CHAPTER FOUR

REORIENTATION AND RECONSTITUTION, 1958-1964

THE BEGINNING OF A TIME OF TURMOIL

When *Sputnik I* was launched near the end of 1957, the United States was entering a period of great agitation. In fact, while the fifties are remembered as a decade of placidity and conformity, the sixties are remembered as a decade of turmoil and turbulence.[1] Names and events capture the agitation of the times: black student sit-ins at lunch counters in Greensboro, North Carolina; police dogs and water cannons attacking peaceful civil-rights demonstrators in Birmingham, Alabama; hippies and the drug culture in Haight-Ashbury; race riots and flames in northern cities and the Watts section of Los Angeles. There were college students rebelling against authorities at Berkeley and Columbia, pitched battles between police and radical youth at the 1968 Democratic convention in Chicago, and young people exploring alternative cultural forms at Woodstock. It was a time of peace marches, draft-card burnings to protest the Vietnam War, and of shocking government violence at Kent State. It was the era of the Bay of Pigs misadventure, of the building of the Berlin Wall, of Gary Powers and the U-2 spy plane incident, and of the Cuban missile crisis. The decade was punctuated with assassinations: Medgar Evers, John F. Kennedy, Martin Luther King, Jr., and Robert F. Kennedy.

Not all the events were dire. Other events were happy, and some even noble: Martin Luther King's "I Have a Dream" speech before the Lincoln Memorial; President Kennedy's request that citizens ask themselves what they could do for their country; President Lyndon B. Johnson's announcement of the passage of a comprehensive civil rights bill barring discrimination on the basis of color, national origin, or gender and his announcement of plans for a "Great Society;" Neil Armstrong's footsteps on the Moon.

Like the blows of a forge hammer on iron, these events beat the society into new legal and cultural forms with lasting effect. Foremost among the legal changes was increased civil rights protection. Although *de facto* equality was still not realized, the basis for achieving complete legal equality was laid. On a cultural level, sexual taboos were overridden, with consequences welcomed by some and abhorred by others. A drug culture was established, with unwelcome ramifications for the whole society. But perhaps the most important cultural change was society's altered attitude toward authority. Where it had once been uncommon to question government, law enforcement, and parents, now such questioning was not only done with impunity, but expected of all thinking citizens.

---

[1] Paul Johnson, *Modern Times: The World From the Twenties to the Eighties* (New York: Perennial Library, 1985). Johnson calls the section dealing with these years "America's Suicide Attempt."

Foreign Affairs

After the flight of the sputniks, Senator Styles Bridges of New Hampshire noted that the "time has clearly come to be less concerned with the depth of pile of the new broadloom rug or the height of the tail fin of the new car and to be more prepared to shed blood, sweat and tears."[2] *Sputnik I* was one factor that began jolting the Nation out of its 1950s complacency. Americans were ready to ascribe to Russia a capability that it did not have and to assert a missile gap that did not exist. This feeling was not assuaged when the Nation's first attempt at orbiting a satellite failed on national television.

President Dwight D. Eisenhower was chagrined and nonplussed by public reaction to the sputniks. He had been under great pressure to increase defense spending but had resisted, for he knew that no missile gap existed.[3] The president knew that the Nation's missile program was on schedule, and through U-2 reconnaissance was aware of the status of Soviet efforts. Indeed, before the end of the decade, Atlas, the first U.S. intercontinental ballistic missile (ICBM), became operational. Titan had begun to be developed in 1955, and the solid-fueled Polaris in 1956. In the early sixties, Minuteman, a solid-fueled ICBM, became ready for use. Convinced that public reaction was out of proportion to the danger, the president travelled around the Nation, attempting to reassure the populace that the sputniks did not represent a threat to national security.

The United States could have launched a satellite a full ten months ahead of the Soviets, but it would have been a military effort that Eisenhower did not want. The President preferred to wait until January 31, 1958, when a civilian satellite, *Explorer I,* would be launched as part of the International Geophysical Year.[4] Although launched by the Army Ballistic Missile Agency using the Jupiter-C rocket designed by that agency and the Jet Propulsion Laboratory, *Explorer I* was a civilian endeavor.[5] But the sputniks sped up the satellite effort, and a full-scale space race was on.[6] In May 1958 Eisenhower proposed the formation of a civilian space agency, and on July 29, 1958, Congress passed the National Aeronautics and Space Act, thereby forming the National Aeronautics and Space Administration (NASA) on the bones of the old National Advisory Committee for Aeronautics.[7]

Three years later, with Kennedy in office, another Soviet space spectacular shook the Nation. On April 12, 1961, the Soviets launched into orbit a satellite carrying Yuri Gagarin, the first cosmonaut. President Kennedy felt frustrated. As reported by the

---

[2] William L. O'Neill, *American High: The Years of Confidence, 1945-1960* (New York: The Free Press, 1986): 271.

[3] James Everett Katz, *Presidential Politics and Science Policy* (New York: Praeger Publishers, 1978): 119; O'Neill, American High: 270-272.

[4] O'Neill, *American High*: 273.

[5] Hugh Odishaw, "International Geophysical Year," *Science* 128 (1958): 1608.

[6] Katz, *Presidential Politics*: 119; O'Neill, *American High*: 273.

[7] *National Aeronautics and Space Act of 1958, U.S. Statutes at Large,* 72 (1958): 426.

An Atlas ICBM was shrouded for security as it moved through an unidentified town en route from San Diego to the Air Force missile testing center in Cape Canaveral, Florida (1957). (AP-Wide World Photos)

Washington bureau chief of *Time*, Hugh Sidey, at a White House meeting, Kennedy stormed: "Is there any place where we can catch them? What can we do? Can we go around the Moon before them? Can we put a man on the Moon before them? . . . Can we leapfrog? . . . If somebody can just tell me how to catch up! Let's find somebody, anybody. I don't care if it's the janitor over there, if he knows how."[8] In May 1961 the president publicly committed the Nation to landing a man on the Moon and returning him safely to Earth by the end of the decade. The Apollo Program was born.

Far more than any other foreign policy event, the sputniks influenced the programs of the Bureau. Three other events, however, recall the tenor of the times. Seeking to establish friendly relations with the Soviets and to promote peace between the two countries, Eisenhower invited Premier Nikita Khrushchev to the United States in 1959. Although miffed because of his inability to visit Disneyland (for security reasons), Khrushchev enjoyed his American tour. In the friendly "spirit of Camp David," he and Eisenhower arranged for a Paris summit conference to be held in August 1960. However, on May 1 of that year, the Soviet Union shot down and captured American pilot Gary Powers in one of the U-2 spy aircraft regularly used to fly over the Soviet Union. Unable to believe that Powers had survived despite the array of devices meant to destroy both the aircraft and its commander in the event the plane was hit, Eisenhower maintained that the U-2 was merely a weather plane. But Khrushchev produced both plane parts and pilot, catching the administration in a web of lies. The summit conference was aborted, and hopes for detente were dashed.

A far more serious encounter faced Eisenhower's successor, John F. Kennedy, in the second year of Kennedy's administration. In the wake of the abortive Bay of Pigs invasion in early 1961, the peace treaty between the Soviet Union and East Germany,

---

[8] Johnson, *Modern Times*: 629; Katz, *Presidential Politics*: 143, gives a slightly different version.

and the construction of the Berlin Wall, Khrushchev began the drastic step of emplacing missiles in Cuba. There was no hope of keeping this action a secret, and on October 15 newly constructed missile sites were photographed by a U-2 aircraft.[9] On October 22, while the armed forces were on "maximum alert," ninety B-52s with multi-megaton bombs were poised over the Atlantic, and nuclear warheads were activated on 100 missiles as Kennedy explained to the Nation what had occurred. He announced that he was instituting a "quarantine" of the island. On October 24, Soviet missile-carrying ships approached but did not cross the quarantine line, whereupon Kennedy wired Khrushchev, to request the removal of the missiles or, in his words, "a restoration of the earlier situation." In return for an American promise not to invade Cuba, Khrushchev removed his missiles from the island.[10] Both sides had faced the abyss of nuclear war but had not fallen in.

A U.S. Navy patrol plane hovered overhead as the destroyer U.S.S. *Barry* pulled alongside the Soviet freighter *Anesov*. The Soviet vessel carried a presumed cargo of outbound canvas-covered missiles. The removal of missiles from Cuba in 1962 by the U.S.S.R. brought an end to the Cuban missile crisis. (AP-Wide World Photos)

---

[9] Johnson, *Modern Times*: 625-626. It was not a small emplacement. It was to have contained 42 medium-range strategic missiles, 24 longer-range (2200 miles) missiles, 24 surface-to-air missile groups, and 22 000 Soviet troops and technicians.

[10] Ibid., 625-627.

But the defining foreign-policy event of the late fifties and early sixties was not the sputniks, the U-2 shoot-down, or the Cuban missile crisis, but the Vietnam War. This was not started by an individual event about which it could be said, "Before it we were not at war, and after it we were." Rather, a series of incremental events led the Nation into the quagmire of Vietnam. Perhaps, as historian Paul Johnson points out, the two events during the fifties and early sixties that were most influential in drawing the United States into the war were Eisenhower's refusal to sign the Geneva accords which called for free Vietnamese elections in two years, and Kennedy's acquiescence to the overthrow of South Vietnam's leader, Ngo Dinh Diem. [11]

National Affairs

When the sputniks flew, the Nation was approaching the end of the placid fifties. It had been a prosperous decade, analyzed by the economist John Kenneth Galbraith in his best-selling book *The Affluent Society*. In Galbraith's view, the modern industrial states had "mastered the difficulty of producing goods," and the days of shortages were over; the only remaining problem was the equitable distribution of goods. This was not only an economic problem, but a political one as well.[12] It was a problem not easily solved. Indeed, from 1958 to 1964, the principal domestic issue was the distribution of economic goods and political rights to African Americans. In short, the issue was civil rights.

Following the successful completion of the Montgomery, Alabama, bus boycotts at the close of 1956, African Americans had a new organization, the Southern Christian Leadership Conference (SCLC), and a charismatic leader who preached nonviolent, Gandhi-like protest, Martin Luther King, Jr. But this success did not mean that equality had arrived. In 1957, Arkansas Governor Orville Faubus refused to allow "black" students into "white" public schools, openly defying the Supreme Court's ruling in *Brown v. Board of Education.* Faubus's hard-line stance necessitated Eisenhower's reluctant use of a thousand paratroopers to force integration past an angry mob of white citizens. As late as 1961, James Meredith's attempt to be admitted to the University of Mississippi caused a riot that could not be controlled by 500 Federal marshals. As a result, President Kennedy federalized the Mississippi National Guard. Scores of marshals were injured, hundreds of rioters were taken into custody, and two bystanders were killed.

Beyond the segregation of schools, the South still had segregated public eating places, toilets, and bus and train waiting rooms. Throughout the Nation, housing was segregated. Young African Americans, most of them college students—largely from black colleges—picked up the struggle in Greensboro, North Carolina, where they worked to desegregate lunch counters. Studiously courteous and nonviolent, their example was followed in many other places in the South, and they gained considerable sympathy

---

[11] Ibid., 632-633.

[12] Ibid., 613.

from at least the northern segment of white society. They were followed by the "freedom riders," whose simple demand that interstate bus terminals be integrated provoked such vicious attacks that Federal marshals had to be called upon to protect them. Other sit-ins, demonstrations, wade-ins and boycotts followed. Sympathy for the black push toward equality mounted slowly in the white population, but the defining incident occurred in Birmingham, Alabama.

It was the SCLC's strategy to carry out "mass demonstrations and store boycotts" in Birmingham in the hope of splitting the business elite from the white leadership.[13] After a few skirmishes between civil rights protestors and the police, Eugene "Bull" Connor, Birmingham commissioner of public safety, obtained an injunction against further demonstrations. When the SCLC persisted, the leaders were thrown into jail.[14] King was held incommunicado, without mattress and blankets, until his wife Coretta contacted President Kennedy. After Kennedy's intercession King's lot improved, but the men, women, and children who marched on the Birmingham city hall were set upon by police dogs, buffeted by water from high-pressure fire hoses, and jailed. King and the other leaders were released, but the marches, dog attacks, hosing, and jailing continued. The scenes of dogs and water cannons assaulting helpless people, many of them women and children, helped to create a great deal of support for the demonstrators, and the White House sent in mediators. Although city officials remained intransigent, the business community, faced with a "paralyzing boycott and damning publicity, agreed to desegregation demands." The SCLC's strategy had worked as intended and it was clearly a victory for the African Americans.[15]

Martin Luther King, Jr., first visited the White House in 1961. He left somewhat disappointed. Concerned about his southern support, Kennedy was slow to move on civil rights. Finally, in June 1963 while southern killings continued, he called for new legislation. Medgar Evers, an NAACP official, was killed in his Jackson, Mississippi, home on June 12, and four black girls were killed in a church bombing in Birmingham in September. On August 28, 250 000 persons of all colors marched on Washington and assembled at the Lincoln Memorial to support civil rights and hear King's "I Have a Dream" speech. It was clear that the time had arrived for something to be done. But, like a historical exclamation point to this era, a killing unrelated to civil rights took place. On November 22 in Dallas, President Kennedy was struck by an assassin's bullets.

Kennedy's civil rights bill had not been passed and, ironically, it might not have passed had he lived. However, the new president, Lyndon B. Johnson, who had been looked upon with skepticism by the civil rights movement because of his southern background, saw the bill through Congress with his masterful knowledge of that institution's workings. On July 2, 1964, a civil rights bill that forbade discrimination in most public facilities was passed and became the law of the land. In 1965 Johnson

---

[13] James MacGregor Burns, *The Crosswinds of Freedom: From Roosevelt to Reagan—America in the Last Half Century* (New York: Vintage Books, 1990): 367.

[14] It was here that King wrote his famous "Letter from Birmingham Jail."

[15] Burns, *Crosswinds of Freedom*: 368.

shepherded through Congress a voting rights bill that banned literacy tests and provided for Federal registrars. There was still a long way to go, but most of the basis for legal equality was now at hand.

During the period the public became more concerned with the environment, an area in which the Bureau would get more deeply involved. The most potent catalyst in increasing this concern was Rachel Carson's publication of *Silent Spring* in 1961, ten years after her other famous book, *The Sea Around Us.* Combining a poetic vision with sound scholarship, Carson laid open to public view the destruction of the environment by booming industries, the dumping of chemicals, and the indiscriminate use of herbicides and pesticides. Also important in awakening public sensitivities to the environment were Jacques Cousteau's vivid pictorial documentations of the beauty—and fragility—of the undersea world.

Congress also became more active on environmental issues. Four pieces of legislation were passed during the period, two dealing with air pollution, one with water pollution, and the fourth dealing with the control of the sale of pesticides and other poisons. In 1959, the original Insecticides, Fungicides, and Rodenticides Act, which controlled the sales of poisons for "insects, rodents, fungi, weeds, and other forms of plant or animal life . . . which the secretary [of agriculture] shall declare to be a pest" was expanded to include nematocides, plant regulators, defoliants, and desiccants; and the power of the secretary was somewhat expanded in determining what was considered proper labelling.[16] Similarly, the Water Pollution Control Act of 1948 was significantly amended to require that research and studies be carried out on the treatment of municipal sewage, on the effects of pollution on water, on the effect of augmented flows on water quality, and on the waters of the Great Lakes.[17]

The first amendment of the original 1955 air pollution law, passed in 1962, was very brief but notable for its recognition of air pollution by motor vehicles. It instructed the surgeon general to study the substances emitted by motor vehicles to determine their effects, both harmful and benign. A little more than a year later, the comprehensive Clean Air Act was passed, having as its purposes the protection of the Nation's resources, the initiation of a national research and development program, and the provision of assistance to state and local governments. The comprehensive act again singled out motor-vehicle pollution. It mandated the formation of a committee that would monitor progress in the automotive and fuel industries and required a semi-annual report on progress from the secretary of health, education, and welfare. Slowly the Nation was becoming serious about the environment.[18]

---

[16] *Federal Insecticide, Fungicide, and Rodenticide Act, U.S. Statutes at Large,* 61 (1947): 163; *Nematode, Plant Regulator, Defoliant, and Desiccant Amendment of 1959, U.S. Statutes at Large,* 73 (1959): 286.

[17] *Water Pollution Control Act, U.S. Statutes at Large,* 62 (1948): 1155; *Federal Water Pollution Control Act Amendments of 1961, U.S. Statutes at Large,* 75 (1961): 204.

[18] *Air pollution control, U.S. Statutes at Large,* 69 (1955): 322; *Air pollution control, U.S. Statutes at Large,* 76 (1962): 760; *Clean Air Act, 77* (1963): 292.

THE GLORY DAYS

Scientists at work in the late fifties and early sixties would later recall these years as a golden age of science. Times were good. The esteem placed by Vannevar Bush on basic research in *Science, the Endless Frontier,* and formalized by the establishment of the National Science Foundation in 1950, was unquestioned. Many industrial corporations gave their scientists new freedom in doing research, and science had wide public support. Most important, research and development expenditures continued the steep upward climb begun at the end of World War II. Between 1957 and 1964, while the gross national product was increasing by a factor of 1.43 (in current dollars), national expenditures for research and development increased by a factor of 1.93, averaging a yearly growth rate of 13.41 percent. Expenditures by the Federal Government increased by a factor of 2.05, an even greater growth rate. And the character of the work was changing, with basic research increasing still faster. Thus, national expenditures for basic research increased by a factor of 2.99, and such expenditures by the Federal Government alone increased by a whopping 3.91. Even the Federal Government in-house expenditures for basic research increased by the substantial factor of 2.98. These were times that were destined to make the research scientist happy.[19]

Science also achieved a loftier position within the Federal Government. In the wake of the sputniks, President Eisenhower formed the President's Science Advisory Committee (PSAC), whose chairman was instantly known as the presidential science advisor. Science now had a presence in the White House with an advocate who had the president's ear.

