Board (NTSB) began an investigation that was to proceed for three years, although the cause of the collapse was known much earlier.[319]



The Point Pleasant Bridge carried U.S. Highway 35 over the Ohio River from Point Pleasant, West Virginia, into Ohio before the bridge collapsed on December 15, 1967. (Courtesy of the West Virginia Department of Highways)

[318] John A. Bennett, "Metallurgical Examination and Mechanical Tests of Material From the Point Pleasant, W. Va. Bridge, Part 1," NBS Report 9981, February 14, 1969; J. A. Bennett and H. Mindlin, "Metallurgical Aspects of the Failure of the Point Pleasant Bridge," *Journal of Testing and Evaluation* 1 (1973): 152-161.

[319] National Transportation Safety Board, "Collapse of U.S. 35 Highway Bridge, Point Pleasant, West Virginia, December 15, 1967," NTSB SS-H-2, October 1968.

Although the bridge was a suspension bridge, its design was unusual in three ways. First, unlike more common suspension bridges in which the suspension members are large cables composed of drawn steel wires, in the Point Pleasant bridge the suspending members were two chains made of pairs of eyebars linked together—completely analogous to a bicycle chain except that the links in the chain were 55 feet long, with shanks 12 inches wide and 2 inches thick, and the eyes in the eyebars were 11.5 inches in diameter. Second, the chains were the top members of a stiffening truss in parts of the main and side spans. Third, the steel used for the eyebars was a relatively new material in the late twenties when the bridge was built. It was a medium carbon steel in the water-quenched and tempered condition. This provided a material of high yield strength, which lowered the construction cost.

Unfortunately, while behaving normally in an ordinary tensile test, the new steel had a low resistance to crack propagation. This was not recognized at the time the bridge was built. In January 1968, the Bureau of Public Roads of the Federal Highway Administration of the Department of Transportation, which was conducting the investigation with the NTSB, requested that the Bureau send a representative to examine the wreckage resulting from the collapse.[320] Hence, John A. Bennett of the Metallurgy Division, a scientist with many years experience in failure analysis, visited the site on January 22 and 23, 1968.

Upon returning, in a state of controlled excitement he reported his findings to the division chief, Elio Passaglia, and his assistant, Harry C. Burnett. He showed photographs of the fracture through one of the eyebars (serial number 330). On one side of the eye, the fracture was perfectly flat and radial, while on the other side there was considerable plastic deformation and tearing. In his report of the examination on January 26, 1968, he wrote:

> I observed only one fracture that I consider to be of primary importance in connection with the collapse of the bridge; that is the fracture through the eye of the eyebar which I believe bears the serial number 330. . . . I believe that this configuration of the fracture could have been produced only by a progressive cracking of the first side by loads whose maximum value did not cause appreciable plastic deformation of the eye. . . . When this first fracture was completed, the other side was subjected to an excessive load and failed rapidly, but with a fairly ductile tearing fracture. . . . I have been unable to conceive of any way in which this fracture could have occurred after any other failure in the eyebar chain. I believe, therefore, that it is almost unquestionably the primary fracture in the collapse.[321]

Bennett's deduction was to prove completely correct, but it took a great deal of laboratory work before the deduction could be proved. While a complete, if not exhaustive, metallurgical examination was carried out,[322] the most crucial examinations were

[320] Bennett, "Metallurgical Examination": 1.

[321] Ibid.

[322] J. A. Bennett and M. R. Meyerson, "Metallurgical Examination and Mechanical Tests of Material from the Point Pleasant, W.Va. Bridge, Part 2," NBS Report 9981, February 14, 1969.

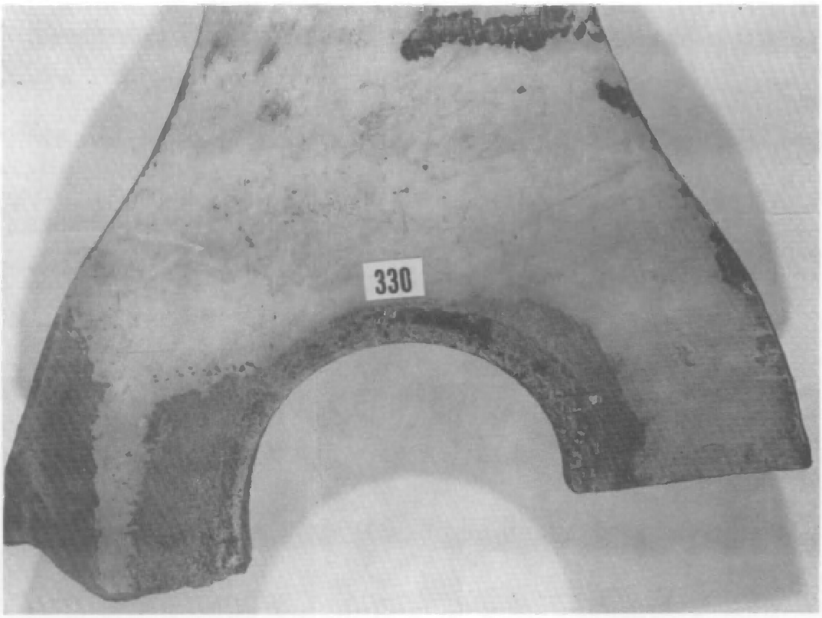


Figure 1: Inboard piece of the broken eye from the Point Pleasant Bridge. The north face is shown, and the lower side of the eye is at right.

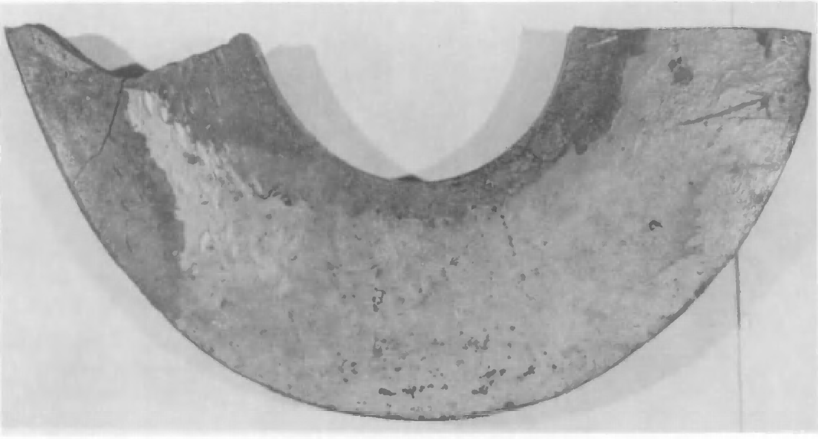


Figure 2: Outboard piece of the broken eye, as received, oriented as in figure 1.

those having to do with the nature of the fracture. Briefly, the fracture surfaces were examined and photographed. The markings indicated that the fracture had started at the surface of the eye hole at a point 0.1 inch from the face of the eye. At this point there was clear-cut evidence of a pre-existing crack which formed the site for the initiation of a crack that proceeded catastrophically from this point across the side of the bar.

The pre-existing crack was clam shell in shape, and only 0.12 inches deep and 0.28 inches long. Finding this crucial, pre-existing crack prompted an extensive search for other cracks. Many were found, all initiating in areas where there was heavy corrosion, suggesting that the mechanism of cracking was stress corrosion.

One of the crucial questions was whether the size of the pre-existing crack was sufficient to cause a brittle fracture. The question could only be answered by fracture mechanics data, and extensive investigations at Battelle Memorial Institute (one of several participants in the investigation), including one full-scale test on an eyebar, led to the conclusion that it was. Thus, the whole failure sequence likely started at this small, pre-existing crack.

The final conclusion was that a combination of factors was responsible for the collapse. These were:

1. The high hardness of the steel made it susceptible to stress corrosion cracking.

2. The close spacing of components in the eye joint made painting impossible, leading to a site where corrosion could take place.

3. The high design load in the eyebar chain resulted in a local stress at the inside of the eye greater than the yield strength of the new steel.

4. The low fracture toughness of the steel caused it to fail catastrophically from a strategically located crack only 0.12 inches deep.

The absence of any one of these would have prevented failure.

The collapse of the Point Pleasant Bridge had important repercussions. It was a case where, using the basic knowledge of materials available at the time of construction, everything was done correctly. But it was not recognized that such a steel could show stress corrosion, and fracture mechanics had not progressed to the point where the disastrous consequences of a 1/8 inch crack could be foreseen. It was a strong argument for basic research in materials science and engineering, and it provided strong support for the effectiveness of linear elastic fracture mechanics, which at that time was not as widely accepted as it is now. There were two important repercussions resulting from the collapse. The first was the closing and subsequent removal of a bridge at St. Marys, West Virginia, which was identical to the Silver Bridge. The second repercussion was that the FHWA undertook an investigation of cracks in all highway bridges. The bridges were not in good shape. And partly as a result of this investigation, and partly to reaffirm an activity it had carried out for most of its history, in 1968 the Metallurgy Division announced a program of service to Government agencies entitled Analysis of Material and Structural Failures.

Atomic Weights and Isotopic Abundances

Atomic weights and isotopic abundances are crucially important in many realms of science.[323] In fundamental-constant work, accurate values are essential to the determination of Avogadro's number, the Faraday, and the gas constant R. Isotopic abundances for lead, argon, strontium, potassium, and a number of other elements are used in geochronology, and a comparison of terrestrial isotopic abundances with those in meteorite material give important clues to the formation of elements in cosmology. A particularly apt illustration, albeit now of limited use, was the relation between the atomic weight of silver, the Faraday, and the definition of the old international ampere, now superseded by the absolute ampere. In modern analytical chemistry, the technique of "isotope dilution mass spectrometry" (IDMS) is becoming a reference method for the determination of the major component in a mixture.[324]

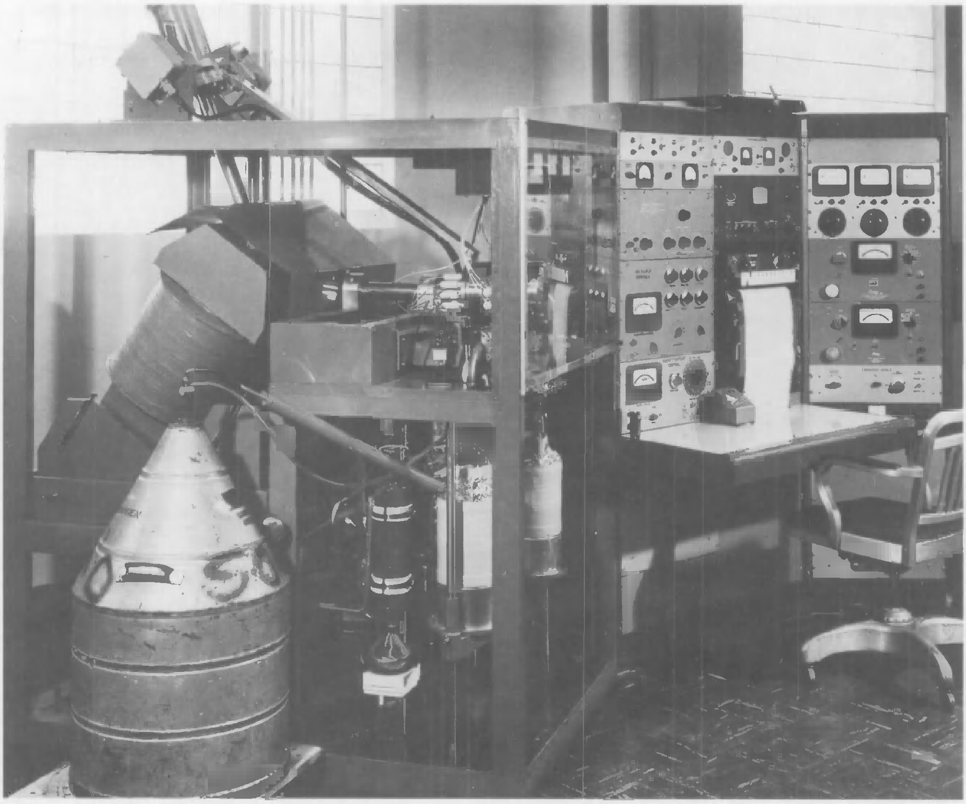
With modern mass spectrometers, the best way of determining the atomic weight of multi-isotope elements is via the relative abundance of its isotopes, and, of course, their atomic masses. Then the atomic weight of the element is obtained as a simple average, which can be scaled to the atomic mass of carbon-12, taken as 12 units. Thus in principle the method is very simple. All that is necessary is to place the element in a mass spectrometer and determine both the masses and the relative abundances of the isotopes. In practice, however, the situation is not that simple. In order for the measurement to be made, the element must be vaporized, ionized, and the atoms counted or otherwise detected after passing through the apparatus. Now the various isotopes of the element do not behave the same way in this process. They will not necessarily evaporate, ionize, or be detected with equal efficiency. In the term of the trade, a "bias" or systematic error can exist.

A legacy of the Manhattan Project, however, provided a means of handling this problem. Nearly pure isotopes of many elements were produced and were made readily available in the project. From these nearly pure isotopes, synthetic mixtures of known composition could be made, and measuring these with the mass spectrometer system could provide a measure of the bias inherent in the measurement process described above. The synthetic mixture provided a means of calibrating the system and procedure. The isotopic abundances (generally expressed as ratios) produced by this laborious but accurate method are called "absolute isotopic abundances." The resulting atomic weights are the most accurate available. It is clear, of course, that samples must come from many different locations in order to check on the geographic variability, and to yield atomic weights that are indeed representative of terrestrial material.

With this capability, in the early sixties the Bureau began a long-term program to determine the atomic weights of various elements by solid-source mass spectrometry. As might be expected, the first element studied was silver. The value for the

[323] J. R. De Laeter, P. De Bièvre, and H. S. Peiser, "Isotope Mass Spectrometry in Metrology," *Mass Spectrometry Reviews* 11 (1992): 193-245.

[324] P. De Bièvre, J. R. De Laeter, H. S. Peiser, and W. P. Reed, "Reference Materials by Isotope Ratio Mass Spectrometry," *Mass Spectrometry Reviews* 12 (1993): 143-172.



Mass spectrometer used for the determination of the atomic weights of silver, chlorine, bromine, and copper. Part of the large magnet used for deflection of the beam can be seen behind the top of the metallic Dewar at left.

abundance ratio was 1.07547 ± 0.0013 for material from a number of different sources. However, material from Cobalt, Ontario, showed a statistically significant variation which later work did not substantiate.[325] The atomic weight was determined to be 107.8685 ± 0.0013 on the $C^{12} = 12$ scale.