A year later, on March 13, 1959, the president formed the Federal Council for Science and Technology (FCST) by Executive Order 10807. Composed of the science advisor and officials from all the departments and agencies concerned with science, the aim of the Council was to provide a means for closer collaboration and cooperation among Federal agencies involved in scientific matters. FCST was to consider the effects of new developments and problems in science, provide more effective planning and administration of programs, identify research needs, avoid duplication, and further promote international cooperation. Its chairman was to be designated by the president, and the position came to be occupied by the science advisor.

Apart from the White House, PSAC assisted in the creation of a new policy body, the Office of the Director of Defense Research and Engineering, under the secretary of defense. In February 1958, DOD formed the Advanced Research Projects Agency (ARPA) to handle its long-range projects. Early in the same year, the National Academy of Sciences (NAS) gave formal recognition to the new area of space science with the creation of the Space Science Board. And the formation of NASA in July 1958 further spurred the new science and provided vehicles for new developments in astronomy, earth sciences, and communications. Closer to home, the Department of Commerce, along with various other agencies, formed a new post of assistant secretary for science and technology, with important consequences for the Bureau. The persons filling these positions generally represented their agencies on FCST.

---

[19] *Historical Statistics of the United States: Colonial Times to 1970. Parts 1 and 2* (Washington, D.C.: Bureau of the Census, 1975): 224, 965.

The election of President Kennedy also gave science a sympathetic ally. In a May 1961 message to Congress, Kennedy announced his plans for a manned lunar landing. In June 1962, with the acceptance of Congress, the President formed the Office of Science and Technology (OST) in the Executive Office of the President.[20] With the science advisor heading OST, science policy advice was no longer denoted as a White House staff function, but rather as a full office. The main aim, originally proposed by Senator Henry Jackson's Subcommittee on Government Operations, was to provide a mechanism for determining and coordinating science activities in the Government. To that end, the National Science Foundation function "to evaluate scientific research programs undertaken by agencies of the Federal Government" was transferred to OST. But while the office could evaluate programs and had responsibility to coordinate them, it had no line authority. It was still an advisory body, although it—and hence all of science—had a voice at the highest level.[21] These were glory days indeed.

The pace of technological change and scientific discovery did not slacken during this period.[22] The electronics revolution based on the transistor and on solid-state physics continued. In 1959 Sony introduced the first solid-state television set, and in the same year the first fully transistorized business computer, the RCA 501, was manufactured. As a harbinger of what was to come, the first integrated circuits were used for gates and logic circuits in computers designed for military purposes. In communications, a marriage of solid-state electronics and space capabilities produced two developments that would "shrink the world." In 1960, *Echo,* the first (passive) communications satellite, was placed into orbit, and two years later, on July 10, 1962, *Telstar* was launched, relaying the first transatlantic television pictures. In a few years, world news would be seen in "real time." Two other developments would revolutionize the office and the home. In 1959 Xerox introduced the first commercial copy machine, bringing joy to the lives of stenographers and paper manufacturers. In 1964 the first permanent-press clothing was introduced, bringing delight to housekeepers and despair to the manufacturers of flatirons and cotton fabrics. In space, the Soviet Union made two advances. Although they would eventually be overshadowed by the United States feat of landing a man on the Moon, these accomplishments made headlines at the time. In 1959 the Soviet *Lunik II* landed on the Moon, the first man-made object to do so, and in the same year *Lunik III* returned pictures of the far side of the Moon—the first time that mankind had seen the back of its celestial neighbor.

Progress in the field of basic science was even greater, if less spectacular. In astronomy a new class of celestial objects, the quasar (for quasi-stellar object), was discovered. These objects, enormously distant, as shown by their large red-shift, and

---

[20] *Reorganization Plan No. 2 of 1962, U.S. Statutes at Large,* 76 (1962): 1253. Ironically, the OST had little input into the lunar landing decision. Kennedy obtained his advice from his line organization, i.e., NASA. Jerome Wiesner, then director of OST, was to state later that this came about because NASA and OST differed on the details of how the mission should be carried out, with NASA planning on a "straight shot" while OST proposed going via a space station. Kennedy could hardly go against his line organization in such a crucial matter. (From author's personal recollection; see also Katz, *Presidential Politics:* 143)

[21] Katz, *Presidential Politics:* 39-40.

[22] Alexander Hellemans and Bryan Bunch, *The Timetables of Science: A Chronology of the Most Important People and Events in the History of Science* (New York: Simon and Schuster, 1988).

enormously bright as shown by their immense luminosity, excited considerable speculation as to their nature. No known source could power such a massive output of energy. Later it would be proposed that the far-away quasars were very early galaxies with immense black holes at their centers.

At the opposite extreme of dimensions, Murray Gell-Mann of the California Institute of Technology (and independently Yu'val Ne'eman at Imperial College, London) introduced what Gell-Mann called the "eightfold way." A means of classifying the observed family of subnuclear particles, this method would lead to the concept of quarks, to quantum chromodynamics, and to the standard model of elementary particles. In other notable discoveries in elementary particle physics:

- Gordon Danby and his group showed that there are two types of neutrinos, the known one associated with the electron and another associated with the muon. It is now known that there is a third neutrino associated with the tau lepton, giving a family of six so-called leptons. Later it would become a part of the standard model that six quarks exist in symmetry with the leptons.
- Gerhart Lüders and Wolfgang Pauli proved the CPT theorem that conversion of a particle into its anti-particle, along with the simultaneous inversion of space and time, leaves the laws of physics intact. The CPT theorem is now recognized as one of the most fundamental theorems in physics.

In the mushrooming field of molecular biology, new discoveries came swiftly. In 1958 Severo Ochoa and Arthur Kornberg shared the Nobel Prize for the artificial production of nucleic acids with enzymes. Then in 1960, using x-ray diffraction techniques, John C. Kendrew and Max Perutz determined the structure of myoglobin and hemoglobin, respectively. In the same year, Jacques Monod and François Jacob proved the existence of messenger RNA, and a year later Robert W. Holley produced transfer RNA, the molecule that actually produces proteins. Marshall W. Nirenberg began breaking the genetic code locked in DNA when he showed that the UUU (uradylic acid) triplet of three bases in a row was the code for the amino acid phenylalanine. And in 1962 Francis H. C. Crick, Maurice Wilkens, and James D. Watson received the Nobel Prize for their determination of the molecular structure of DNA. The concept of a code for life had become reality, with a future of promise and threat from genetic engineering.

But from the point of view of the Bureau, two developments of central importance to basic measurement standards were the most important advances in physics during the period. The first of these was the production by Theodore H. Maiman of the pulsed ruby laser.[23] Based on a fundamental paper by James P. Gordon, Herbert J. Zeiger, and Charles H. Townes on the maser, this work would lead to a host of other types of pulsed and continuous-wave lasers, making possible unparalleled applications in metrology.[24]

---

[23] T. H. Maiman, "Stimulated Optical Radiation in Ruby," *Nature* 187 (1960): 493-494.

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

The other development was the work of Brian D. Josephson at the Cavendish Laboratory of Cambridge University. In a purely theoretical investigation of electron tunneling through an insulator that separated two superconductors, Josephson, while still a graduate student, showed that a voltage between the superconductors should produce an alternating current across the insulator.[25] The frequency of the current was predicted by a remarkably simple equation. The frequency was simply proportional to the voltage, with the proportionality constant being twice the ratio of two fundamental constants: the electronic charge and Planck's constant. There were thus two uses for the effect. One was the determination of the ratio of the two constants directly in terms of the basic standards of frequency and voltage. The other—which is really the inverse of the first—was to develop a measurement standard for voltage based only on the basic standard for frequency (the most accurate measurement known to science) and the ratio of the two constants.[26] It was another example of using a natural phenomenon as the basis for the realization of a unit of measurement. On January 1, 1990, a new reference standard for the volt based on the Josephson effect was adopted internationally.

THE BUREAU

Well on its way to implementing the welcome changes inspired by the Kelly Committee Report and, to some degree by the sputniks, the Bureau was about to enter its own "golden age." In the period from 1957 to 1964, its history, like that of the rest of science, did not mirror the growing turbulence of the times. Rather it was to be a time of great growth for the Bureau in which the character of its work was to change significantly. By the end of the period, all the recommendations of the Kelly Committee Report would be accomplished. No longer would transferred funds be the dominant source of financial support. Appropriations would rise dramatically, albeit insufficiently to permit the Bureau to carry out all that was demanded of it. New, talented, younger staff would be added. Most significantly, the character of the work would change, becoming more basic and more fundamental with a substantial theoretical component, a striking departure from its historical condition. Construction of a spacious modern home with several major new facilities—an immense mechanical testing facility, a linear accelerator for electrons, a nuclear research reactor—would be under way in Gaithersburg, Maryland. The Bureau would undertake a new and crucially important program on the production, analysis, and dissemination of scientific data. Most importantly, its scientific productivity would flourish.

These changes did not take place without trauma. Significant reorganization of divisions occurred, and new divisions were formed. This reorganization was accompanied by the replacement of old leaders with new, young ones. Mostly—especially at the

---

[25] B. D. Josephson, "Possible New Effects in Superconductive Tunnelling," *Physics Letters* 1 (1962): 251-253.

[26] The use of the Josephson effect as a voltage standard is dependent on a further, but related, property of the effect. If, in addition to a direct voltage, an alternating voltage is applied across a superconductor-insulator-superconductor junction, then at frequencies which are sub-multiples of the above defined frequency, the voltage-current curve of the junction shows plateaus at constant voltage.

division chief level—this replacement was done when retirement provided the opportunity. Even so, potential replacements from within were often passed over in favor of new persons from outside or younger leaders from inside, inevitably causing some resentment. New programs were undertaken, important established programs strengthened, and some established programs stopped or curtailed.

Underlying all this was a yearning by Director Astin for a statement of the Bureau's mission, a definition of the Bureau's place in the economic and social fabric of the Nation. This quest produced a series of statements of the Bureau's role and eventually led to an even more important development. Despite all the programmatic changes, the organizational structure of the Bureau remained stable through most of the period. Then in 1964, partly as a result of the quest for the definition of a role and partly as the result of an analysis by a second Kelly Committee of the role of the Department of Commerce in science and technology, the Bureau would undergo the first major structural change in its history.

Budget Matters

The key to all these changes was an increase in direct appropriations, almost certainly brought about by the effect of the 1957 launch of the sputniks. During the previous seven years, Bureau appropriations had actually decreased.[27] Appropriations in 1950 had totaled $8.8 million; they dropped to a low of $6 million in 1954, then began to rise, reaching $8.75 million in 1957. The next seven-year period was to prove greatly different. By 1964, appropriations had grown to $28.7 million, more than three times the 1957 appropriations. While substantial and surpassing the increases for research and development described in the preceding section, these increases lose some of their impact when the funding history is examined in more detail. The Bureau had been starved for funds since 1950, thus the gains after 1957 were doubly welcome.

The decreasing-increasing course of Bureau funding was mirrored by staffing levels, although the numbers are misleading. Bureau employment reached a peak of 4852 in 1953, though only 1132 were supported by direct appropriation. With the divestiture of the ordnance divisions, total employment dropped precipitously to about 2800. By 1957, total employment was 3024, with 1182 supported by the direct appropriation. Then began a steady climb that in 1964 brought the total level of permanent staff to 3905, with 2043 supported by the direct appropriation. By this time, the staff at Boulder had grown to 1230.[28]

---

[27] The figures given here are for direct appropriations for research and development. The appropriation total for each year defines the base to which the next year's increases (or decreases) are made and defines the monies available for the Bureau to carry out research and development for its measurement and standards responsibilities. It does not include special one-time appropriations for the purchase of equipment, maintenance and plant services, or the construction of facilities, which can be substantial—the cost of the Gaithersburg facilities was over $100 million. It also excludes funds from other agencies and income from calibrations, the sales of standard samples, and the reimbursement for administrative services, except as noted in the text. All figures are given in current dollars. Conversion to constant dollars does not change any of the conclusions drawn. Later in the period there were special non-base appropriations for the Civilian-Industrial Technology program and from a Special Foreign Currency Program.

[28] See Appendix G.

These numbers do not represent all the persons working in the Bureau laboratories. For example, in 1964 the total NBS population was 4557, the number above full-time employment being composed of intermittent and temporary employees, consultants, students, teachers, research associates, and guest workers.[29]

The effect of the sputniks was not felt immediately. Because the Government's fiscal year ran from July 1 to June 30, the sputniks occurred in the 1958 fiscal year. Appropriations for that year were made before July 1, 1957. Thus, *Sputnik I* was launched after the budget for FY 1958 had already been passed. Moreover, since budget hearings were held in the spring, budget documents were always prepared in the previous fall. By the fall of 1957, the budget documents for FY 1959 had already been completed. Since, as we have seen, Eisenhower was against crash programs and, in addition, did not feel that the sputniks offered any threat, those budget requests for 1959 were not changed. But the Nation could not stand to be in second place, and the Congress wanted action. In particular, the House Appropriations Committee, under the sympathetic Prince H. Preston of Georgia, wanted to give NBS more money. As the committee well knew, Astin could not ask for more than the administration permitted and so had to defend a smaller budget than Congress would have approved. In early 1958, the Bureau requested an increase of $2.07 million for FY 1959. Preston deftly drew out that the original Bureau request was for an increase of approximately $3.5 million, but that this amount had been reduced by the department. Having a little fun at the expense of Astin and the department, Preston asked:

> MR. PRESTON: It is not likely that you would have materially altered this budget had sputnik been launched at that time; is it?
>
> DR. ASTIN: I doubt that it would have been materially changed.
>
> MR. PRESTON: There seems to be general agreement between the Department, yourself, and the Bureau of the Budget on this?
>
> DR. ASTIN: Yes, sir.
>
> MR. PRESTON: If the Congress approves the amount that you have requested, in toto, will you essentially be able to fulfill your mission as the Bureau of Standards?
>
> DR. ASTIN: We will not be doing all of the things we should be doing but it is an important first step.[30]

---

[29] These figures come from the following sources, respectively: Kelly Committee Report: 105; MFP Appendix H; 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, House of Representatives,* 85th Cong., 1st sess., National Bureau of Standards, 12 March 1957: 166; Annual Report, 1964: 236; House Committee on Appropriations, 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 1965: Hearings Before a Subcommittee of the Committee on Appropriations, House of Representatives,* 88th Cong., 2d sess., National Bureau of Standards, 21 February 1964: 588.

[30] House Committee on Appropriations, Subcommittee on Department of Commerce and Related Agencies, *Department of Commerce and Related Agencies Appropriations for 1959: Hearings Before a Subcommittee of the Committee on Appropriations, House of Representatives,* 85th Cong., 2d sess., National Bureau of Standards, 23 April 1958: 420-423.

Later in the same hearings the subject of high-temperature measurements came up, led by Congressmen Daniel J. Flood of Pennsylvania and Sidney R. Yates of Illinois. It was believed, or perceived, that the Soviet Union had far greater capabilities for measuring high temperatures than did the Bureau—6000 °C as contrasted to 3000 °C. Astin explained that in order to extend the range of measurements, a new facility would have to be built. Mr. Yates asked:

> MR. YATES: Why do you not ask for the facility then?
> DR. ASTIN: I hope that ultimately we will be permitted to ask for it.
> MR. FLOOD: Permitted to ask for it?
> MR. YATES: Who is preventing you from asking for it?
> DR. ASTIN: We have prepared an analysis of major needs, at the request of the Secretary of Commerce, and this is under consideration.
> MR. YATES: And in the meantime you are falling behind every day?
> DR. ASTIN: Our requests were rather substantial.[31]

It was clear that the way was open for significant budget increases. Congress would certainly not stand in the way. That year the Congress approved the full $2.07 million increase. Next year the Bureau asked for an increase of $6 million and received $5.75 million. In the two years following the sputniks, the Bureau appropriation increased from $9.73 million to $17.25 million. With Astin explaining how the demands on the Bureau exceeded the Bureau's ability to fulfill them and comparing Bureau capabilities to those of the Soviet Union when possible and strategically advantageous, the budget continued to increase, although not as greatly as the demands. By FY 1964, the Bureau appropriation was $28.7 million.

In 1957 the Bureau, of course, did not know that its appropriation would expand in the manner it did. Moreover, one of management's greatest concerns was to bring transferred funds into balance with appropriated funds in the manner recommended by the Kelly Committee. There was no evidence that this balance would occur of its own accord. Indeed, in fiscal years 1956-1958 the ratio of transferred to the total of transferred plus appropriated funds remained relatively constant at 63 percent to 65 percent. To try to arrive at a policy on transferred funds, Nicholas E. Golovin, then associate director for planning, undertook a study of work performed for the Bureau of Aeronautics (BuAer) as representative of other-agency work in general.[32] The BuAer was a large client of the Bureau, with a $1.43 million program spread through nine of the Bureau's divisions. Golovin found that 26 percent of the BuAer funds were "convertible" and the remainder "nonconvertible." In accordance with definitions contained in the Kelly Committee Report, the term convertible meant that the work was such that the Bureau could justify it as part of its measurement and standards

---

[31] Ibid., 433.

[32] Memorandum, N. E. Golovin to A. V. Astin, "Bureau of Aeronautics Programs," August 2, 1957. (NARA; RG167; Astin file; Box 17; Folder Golovin Correspondence Relating to NBS Programs and Administrative Problems, 1956-1958)

mission hence desirable because it added to basic Bureau capabilities. The nonconvertible work was quite different. The Bureau could not justify it as part of its basic mission; it was purely a service to the agency requesting it. But the Bureau went a step beyond the Kelly Committee Report. Not only would it undertake to carry out the desirable convertible work for other agencies, it would, in fact, seek direct Congressional appropriations to carry out the work, much as was done with the conversion of the Interservice Radio Propagation Laboratory into the Central Radio Propagation Laboratory (CRPL).

Golovin suggested a policy statement for the acceptance of work of other Federal agencies. The policy stated that NBS would not undertake industrial-type product or device development, evaluation, or testing work unless [1] there were a national emergency, [2] there were no "alternative source of capabilities . . . inside or outside the federal government," [3] the Bureau had been assigned a central mission for doing the work, or [4] if "test and evaluation work is limited in extent or duration, and is definitely subordinated to . . . developing data and/or test procedures of sufficient interest to warrant publication. . . . " If the third or fourth conditions applied, it was understood that the work would eventually be financed by direct appropriations.[33]

Upon receiving this memorandum, Astin appointed a committee under the chairmanship of Robert D. Huntoon to study the application of this policy, first to specific divisions and then to the rest of the Bureau.[34] After considering a number of options, the committee came up with a remarkable proposal.[35] All the nonconvertible other-agency work would be carried out in a separate organizational unit with the tentative title Institute for Applied Science. Reporting to an associate director for applied science, "the institute would be part of NBS with Civil Service Commission employees, but would be financed entirely from transferred funds and would not ask for Congressional support." An important point made by the committee was that the institute would be similar to the Diamond Ordnance Fuze Laboratory before its divestiture by the Bureau. Administrative services would be shared, employees of the "regular" Bureau could occasionally go to work in the institute, and special NBS capabilities (such as vacuum-tube fabrication) could be utilized via work orders. Convertible transferred-funds work would be carried out as usual except that steps would be taken to convert it to appropriated-funds work.

---

[33] Memorandum, N. E. Golovin to A. V. Astin, "Working Fund Work Policy Statement," September 30, 1957. (NARA; RG167; Astin file; Box 17; Folder Golovin Correspondence Relating to NBS Programs and Administrative Problems, 1956-1958)

[34] Memorandum, A. V. Astin to Committee Members, "Special Study of NBS Programs Supported by Other Federal Agencies," October 16, 1957. (NARA; RG167; Astin file; Box 17; Folder Golovin Correspondence Relating to NBS Programs and Administrative Problems 1956-1958). The members of the committee were R. D. Huntoon, associate director for physics; N. E. Golovin, associate director for planning; I. C. Gardner, chief, Optics and Metrology Division; W. Ramberg, chief, Mechanics Division; I. C. Schoonover, chief, Mineral Products Division; F. B. Silsbee, chief, Electricity and Electronics Division; and Carroll C. Stansbury, chief, Electronic Instrumentation Section. Later, Dean Judd, the Bureau's expert on color measurements, was added to the committee.

[35] Memorandum, R. D. Huntoon, chairman, Special Committee on Programs Supported by Other Agencies to A. V. Astin, "Report of Committee," January 20, 1958. (NARA; RG 167; Astin file; Box 18; Folder Testing by NBS 1952-58)

All in all, the proposal to establish an institute whose sole purpose was to conduct—with transferred funds—scientific work that the Bureau would not carry out under its basic measurement mission was a strange proposal indeed. Only a few years earlier the Bureau had such an institute in the Diamond Ordnance Fuze Laboratory. Admittedly, under the pressure of the Korean emergency, the laboratory had grown so big that the Bureau had to divest it lest the Bureau lose its identity. Now the committee was proposing to create another such institute. As the committee pointed out, the clear identification of this entity as separate and bearing only an administrative relation to the Bureau would permit complete budgetary separation between direct appropriations and convertible transferred funds on the one hand, and nonconvertible transferred funds on the other. Perhaps it was felt that this clear separation would protect the Bureau's measurement mission. The separation would be a sign to all that the Bureau was doing this work purely as a service to other agencies and would not do it otherwise, except in a national emergency, under direct orders, or if no other organization was capable of doing the work.

Whatever the reasons for the proposal, it does not appear to have led anywhere. In fact, the rising appropriations decreased the need to rely on transferred funds. While appropriations rose to $28.7 million in 1964, transferred funds were reasonably constant between 1957 and 1964, averaging $15.26 million per year—a maximum of $17.88 million in 1964 and a minimum of $13.22 million in 1961. Indeed, in 1960, for the first time since before World War II, appropriated funds exceeded transferred funds, being 53.8 percent of the total. By 1961, transferred funds were less than 40 percent of the total and remained near that figure for the rest of the period. There is also some evidence that the character of the transferred-funds work was changing. The criteria applied to the acceptance of transferred-funds work in 1957 were that it have a close relationship to the basic functions of NBS, show a prospect of producing results of general value to science, or that it could not be undertaken as effectively elsewhere.[36] But no one knew the optimum amount of transferred-funds work, or even if there was an optimum amount. Hence the subject continued to be discussed by Bureau staff members in memoranda to their superiors in the Bureau and in the department.[37] Despite all this study and communication, something like the policy as stated in Golovin's original memorandum remained the guiding policy on transferred-funds work. The subject would continue to concern Bureau management for the remainder of this history.

---

[36] Report, "Analysis of Program Conversion." (NARA; RG 167; Astin file; Box 20; Folder Correspondence 1958). This unsigned, undated eight-page document appears to be an analysis for Bureau management of transferred funds from 1947 to 1958. The context indicates that it was written in 1958.

[37] Memoranda: N. L. Christeller to A. V. Astin, "Conversion Policies," January 27, 1962; Under Secretary E. Gudeman to A. V. Astin, "Conversion Program in National Bureau of Standards," January 18, 1962; L. M. Branscomb to A. V. Astin, "Transferred Fund Policies," October 30, 1961. (NARA; RG 167; Astin file; Box 15; Folder Transfer of Funds)

In 1960, funds from an unusual source became available for the Bureau to carry out a part of its program.[38] Through its Commodity Credit Corporation (CCC), the United States sold surplus agricultural commodities to various nations and was paid by the purchasing country in the local currency. By the terms of the law governing these sales, the United States was obligated to spend these funds in the purchasing country. The money could be used for such essential expenses as support of the U.S. embassy.[39] In some cases the sales exceeded these necessary expenses and a surplus of funds in the local currency accrued to the CCC account. The NSF requested that the Bureau of the Budget permit various Government agencies to use these funds to support research in the foreign country to further those agencies' programs. Such permission was granted, and thus was born the Special Foreign Currency Program, or, less formally, "PL-480 money."

However, by the terms of PL-480, the agencies could only use these surplus funds if the Congress appropriated U.S. dollars to the agencies for "purchase" of the funds from the CCC. In effect, U.S. funds were transferred from the Treasury's general account to the CCC account. The net result was to use surplus commodities for the purchase of research services, but the appropriations committees remained in charge of the agencies' funds.

The Bureau, with excellent knowledge of the scientific work in other countries and seeing a way of carrying out work that it could not do itself, was naturally interested in obtaining PL-480 money. The funds did not permit the addition of staff, but they permitted the support of foreign workers in fields where knowledge was important to the Bureau program and, in a roundabout way, permitted more foreign travel than was possible under the chronically inadequate foreign-travel allotment. In 1960, the Bureau asked for $5.17 million for work in Spain, Yugoslavia, Poland, Israel, India, and Pakistan, but received nothing. In 1961, NBS asked for $5 million, now omitting Spain from its list of countries, but the Joint House and Senate Conference Committee deferred the appropriation pending further studies. In 1962, proposing projects only in Israel, India, and Pakistan, it asked for and received $1 million. In 1963 and 1964, the Bureau again asked for $1 million, and received half the requested amount. The work included such studies as the calculation of atomic spectra (Israel), the propagation of electromagnetic waves in the earth (India), standard reference materials (Pakistan), and translations—particularly from the Russian literature—on highly specialized topics of little interest to U.S. publishers. It was a useful program, expanding the Bureau's international activities. The Bureau continued to receive funds from this source into the 1980s, when the CCC funds in excess were essentially depleted.

---

[38] 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, House of Representatives,* 86th Cong., 2d sess., National Bureau of Standards, 13 January 1960: 264-281.

[39] *Agricultural Trade Development and Assistance Act of 1954, U.S. Statutes at Large,* 68 (1954): 454.

An Evaluation and a Status Report

Doubtlessly pleased with the results of the Kelly Committee Report in 1953, Secretary Weeks, as one of his last acts at Commerce, in late 1958 asked the National Academy of Sciences to carry out a similar study of science and technology in the whole Department of Commerce, not merely on the Bureau alone. Weeks requested that a committee be appointed "to undertake an up-to-date evaluation of the functions and operations of the Department of Commerce in relation to present national needs,"[40] and Detlev W. Bronk, then president of the Academy, appointed Mervin J. Kelly as chairman of the committee.[41] Their report, *The Role of the Department of Commerce in Science and Technology,"* was delivered on March 2, 1960, to then Secretary of Commerce Frederick H. Mueller, the second secretary to hold the office after Weeks had commissioned the second Kelly study shortly before he returned to private life.

The objective of the study was "to evaluate the functions and operations of the Department of Commerce to insure that it is fulfilling its responsibilities in the interest of science and technological progress."[42] For this purpose the committee studied all the agencies of the department that had some bearing on science.[43] In addition, the committee investigated the overall management of science at the secretarial level. In its analysis, the committee found that in comparison with other nondefense science and technology institutions, financial support in the Department of Commerce was relatively inadequate and had not progressed as rapidly as that of other institutions. Most important, the committee found that "the explanation lies largely in Commerce's organization structure and personnel at the administrative level."[44] The administration of the seven agencies in the study was divided between two "assistant secretarial offices." These assistant secretaries had other competing responsibilities and, in addition, the offices had not been "filled . . . by men with background in science and technology and understanding of the operations and needs of research and development." To rectify this situation the committee recommended the formation of "an assistant secretaryship for science and technology" to provide "professional leadership at the top administrative and policy levels." It was the only department-wide recommendation made by the committee.

---

[40] Letter, D. W. Bronk to Secretary of Commerce F. H. Mueller, March 2, 1960. This is the cover letter for the committee report.

[41] The other members of the committee were Horace R. Byers, Michael Ference, Jr., Paul M. Frye, Frank W. Herring, Augustus Kinzel, H. A. Leedy, Dale F. Leipper, C.G. Suits, and Abel Wolman.

[42] *The Role of the Department of Commerce in Science and Technology: A Report to the Secretary of Commerce by a Special Advisory Committee of the National Academy of Sciences* (Washington, D.C.: NAS-NRC, 1960): 3. Hereafter referred to as the Second Kelly Committee Report.

[43] Besides the National Bureau of Standards, the agencies were the U.S. Coast and Geodetic Survey, Maritime Administration, Patent Office, Bureau of Public Roads, Office of Technical Services, and Weather Bureau. The Bureau was by far the largest, with appropriated expenditures equal to the combined scientific expenditures of all the other agencies.

[44] Second Kelly Committee Report: 6.

The recommendation was adopted, the position established, and in May 1962 its first occupant, J. Herbert Hollomon, was installed. Hollomon was a forceful and dynamic research manager-metallurgist from the General Electric Research Laboratory who would have a profound effect on the organization, programs, and manner of operating at the Bureau.

Earlier, in 1958, at about the time Astin learned that Secretary Weeks had requested the formation of an evaluation committee, he had been discussing the major needs of the Bureau with Under Secretary Walter Williams.[45] At the request of the under secretary and doubtless spurred by the impending formation of the second Kelly Committee, Astin prepared a study paper for use by the secretary of commerce in planning for Bureau activities.[46] Along with a three-page memorandum for the secretary, the study paper provided an analysis of both the legislative and the financial needs of the Bureau. In it Astin clearly pointed out that certain very important activities carried out by the Bureau did not arise directly from its organic act, but were carried out because of frequent ad-hoc assignments or other-agency arrangements. These activities needed a base of mission statements with legal impact. For example, the functions of the CRPL were not clearly defined, leading other agencies to try to duplicate some of them. Astin felt that an Executive Order, perhaps initiated by the science advisor, would simplify the operation of the CRPL for both the Bureau and the Department of Defense.

Other activities in which the Bureau's role required greater clarification were Boulder's Electronic Calibration Center, under construction with appropriated funds at the request of the DOD but without firm commitment from Defense for its support; the Building Technology Division, which was becoming a central government building-research activity with no authoritative definition of its scope and objectives; the Cryogenic Engineering Division, set up with AEC funds and subsequently transferred to the Bureau when AEC needs diminished; the Data Processing Systems Division, which was becoming a sort of central government service organization for computers, but without an authoritatively defined mission; the Applied Mathematics Division, which was in much the same situation; and the National Hydraulics Laboratory, which had never received sufficient support to become a truly national research facility and whose facilities were being duplicated and surpassed in other agencies.[47]

Astin made two other points in this administrative portion of his study. Both had to do with the difficulty of hiring and retaining staff, particularly at the higher levels. Government pay scales were simply not high enough to compete with the private

---

[45] Memorandum, A. V. Astin to W. Williams, "Major Needs of the National Bureau of Standards," March 3, 1958. (NARA; RG167; Astin file; Box 36; Folder "A"-NBS Functions)

[46] "Study Paper on Major Needs of the National Bureau of Standards," February 25, 1958. (NARA; RG167; Astin file; Box 36; Folder "A"-NBS Functions). The first page of the study paper is unfortunately missing from the record.

[47] A national hydraulic laboratory facility was authorized on May 14, 1930. It was to have been a facility available to all agencies of the Government to obtain fundamental data in hydraulic research and engineering. (*Bureau of Standards, hydraulic laboratory to be established, U.S. Statutes at Large,* 71 (1930): 327)

sector, and Astin requested an increase in the number of PL-313 positions allocated to the Bureau.[48] In addition, he strongly recommended legislative requests that would increase the general pay scales of scientists.

In a long portion of the study Astin asked for funds for specific equipment, facilities and programs. Stating that this was not an exhaustive inventory he listed twenty-three specific items, ranging from $81 million for new facilities at Gaithersburg to $250 000 in yearly operating costs for a program in operations research. While most of the requests were the type normally made by research directors, three illustrate some of the new directions the Bureau was taking and the change in its character. There was a request for $4.7 million for a high-intensity linear electron accelerator and $4 million for a companion research nuclear reactor. These two facilities would in due course be realized, and the Bureau would become a leader in their use.

But one item more than any other illustrated how the nature of the Bureau's work was changing in 1958. In item number 6, the Bureau requested $500 000 as yearly operating costs for theoretical studies. Stating that "one of the current weaknesses of the Bureau's programs is inadequate highly specialized attention to the theoretical aspects of experimental work . . . ," Astin proposed to "establish strong theoretical groups at both Washington and Boulder on a sufficiently large scale to attract the desired and needed talent."[49] No such autonomous groups would be formed, but theoretical work was rapidly becoming a strong—if decentralized—component of Bureau activities.

As did the first Kelly Committee, the second carried out an intensive, division-by-division investigation of the Bureau. It gave the Bureau high praise, writing "the Bureau represents an important and permanent scientific resource of government, and should be given the support warranted by so valuable an asset that insures the maintenance and enlargement of its scientific strength and effectiveness."[50] More

---

[48] Enacted on August 1, 1947, Public Law 313 (*War and Navy Department, professional and scientific service, U.S. Statutes at Large,* 61 (1947): 715) established a new class of Civil Service positions within the War and Navy Departments. To "effectuate those research and development functions, relating to the national defense . . . ," these positions could be granted by the secretaries of the departments, upon approval by the Civil Service Commission, without competitive examination. The law specified the number of positions allotted to each department, as well as the minimum and maximum salaries permitted. The salaries were higher than those for the GS-15 level, the normal top of the grade. From time to time amendments to the act extended the authority to other agencies, revised the number of positions allotted, and the salary range.

In a similar manner, Public Law 429 (*Classification Act of 1949, U.S. Statutes at Large,* 63 (1949): 954) specified the number of positions for the whole Civil Service in grades GS-16, GS-17, and GS-18—the so-called "Super Grades." Again periodic revisions in number and salary were made. In 1965, the Bureau had 39 appointees in GS-16, and 29 in GS-17. It also had 12 under Public Law 313. Memorandum, G. R. Porter to A. V. Astin, June 14, 1965. (NARA; RG 167; Astin file; Box 20; Folder Correspondence 1965)

[49] Study Paper: 8.