With this work began a steady stream of other determinations. By 1982, values for twelve elements had been determined.[326] The NBS values were adopted (sometimes

[325] W. R. Shields, D. N. Craig, and V. H. Dibeler, "Absolute Isotopic Abundance Ratio and the Atomic Weight of Silver," *Journal of the American Chemical Society* 82 (1960): 5033-5036. W. R. Shields, E. L. Garner, and V. H. Dibeler, "Absolute Isotopic Abundance of Terrestrial Silver," *Journal of Research of the National Bureau of Standards* 66A (1962): 1-3.

[326] A core group of NBS scientists, which included William R. Shields, Thomas J. Murphy, Ernest L. Garner, Vernon H. Dibeler, Edward Cantazaro and, later, John W. Gramlich, Paul J. Paulsen, I. Lynus Barnes, Larry J. Moore, and Lura P. Dunstan, made determinations for silver, chlorine, copper, bromine, chromium, magnesium, lead, rubidium, rhenium, potassium, thallium, and strontium.

with minor revision) by the Commission on Atomic Weights and Isotopic Abundances, a standing committee of the International Union of Pure and Applied Chemistry.

The Bureau not only carried out the determinations, but it sold samples of some of the materials as SRMs. In 1965 it offered chlorine (as sodium chloride), copper metal, bromine (as sodium bromide), and silver (as silver nitrate). By 1970 it offered nine elements in twelve different SRMs, and by 1988 the numbers had increased to fifteen elements in twenty SRMs. Up to 1987, the Bureau sold certified isotopic-abundance SRMs for uranium, both depleted in U^{235} and enriched up to 93 percent U^{235}. In that year the Brunswick Laboratory of the Department of Energy began issuing Certified Reference Materials. These include the plutonium and uranium materials previously issued by the Bureau.

A Program on Crystal Growth

In 1962, two years before the formation of the Institutes, the Bureau had quite a number of activities, both theoretical and experimental, on the growth and characterization of crystals. The work ranged from the crystal growth of organics (primarily polymers), to inorganic materials and metals. Growth was studied, as appropriate to the system, from solution, from the vapor, and from the melt. Characterization of crystals, or more broadly, materials, was concerned with the study of crystal defects—dislocations, point defects, stacking faults—and their effect on bulk properties. And purification by crystallization was actively pursued. This work, however, was not considered to be a program. Rather it was carried out by individual scientists furthering the mission of their individual organizational units. It was not an unproductive mode of operation.

Aware of the crucial importance of certain highly perfect crystals—and indeed all advanced materials—to military technology, Charles F. Yost, assistant director of materials science for the Advanced Research Projects Agency (ARPA) of the DOD, worked out with Irl C. Schoonover, then Bureau deputy director, "a program for accelerated research in those areas of crystal research that [are] judged to be of critical importance to the entire field of materials research." A joint NBS/ARPA program on crystal growth and characterization was born, with ARPA funds providing for the expansion of the Bureau's existing effort.[327]

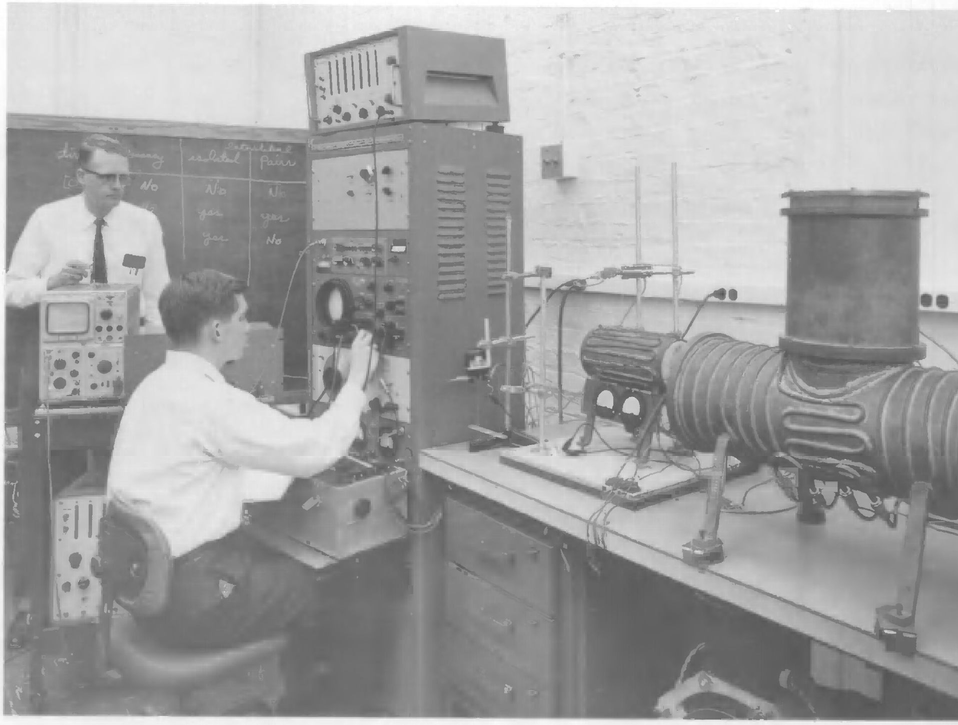
While at first the program had only two categories of work—crystal growth and crystal characterization—it was expected that the work would develop "theory on the mechanism of crystal growth, experimental techniques for growth and study of crystals, interpretive analysis of observations on crystals examined by diverse methods, and data from the measurement of defect-sensitive properties of crystals."[328] H. Steffen Peiser was the program coordinator, whose main function—at least in the early days—was to

[327] *Research on Crystal Growth and Defect Characterization at the National Bureau of Standards During the Period July to December, 1962*, H. Steffen Peiser, ed., Natl. Bur. Stand. (U.S.) Technical Note 174; March 15, 1963. Similar Technical Notes were issued semi-annually until 1967.

[328] Ibid., 1.

Avery Horton of NBS water-polished a slice from a crystal of ammonium dihydrogen phosphate grown from solution in the NBS-ARPA crystal program. Strains and dislocations introduced into the crystal by abrasive polishing methods were avoided by this technique. After polishing, the crystal was examined by x-ray topography. These crystals were used to determine the inherent imperfections caused by growing variables.

imbue the program participants with the sense of belonging to a unified activity, for in the early days the work was no more than a continuation of what they had been doing. But the extra funds provided by ARPA were very welcome and soon led to an expansion of the effort.



John B. Wachtman, Jr. (left), and Tomas Fridinger (center) made flexural measurements of rutile crystals at NBS. The specimen was held at the desired temperature (20 °C to 600 °C) in the furnace (right) and was oscillated at particular frequencies. The crystal resonated and any changes in the physical properties of the crystal were detected electronically and recorded. From these data, values for such data as internal friction were calculated.