[50] Second Kelly Committee Report: 87.

important, as part of its investigation, the committee asked Astin to prepare for them a statement of the missions of the Bureau, and what resulted was the first clear statement during Astin's tenure of these missions.[51] Included as an appendix to the committee report, the statement won high praise from the committee, which wrote: "[the paper] gives evidence of a clarity of vision of the Bureau's place in government and in the nation's scientific and industrial communities."[52]

The statement was based largely on the 1958 study paper Astin had written for the secretary but with only an abbreviated list of desired projects and considerably more on the mission of the Bureau. Using the 1950 version of the Organic Act as the basis for his discussion of the Bureau's mission, Astin pointed out that the legal functions of the Bureau were very broad and permitted "almost any sort of technical activity" but were not much help in setting priorities.[53] Astin looked at the mission statement as useful in setting priorities. After a cogent discussion of the functions listed in the enabling legislation, Astin arrived at a single statement for the Bureau's primary mission:

> The National Bureau of Standards seeks to provide the central basis for a self-consistent and uniform system of physical measurement throughout the United States.[54]

Having such a statement, the relevance of a program to a "particular measurement problem and its importance to scientific and technological industrial progress" may be judged and hence priorities set.[55]

But this statement did not encompass all the activities of the Bureau. In particular, it did not come to grips with the other-agency problem. Here Astin felt an obligation to perform technical services for other agencies but only to the extent that such work did not interfere with the primary mission. There would be no going back to the ordnance development days. Moreover, if a particular task were to be substantial and continuing, a formal assignment of responsibility would be desirable, as Astin had already proposed in his study paper. Again he stressed the need for better definitions of the missions of CRPL and other Bureau programs. In smaller, miscellaneous projects for other agencies, Astin's policy was to give preference to those jobs where the Bureau had some unique competence or which enhanced its measurement-standards competence. Finally, Astin noted a very important point. In the field of industrial standards, codes, and specifications, the Bureau had no primary responsibility for the development or promulgation of these standards, but it made its competence available to the organizations that had the responsibility.

---

[51] Minutes of associate director meetings, November 3 and November 5, 1958, and November 6, 1959. (NARA; RG167; Astin file; Box 29; Folder Old—AD Minutes). This mission statement was worked on by the associate directors in late 1958 and finished in late 1959 for publication in the Annual Report, 1960.

[52] Second Kelly Committee Report: 83.

[53] Ibid., 97.

[54] Ibid., 98.

[55] Ibid.

The committee took good note of Astin's paper. Their recommendations reflected the desires of the Bureau. The committee called for: yearly expansion of 15 percent in the measurement standards area (a rate that could not long be sustained); rapid completion of the Gaithersburg installation; an Executive Order for the CRPL; special committees of scientists and engineers to review the scope and activities of the Cryogenic Engineering Laboratory, the National Hydraulics Laboratory and the Building Technology Division; appropriated funds to be used for measurement and standards activities of broad national interest; a substantial increase in the number of PL-313 positions; more effective technical advisory committees; and an analytical review and report for the Statutory Advisory Committee, by the director, of the Bureau's various activities for other agencies. Not all of these recommendations would be carried out, but they were an eminently reasonable set.

The interest in a well-articulated mission statement did not end when Astin framed one for the second Kelly Committee. On September 9, 1960, the Bureau issued a mission statement and published it in its annual report for 1960. An expansion of what was provided for the committee and less pedagogical in tone, the statement was built around the measurement-standards functions of the Bureau but did not contain the condensed single-sentence version noted above. Nor did it come to grips with the Bureau's special responsibilities, instead promising that the Radio Propagation Laboratory, the National Hydraulics Laboratory, the data processing systems program, cryogenic engineering, building technology, and fire research would be "dealt with in later separate statements."[56]

In cryogenic engineering and building technology, the special statements would take the form of reports of the National Academy of Sciences. Following the recommendations of the Kelly Committee, the Bureau asked the academy to appoint committees of experts to "determine the scope of the Bureau's responsibilities in these areas." The academy did so, appointing an ad-hoc committee to investigate the cryogenic-engineering area and a committee from the Building Research Advisory Board of the National Research Council (NRC) to investigate the building-technology area. The two reports were delivered in 1962, the cryogenics on January 15 and the building technology on May 21.[57] Both reports, though each with a different emphasis, cautioned that the Bureau should not compete with industry in the development of proprietary products but found that the Bureau did have a role in these areas.

The cryogenics panel in effect wrote a mission statement for the Bureau's cryogenics work. Pointing out that the competence of the Bureau was utilized in reinforcing

---

[56] Study Paper: 4-5; Second Kelly Committee Report: 95; Annual Report, 1960: 150.

[57] "Report of the Ad-Hoc Cryogenic Engineering Panel," Dr. Clyde McKinley, chairman, January 14, 1962 (Washington, D.C.: National Academy of Sciences–National Research Council, 1962). (NARA; RG167; Astin file; Box 21; Folder NAS/AVA-1953-1969); "A Program for Building Research in the United States, a Report for the National Bureau of Standards by a Special Advisory Committee of the Building Research Advisory Board," Richard G. Folsom, chairman, May 21, 1962. Publication 994 (Washington, D.C.: National Academy of Sciences–National Research Council, Building Research Advisory Board, 1962). (History Project File, Chapter 4, Folder Building Res. Ch. 4). See also Bascom W. Birmingham, "Program of the Cryogenic Engineering Laboratory Division," February 1964. A "white paper" courtesy of the author. (History Project File, Chapter 4, Folder Birmingham White)

the capabilities of other Government agencies to further the national need in cryogenics, the panel noted that in carrying out this function the agency's "activities are broadly basic in character, in keeping with the primary measurement mission of the Bureau." The committee then listed five specific types of activities, all concerned with providing information on cryogenic engineering, including the maintenance of a National Cryogenic Data Center and the furnishing of advisory and consultative services to other government agencies and the general public. It is clear that the cryogenics panel saw a useful role for the Bureau in the cryogenics area.[58]

The Building Research panel made a very different recommendation. It recommended that there be established a National Institute of Building Research with the mission of "stimulating and sustaining a correlated and continuing national program of building research," that the Institute be organized under the Bureau, and that the Bureau's research activities be incorporated in the Institute. It was a wide-ranging recommendation but showed clearly that there was an important role for the Bureau in building research.[59]

However, Astin did not accept the recommendation of the panel. He wrote to Hollomon, "I am skeptical that a National Institute of Building Research, established within the NBS and subordinate to it, could effectively attain the full stature and importance which the recommended program elements demand. . . . It should be essentially on the same reporting level as NBS."[60] How Astin proposed to deal with the special mission for building research is discussed in a later section of this chapter.

Fire research was in somewhat the same state as building research. An NAS-NRC panel, under joint sponsorship of the U.S. Forest Service, the DOD, the NSF, and the Bureau, studied the fire problem during several weeks in the summer of 1961 and recommended that a fire group should be formed in the Federal Government. This group would continually assess the fire program in the whole Nation and "arrange for the execution of work not now adequately supported."[61] The group should have a full-time director and a full-time technical staff. A budget of $3 million was suggested as a start, rising to perhaps three times that amount. It is not difficult to think of this as an institute as well. Indeed, the recommendations were referred to the Federal Council for Science and Technology (FCST), which gave this responsibility to the Department of Commerce. The Bureau was then designated "a central agency for fundamental fire research in the physical sciences . . . " and, with funds provided by the DOD and the Office of Civil and Defense Mobilization, it began to support fundamental research in

---

[58] "Report of the Ad-Hoc Cryogenic Engineering Panel," 5.

[59] "A Program for Building Research in the United States:" 7.

[60] Memorandum, A. V. Astin to J. H. Hollomon, "NAS-NRC report 'A Program for Building Research in the United States,'" February 21, 1963. (DOC; Assistant Secretary for Science and Technology; Accession 40-68B-6087; Box 28; Folder National Bureau of Standards)

[61] *A Study of Fire Problems: a Study held at Woods Hole, Massachusetts, July 17 to August 11, 1961 under the guidance of the Committee on Fire Research of the National Academy of Sciences-National Research Council, Division of Engineering and Industrial Research* (Washington, D.C.: NAS-NRC, 1961) NAS-NRC publication 949: 1. See also NAS Annual Report, 1961-1962: 79-80.

other laboratories.[62] Plans were made to form a National Center of Fire Technology to report to Hollomon, but this did not to come to pass.[63]

It was not clear to everyone that the Bureau had a role in these areas. On September 25, 1962, eight months after the cryogenics panel report, Hollomon wrote to Astin: "I wish to initiate the necessary steps and come to a decision concerning the Cryogenics Laboratory at Boulder. Further, I think we agree that the work underway there does not fall within the responsibility of the Bureau of Standards. . . . I think it very likely that it may be carried out by private enterprise rather than by government and it is in competition with private enterprise. Thus, I would appreciate your indicating the steps that are necessary for the government to cease operating the Cryogenic Laboratory."[64]

Astin's thoughts on receiving this letter are not known. What happened was that Bureau management, with these two reports in hand, took a different course and rewrote the mission statement extensively. It produced a strikingly different document from that prepared in 1960. On June 20, 1963, Deputy Director Huntoon wrote to the division and section chiefs on the restatement of the mission:

> The mission of the National Bureau of Standards is to conduct research and perform essential central national services in the area of physical measurement which are performed with the objective of facilitating the reliable and efficient exchange of quantitative data and of technological products and services in science, engineering, industry and commerce.
>
> The research and services aspects of the mission are considered as equal in priority since neither can exist without the other. Without the service function there is no solid justification for NBS to continue as a Federal research institution. Without the research function the service function cannot be effectively performed. Therefore, both must be emphasized and neither to the detriment of the other.[65]

The document then went on to list eight mission components: direct services, physical measurement standards or measurement systems, standard reference data, standard reference materials (previously called standard samples), engineering measurements and standards, special central responsibilities, general informational research (undirected basic research), and technical services, such as mathematical, instrumental and analytical services.[66]

---

[62] Annual Report, 1960: 109.

[63] "Minutes of the Annual Meeting of the NBS Visiting Committee," May 14, 1962: 4. (NIST RHA; Director's Office file; Box 354; Folder 1962—Visiting Committee)

[64] Memorandum, J. H. Hollomon to A. V. Astin, "Cryogenics Laboratory," September 25, 1962. (NARA; RG 167; Astin file; Box 17; Folder Correspondence re. Senior Appointments)

[65] Memorandum, R. D. Huntoon, deputy director to division and section chiefs, "Revised Mission Statement and Associated Programming Instructions," June 20, 1963: 1. (NARA; RG 167; Astin file; Box 15; Folder Mission)

[66] Ibid., 1-3.

The most interesting of these components was "special central responsibilities." The memorandum states, "The enabling legislation of the Bureau and the stated mission are broad enough to permit either temporary or long term activity in connection with central national responsibilities. . . . When authorized and activated these will be included in one component of the mission, called . . . Special Central Responsibilities."[67] Only three examples of these were listed: radio-propagation research and services, building research, and data-processing-systems research and services. Missing from the list were cryogenic engineering and the National Hydraulics Laboratory. With this new mission, the cryogenic-engineering activities fell under direct services, standard reference data, and engineering standards, so that by redefining the mission, the activities of this program fell under three of the defined mission components, thereby obviating the need for a special mission. The three listed still needed special mission justification.

The analyses of the Hydraulics Laboratory's functions and fate came about in a somewhat different manner. Astin had been concerned for some time about what to do with this laboratory, particularly since the impending move to Gaithersburg made a decision on facilities at the new site imperative.[68] Thus, doubtless at his instigation, in November 1960 the FCST appointed a committee with Astin as chairman to decide the fate of the laboratory.[69] After a few meetings, Astin reported to the FCST on the decisions the committee had reached. The committee recommended that the legislation establishing the Laboratory be repealed, provided that all agencies involved in hydraulics strengthened their basic research programs; it provided a list of recommendations for strengthening basic research in hydraulics; and it recommended that a limited research hydraulics facility be provided for the Bureau at the Gaithersburg site. This was in fact done.

---

[67] Ibid., 3.

[68] Memorandum, C. N. Coates to A. V. Astin, "Advisory Committee Recommendations Regarding National Hydraulics Laboratory," September 8, 1958. (NARA; RG 167; Astin file; Box 16; Folder National Hydraulics Laboratory, Army Corps of Engineers). Authorized in 1930 (*Bureau of Standards, hydraulic laboratory, U.S. Statutes at Large,* 46 (1930): 327) as a laboratory to obtain fundamental data in hydraulics research and to serve as a central Government laboratory in this field, the then-not-inconsiderable amount of $350 000 was authorized and appropriated for the facilities. However, operating funds commensurate with this broad authority were not provided and the laboratory operated principally on transferred funds. In 1961 it had a staff of fifteen and expenditures of $69 000 from direct appropriations, and $109 000 from transferred funds. By comparison, the Navy spent $1.41 million, the U.S. Geological Survey $330 000, and the USDA Soil Conservation Service $1.85 million, only slightly less than half in basic research. The laboratory did regularly publish a directory of all the hydraulics research carried out in the United States and Canada. From 1933 to 1942 these directories were published as the *Bulletin. Series A: Current Hydraulic Laboratory Research in the United States,* from 1947 to 1971 as *Hydraulic Research in the United States,* and from 1974 to 1980 as *Hydraulic Research in the United States and Canada.*

[69] Letter, L. Carmichael, chairman of the Standing Committee of FCST, to A. V. Astin, December 1, 1960. (NARA; RG 167; Astin file; Box 16; Folder FCST Hydraulics Lab. Panel). The committee members in addition to Astin were, A. L. Cochran, Corps of Engineers; L. B. Leopold, U.S. Geological Survey; F. D. Rigby, Office of Naval Research; G. B Schubauer, National Bureau of Standards; R. J. Seeger, National Science Foundation; T. W. Edminster, U.S. Department of Agriculture; and J. Westrate, FCST Standing Committee.

The Bureau did, however, pick up a new amendment to its organic act on September 7, 1958.[70] Unlike the 1950 revision, it had nothing to do with the basic responsibilities and authority of the Bureau. Rather, along with other similar housekeeping matters, it clarified the secretary of commerce's authority to acquire or lease land for field sites and to undertake the construction of buildings. The law had no effect on the work the Bureau was authorized to carry out and, hence, was of no historical consequence.

An Organization Changes

As with any organization, the Bureau underwent periodic alterations in the makeup of its organization when individuals left for retirement or other reasons, and organizational units were added, subtracted, or had functions modified as the aims of the institution changed. During the period 1957-1964 there were a number of the former type of changes, and many of the latter type as management strove to redirect the nature of the research carried out. However, in these changes the basic line structure of section-division-director was not altered. In 1964, a major change occurred in which a new level of "institute" was added, so that the line structure was now section-division-institute-director.

Along with this new line structure, there was, of course, a support and staff structure, sometimes organized as divisions, sometimes as offices, and sometimes simply by title. One of the most important changes made during the period occurred in 1958, when the position of deputy director was formed. Robert D. Huntoon was the office's first occupant. It is remarkable that in its first fifty-seven years the Bureau had not felt the need for this position. With "fully delegated authority in the direction, coordination, and review of Bureau programs and administration,"[71] the position freed the director from day-to-day operations and allowed him to devote himself to broad policy matters and to contacts with the outside.[72] It was a significant change.

The Office of the Director always had a number of consultants, special advisors, special assistants, and persons with similar functions. Then in 1961, a new position of senior research fellow was created. Similar to the movement in many other research organizations at that time to form a "parallel ladder" recognizing—and paying—scientists as much as managers, this position was to "afford recognition to distinguished scientists and to enable them to do independent research and consultation of a broad character beyond the scope of a particular division."[73] The first holder of one of these positions was Ugo Fano, the Bureau's senior theoretical physicist. By July of

---

[70] *An Act To amend the Act of March 3, 1901, U.S. Statutes at Large,* 72 (1958): 1711.

[71] Annual Report, 1959: 14.

[72] As already discussed in previous chapters, the Bureau had a number of associate directors in several disciplines, including planning, but these positions combined line and staff functions and cannot be considered as true line positions. In 1961, the "Associate Directors were relieved of responsibility for supervision of particular divisions so that they could spend full time in staff work for the Director and Deputy Director." (Annual Report, 1961: 17)

[73] Annual Report, 1961: 17.

Robert D. Huntoon joined the Bureau staff in 1941 as one of the principal scientists working on the development of the radio proximity fuze, a major scientific achievement bearing on Allied victory in WWII. Over the course of his career, Huntoon held many high-level positions at the Bureau, including chief of the Electronics Division, chief of the Atomic and Radiation Physics Division, acting chief of the Central Radio Propagation Laboratory, coordinator of Atomic Energy Commission projects at NBS, director of the NBS Corona Laboratories, and associate director for physics. In 1958, Huntoon was appointed deputy director, becoming the first occupant of the newly created position.

1964, he had been joined by chemist-theoretician Kurt E. Shuler, mathematical statistician Churchill Eisenhart, and James R. Wait, the principal theoretician of the CRPL. All were administratively assigned to the Office of the Director.

This new-found concern for the staff was illustrated by the establishment of awards. The few awards then available were of recent establishment. In 1949, the Department of Commerce had initiated two awards to recognize outstanding performance by members of the staff. These were the Gold Medal award, Commerce's highest honor, conferred upon an employee of the department for "distinguished achievements of major significance to the department and the Nation," and the Silver Medal award, the second highest award of the Department, bestowed for "meritorious contributions of exceptional value to the Department." Each of these consisted of a medal, a lapel emblem, and a certificate. They were naturally highly prized.

Although members of the Bureau staff were frequent winners of these awards, in the first sixty years of its history the Bureau had not instituted any awards of its own. Then in 1960, the Bureau Personnel Committee,[74] through its chairman Irl C. Schoonover, wrote to Astin recommending the establishment of the Samuel Wesley

---

[74] The Bureau Personnel Committee met periodically to consider grade increases for personnel and to go over any personnel problems. Each of the divisions also had a personnel committee whose recommendations went to the Bureau committee.