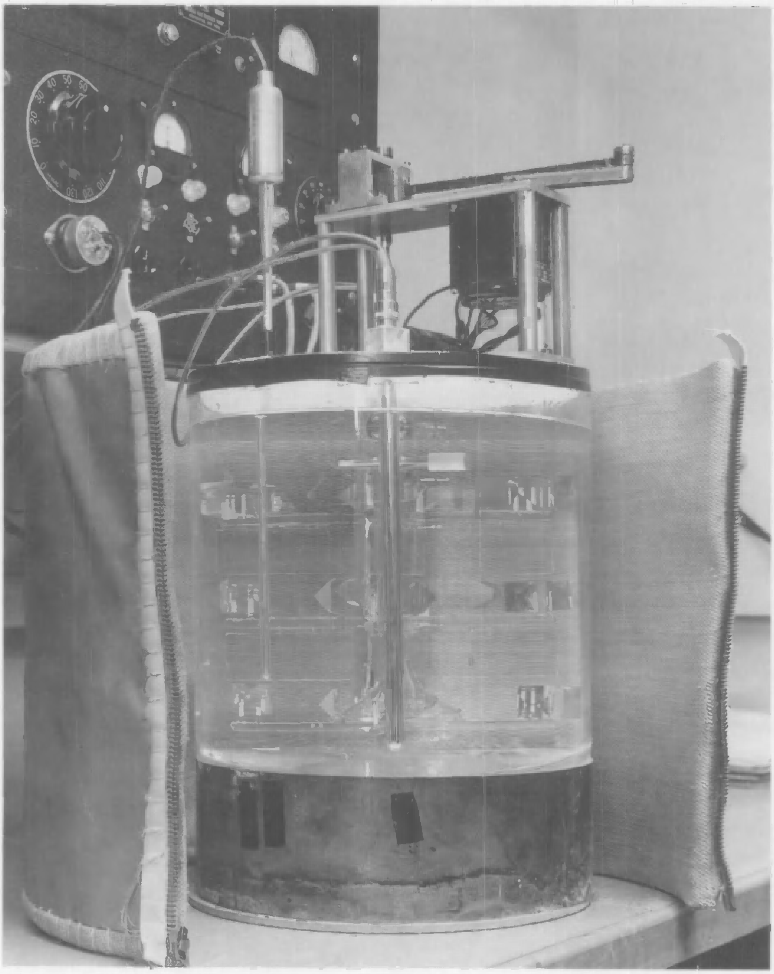
The effort was not small. In July 1962 there were twenty-one projects in crystal growth and twenty-five in crystal characterization, with sixty-one scientists participating.[329] By December 1964, the program had swelled to twenty-five projects in crystal growth, twenty-seven in defect characterization, twenty-three in physical properties of crystalline materials, and sixteen in crystal chemistry. By then there were 181 participants—not all full-time—from the Washington Laboratories, and sixteen from Boulder, mostly from the Cryogenics Division.

With such a massive effort, we can give only a fleeting glimpse of the investigations, and the glimpse is as of mid-1964.[330] Thus, in crystal growth there were projects on the growth of dislocation-free metal crystals from the melt; on the kinetics of growth from the melt of metal crystals; on the theory of dendritic crystallization, which

[329] Ibid., ii, 28-31.

[330] *Research on Crystal Growth and Characterization at the National Bureau of Standards January to June 1964*, Howard F. McMurdie, ed., Natl. Bur. Stand. (U.S.) Technical Note 251; October 19, 1964.

led to powerful work on morphological stability; field emission studies of crystallization; crystal growth by electrodeposition; the growth of large and highly perfect ammonium dihydrogen phosphate crystals from solution; spherulite growth from relatively pure systems; the crystallization of polypropylene from solution, which led to crystals whose habit can be described as a loosely woven place mat; and theoretical studies of whisker growth.



Nearly perfect single crystals of ammonium dihydrogen phosphate were grown from solution in a crystal growing bath. The zippered insulation jacket was removed only during inspection periods. The temperature-control mechanisms (background) and a mixing device (upper right) provided uniform temperature and concentration throughout the solution.

In the study of defects in metal crystals, dislocations and stacking faults received considerable attention. The high-temperature motion of dislocations in aluminum oxide was studied in the electron microscope using a special high-temperature stage. The fundamental electrical properties of commercially important semiconductor crystals were studied with very high precision, and standard measurement methods proposed. The effect of point defects on the dynamic properties of crystals was a continuing project. And the characterization of crystals by x-ray diffraction topography would eventually lead to a re-determination of Avogadro's constant.

In the broad category of physical properties of crystalline materials, which appears to include everything about all solids other than glasses, there were a number of solid-state studies of electronic properties, electron spin resonance and nuclear magnetic resonance studies, soft x-ray studies, and optical properties of some specialized compounds. Another broad field, crystal chemistry, was represented by studies of polymorphism in bismuth trioxide and other systems, the crystal chemistry of mineralized tissue (of great interest to the dental researchers), the radial distribution in glasses, and computer models for amorphous and crystalline phases in simple substances.

This broad and diverse program continued until 1966. At that time ARPA's interest began to change. Feeling it had sufficiently stimulated crystal growth research at the Bureau and elsewhere, it became more concerned with high-temperature and laser materials, and its support dropped accordingly. But much of the crystal growth and characterization research continued, with direct support by the Bureau and a few other agencies.

Silicon Resistivity

In 1960, at the request of the American Society for Testing and Materials (ASTM) Committee F-1 on Materials for Electron Devices and Microelectronics, the then Instrumentation Division formed a program to investigate the problems associated with measurements on silicon wafers to be used in the manufacture of solid-state electronic components.[331] Supported at various times by ARPA, the United States Air Force Cambridge Laboratories, and NASA's Electronic Research Center, and with assistance (through ASTM) by the semiconductor industry, this program was to be technically fruitful, and in many ways it was a prototype for the interaction of the Bureau with other Government agencies and industry.

The most important of the measurements made on silicon wafers is room-temperature resistivity.[332] The reason this property is so important is that it is a measure of the impurity concentration, and this parameter is in turn the most important consideration

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[331] In 1966 the name of the division was changed to Electronic Instrumentation, and Myron G. Domsitz replaced G. Franklin Montgomery as division chief. In 1969, the name and emphasis were changed to Electronic Technology Division. Domsitz remained chief.

[332] W. Murray Bullis, *Measurement Methods for the Semiconductor Device Industry—A Summary of NBS Activity,* Natl. Bur. Stand. (U.S.) Technical Note 511; December 1969; "Improved Semiconductor Resistivity Measurements," *Technical News Bulletin* 54 (1970): 198-200.

in device design. There were basically two methods in use for the measurement of resistivity: a two-probe method, deemed the more accurate but requiring more work; and a four-probe method which is much easier to carry out. For both methods, the resistivity is calculated from the voltage developed along the path of a known current through the specimen, and the geometry of the specimen. In the two-probe method, a rectangular parallelepiped is cut from the silicon wafer or boule. After polishing, the two ends of the specimen are coated with metal, making them equipotential surfaces, and current is passed from one of these surfaces to the other. Then, on the reasonable assumption that the current density is uniform across all intermediate planes, the resistivity is easily calculated from the dimensions of the specimen and the voltage developed across two planes parallel to the end surfaces, and separated by a known distance. The method is in principle exact and hence yields an accurate measurement, but suffers because the preparation of the rectangular bars is a long and tedious task. It is not useful for routine measurements.

The four-probe method is quite different. In it, four contact points, arranged in a straight line and with accurately known spacing, are pressed onto the silicon surface, and a known current is passed between the exterior probes. Voltage is measured between the two interior probes. No special cutting of a specimen is necessary, only the preparation of a polished surface. The problem here is to know the current distribution. The solution for a semi-infinite space is well known and results in a particularly simple formula for the resistivity. For finite-size circular specimens, correction factors to this formula to account for finite diameter and thickness and the location of the probe with respect to the center of the specimen had to be developed. The problem was solved, but not in closed form, so that tables had to be published.[333] Attesting to the interest in the topic, more than 1500 copies of the report were requested by industry. Also calculated were four-probe correction factors for the use of the four-probe method on rectangular bars.[334] The calculation required machine computation, but it led the way to a direct comparison between two- and four-probe methods on the same specimen.

Besides these results, a number of other factors needed investigation. These were the probe force, probe material, surface preparation, and probe wander—i.e., movement of the probe points from their expected positions. And, of course, the effect of temperature had to be considered. It was not trivial, for experiments showed that at room temperature, the temperature coefficient was about 1 percent per Celsius degree. Eventually, an electrical measuring circuit of proper input impedance and good accuracy was developed.

[333] Lydon J. Swartzendruber, *Correction Factor Tables for Four-Point Probe Resistivity Measurements on Thin, Circular Semiconductor Samples*, Natl. Bur. Stand. (U.S.) Technical Note 199; April 1964.

[334] Lydon J. Swartzendruber, *Calculations for Comparing Two-Point and Four-Point Probe Resistivity Measurements on Rectangular Bar Shaped Semiconductor Samples*, Natl. Bur. Stand. (U.S.) Technical Note 241; June 1, 1964.

While a standard for publication by ASTM was in preparation, several round-robin tests on the new techniques were run. Experienced laboratories achieved a standard deviation of somewhat less than 1 percent, five times better than was achieved with a 1964 standard, and sufficiently good for transactions at the buyer-seller material interface. Inexperienced laboratories, however, had considerable difficulty, caused primarily by unsatisfactory electrical measuring equipment.

One part of the planned program did not come to pass. It had been planned from the beginning that the Bureau would offer silicon SRMs certified for resistivity to be used in calibrating probes. However, a study showed that the available material was not sufficiently uniform "to provide the accuracy and reproducibility required by industry."[335] But the whole project was a fine example of cooperation between the Federal Government, in the form of the Bureau and interested other agencies, and the private sector in the form of producers and users of circuit-grade silicon, and a voluntary standards organization. By this cooperation, and largely because of the Bureau's work, an important industrial measurement in the Nation's National Measurement System had been substantially improved.

Evaluating Nuclear Radiation Detectors

While work was going on to develop new measurement methods for the resistivity of silicon, the Electronic Technology Division, with support from the Division of Biology and Medicine of the Atomic Energy Commission (AEC), entered into a new program on the evaluation of semiconductor detectors for gamma radiation.[336] The problem was that while some detectors worked well, others, ostensibly the same, were unsatisfactory. And the only way to determine if a sample of starting material had the properties to yield a good detector was by manufacturing a detector and trying it out. Because of the manner of fabrication, this was a long and tedious process that required 8 to 10 weeks.

These detectors consisted of a relatively thick slab of intrinsic germanium (essentially the same number of electrons and holes) sandwiched between thin layers of p-type (current carriers are holes) and n-type (current carriers are electrons) material. The detector acts like a "solid-state ionization chamber." In use, a reverse bias is applied to the device and gamma rays reach the intrinsic region creating holes and electrons which flow to the p- and n-type layers, respectively. The total charge collected is proportional to the energy of the incident photon—when everything works properly.

The reason production took so long was the painstaking manufacturing process. Obtaining "essentially intrinsic" germanium meant the achievement of an unattainable level of purity in the starting material. As a result, it was slightly p-type, with a minute

335 "Improved Semiconductor Resisivity Measurements," *Technical News Bulletin* 54 (September 1970): 198.

336 A. H. Sher, "Lithium-Ion Drift Mobility in Germanium," *Journal of Applied Physics* 40 (1969): 2600-2607; "Evaluating Ge(Li) Nuclear Radiation Detectors: Cause of Defects Sought," *Technical News Bulletin* 53 (December 1969): 268-269.

amount of boron (which forms holes) added to make it so. Then the holes were filled by adding lithium, which enters the germanium lattice interstitially and is an electron donor. The process is called compensation, with the lithium electron compensating for the hole-forming boron. The lithium was introduced by field-aided diffusion, that is, by applying a very thin layer of lithium to one surface (in the form of a suspension in oil), applying a voltage of up to 1000 volts across the slab, and heating for as long as several weeks. The lithium must diffuse just enough, but not too much, for the device to work properly. The process is called "drifting" and these devices are labelled Ge(Li) detectors. Clearly, this "drifting" or "compensation" step was the most critical step in the fabrication process.

For this reason, Alvin H. Sher undertook to re-determine the mobility of lithium in germanium. Without explaining the process here, he obtained results that were significantly higher at room temperature than two results obtained previously. However, the germanium crystals used in the experiments were specially selected to be free of "impurities that might reduce the lithium mobility."[337] Other samples showed results that were a factor of 100 lower in lithium diffusion at room temperature. Other studies, such as resistivity, photoconductive decay carrier lifetime, and etch pit studies, were inconclusive. The only tenable conclusion was that the scatter in performance of these detectors was caused by variations in the impurity content, which in turn caused differences in the lithium mobility.

The Bureau did not solve the problem of germanium. It did provide six nomographs to "facilitate the fabrication and testing of" these detectors.[338] Two of these nomographs were concerned with processing and four with testing. The parameters identified in the nomographs concerned with processing were (1) time, temperature, applied bias, and drifted depth; and (2) lithium mobility, crystal resistivity, and oxygen concentration. For the other four, the parameters were (3) area, capacitance, and drifted depth for planar detectors; (4) the same parameters for coaxial detectors; (5) total spectral resolution, system noise, and detector resolution; and (6) detector resolution, gamma ray energy, and effective Fano factor.[339] The titles indicate which parameters the Bureau considered to be the critical parameters in the fabrication and testing of Ge(Li) nuclear radiation detectors.

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[337] Sher, "Lithium-Ion Drift": 2605.

[338] Alvin H. Sher, *Nomographs for Use in Fabrication and Testing of Ge(Li) Detectors,* Natl. Bur. Stand. (U.S.) Technical Note 537; August 1970.

[339] The Fano factor gives the relationship between the number of pairs produced by the ionizing radiation and the variance of that number. Multiplied by 100 it could be called " % variance." It can be calculated theoretically from models of the fluctuation of the number of pairs produced per event, and thus is a measure of the theoretical accuracy possible in ionization methods of measuring the energy of radiation. U. Fano, "Ionization Yield of Radiations. II. The Fluctuations of the Number of Ions," *Physical Review* 72 (1947): 26-29.

Building Research and the Performance Concept

One of the principal policies of the Institute for Applied Technology was variously called the "performance concept" or "performance criteria." More commonly known as performance standards, the concept specifies the function an item is to perform, rather than the materials from which it is made and the details of its construction. For example, a performance standard for a wall might enumerate such functions as the load it is to bear, the maximum rate of heat and sound transfer through it, and other such functions. Mention of the material of construction and method of manufacture would not necessarily be made. At the other extreme, existing construction standards were typically based on "narrowly drawn engineering specifications," such as 2×4 studs on 16 inch centers, with 3/4 inch exterior plywood siding, interior dry wall, and so forth. The importance of the concept was that it "provides for maximum expression of creativity and innovation among builders and manufacturers because attention is focused on the function of a particular system rather than on the system itself."[340]

Implementation of the performance concept is not simple. The performance in mind when the item functions is, of course, performance in service. To assess this performance, however, it is necessary to build the item, place it in service, and determine—either objectively by measurements or subjectively—how it performs. Such information is difficult to obtain, and the existence of a performance concept is not necessary to carry it out. It is relatively easy to assess performance in a test, as, for example, the thermal conductivity of a candidate wall construction. However it is hard to relate performance in a test to performance in service. The better the scientific knowledge relating performance to composition and structure, the easier and more valid will be the determination of the relationship between performance in a test to performance in service.