Stratton Award to recognize "truly outstanding scientific accomplishment. Recipients selected to receive this honor should be presented with a bronze plaque and an honorarium at an appropriate ceremony." The committee made provisions for two awards each year, but also for omitting years in which no truly outstanding accomplishment was available.[75]

After some discussion with the department, the establishment of such awards was authorized on November 7, 1960, and on September 4, 1962, Astin wrote to the department with the committee's choice of recipients for the first two Stratton Awards. They were James R. Wait, the senior theoretician of the CRPL, "in recognition of his contributions to a better understanding of the mechanisms of electromagnetic radiation and radio-wave propagation" and, in a joint award, Peter L. Bender and Raymond L. Driscoll "in recognition of their contributions to precision electromagnetic measurement and particularly the determination of the gyromagnetic ratio of the proton." Each

James R. Wait came to NBS, Boulder from Ottawa, Canada, in 1955 to examine theoretical aspects of radio-wave propagation. He received a Department of Commerce Gold Medal for Exceptional Service in 1959 and one of the first Stratton Awards in 1962. In the following year, Wait was honored with an appointment as a Senior Research Fellow at NBS and was granted an Arthur S. Fleming Award by the Washington Junior Chamber of Commerce, recognizing him as "one of the ten outstanding young men in the Federal service, 1963." These early awards and honors would be augmented by many more over the course of Wait's distinguished career.

---

[75] Memorandum, I. C. Schoonover to A. V. Astin, "Stratton Awards." (NARA; RG 167; Astin file; Box 18; Folder Stratton Awards 1961-62). Date on memo is not legible.

Peter L. Bender, a joint recipient of one of the first Stratton Awards in 1962, was recognized for contributions to precision electromagnetic measurement and particularly the determination of the gyromagnetic ratio of the proton." Bender was among the first group of seven Postdoctoral Research Associates that arrived at the Bureau in 1955-1956. He became a regular staff member in 1957 and, just two years later, received a Gold Medal for Exceptional Service from the Department of Commerce for his application of the principle of optical pumping to measurements of atomic constants and to the development of a rubidium clock.

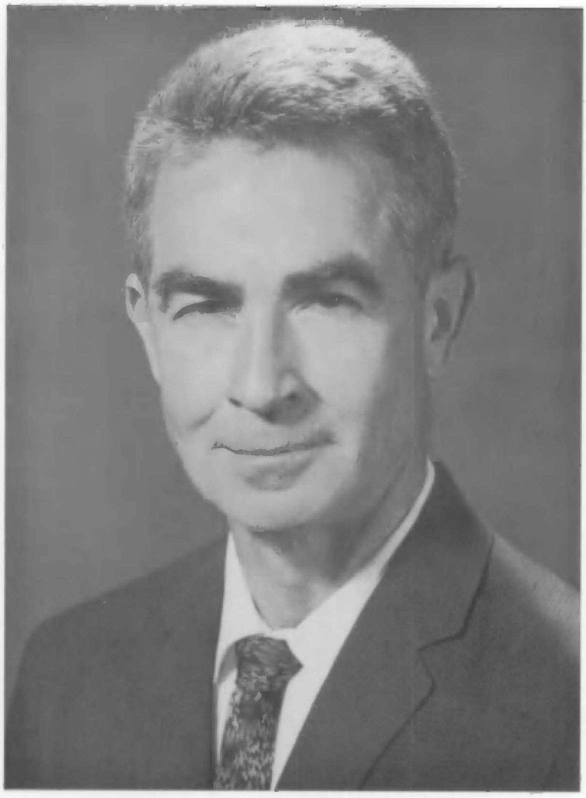
recipient received a plaque and a $1500 honorarium.[76] The establishment of the award and the work of the recipients illustrate well the emphasis management was placing on fundamental research.

These were not the last awards to be established. Since the Stratton Award was for basic science, other areas of the Bureau's work were in danger of being neglected. In 1964 the Edward Bennett Rosa Award was instituted for "outstanding achievements in the development of meaningful and significant standards of practice in the measurement field." This award recognizes another major aspect of the Bureau's activities and consists of an honorarium and a brass plaque.

Other awards followed: in 1966 the Bronze Medal for "work that has resulted in more effective and efficient management systems as well as the demonstration of

---

[76] Memorandum, A. V. Astin to C. Hayward, "Stratton Awards," September 4, 1962. (NARA; RG 167; Astin file; Box 18; Folder Stratton Awards 1961-62)



Raymond L. Driscoll came to NBS in 1936 and proceeded to accrue honors for his work in the area of experimental absolute electrical measurements and for the application of such measurements to the determination of atomic constants. In 1950 Driscoll won a Silver Medal for Meritorious Service from the Department of Commerce, and in 1959 he was awarded a Gold Medal for Exceptional Service. In 1962, the year the Stratton Award was inaugurated, Driscoll was a joint recipient for "contributions to precision electromagnetic measurement and particularly the determination of the gyromagnetic ratio of the proton."

unusual initiative or creative ability in the development and improvement of methods and procedures" (bronze medal, lapel emblem, certificate); in 1967 the Eugene Casson Crittenden Award for employees "who perform supporting services that have a significant impact on technical programs beyond their own offices" (honorarium and certificate); in 1974 the Edward Uhler Condon Award recognized "distinguished achievement in written exposition in science and technology" (aluminum plaque and honorarium); in 1975 the Applied Research Award recognized the "practical application of the results of scientific or engineering research" (mixed metal plaque and honorarium); in 1977 the Equal Employment Opportunity Award recognized "significant contributions to EEO which have been performed in an exceedingly outstanding manner" (anodized brass plaque and honorarium); in 1979 the NBS Safety Award, recognized "unusually significant contributions to the NBS Occupational Safety and Health program" (honorarium and certificate for individual recipients, certificate and plaque for group awards); in 1984 the Allen V. Astin Measurement Science Award recognized "outstanding achievement in the advancement of measurement science or in the delivery of measurement services" (bronze plaque and honorarium); in 1992 the William P. Slichter Award recognized "outstanding achievements by NIST staff in building or strengthening ties between NIST and industry" (certificate and honorarium); and in 1996 the George A. Uriano Award recognized "outstanding achievements by NIST staff in building or strengthening NIST extramural programs, with emphasis on fostering U.S. competitiveness and business excellence" (certificate and honorarium). Presented at a special annual ceremony, the awards provide some happy occasions and result in stimulation for members of the staff.

In another arena, the civil rights struggles of the time shaped Government personnel action. In the Federal Government, equal opportunity had long been a policy.[77] Beginning in 1940 with Executive Order 8587, which was the first public statement of the principle that persons must not be discriminated against on the basis of race, color, or creed, and with the Ramspeck Act,[78] a series of other executive orders and legislation effectively barred such discrimination in the Federal Service. In 1955, with President Eisenhower's Executive Order 10590, which stated that equal opportunity was to be afforded to all qualified persons, a new active phase of EEO began, heightened by President Kennedy's introduction of affirmative action in 1961. Then the Congress produced legislation that went beyond the confines of Federal Government employment when the Equal Pay Act of 1963 prohibited "discrimination on account of sex in the payment of wages by employers engaged in commerce or in the production of goods for commerce." [79] In that same year, just before the landmark Civil Rights Act of 1964, the Department of Commerce began an EEO program. As a result, the Bureau sent a monthly memorandum to Assistant Secretary Hollomon listing the actions it had taken to improve EEO. A typical example lists six African Americans interviewed, two appointed, eight promoted, and one who received a cash award. The highest grade involved was GS-8, and most of those cited were at the GS-2 or GS-3 level. In addition, high-level management met with six black employees on housing problems in connection with the Gaithersburg move and other aspects of the Bureau's EEO activities of concern to minority employees.[80] The Bureau itself did not form an EEO Committee until May 1968. Hollomon was critical of the Bureau's EEO program, stating that it was concerned with only long-range problems and was not specific enough about goals on dates and numbers.[81]

* * *

Outside the Office of the Director a number of changes were made before 1960, the most notable of them being the formation of the Radio Communications and Systems Division in 1959, with Richard C. Kirby as chief, in the Central Radio Propagation Laboratory. But the most significant changes occurred in mid-1960, and they were extensive indeed. After the retirement in 1959 of its long-term chief Francis B. Silsbee, the Electricity and Electronics Division was split into its component parts, reversing a merger made in 1953. The Electricity Division was formed under Chester H. Page,

---

[77] Equal Employment Opportunity Commission, Office of Equal Employment Opportunities, "Generational EEO Developments," [1991].

[78] *An Act Extending the classified executive civil service, U.S. Statutes at Large,* 54 (1940): 1211-1216.

[79] *Equal Pay Act of 1963, U.S. Statutes at Large,* 77 (1963): 56-57.

[80] Memorandum, A. V. Astin to J. H. Hollomon, "Progress Report on the NBS Equal Opportunity Program," December 17, 1963. (NIST RHA; Director's Office file; Box 381; Folder Chrono 7/63—12/63)

[81] Memorandum, J. H. Hollomon to A. V. Astin, "Equal Employment," June 19, 1963. (DOC; Assistant Secretary for Science and Technology; Accession 40-70A-6988; Box 11; Folder June 1963)

and the electronics component was merged with the Office of Basic Instrumentation from the Office of the Director, and the Mechanical Instruments Section from the Mechanics Division to form a new Instrumentation Division under G. Franklin Montgomery.

Following the retirement of its chief, Irvine C. Gardner, in 1959, Optics and Metrology became simply Metrology under Alvin G. McNish, picking up the Mass and Scale Section under H. Steffen Peiser and the old Capacity, Density, and Fluid Meters Section under Charles T. Collett from the Mechanics Division in the process.

Very important changes occurred in the Heat Division under Charles M. Herzfeld. The Engine Fuels Section, the last holdover from the automotive laboratory, was abolished. The Rheology Section under Robert S. Marvin was moved to the more appropriate Mechanics Division, and the Free Radicals Research Section under Arnold M. Bass, its work completed, was also abolished. However, two new sections were formed, and they were to become stellar performers. These were the Equation of State Section under Joseph Hilsenrath, and the Statistical Physics Section under Melville S. Green, which brought statistical mechanics research to the Bureau in a big way.

Condon's division, Atomic and Radiation Physics, had grown greatly and was replete with capable, enthusiastic, and ambitious young scientists. It was simply split into its two components: Radiation Physics under its vigorous long term chief, Lauriston S. Taylor, and Atomic Physics under the natural leader Lewis M. Branscomb. It was a pleasant separation for all concerned.

In a split that was not accepted with complete enthusiasm, the Chemistry Division was divided into Analytical and Inorganic Chemistry, under the temporary tutelage of Irl C. Schoonover, and Physical Chemistry, under the capable stewardship of relative newcomer Merrill B. Wallenstein. In the process, the Organic Coatings Section was abolished, and the Electrodeposition Section was moved to the Metallurgy Division, both moves consistent with the recommendations of the first Kelly Committee Report. Physical chemistry, with new sections on Molecular Spectroscopy under David E. Mann, Molecular Kinetics under Robert E. Ferguson, and Mass Spectrometry under Vernon H. Dibeler, was to become one of the stellar scientific divisions of the new Bureau.

The Mechanics Division was also extensively reorganized, but remained under the leadership of Bruce L. Wilson. Its Mechanical Instruments Section was transferred to the new Instrumentation Division; two sections were transferred to Metrology as described above; and two sections—Rheology under Robert S. Marvin from the Heat Division and a new section, Pressure and Vacuum under Daniel P. Johnson—were added.

In 1960, Boulder's Radio Propagation Physics Division under Ralph J. Slutz was split into Ionosphere Research and Propagation Division under Ernest K. Smith, Jr., and the Upper Atmosphere and Space Physics Division under C. Gordon Little. No new sections were formed, nor any old ones abolished. In 1962, the Radio Standards Laboratory also split into two divisions: the Radio Standards Physics Division under L. Yardley Beers and the Radio Standards Engineering Division under George E. Schafer. By 1963, Boulder had seven technical divisions, four in the Central Radio Propagation Laboratory, two in the Radio Standards Laboratory, and one in the Cryogenic Engineering Laboratory.

The Annual Report for 1961 described these changes as having been made "as part of the Bureau's efforts to meet the expanding needs of modern science and technology," but the changes were not yet complete.[82] In particular, the reorganization of the three materials divisions had not yet been accomplished. For two of these divisions, the changes were made less by movement of division parts than by installing new leadership at the division level. Thus, in late 1959, Alan D. Franklin was installed as chief of the Mineral Products Division, and in 1960 Lawrence M. Kushner, who had been chief of the Metal Physics Section, was appointed chief of the Metallurgy Division. Both were younger men who reorganized and re-oriented their divisions as time passed. For the Organic and Fibrous Materials Division, the changes were more substantial. In early 1962 it was renamed the Polymers Division in recognition of the fact that the industrially important organic materials were in fact synthetic polymers. The division remained under Gordon M. Kline, its long-time head, but five of its eight sections were abolished and replaced with a new set of four under new leadership. Robert B. Hobbs, however, remained in a leadership role, becoming chief of the Applied Polymers Standards and Research Section upon the abolition of the Paper Section which he had headed.[83]

These major changes did not occur without some rancor on the part of the staff. Older staff members who were passed over by younger or more capable persons were understandably upset, and those who were replaced—sometimes summarily—were naturally hurt. Many left the Bureau. The management, however, was serious in its desire to transform the Bureau and proceeded with its plans. The younger staff who had taken over positions of leadership were happy with the turn of events and brought a heightened vigor to their jobs. In a few short years the composition of middle management at the Bureau had been substantially recast.

Not planned and not part of this reorganization effort, a major change in leadership occurred in 1962 with the resignation of Frederick W. Brown, director of the Boulder Laboratories since their establishment in 1954. This resignation was not occasioned by the desire of management to re-orient the work at Boulder. Consisting of the Cryogenic Engineering Division, the Central Radio Propagation Laboratory, and the Radio Standards Laboratory, the work in Boulder was basic and scientifically modern. Rather, Brown's resignation was caused primarily by the nature of his position, although personality differences between him and Astin played a part. While he bore the title of director, he was not in charge of the technical work. The division chiefs in Boulder reported directly to the Bureau director, not the Boulder director. As a result, the Boulder director was not a great deal more than a caretaker, looking after administrative matters, taking care of the physical plant, and acting as the interface with the

---

[82] Annual Report, 1961: 17.

[83] The sections abolished were Rubber under Lawrence A. Wood, Textiles under Herbert F. Schieffer, Paper under Robert B. Hobbs, Leather under Joseph R. Kanagy, and Plastics under Frank W. Reinhart. The new sections were Macromolecules: Synthesis and Structure under Donald McIntyre, Polymer Chemistry under Leo A. Wall, Polymer Physics under Elio Passaglia, and Applied Polymers Standards and Research under Robert B. Hobbs. Dental Research under William T. Sweeney, Polymer Characterization under Norman P. Bekkedahl, and Polymer Evaluation and Testing under Robert D. Stiehler were not changed.

Frederick W. Brown was the first director of the Bureau's Boulder Laboratories from 1954 until 1962. Before coming to NBS, Brown served as technical director of the U.S. Naval Ordnance Test Station at China Lake, California. He was widely recognized as an outstanding administrator of scientific research and development programs as well as a technical expert. While employed by the Bureau of Mines during World War II, Brown authored what was long considered a classic in the field of theoretical calculations for explosives.

outside community. The direction of the technical work was determined by the division chiefs and the Bureau director with the associate director involved. The Boulder director doubtless had input in this process, but he was not a formal part of the technical line organization. Even in administrative matters, the amount of leeway the Boulder director had in interpreting Bureau administrative policies, and in making his own, was a source of some difficulty.[84]

But the Boulder problems were deeper and more endemic than merely the relationship between Brown and Astin. The Boulder staff chafed under what they thought to be restrictions on their freedom to select projects. Indeed, in 1958, an audit by the General Accounting Office found that the Boulder staff were eager to complain that Washington did not understand Boulder's problems and thus were not sympathetic to

---

[84] Memorandum, A. V. Astin to F. W. Brown, "Review and Definition of Washington-Boulder Relationships." (NARA; RG 167; Astin file; Box 20; Folder 1956). This memorandum is undated, and it is not known if it was ever sent. This same box contains a great deal more material on Boulder-Washington relationships.

Boulder's needs. Because their work mostly came under the "special missions" category, they felt that Washington was not as supportive of them as it should have been. The situation festered to the point that in early 1962 all the Boulder division chiefs, with Brown's knowledge, wrote memoranda to Astin expressing their views on the Boulder-Washington relationships. While some of the memoranda were quite moderate, others were more forceful. One division chief entitled his memorandum "My Reasons for Believing that CRPL Should Now Explore the Possibility of Finding a New Home Outside of the NBS."[85] Russell Scott, chief of the Cryogenic Engineering Laboratory, who would succeed Brown, wrote, "The activities of CEL are relegated by NBS policy to the status of 'secondary missions.' . . . [This] has hurt morale."[86] A third division

Russell B. Scott played a leading role in the development of the science of cryogenics. He joined the staff of the Bureau's Low Temperature Section in 1928 and became its chief in 1948. Following the establishment of the Cryogenic Engineering Laboratory in Boulder, Scott was transferred there as its chief. In 1963, Scott succeeded Brown as Boulder's director.