While the concept was applied to all IAT standards when possible, its greatest expression came in building research. James R. Wright, chief of the Building Research Division, in a discussion of the performance concept, recognized a number of subcategories under the performance concept: Performance Requirement ("a qualitative statement describing a problem for which a solution is sought"); Performance Criteria ("give the set of characteristics that solutions must have"); Performance Specification ("comprehends all of the information in the underlying requirement and criteria"); and finally Performance Standards and Performance Codes.[341]

[340] Annual Report for 1966: 86.

[341] James R. Wright, "Measurement—Key to Performance," in *Performance of Buildings—Concept and Measurement: Proceedings of the 1st Conference in a Series of Conferences on Man and His Shelter Held at Gaithersburg, Maryland, September 23-25, 1968,* W. W. Walton and B. C. Cadoff, eds., Natl. Bur. Stand. (U.S.) Building Science Series 1; 1970: 11-19.

Some of these subcatagories were put to the test in a series of investigations on the development of performance criteria and test methods for various building components: sanitary plumbing systems; exterior walls with respect to their air, moisture, and heat transfer, and with respect to their structural properties; and floor coverings.[342] The conclusions reached by Paul R. Achenbach during the course of a study of plumbing fixtures were generally applicable to all the investigations. Tests were, of course, carried out on full-size specimens, and in all cases the "adequate simulation of use conditions is a key ingredient in developing test procedures applicable to widely different materials." He also pointed out that many of the problems were multidisciplinary, and multidisciplinary laboratory efforts were necessary in the development of test methods. Developing performance measures was no easy matter.

More ambitious was the performance testing of a whole housing system.[343] In an investigation carried out for the Department of Housing and Urban Development, the Bureau evaluated the full-scale performance of a low-income housing system planned for construction in Detroit. The system used new concepts developed by Neal Mitchell of Harvard's Graduate School of Design. Using prefabricated, lightweight components, the housing system obtained its stiffness and rigidity by the interaction of its structural elements. This provided a lightweight, easily erected and expandable structure.

The Bureau erected the first floor of one unit in its structures laboratory since the full three stories of the unit exceeded the capacity of the laboratory. Stresses were applied to the first floor to represent those that would be present in the full structure under both normal service and potential ultimate loading conditions, and additional tests were carried out on system components. As part of the work, performance criteria for the evaluation of the structural safety of the design were developed. The work indicated that the design did not sacrifice safety for economy. In view of the signifiance of the Bureau's findings, the city of Detroit waived code requirements and issued a building permit for the project.

In a similar project, another prefabricated building was erected and evaluated in the Building Research Division environmental test chamber.[344] Prefabricated in Florida and trucked to the Bureau, this building was being developed for use as an advanced-base, relocatable structure by the U.S. Navy, which sponsored the Bureau's work.

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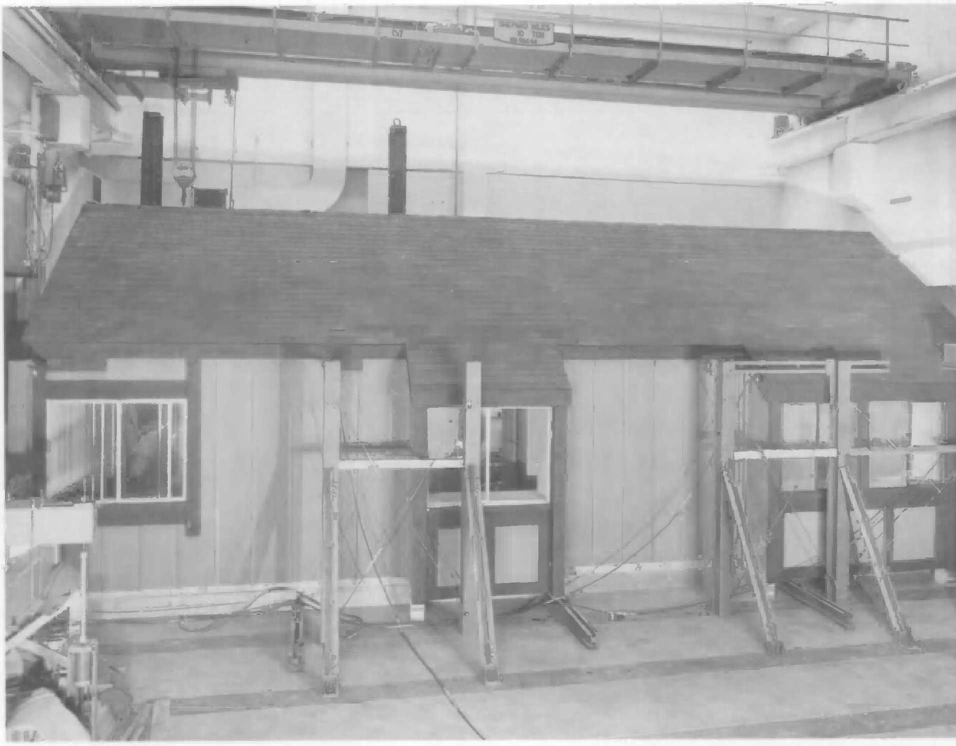
[342] Paul R. Achenbach, "Performance Testing of Sanitary Plumbing Fixtures," in *Performance of Buildings*: 65-73; Henry E. Robinson, "Performance Testing of Exterior Walls: Air, Moisture, and Heat Transfer," in *Performance of Buildings*: 75-76; Robert G. Mathey and Thomas W. Reichard, "Performance Testing of Exterior Walls: Structural Properties," in *Performance of Buildings*: 77-84; Winthrop C. Wolfe and William C. Cullen, "Performance Testing of Floor Coverings," in *Performance of Buildings*: 85-92.

[343] Edward O. Pfrang, "Performance Testing of a Low-Cost Housing System," in *Performance of Buildings:* 49-60; Edward O. Pfrang and Felix Y. Yokel, *Structural Performance Evaluation of a Building System,* Natl. Bur. Stand. (U.S.) Building Science Series 25; November 25, 1969; "Performance of Low-Income Housing: Phoenix Project Studied," Technical News Bulletin 53 (1969): 84-85.

[344] Annual Report for 1969: 149; "Prefabricated Building Undergoes Evaluation," *Technical News Bulletin* 53 (1969): 154-155.



The first factory-built module constructed for the Department of Housing and Urban Development Operation Breakthrough program was tested and evaluated by the Bureau. The full-scale structure was subjected to racking and floor and wall impact tests.

The building was constructed of panels made of sheet aluminum bonded to a paper honeycomb structure, with the panels set on an I-beam foundation. Loads were applied to the building to simulate snow and wind loads, and other tests measured air leakage and heat loss. With the capability of the chamber to provide ambient exterior temperatures from –50 °F to 150 °F, and exterior relative humidities from 10 percent to 90 percent, performance was evaluated over the whole range of environmental conditions anticipated in service. As a result of the tests, the Bureau recommended design changes to improve the thermal and structural performance of these buildings.

Disaster Investigation

We saw in the investigation of the Point Pleasant Bridge collapse how the Bureau was requested to visit the disaster site, and subsequently to participate in an investigation of the cause of the failure. We also saw how the Metallurgy Division reaffirmed its long-standing policy of assisting other agencies in the investigation of material and structural failures. Generally speaking, the work involved in this failure analysis activity, in which a single structure or piece of equipment had failed, was laboratory work designed to determine the initial cause of the failure—to find the responsible flaw or malfunction. As such, the work was concerned with fractographic investigations, the determination of how well material properties met specifications, and similar activities. The investigation of the accident site—reassembly of pieces, questioning of witnesses, and other such activities—was not part of the Bureau's investigations.

In the period covered by this chapter, the Building Research Division, became involved in a related activity: disaster investigation. Here the concern was—and remains—to determine how different types of structures withstood natural disasters: floods, hurricanes, tornadoes, and earthquakes. The concern was to obtain information which could be used in new building codes to prevent—as far as possible—damage or destruction in such harsh environments. Unlike the failure-analysis investigations, these disaster studies were almost entirely field work, determining first of all the character of the natural phenomenon causing the disaster, and then the response of various structures to it. It was, therefore, not necessarily concerned with the detailed analysis of an individual structure, but rather on the behavior of classes of structures—why some withstood the phenomena and others did not.

As if designed to illustrate the scope of the work, natural disasters of four different types sparked investigations within a few years' time:

- A flood caused extensive damage in Fairbanks, Alaska, in August 1967.[345]
- Hurricane Camille occured in August 1969.[346]
- A tornado hit Lubbock, Texas, on May 11, 1970.
- An earthquake measuring 6.6 on the Richter scale occured in the San Fernando area of California on February 9, 1971.

We will illustrate this type of work by briefly describing what the Bureau did in the last two investigations.

In 1970, Lubbock was a city of about 150 000 persons in the Texas Panhandle, 115 miles south of Amarillo.[347] At 9:15 p.m. on May 11, a radar echo was picked up, indicating a tornado ten miles east of the city. About twenty minutes later, another radar echo and visual sighting placed the tornado near the center of the city, from whence, as indicated by subsequent damage, it followed a course north by northeast. At 10 p.m. it passed the Weather Bureau office at the Lubbock airport northeast of the city, where at 10:02 p.m. winds measured 89 mph, and 1.8 in of rain fell in one hour. The tornado continued on a northeast path and eventually left the city.

[345] Annual Report for 1968: 126.

[346] Annual Report for 1970: 132-133.

[347] N. F. Somes, R. D. Dikkers, and T. H. Boone, *Lubbock Tornado: A Survey of Building Damage in an Urban Area,* Natl. Bur. Stand. (U.S.) Technical Note 558; March 1971.

It was a severe tornado. The death toll was twenty-six, and property loss was estimated at $200 million, with 460 single-family detached homes destroyed, 489 severely damaged, and 754 sustaining minor damage. In addition, 80 mobile homes were destroyed and 30 were severely damaged. Because such natural disasters provide unparalleled full-scale tests of building construction, the Building and Research Division sent a three-man team (the authors of the cited report) to carry out an investigation. On May 14-16, the team carried out thorough photographic, ground, and helicopter surveys of the damage along the path of the tornado.

The survey found that the "predominant type of building damage . . . was the loss of roof coverings and roof structures," and goes on to detail the differences in the behavior of various kinds of asphalt shingle roofs, clay tile roofs, and metal roofs, where inadequate fastening resulted in large areas of the roof being stripped from its support. In many places complete roof structures were lost, indicating that uplift forces had not been sufficiently considered in design. Other areas investigated were glazing, masonry veneer, flying debris, and mobile homes, where the level of the damage could have been reduced by using over-the-roof ties. The two principal conclusions were that "currently accepted good practice for the design and construction of buildings . . . against wind loads . . . would . . . have greatly reduced the damage observed at Lubbock; and, following the theme of the research program of the division, "research is needed to develop performance criteria with respect to wind loads."

The San Fernando earthquake investigation followed much the same lines.[348] The quake occurred on February 9, 1971, at 6:41 a.m., killing sixty-four and causing $500 million in damages. Within twenty-four hours, at the request of the White House Office of Emergency Preparedness, four members of the Building and Research Division were at the site, examining homes, schools, hospitals, roads, bridges, public services, and flood control facilities. An abbreviated list of their findings follows:

1. Present procedures used to update design regulations should be evaluated to find more expeditious ways to incorporate new knowledge into design. To put it another way, changes in the building codes had not kept up with increases in knowledge.[349]

2. As a corollary to this observation, the Bureau team recommended that the earthquake hazard evaluation of older structures built under older codes begin immediately.

[348] H. S. Lew, E. V. Leyendecker, and R. D. Dikkers, *Engineering Aspects of the 1971 San Fernando Earthquake,* Natl. Bur. Stand. (U.S.) Building Science Series 40; December 1971. A synopsis of this is in "Report on the San Fernando Earthquake," *Technical News Bulletin* 56 (1972): 99-100.

[349] Originally this recommendation read, "An immediate review should be made of the adequacy of present design requirements for seismic design." Such a review began in 1972 under NSF and NBS sponsorship. (Richard N. Wright, memorandum to Elio Passaglia, February 23, 1993. History Project File; Chapter 5; Folder Disasters)

3. Deformation and deflection should be considered along with strength in the design of earthquake-resistant structures. Since it is difficult for a rigid structure to withstand the large displacements imposed on it by the earthquake, design should be flexible enough to accept the imposed large displacements.

4. Hazards created by overhead objects such as ceiling fixtures, emergency lights, suspended ceilings, and similar components should be kept in mind in their design and placement.

5. Walls with large openings, such as required for garages, should be given adequate strength and stiffness.

6. The report also recommended use of flexible joints and automatic cutoff valves to reduce and limit damages to underground water, sewage, and gas lines.

7. Elevator design should be reviewed to improve the likelihood that elevators perform adequately during and after disasters.

From the initiation of the disaster investigation program, the Building Research Division and its successors have kept a team of engineers at the ready to investigate such natural disasters.

Experiments in Fire and Smoke

In 1969 John W. Davis, chairman of the Subcommittee on the National Bureau of Standards of the House Committee on Science and Astronautics, requested that the Bureau prepare a report entitled, "A Review of the Fire Problem and a Proposed Program to Implement the Fire Research and Safety Act of 1968." Authored by John A. Rockett, Alexander F. Robertson, and John F. Christian of the Fire Research Section, the report was printed as a Committee Print in 1970. The authors provided a description of the program under the main headings "Programs Designed Primarily at Reducing Death and Injuries," " Programs Directed at Fire Department Operations," "Fire Losses" (a data collection program), and "Incremental Building Costs." Much of the program was nontechnical, but featured within it were specific projects on fires in buildings and the hazards of smoke. These topics led to a number of technical questions. The Bureau had been at work in these areas even before the enactment of the Fire Research and Safety Act, and a quick summary of three of them follows.

In 1968 the Bureau had an unparalleled opportunity to study the effects of controlled fires in buildings at little cost to itself.[350] Briefly, the Pratt Institute's School of Architecture, with a grant from Housing and Urban Development, built a two-story building with two wings: one of concrete construction and the other of steel frame construction. The two-story structure located in Carteret, New Jersey, was planned to permit technical evaluation of construction materials being considered for use in high-

[350] "NBS Studies Fire Safety of Apartment Units," *Technical News Bulletin* 52 (1968): 3-4.