---

[85] Memorandum, chief, Radio Propagation Engineering Division to A. V. Astin, "My Reasons for Believing," February 6, 1962. (NARA; RG167; Astin file; Box 20; Folder Correspondence 1962)

[86] Memorandum, chief, Cryogenic Engineering Laboratory to A. V. Astin, "NBS Boulder-Washington Relations," February 6, 1962. (NARA; RG167; Astin file; Box 20; Folder Correspondence 1962)

chief did not feel that morale had deteriorated below the division chief level, but he made the point, "I suspect CRPL is considered more of a nuisance than a jewel by Washington management," and then continued, "I can also see how we would tend to grow more apart with Boulder increasingly reflecting the views of local management due simply to the separation in distance."[87] Yet another division chief recommended "complete delegation of authority to Boulder Laboratories' management for all policy formation, program planning, manpower, and fiscal controls, etc., pertaining to Boulder Laboratories."[88] He did not, however, say what was left for Washington to do. That was the rub of the matter. The installation 1700 miles from headquarters naturally began to feel more and more independent. However, complete independence was not possible without destroying the reason for existence of headquarters, save perhaps for the defense of the budget. Power necessarily had to be shared, but the final authority had to reside at headquarters. This placed the director of the Boulder Laboratories in a difficult position, with authority over some things but not over others. It appears that for Brown, who was very anxious to create the best research climate possible, his authority did not extend far enough, and so he resigned.[89]

Brown was replaced by Russell Scott, chief of the Cryogenic Engineering Laboratory. Significantly, his title was changed to manager of the Boulder Laboratories; there was now only one director, and he was in Washington. Under Scott unrest lessened. Then in 1965 the CRPL was transferred to the newly formed Environmental Science Services Administration (ESSA) and all questions of "secondary missions" ceased. The CRPL laboratories remained in the same location despite the fact that they were no longer part of the Bureau, and it is significant that the name of the site remained "Boulder Laboratories." Scott now managed Boulder for both ESSA and the Bureau.

JILA, The Joint Institute for Laboratory Astrophysics

The Bureau's annual reports for 1959-1961 feature discussions of a new program in plasma physics and astrophysics that the Bureau intended to emphasize. With the development of space science brought about by rocket and satellite capabilities and of research in thermonuclear power, interest in the behavior of very hot gases and plasmas had grown substantially. But the relevant fields of physics were poorly understood and, as a result, progress was being held up in the fields of space exploration and astrophysics, thermonuclear power and plasma physics, rocket re-entry problems,

---

[87] Memorandum, E. K. Smith to A. V. Astin and R. D. Huntoon, "Boulder/Washington Relations," February 2, 1962. (NARA; RG167; Astin file; Box 20; Folder Correspondence 1962)

[88] Memorandum, chief, Division 87 to A. V. Astin, "Recommendations Regarding the Management of Boulder Laboratories," February 6, 1962. (NARA; RG 167; Astin file; Box 20; Folder Correspondence 1962)

[89] Brown went to the U.S. Embassy in Buenos Aires, and his departure was without rancor. A few years later Astin was to help Brown in his attempt to find a new position. Letter, A. V. Astin to F. W. Brown, August 20, 1964 (NIST RHA; Director's Office file; Box 381; Folder 5/64–8/64); Letter, A. V. Astin to F. W. Brown, April 28, 1965 (NIST RHA; RG 167; Director's Office file; Box 381; Folder 1/1/65–4/30/65)

ultra-high temperature research, and atmospheric research. Lacking were "precise measurement techniques, standards, and basic data on the fundamental properties of the hot gas or plasma."[90] The Bureau's new program was to emphasize these areas, and the implementation of the program would eventually lead not only to a decentralized but coordinated Laboratories for Astrophysical and Plasma Research involving some 100 senior staff members, but also to the formation of a wholly new and novel organization called the Joint Institute for Laboratory Astrophysics (JILA).[91]

The Bureau was not starting from scratch in this field. Along with work in the measurement of very high temperatures and with astrophysical and ionospheric studies in Boulder, it had a small program in laboratory astrophysics, loosely defined as the laboratory study of the atomic properties and processes of importance to astrophysical phenomena. The program was centered in the Atomic Physics Section under Lewis M. Branscomb, who had been chosen in 1959 to coordinate the development of a larger program.[92] In 1960, a presentation of the Bureau's present program and plans for its future program was made to the Space Science Board of the National Academy of Sciences, whereupon that body sent a resolution to the secretary of commerce:

> The Board foresees that a strong limitation to progress in physical interpretation of experiments and observations of the terrestrial, planetary, solar and stellar atmospheres is the lack of sufficient understanding of basic physics of atoms and molecules in the environment which they encounter in these atmospheres. The Board feels that basic work on atomic cross sections, reaction rates and interaction with radiation fields both individually and cooperatively should be encouraged wherever interest exists or may be stimulated.
>
> The Board is aware of the excellent work in various such aspects of laboratory and theoretical astrophysics done by groups at the National Bureau of Standards, and, as a supplement to the above, believes that the Government should recognize in a formal way this potential in a federal laboratory for a coordinated and relatively comprehensive approach to these problems which are so important to space science.[93]

---

[90] Annual Report, 1961: 6.

[91] Memorandum, A. V. Astin to R. E. Giles, "The NBS Program in Laboratory Astrophysics, Appendix I," February 28, 1962. (DOC; Assistant Secretary for Science and Technology; Accession 40-68B-6087; Box 28; Folder National Bureau of Standards). The defining document for JILA is "Memorandum of Understanding between the National Bureau of Standards and the University of Colorado concerning the collaborative establishment of a Joint Institute for Laboratory Astrophysics." (History Project File; Chapter 4; Folder JILA). The document will be referred to as MOU.

[92] In 1960 Branscomb became chief of the new Atomic Physics Division and Stephen J. Smith became chief of the Atomic Physics Section. A description of the program Branscomb coordinated is given in *Measurements and Standards in Plasma-Physics and Astrophysics at the National Bureau of Standards,* Lewis M. Branscomb, ed., Natl. Bur. Stand. (U.S.) Technical Note 59; (July 1960).

[93] Annual Report, 1960: 10-11.

Similar opinions were expressed in 1962 by the FCST, which had appointed a panel to study the problem in 1960: the Bureau's "statutory responsibility for precise measurements . . . [make] the NBS a focal point for laboratory astrophysics. Directly appropriated funds for operations and facilities should be made available to NBS to complete the establishment of a long range interdisciplinary laboratory and theoretical astrophysics program."[94]

Earlier, Branscomb had developed unique plans for an effort in laboratory astrophysics that did not involve the Bureau. The recipient of a Rockefeller Public Service Award, he had spent the 1957-1958 academic year at University College, London, gathering material for a book on negative ions which he never wrote. He was not unaware of the connection between atomic physics and astrophysical problems. Through his colleagues William Meggers and Charlotte Moore Sitterly, Branscomb had contact with the International Astronomical Union (IAU). His association with Professor Michael Seaton of University College strengthened that contact. Moreover, the study of material for his proposed book sharpened his interest in astrophysics, turning his attention to the role of negative ions in stars. Thus, with his way eased, Branscomb obtained an invitation to the General Assembly of the IAU in Moscow. There he met Richard N. Thomas, one of two astrophysicists from the Boulder Laboratories. With the other Boulder astrophysicist, John T. Jeffries, Thomas was administratively housed in the Office of the Director of the Boulder Laboratories. Branscomb had known Thomas slightly at Harvard, and in their discussions in Moscow, they conceived the idea of a "proper group of atomic physicists interested in astrophysical applications, and astrophysicists who wanted to do the astrophysics, not in the classical way, but in the modern quantum mechanical way. . . . We cooked up the idea, the two of us, that if somehow we could take the atomic physics group in Washington and these two astronomers [Thomas and Jeffries, a guest worker from Australia] in Boulder, and marry them up, we would leave the Bureau and we would go somewhere, and we would do this great thing." The main impetus behind this idea was that Branscomb was not satisfied with simply measuring and publishing properties of matter. He wanted to see his work applied. The marriage of atomic physicists and astrophysicists would help assure that the work of the atomic physicists would be used by astrophysicists, and doing the work in the free environment of a university would also attract workers in other fields such as aerodynamics. Thus, notwithstanding its fundamental nature, the work of the atomic physicists would be "applied research."[95]

When Branscomb returned from his sabbatical, he told Astin of the ideas that he and Thomas had and gave notice that he would like to try to "find someplace to go and try to do all this stuff." Astin pointed out to Branscomb that he need not leave the Bureau to do this and reminded him that the Bureau had at one time set up the Institute for

---

[94] Memorandum, A. V. Astin to E. Gudeman, "The NBS Program in Laboratory Astrophysics," February 14, 1962. (DOC; Assistant Secretary for Science and Technology; Accession 40-68B-6087; Box 28; Folder National Bureau of Standards)

[95] Interview with L. M. Branscomb, July 11, 1988: 70. (NIST Oral History File); Letter, L. M. Branscomb to E. Passaglia, August 23, 1991. (History Project File; Chapter 4; Folder JILA)

Numerical Analysis as a joint effort between the Bureau and UCLA. Why not do the same thing in this field? Thus was the idea of a Joint Institute for Laboratory Astrophysics born.[96]

While coordinating the work of the Atomic Physics Division, Branscomb's ideas for the joint institute were sharpened. The institute would consist of astrophysicists and atomic physicists already on the Bureau staff and faculty members, fellows, and students from appropriate departments of a university. The senior members of the NBS component would be professors adjoint in the university. Visiting scholars, important in the institute, were expected to number about ten per year.

The disciplinary core of the institute was to be atomic and molecular physics, radiative transfer theory, and eventually quantum electronics, while the applications were to be in atmospheric chemistry, missile wake dynamics, plasma physics, and astrophysics. Particularly important was the emphasis that Thomas placed on the solution of the nonequilibrium, nonlinear radiative transfer problem. All of this required knowledge of atomic collision cross-sections and nonequilibrium radiative transfer theory.[97]

Thus, the institute would carry out research in astrophysics and low energy atomic physics, do theoretical analysis of astronomical observations, and study the physics of the astronomical medium. In short, it was to be a collaboration of atomic physicists and astronomers/astrophysicists. From the point of view of the NBS participants, the university environment promised greater academic freedom, the availability of graduate students, interaction with visiting scholars, and the opportunity to see their results applied. From the point of view of the university, the Bureau could provide sound, experienced administrative personnel, superb shop facilities, and teaching, for an important function of the institution was the training of students in these fields.[98] The university could get grants from agencies such as the NSF where the Bureau could not. In addition, for the university it would be an interdepartmental-interdisciplinary research facility with new opportunities for students and faculty.

The final Bureau plan for its effort in plasma and astrophysics was that the Bureau would form the Laboratories for Astrophysical and Plasma Research, with work to be performed by appropriate members of various divisions in Washington and Boulder.[99] Then, following the lines set out by Branscomb, there would be formed with a yet-to-be-chosen university an organization first called the Joint Institute for Astrophysics and Atomic Physics and finally called the Joint Institute for Laboratory Astrophysics.[100] It was this latter part of the program that made it unique.

---

[96] Interview with L. M. Branscomb, July 11, 1988: 71. (NIST Oral History File)

[97] Personal communication, L. M. Branscomb to E. Passaglia, August 23, 1991.

[98] Much of this material is from "Joint Institute for Astrophysics and Atomic Physics," December 19, 1961, an early planning document written by Branscomb. (History Project File; Chapter 4; Folder JILA)

[99] A formal program of this kind appears not to have been instituted, although the planned work was carried out.

[100] Interview with L. M. Branscomb, July 12, 1988: 10-28. (NIST Oral History file); proposal, L. M. Branscomb, December 19, 1961, "Joint Institute for Astrophysics and Atomic Physics"; memorandum, A. V. Astin to E. Gudeman, "The NBS Program in Laboratory Astrophysics," February 14, 1962. (DOC; Assistant Secretary for Science and Technology; Accession 40-68B-6087; Box 28; Folder National Bureau of Standards)

Even before the idea had been completely approved, the Bureau began to implement the formation of the joint institute. The problem was to find a suitable university, define the relationship between the two parties, determine the organizational position of the institute in the NBS hierarchy (Branscomb was insistent that it have division status), and tend to the myriad administrative and financial details that the formation of such an organization entailed.[101] Approaches were first made to Harvard and to the University of California at San Diego. Both universities were anxious to have the organization, but both universities wanted their faculty to decide which Bureau people would be given faculty positions. The Bureau people would not accept this, and negotiations were effectively killed.

The next school approached was the University of Arizona. With the Kitt Peak National Observatory established there and observational astronomy sure to be strong and growing, this seemed to be an excellent partner. The only problem was that the physics department at the University was weak, awarding degrees only up to the masters. The NBS group felt that in this situation they would be only a service group to the astronomers. Prizing their independence and autonomy, Arizona was rejected.

The last university investigated was the University of Colorado in Boulder, which, with its High Altitude Observatory and its Laboratory for Atmospheric and Space Physics, had capabilities complementary to those of the NBS group. The presence in Boulder of the National Center for Atmospheric Research was an added bonus. Here a happy marriage was made, though not because the Bureau already had an establishment in Boulder. In Branscomb's words, "[that was] a negative factor. We would have been happier to go to Boulder without it, because we really wanted not to be in the Bureau's administrative environment. We wanted to be in an academic one."[102] With the leadership of University President Quigg Newton, who was very receptive to the union of the two institutions, with the advantage of Thomas and Jeffries already being in the area, and with complementary capabilities on the two sides, a Memorandum of Understanding (MOU) was drawn up and submitted to the two parties. On the Bureau side, approval had to be obtained from the department, and Astin had largely cleared it with Under Secretary Gudeman.

What made the proposed organization unusual and easier to sell was its unique concept. Neither the Bureau nor the university would lose any autonomy by forming the institute. All the workers in the institute (including administrative and supporting personnel) would remain the employees of one or the other institution. Since it was planned that the institute would be located in a new building on the university campus, the space required would be owned by the university and the Bureau would simply

---

[101] Branscomb Oral History, July 12, 1988: 10-28.

[102] Ibid., 12-13.

pay a fee for its use.[103] While not specifically mentioned in the MOU, title to equipment would also reside in one or the other institution. Under this arrangement the proposed institute owned no property and had no employees, hence in this sense did not legally exist. The Bureau would simply carry out one of its programs in an admittedly unusual place, a place for which it made a payment, but spent no money inappropriately. It retained complete control of its program. The Bureau did have the responsibility for providing funds for the visiting scientists. These funds were provided as a grant to the university, which then dispersed them to the visiting scholars.

The fact that some Bureau staff would do some teaching did require justification. The Bureau argued that the teaching would consume only a very small fraction of a scientist's time, that it would be done only at the postgraduate level, and that it was, moreover, an integral part of doing research.[104] The idea was accepted and the road to the formation of JILA was cleared.

On the university side, the memorandum had to be cleared by the Board of Regents. After a Saturday presentation to them by Branscomb, the clearance was obtained and JILA was formed. On April 18, 1962, the MOU was signed by Astin and on April 25 by President Newton. Even before these signings, with approval by the Board of Regents, a public announcement of the joint venture was made on April 13, 1962. It was accepted enthusiastically by the Boulder community.

It was not a large organization when formed. There were nine members from Washington, Thomas and Jeffries from Boulder, and three appointed by the university. But by 1967, the permanent scientific staff numbered twenty-four, with forty graduate students and twenty visiting scientists.[105]

The management of the institute was novel for a hierarchical organization like the Bureau, but rather more common in the collegial environment of universities. The MOU called for two types of appointments: fellows and members. Fellows were defined as Bureau staff members who were professors adjoint or who held equivalent civil service grades, and university associate professors and professors. Visiting scholars

---

[103] The rent situation was not simple. Briefly, the National Science Foundation was willing to fund half the cost of the JILA building if the University had the funds for the other half. The University did not have these funds, but it arranged to borrow them from the State Escrow Fund, essentially the retirement fund. As security it used a letter from Branscomb "pointing out that the Bureau of Standards was going to occupy half of this building and was going to pay a payment in lieu of rent of an agreed amount which the Bureau auditors had agreed to as a reasonable compensation for the space we would use, and that this revenue would be more than sufficient to amortize this university loan in a reasonable period of time. In effect . . . [the Bureau] matched . . . [NSF's] money . . . but the Bureau of Standard's money was paid out over a period of time." Moreover, since the Bureau and university people were completely intermixed, there was no way to determine what space was assigned to which group. Thus the Bureau did not pay rent; it paid a fee in lieu of rent. Branscomb Oral History, July 12, 1988: 26-28.

[104] Memorandum, A. V. Astin to R. E. Giles, "Proposed agreement between the NBS and the University of Colorado," April 9, 1962. (DOC; Assistant Secretary for Science and Technology; Accession 40-68B-6087; Box 28; Folder National Bureau of Standards)

[105] R. H. Garstang, "The Joint Institute for Laboratory Astrophysics," *Sky and Telescope*, 23(3) (March 1967): 2-4.

were all members. The MOU specified that the university contingent would come from the Department of Physics or be "appropriate members of the University faculty in high temperature aerodynamics and fluid physics." The Fellows of JILA formed a small council, which in theory was advisory only, but in practice was able to make decisions that could be carried out by either the chief of the Laboratory Astrophysics Division on the Bureau side or the chairman of the Physics Department on the university side. The fellows elected a chairman from their members, the chairmanship rotating back and forth between the Bureau and university contingents. The system is still in effect.