NBS engineers and technicians made last-minute inspections of the instrumentation used to monitor the full-scale burnout of the test building designed by the Pratt Institute. The steel wing (left) and the concrete wing (right) were joined by wooden stairs and a platform.

rise structures. The primary aim of the fire studies was to obtain information on the protection of occupants, and the prevention of fire spread to other apartments and buildings. The Bureau was called in to carry out the fire tests.

In one of the rooms of an apartment, lattice-type cribs constructed of 2×4 Douglas fir were placed so as to provide a fuel loading of six pounds per square foot—about average for an apartment. Extensive temperature measurements were provided in the fire room and in adjacent rooms. Windows were open in the fire room but closed in the others. Other measurements included floor deflection, smoke density, and the detection and measurement of toxic fumes. The floor above the fire room was loaded to forty pounds per square foot to represent the structural design load. Fire tests were conducted in both the concrete and steel structures on the ground and second floors.

The tests showed that a small amount of flaming penetrated to the room above, primarily through the development of separations between ceilings and walls, and some smoke and fire penetrated through openings for electrical outlets. Toxic gas measurements were not sufficiently accurate to provide a measure of the hazard from that source. In both the steel and concrete constructions there was no evidence of structural failure, although premature failure of a suspended ceiling did occur. These tests provided valuable experience and data on full-size housing units.

Since the costs of such tests were significant, it was important to study the development of fires in small enclosures where controlled conditions could be used and possibly provide useful information at relatively low cost. Also, it was important to try to find a way to scale the experiments so that fire behavior in large, room-sized enclosures could be predicted.

A series of such experiments was carried out by Daniel Gross and Alexander F. Robertson.[351] Enclosures of three different sizes, but with the same 1:1:2 width:height: length ratios, were constructed of nonflammable structural material. A rectangular opening (a "window") of variable area was made in one end to provide air for the fire. Within the enclosure a crib of sticks of unfinished cellulose-based fiberboard was constructed. The enclosure was placed on a platform scale and, during the burning, measurements were made of the mass burn-up rate as well as changes in the concentration of O_2, CO, and CO_2. By adjusting the window opening area, the burning could be controlled from smoldering to fully developed burning.

The importance of the results lay in the development of scaling relationships. During fully developed flaming, the burning rate was approximately constant, and a "ventilation parameter" of the window area multiplied by the square root of the window vertical dimension (to take into account the convective air flow in and out of the window) was developed. A log-log plot of all the burning rates against this parameter produced approximately a single straight line for all the data. However, the data were segregated depending on the nature of the enclosure, so that the data lay in three separate regions on the line. Further, normalizing those data by dividing by the square of the linear dimension ratio produced two nearly straight lines, one for smoldering and another for flaming. All the data fell together on these two lines. The effect of enclosure size had been approximately scaled into the fire behavior. Evident on the plots was a region at high burning rates where the burning rate was rapid and independent of the window area, as if there were no enclosure. An approximate method of scaling fires for room size had been achieved.

The final area of concern was smoke density measurement.[352] In recognition of the critical nature of smoke in causing death and injury in accidental fires, two projects (among others) emphasized the need for better definition and measurement of the properties of smoke aerosols. One HUD-sponsored project sought to provide a convenient and technically sound method for the laboratory measurement of the smoke generated by burning interior finish materials in building fires. This led to the development of a "Smoke Density Chamber" which subsequently became commercially available and

[351] D. Gross and A. F. Robertson, "Experimental Fires in Enclosures," in *Tenth Symposium (International) on Combustion* (Pittsburgh, Pa.: The Combustion Institute, 1965): 931-942.

[352] The author is grateful to Daniel Gross for the following paragraph, which he wrote and is used here with minor editorial change.

was adopted as an ASTM Standard.[353] Features of the method included the use of both flaming and nonflaming exposures, the use of an optical density scale for measuring light transmission through the enclosed aerosol, and expressing the results (total smoke generated or instantaneous generation rate) in terms of a nondimensional factor involving the appropriate geometrical parameters: the "specific optical density." This development led to an FAA-sponsored project in which the Smoke Density Chamber was used to measure the smoke generated by materials used on the interior surfaces of passenger aircraft. The project followed shortly after a survivable crash in which half the plane's occupants died from toxic and vision effects of smoke.

SUMMARY

In the period covered by this chapter, there were important changes in the Bureau and in its role in the Nation. While its traditional concerns had been toward the development of science, technology, and industry, a spate of new legislation gave the Bureau added responsibilities arising from social and consumer equity and safety considerations. These new responsibilities occasionally brought the Bureau to the uncomfortable position of being a quasi-regulatory agency, writing mandatory standards. These new responsibilities had a sufficiently profound impact on NBS that its director, cognizant of the Bureau's traditional functions and not knowing where these new trends would lead, labelled the Bureau an "evolving institution." From the point of view that the Bureau was the Nation's corporate laboratory, and that there was no other institution with the requisite capabilities to shoulder these responsibilities, the addition to the Bureau's duties seems understandable, indeed even natural. However, temporarily, at least, they brought the Bureau to a new but uncomfortable arena.

During this period the Bureau moved to a spacious and beautiful new home where the facilities far surpassed those of its old but beloved (by many of the "old timers") home at Van Ness and Connecticut Avenues in Washington, D.C. In the process it acquired two outstanding new facilities, the LINAC and a nuclear reactor, along with a great deal of modern equipment. With the infusion since Condon's days—and continued by Astin—of the thrust toward basic research, and of scientists conversant with modern methods, the new facilities made the Bureau a world-class scientific institution in every sense of the phrase.

The end of the period marked a historical turning point in the Bureau's existence. In 1969, Allen V. Astin, the Bureau's director for seventeen years, retired. Except for his Director of Administration Robert S. Walleigh, Astin was the last person in an upper management position who had been at the Bureau in prewar days. The leadership of

[353] D. Gross, J. J. Loftus, and A. F. Robertson, "Method for Measuring Smoke From Burning Materials" in *Symposium on Fire Test Methods—Restraint and Smoke 1966,* American Society for Testing and Materials (ASTM) Special Technical Publication 422; 1967: 166-204; *Standard Test Method for the Specific Optical Density of Smoke Generated by Solid Materials,* American Society for Testing and Materials (ASTM) Standard E 662.

the Bureau was entrusted to a new cadre of vigorous young leaders, carefully assembled and nurtured since the late forties and early fifties. But the Bureau never broke with its past. The new leadership was too aware of its history and tradition to break away from them summarily. Rather, they built on those traditions following the demands of the times and did not weaken the institution.

As it did in many other institutions, the increasing importance of relevance forced changes on the Bureau. Beginning with Planning, Programming, and Budgeting, the new demands for justification of old and new programs forced upon the Bureau new activities and structures. This gave rise to the Office of Program Planning and Evaluation, which began haltingly and in a small way, but exercised an increasingly profound effect on Bureau management.

Through the whole period the technical work flourished. From the measurement of laser frequencies to the Josephson effect; from fracture mechanics to critical phenomena; from disaster investigations to electron scattering by nuclei; from clinical SRMs to bridge failures; from superconducting semiconductors to new radiometric standards; from the resistivity of silicon to performance standards for buildings, the technical work was sound, interesting, and to the point. It further enhanced the Bureau's reputation.