The new building for JILA near the center of the campus was planned by the university as part of a physics and astrophysics complex, but construction was not completed until 1966. The Bureau people, however, came out to Boulder in the summer of 1962, setting themselves up in the same armory their predecessors had occupied almost ten years earlier when the cryogenics and radio propagation staffs first came to Boulder.

While most of the staff from Washington were atomic physicists and spectroscopists, Branscomb had longer range visions than merely laboratory astrophysics. With the urging of Peter L. Bender, one of the recipients of the first Stratton Awards, he also brought John L. Hall, whose field was the then relatively new one of lasers. Not only were lasers expected to be useful in the study of the nonlinear properties of atoms, Branscomb properly viewed the laser as a premier tool for fundamental metrology. Indeed, by 1976 the JILA program had evolved to the point where an addendum to the MOU was made. The program now included:

> laser physics, precision measurements, geophysics, and data and measurements necessary for the understanding of reaction mechanisms in the atmosphere; to the collection and evaluation of the scientific data; and to the education of scientists.
>
> In view of these contributions . . . the purpose and role of the Joint Institute for Laboratory Astrophysics shall continue to evolve and expand beyond the areas of science outlined in the original Memorandum of Understanding.[106]

As an almost humorous aside, JILA was formed less than a month before Hollomon assumed his position as assistant secretary for science and technology. It appears that he did not believe that the Bureau should be involved in such an effort. He sent Astin a copy of an article by Branscomb and Thomas that had appeared in *Physics Today* with notations questioning the scientific objectives as described by the authors.[107] In response, Astin wrote him a memorandum about JILA. In the margin of this memo Hollomon wrote, "Allen, honestly I guess I don't see how it connects with the

---

[106] "An Addendum to the Memorandum of Understanding Between the National Bureau of Standards and the University of Colorado Concerning the Collaborative Establishment of a Joint Institute for Laboratory Astrophysics, March 1976." (History Project File; Chapter 4; Folder JILA)

[107] L. M. Branscomb and R. N. Thomas, "Laboratory Astrophysics," *Physics Today* 15(11) (November 1962): 42-46.

Standards business—Isn't this just the sort of thing NSF is supposed to do—H," and returned the memo to Astin.[108] Nothing came of this exchange; JILA was an eminently successful example of interaction between the Government and academia. Its MOU was used by several other Government agencies as a model of such collaboration. To this day it remains a thriving and successful institution. One, however, cannot help but wonder what would have happened had Hollomon arrived a year earlier.

The National Standard Reference Data System

Well-known and accurate data are the lifeblood of science and engineering. Whether the problem is the design and execution of an experiment in science or the design of a nuclear reactor in engineering, accurate data are essential. These self-evident facts were formalized when, at the organizational meeting of the International Union of Pure and Applied Chemistry in 1919, the new organization approved the production of the *International Critical Tables of Numerical Data of Physics, Chemistry and Technology.* The task of organizing the compilation of these tables was given to the United States, whereupon the National Research Council took on executive, editorial, and financial responsibilities for the project. A large part of the funding came from industry. A board of trustees and board of editors were instituted with the help of the American Chemical Society and the American Physical Society, and in 1926 the first of seven volumes of critical—meaning well-evaluated—data was published.

The Bureau had a significant role in this effort. The then director, George K. Burgess, was a member of the board of editors and Edward W. Washburn, chief of the Bureau's Chemistry Division, was editor-in-chief. By 1933, the first seven volumes and the index to the first edition were finished but, with the onset of the Great Depression, the lack of funds caused work on the critical tables to lapse. Then in 1955, with stimulus by Astin, the Office of Critical Tables was founded in the National Academy of Sciences-National Research Council to encourage the formation of new groups and to develop standards of criticality. However, the office had no funds; its role was purely advisory and no directly sponsored program resulted.[109]

By the early sixties the mushrooming of science made action on standard reference data essential. Indeed, in 1962 Astin acted unilaterally to form a committee under Merrill B. Wallenstein, chief of the newly formed Physical Chemistry Division, charging it to lay out what a suitable Bureau role in standard reference data would be.[110] The Bureau was thoroughly familiar with reference data. Not only did it have the history of participation in the International Critical Tables Project, but NBS periodically published compendia of reference data from its own work and from data obtained from the literature. It had background in obtaining data, compiling and evaluating them.

---

[108] Memorandum, A. V. Astin to J. H. Hollomon, "The Joint Institute for Laboratory Astrophysics (JILA)," December 7, 1962. (NARA; RG167; Astin file; Box 17; Folder Correspondence re Senior Appointment)

[109] Interview with Edward L. Brady, August 10, 1987: 2. (NIST Oral History File)

[110] Ibid., 2-3.

This Bureau study and those of others in the scientific community pointed to the need for a decentralized program managed by a small office at the Bureau, but having data centers throughout the country.[111] Learning of this plan through its Committee on Scientific Information, the Federal Council for Science and Technology issued a "Federal Policy on National Standard Reference Data System" on May 28, 1963.[112] The policy read:

> There will be established a National Standard Reference Data System (NSRDS) to provide on a national basis critically evaluated data in the physical sciences. The NSRDS will consist of a National Standard Reference Data Center (NSRDC) at the National Bureau of Standards and such other Standard Reference Data Centers as may be required.
>
> The National Bureau of Standards will be charged with the administration of the National Standard Reference Data System. This assignment will include the establishment of standards of quality, methodology including machine processing formats, and such other functions as are required to ensure the compatibility of all units of NSRDS.

The policy went on to state that centers could be assigned to departments or agencies, in which case that organization would administer the center and bear the costs but would have to "meet the quality standards and other requirements of the NSRDS." Centers could also be established at universities and research institutes, but to be included in the NSRDS they would have to meet the standards and requirements of the system.[113] Nine days later the FCST policy was followed with a press release from the Office of Science and Technology announcing the policy and pointing out that the responsibility for data compilation held by NBS, the Department of Defense, the AEC, NASA, the NSF and several other agencies was concentrated at a single point. The Bureau had a new function.[114]

But the Bureau was prepared. As a result of the study begun by Astin, John D. Hoffman, then chief of the Dielectrics Section, contacted Edward L. Brady, a friend with whom he had worked at the General Electric Research Laboratory. Brady had considerable experience in international atomic energy circles and was now at the General Dynamics Corporation in San Diego. When asked about heading up the NSRDS, a position that clearly required great tact and diplomacy, Brady felt the "concept sounded very good to me, and I was excited at the opportunity. . . ."[115]

---

[111] Committee on Science and Astronautics, Subcommittee on Science, Research, and Development, *A Bill to Provide a Standard Reference Data System: Hearings Before the Subcommittee on Science, Research, and Development of the Committee on Science and Astronautics, U.S. House of Representatives,* 89th Cong., 2d sess., on H.R. 15638, superseded by H.R. 16897, June 28, 29, and 30, 1966: 31. Testimony of A. V. Astin.

[112] "Appendix A" in Edward L. Brady and Merrill B. Wallenstein, *National Standard Reference Data System Plan of Operation,* Natl. Bur. Stand. (U.S.) National Standard Reference Data Series 1; December 1964.

[113] Ibid.

[114] Ibid., Appendix B.

[115] Brady Oral History: 2.

Upon joining the Bureau in 1963 Brady formed the Office of Standard Reference Data (OSRD) to manage the program, staffed the office, organized a review committee—later called an evaluation panel—and began the interaction with the national and international communities that were involved in the data collection, evaluation and dissemination efforts that were to always characterize the OSRD. Most important, Brady did everything in the open, writing with Wallenstein a document giving the plan of operation of the system and its philosophy.[116] In that document the scope of the NSRDS was laid out, along with a discussion of appropriate activities, the organization and management of the system, the budgetary plans, and a plea to the technical community for its cooperation.

The philosophy of operation of the system was perhaps best laid out in a discussion of the definition of standard reference data. The definition states that "Standard Reference Data means critically evaluated quantitative information relating to a property of a definable substance or system."[117] Illustrative of Brady's diplomatic experience, this is clarified:

> To obtain complete precision of meaning, most of the terms used in the above definition would themselves require definition. However, the purposes of the National Standard Reference Data Program do not require such precision. General, flexible guidelines on the scope of the program and appropriateness of specific activities are quite sufficient. Decisions will be made taking into consideration all relevant circumstances.[118]

Importantly, priorities for choice of properties and substances were to be determined by the needs of the U.S. technical community.

Despite a chronic shortage of funds—somewhat alleviated by funds from various agencies and by cooperative programs with industry—the system flourished. When the office was formed there were five data centers in the Bureau: chemical thermodynamics, atomic transition probabilities, atomic cross sections, ceramic phase equilibria, and cryogenics.[119] In 1988, there were seventeen centers in the Bureau and seven outside.[120] Moreover, the OSRD funded many short-term projects carried out by scientists in outside institutions.

Despite the fact that the assignment was given to the Bureau by the White House, it did not have the legal force of an Executive Order. Ever conscious of his situation with CRPL and other special assignments, Astin wanted the task to have a firm legal

---

[116] Brady and Wallenstein, National Standard Reference Data System Plan of Operation.

[117] Ibid., 4.

[118] Ibid.

[119] Annual Report, 1963: 8.

[120] Office of Standard Reference Data, *1988 Annual Report Submitted to the Panel for the Office of Standard Reference Data, Board on Assessment of NIST Programs, National Research Council, December 5-6, 1988* (Washington, D.C.: U.S. DOC, NIST, 1988): 37-41. (History Project File; Chapter 4; Folder Standard Reference Data). The OSRD was not involved in the administration or funding of three of the outside data centers but assisted in dissemination of their outputs.

basis.[121] He began an effort to obtain passage of a law that would do this. Astin was successful and, in July 1968, Congress passed the Standard Reference Data Act, which largely repeated the functions specified by FCST, but assigned them to the secretary of commerce.[122] The law also spelled out some of the procedures to be followed, such as publishing in the *Federal Register* "such standards, criteria, and procedures for the preparation and publication of standard reference data as may be necessary to carry out the provisions of this act." The law made provision for the sale of documents and, rather importantly, for their copyright. In an unusual step, the secretary of commerce was given the authority to "secure copyright... of any standard reference data which he prepares or makes available under this Act." This authority, which is almost unique in the Federal Government, stood the Bureau in good stead in arranging the means of publication of reference data.

It was clear from the outset of the NSRDS that the aim of the program was not the production of a new or continuing set of *International Critical Tables.* Science had become too big and diverse. Moreover, scientific data were not static, and more accurate values became available as measurement techniques improved. Data could not be codified once and for all in a set of numbers buried in books enshrined on library shelves. In addition, the concept of critical evaluation required that the sources of the data and their validity be discussed and documented—an OSRD innovation and not a feature of earlier compendia. Thus, the form of the evaluated data outputs was expected to vary with the subject, and to range from tables of numbers through critical reviews of the status of data in particular fields to complete monographs containing graphs and figures. In the beginning, most of this output was published in the NBS-NSRDS series, but some was published in appropriate scientific journals. Later, machine-readable data bases covering many types of data were made available. The OSRD and individual data centers also responded to individual inquiries for specific information.

Despite the diversity of means of output, or perhaps because of it, it became clear to David R. Lide, who in 1969 succeeded Brady as head of OSRD, that some identifiable archival method of publication of reference data was desirable. Under his leadership, the Bureau entered into an agreement with the American Chemical Society (ACS) and the American Institute of Physics (AIP) for the publication of a journal devoted to standard reference data. Named the *Journal of Physical and Chemical Reference Data* (JPCRD), publication began in 1972. Initially published quarterly, but later bimonthly, with offprints of individual papers available for separate purchase, the journal provided

---

[121] When asked about need the for the proposed legislation at the House Hearings on the Standard Reference Data Bill, Donald F. Hornig, then director of OST, replied, "I think the main answer I would give... is that when we talk about the provision of a general service for many agencies of the Government, it becomes very hard for the agency providing it to justify it in terms of its own particular missions.... [T]he most important thing that is involved here is the general expression of intent by Congress that this general service should be performed and not justified strictly in terms of the Department of Commerce's own needs in this case." The statement clearly reflects Astin's own feelings. (NSRDS Hearings: 16)

[122] *Standard Reference Data Act, U.S. Statutes at Large,* 82 (1968): 339-340.

a continuing and identifiable means for the publication of standard reference data. The involved institutions shared responsibilities: the Bureau for editing and content, the AIP for production, and the ACS for dissemination. Proceeds were shared among the three organizations. Using the authority given him by the Standard Reference Data Act, the secretary of commerce copyrighted the contents of the journal and then assigned the copyright jointly to the AIP and the ACS. The government, however, retained the right to unlimited copying for its own use of materials originating in its own laboratories. Today the JPCRD continues to be the leading source of evaluated reference data and publishes contributions from scientists throughout the world as well as from groups that are part of the NSRDS.

It was always recognized that some costs of the NSRDS operation would be recovered by the sale of documents, and this was the principal reason for granting the secretary the power of copyright in the law. The question was always how much could be recovered. All printing and publication costs could be recovered without charging excessive prices, but it was impractical to recoup the cost of the research that led to the production of the data. Only judgment could decide how much of the compilation and evaluation costs (i.e., the costs of the OSRD and the data centers) to try to recover. Brady estimated that only 5 percent to 10 percent of the total OSRD budget could be recovered in this manner. This, though, was not enough for Hollomon, who was a strong supporter of the program. Wanting to impress the Senate and promising to produce $5 million in fees, the assistant secretary estimated 25 percent, which dismayed Brady. Determined by a trade-off between high prices with high cost recovery and low prices to increase availability, cost-recovery in fact averaged 5 percent to 6 percent.[123] In 1964, appropriations for the program were $3.5 million but, over the years, they remained quite static.[124] Despite the fact that the inflation-adjusted appropriation steadily decreased, the SRD program was able to increase its output through cooperative projects with industry, other government agencies, and foreign data centers.

Civilian Industrial Technology—The Bureau Gets a Small, Short Program

The first significant action J. Herbert Hollomon undertook when he became assistant secretary for science and technology in May 1962 was the development of a program called Civilian Industrial Technology (CIT). It was not that he was the originator of the idea for the program. In fact, prompted by a memorandum from Science Advisor Jerome Wiesner and economist John Kenneth Galbraith, President Kennedy formed a White House Panel on Civilian Technology in the summer of 1961. Composed of Wiesner, Secretary of Commerce Luther Hodges, and Chairman of the Council of Economic Advisors Walter Heller, the panel was "assigned the duty of encouraging the utilization of technology in the civilian economy."[125] The Department of Commerce was chosen as the standard bearer for spurring economic growth.

---

[123] Brady Oral History: 8-9.

[124] Appropriations Hearings for 1965: 600; Brady Oral History: 7.

[125] Katz, *Presidential Politics:* 139.

Thus, when Hollomon joined the department, at least one of his tasks was laid out for him. The new assistant secretary was not a stranger to the White House activity. On March 6, 1962, just two months before joining the department, Hollomon addressed a meeting of the White House Panel on Civilian Technology on the related work he had been carrying out with the Engineers' Joint Council's Engineering Research Committee.[126] Moreover, his own beliefs on the role of science and technology were nicely parallel to the aims of the panel. "Scientific and technological resources are a major basis for economic development and for national power, and we do not yet know how best to deploy them," Hollomon wrote in *Science.* He continued, "The relative roles of private and public participation in the use of science and technology for practical purposes are not clear, nor do we know how to employ fully the fruits of science for the improvement of our society."[127]

With this background, Hollomon "hit the ground running." By the fall of 1962 he had put together a program. However, contrary to the advice of the Bureau of the Budget that he seek legislative authority for his program, Hollomon went to Congress for a supplemental appropriation—normally used only for unexpected needs, not for the initiation of programs. He was using the appropriation route to obtain authority for the program, which in effect circumvented the legislative route. This tactic was to prove costly.[128]

Both Hollomon's program and the concepts of the White House Panel were meant to correct four problems. First, Government funding of science and technology had led to an unbalanced condition in which most of the money was going to the military and "space," and with the maturing of those fields there had been a significant drop in the spin-off of technology useful in the civilian sector. Second, this imbalance led to the hiring of the bulk of science graduates by the military and "space." Third, the technological pre-eminence of the Nation in international commercial markets was in danger of being eroded because other nations, unencumbered by military and "space" requirements, could devote an increasingly greater share of their resources to civilian technology. And fourth, the program hoped to correct the inertia or structural inability to develop technology of certain industries and firms.

Hollomon's program had two broad objectives. Technical advances were to be stimulated in the segments of the economy which most needed the benefits of technology, thereby enhancing economic growth and the ability to compete on the world market. Since the bulk of research and development funds were expended by large firms, the program was directed at those industries that were made up of small firms which could not support "even jointly, any significant research." The other objective was to "encourage the rapid diffusion of technology" which would "help close the gap

---

[126] Letter, M. Michaelis to A. V. Astin, February 19, 1962. (NARA; RG167; Astin file; Box 14; Folder untitled). Included is the program for the three-day meeting. Astin was asked to discuss "New Goals for the National Bureau of Standards: A National Institute of Applied Science and Technology."

[127] Letter to the editor, J. H. Hollomon, "Government and Science: How Science Policy is Developed." *Science* 143 (January 31, 1964): 429.

[128] Katz, *Presidential Politics:* 140.

between the technically leading and lagging industries and firms."[129] Widely perceived to be a program that would help the laggards at the expense of the leaders, CIT's objectives caused consternation in some sectors, particularly the building industries. To achieve these objectives, Hollomon proposed a program with seven elements:[130]

1. Support of university activities for the creation of more scientists.
2. Support of research institutes serving industry.
3. Dissemination of technical information in a useful form for specific industries.
4. Support of research and development projects for needy industries at universities and research institutes.
5. Application of systems-research to whole industries.
6. Research conferences in chosen industrial fields.
7. Support of journals and other means of dissemination of information where necessary.

The industries chosen for specific support were textiles and apparel, building technology, and machine tools. Another group of industries was not supported at that time because not enough data were available to decide on the specific requirements for them. These were leather and leather products, lumber and wood products, and foundries and castings. They would be handled later.

In the request for a supplemental appropriation, Hollomon asked for $3.8 million and received $665 000 from the Congress.[131] The following spring he went before the House Appropriations Committee to ask for enough to make the program a $7.4 million effort. Hollomon should have known that he was in trouble when Chairman Rooney of New York, angered by the supplemental subterfuge, refused to address him as "Doctor." The exchange went as follows:

> MR. ROONEY: Did I not read something over the weekend that we should not call doctors "doctors" anymore?. . . . Did you read something about that?
> MR. HOLLOMON: No, sir; I did not.
> MR. ROONEY: It pointed out that the only ones entitled to be called doctors are M.D.'s and D.D.S.'s. We have a government of doctors now.[132]

And for the rest of his testimony Hollomon did not receive the honorific.

---

[129] House Committee on Appropriations, Subcommittee on Department of State, Justice, and Commerce, the Judiciary, and Related Agencies Appropriations, *Departments of State, Justice, and Commerce, the Judiciary, and Related Agencies Appropriations for 1964: Hearings Before a Subcommittee of the Committee on Appropriations, House of Representatives,* 88th Cong., 1st sess., National Bureau of Standards, 25 February 1963: 750.

[130] Ibid., 755.

[131] Appropriations Hearings for 1964: 764; *Supplemental Appropriation Act, 1963, U.S. Statutes at Large,* 77 (1963): 35.

[132] Appropriations Hearings for 1964: 763.

Hollomon came a cropper on building research. His written justification stated that the NAS-NRC report *A Program for Building Research in the United States* and the report on the same topic by the White House Panel on Civilian Technology "provide[d] the technical framework for this program." Then it continued, "Special studies will be performed to bring new technology to bear on ways to establish man's physical environment. For instance, more economic solutions to the needs for the control of humidity, ventilation, temperature, sound, and light, require the cooperation of several disciplines and industrial segments."[133] Widely perceived to mean that the program would engage in innovation and technology development, these statements brought serious criticism from members of the industry. William H. Scheick, executive director of the American Institute of Architects, wrote to Chairman Rooney, "We are absolutely opposed to the use of Federal Government funds for any support, by matching funds or otherwise, of research projects connected with the innovation or development of building materials or products by industry, trade associations, or of individual firms."[134] Richard H. Tatlow III, chairman of the NAS-NRC Building Research Advisory Board which had prepared the building research report, wrote to Frederick Seitz, chairman of NAS, "The Board specifically concluded that there is no identifiable need for Government to concern itself directly with industrial product or process innovation, and that any effort to do so could very easily upset the sensitive balance within the industry."[135] But these were mere love pats compared to the cudgeling by Douglas Whitlock, chairman of the board of the Structural Clay Products Institute. A close friend of Congressman Clarence Bow, ranking Republican member of the Appropriations Committee, Whitlock testified for the U.S. Chamber of Commerce. After extensive criticism of the program, he concluded, "The national chamber opposes the creation of a subsidized research and development program in the construction industry and recommends that the subcommittee reject this portion of the proposed civilian industrial technology program."[136] As if this were not enough, Congressman Bow attacked the program on the floor of the House:

> I want to call the immediate attention of the Congress to a clumsy and highly suspect attempt by a major Federal agency to undertake on behalf of the vast U.S. construction industry and without its invitation, participation, or guidance, an ill-conceived and ill-defined research program that would tamper with the delicate free enterprise mechanisms of that highly competitive $80-billion-a-year industry."[137]

---

[133] Ibid., 755-756. The report of the White House Panel on Civilian Technology, "Technology and Economic Prosperity," was delivered to President Kennedy on December 3, 1962. The authors were Luther H. Hodges, secretary of commerce; Walter W. Heller, chairman, Council of Economic Advisors; and Jerome B. Wiesner, special assistant to the president. (DOC; Assistant Secretary for Science and Technology; Accession 40-72A-7166; Box 10; Folder Internal and Miscellaneous)

[134] Ibid., 774.

[135] Ibid., 780.

[136] Ibid., 1538.

[137] *Congressional Record,* 88th Cong., 1st sess., 1963, 109, pt. 2: 2754.

This criticism from the building industry, along with that from other industrial groups, left the CIT program floundering; it had not sunk, but neither was it sailing very fast.[138] The Senate-House conference allowed a new appropriation of $1 million in addition to the $665 000 already appropriated in the "supplemental." This was to be used for the textile industry which, unlike housing, faced intense international competition and did not oppose government help in innovation. But the funds were pointedly made available only for the completion of the program, and no funds were allotted for any other purpose.[139]

The program was to have been a Department of Commerce one located in Hollomon's office. It would not be a laboratory-based activity but one that made grants to universities and other institutions. Thus, aside from giving advice to the assistant secretary, the Bureau would be treated as any other prospective contractor, receiving perhaps $100 000 or so primarily for the dissemination of technical information to the industry. Astin quite properly felt that the Bureau could not on the one hand be an agency that made grants, and on the other be a grantee.[140] However, with the reduction of the program and its limited duration, the Civilian Industrial Technology program, now concerned solely with textiles, was established at the Bureau in FY 1964. It remained until 1970 when the program ended after expenditures totaled $1.37 million.

During its stay, the program was directed by the Textile and Apparel Technology Center in the Institute for Applied Technology. Except for a small contract with the Applied Mathematics Division, all the funds were let in contracts with external organizations. The work was a combination of applied research and infrastructure development. A few examples illustrate its nature.[141] A contract for a study of fiber surface properties in relation to textile products and processing was awarded to the Textile Research Institute, Princeton, New Jersey. A small award was made to the Fashion Institute of Technology in New York to survey the possibility of setting up programs that would bring new technical information to the apparel industries. This small program led to the establishment of the American Apparel Manufacturers Association, which was formed under another contract but became self-sufficient. A 2-year program at MIT was designed to provide managers and research engineers with a bibliography of the world literature on mechanical processing of textiles. The MIT program developed a thesaurus that provided a link to the scientific literature.

---

[138] Katz, *Presidential Politics:* 140.

[139] D. S. Greenberg, "Civilian Technology: Program to Boost Industrial Research Heavily Slashed in House," *Science* 140 (June 28, 1963): 1380-1382.

[140] Appropriations Hearings for 1964: 774-775.

[141] R. L. Stern, "Current Status of Program Activities and Proposals Under the Textile and Apparel Technology Center." (DOC; Assistant Secretary for Science and Technology; Accession 40-68B-6087; Box 53; Folder Correspondence 1965-66 Filed by Bureau: (b) National Bureau of Standards—General 1966)

A Bold Proposal Leads to a Major Reorganization

Astin had pondered deeply the role of the Department of Commerce in science and technology. Somewhat more than a year after the report of the second Kelly Committee, and while a program for civilian technology was being discussed in the White House, the Bureau director made a bold proposal to then Secretary of Commerce Luther Hodges. It was transmitted to Hodges on August 30, 1961, under the rather cumbersome title "A proposal to strengthen the role of the Department of Commerce in the promotion and development of the Nation's commerce and industry by means of a systematic stimulation and utilization of science and technology."

While agreeing with the second Kelly Committee that science and technology were important to the department's primary functions, Astin did not feel that simply strengthening the existing activities of the department went far enough, but that "the great dependence of commercial and industrial growth upon science and technology requires a more unified and dynamic approach." He made the hardly contestable argument that modern industry depended on new products and processes based on engineering advances, and that those advances were based, in turn, on scientific developments. Thus, in order to carry out its mission of stimulating and fostering commerce and industry, the department had to be deeply concerned with science and technology.[142]

Now, there were other departments for which science and technology were of fundamental importance. These were Defense; Agriculture; and Health, Education and Welfare through its National Institutes of Health. Each of these had far-flung, highly coordinated and successful research efforts in furtherance of their missions to provide, respectively, for the Nation's defense, its agriculture, and its health. With regard to the DOC, however, Astin wrote, "In spite of the fact . . . that new industries and related commercial activities have been major users and exploiters of scientific research and development, we find no comparable program within the government department entrusted with promoting the Nation's commerce and industry." True, there were activities in various bureaus, and these were important, but they fell far short of "a leadership-type utilization of the impact of science upon commerce and industry."

Moreover, competition among nations shaped Astin's consideration of the department's role in science and technology. According to Astin, while "most industrial research and development has been and most likely will continue to be privately supported there are increasing numbers of important new areas where government stimulation and support are necessary. Industrial strength and technological leadership are such important parts of national policy that the Federal Government can afford to do no less here than in [Defense, Agriculture, and Health]."

As a result of this reasoning, Astin proposed that the DOC establish a major new research and development agency called the National Institutes for Physical Sciences (NIPS). Clearly modeled after NIH, these institutes would have as their primary purpose "the stimulation, conduct and support of scientific research and development that are important to industrial and commercial activities, and not adequately provided"

---

[142] Memorandum, A. V. Astin to L. H. Hodges, "A Proposal to Strengthen the Role of the Department of Commerce," August 30, 1961. (NARA; RG167; Astin file; Box 14; Folder untitled).

for either by Government or the private sector. Then Astin listed six specific functions of the new organizations: (1) The operation of research institutes in major areas of industrial technology, such as new materials, automation and production processes, construction, transportation, communication, fire research, quality control, and engineering standards; (2) the operation of a national center to provide services to science and technology in such areas as measurement services, high-precision data, and information; (3) the provision of technical services, such as research and development, surveys and advisory assistance to other agencies of the Government; (4) the conduct of research in areas of engineering or physical science not sufficiently supported in other parts of the Government; (5) the support in private institutions through contracts or grants of applied research of importance to industrial and economic growth; and (6) the operation in the DOC of bureaus whose programs are primarily scientific or technical.

Astin had in mind that all of the work of the Bureau would be included and expanded in the new institutes along with that of some of the other bureaus of the department. And some new functions would be added. In fact, the first three of the enumerated functions were already being carried out at the Bureau in manners ranging from complete and profound to rudimentary. The fourth was clearly meant to provide for the "special missions" activities that so concerned Astin, while the fifth was a completely new departure, for the Bureau had never been a contracting or granting agency. Finally, the last function was meant to accommodate the scientific work in other agencies of the department, primarily the Weather Bureau, the Census Bureau, the Patent Office, the Coast and Geodetic Survey, and the Office of Technical Services. Thus the Bureau and, to a lesser extent, the other agencies of the department would provide the source or nucleus for a number of institutes. The unique measurement-standards function would become an institute, while the Bureau's work on materials could provide the nucleus for a Materials Institute. The CRPL could become an Institute for Communications Research, and Building Research could develop into a Construction Institute. Data Processing could give birth to an Automation and Control Institute, and product testing could be included in a Quality Control and Engineering Standards Institute.

The relation of the institutes to the National Science Foundation was, of course, a matter of concern. Astin felt that rather than competing with NSF's mission of supporting basic research, the institutes would complement that agency since they would primarily support applied research and "operate such basic physical science laboratories as are beyond the scope of the Science Foundation's non-operational mission." His proposal would provide a means for the conduct of "technological activities which do not fall clearly within the responsibilities of existing science agencies," and preclude the often-suggested formation of a Department of Science.[143] He cited examples of how the department and the Bureau had frequently been asked to undertake work that did not result directly from their missions and, thus, were already performing some of the functions of a Department of Science.

---

[143] "Senate Committee on Government Operations: Press Release," from Senator John L. McClellan and Senator Hubert H. Humphrey, Subcommittee on Reorganization, January 20, 1958. This was the most recent proposal for a Department of Science when Astin made his proposal for a National Institutes for Physical Sciences. (DOC; Assistant Secretary for Science and Technology; Accession 40-76-4; Box 1; Folder State Federal Technical Services Act)

The kind of organization that Astin had in mind is further clarified by his wistful statement, "A proposal for establishing such National Institutes is made with mixed feelings on my part since it involves, if accepted, a loss of most of the present programs of the NBS and at least a major part of its present identity. Also, the NBS would be at a lower organizational echelon than it is now." Clearly, Astin referred to a loss on the Bureau's part of its basic measurement standards activities for these would become an institute in the new structure. His NBS would not become NIPS but only a piece of it.

Astin asked for permission to work with the assistant secretary designate and the other agencies of the department on this matter. By the time he took office, Hollomon had already heard about Astin's proposal. When Astin spoke before the White House Panel on Civilian Technology he appears to have presented at least some of his own ideas for NIPS.[144] Hollomon was the immediately preceding speaker on the program and it is safe to assume that he had stayed to hear Astin. Then, less than two months after Hollomon assumed his duties, Astin answered a request from the secretary on how the Bureau could respond to the department's program of promoting economic growth by recommending that the department move rapidly in the direction pointed out in his NIPS proposal.[145] He noted that he had had discussions with Hollomon on the matter and was detailing Associate Director Irl Schoonover to work full time with Hollomon to develop a "specific plan for action."[146]

Exactly what happened between Schoonover and Hollomon may never be known. Schoonover's many virtues did not include writing (nor often reading) memoranda. He much preferred face-to-face discussions and man-to-man deals closed with a handshake. As a result of these predilections and the natural propensity to keep delicate negotiations secret, no written record of the interaction has been found despite assiduous searching. The account to be given has been pieced together from the oral histories of Astin, Schoonover, and Huntoon, as well as from discussions with persons such as John D. Hoffman, Robert L. Stern, and Churchill Eisenhart, who were in positions to know something of the negotiations.

---

[144] The title of Astin's talk was "New Goals for the National Bureau of Standards: A National Institute of Applied Science and Technology." Letter with attachment, Michaelis to Astin, February 19, 1962. (NARA; RG 167; Astin File; Box 14; Folder untitled)

[145] Memorandum, A. V. Astin to L. H. Hodges, "'Will it Promote Economic Growth?' Program," June 25, 1962. (NARA; RG167; Astin file; Box 36; Folder A. Secretary of Commerce 1953. 1961)

[146] Astin had to be careful in his choice of a person to work with Hollomon. While respected for his intellect, Hollomon was variously described by many as domineering, contentious, arrogant, and disdainful, and he quickly caused antagonism in many people. In fact, it was these character traits that alienated some congressmen and members of industry, and were partly responsible for the failure of the CIT program. (Katz, *Presidential Politics:* 140). Schoonover, called "the swamp fox" by his acolytes and possessor of a great sense of humor, could work with Hollomon. Indeed, some who were able to work with him found him a hard taskmaster but one who could improve their performance.

Irl C. Schoonover joined the staff of the Bureau in 1928 as a junior chemist and eventually served in such positions as chief of the Dental Materials Section, chief of the Polymer Structure Section, chief of the Mineral Products Division, associate director for planning, acting director of the Institute for Materials Research, and deputy director. Schoonover played a pivotal role in creating the Bureau's modern materials research program.

Hollomon was very much taken with Astin's NIPS concept, and it influenced his thinking with regard to the interaction between Government and industry in stimulating innovation and the development of technology as articulated in his CIT proposal. Thus, from the very beginning, Hollomon wanted an entity called the Institute for Applied Technology (IAT) included in NIPS to carry out his aims. The rest of the proposed organization was a matter for negotiation. It appears that at one time the proposal was made that there be a National Bureau of Standards consisting of essentially all the measurement-standards work, and an Institute for Applied Technology containing essentially everything else. This view is confirmed in a letter from Kelly to Astin following a meeting of the NBS Visiting Committee, which Kelly chaired. He wrote, "You will note, especially my letter to the Committee members, that I am somewhat concerned with your report on the planning and progress for new scientific programs in the Department of Commerce. I am especially concerned about the possible inclusion of the Bureau as a component of an Institute of Civilian Technology. If this becomes a matter of serious consideration, I certainly believe that those familiar with the Bureau's operations and knowledgeable as to the value of the 'standards' function should have opportunity to comment."[147]

Moreover, there was the serious question of the autonomy of the institutes. Hollomon appears to have pushed for their total budgetary independence so that they would receive separate budgets which he could then control. The skeptical Bureau side feared that Hollomon would re-direct the funds so as to continuously enlarge IAT. Probably seeing that his concept of a NIPS would not be fulfilled, Astin vetoed this idea, and because of the strong position he occupied as a result of the battery additive controversy, such a veto could not be argued with.

As an almost incidental point during the negotiations, the name National Institutes for Physical Sciences (NIPS) was changed to National Institutes for Science and Technology (NIST), a title more in keeping with what both Astin and Hollomon had in mind. While neither of these names was ever adopted, the new acronym would arise again twenty-six years later when the National Bureau of Standards itself, with new legislation, became the National Institute of Standards and Technology. Many of the functions envisaged for NIPS/NIST would be incorporated into the resulting institution.

What resulted from more than a year of discussions was not the formation of the National Institutes for Science and Technology, with NBS as a part of them, but instead, a Bureau reorganized into institutes. The announcement of the reorganization was made on January 30, 1964.[148] There would be an Institute for Basic Standards (IBS), an Institute for Materials Research (IMR), and the Central Radio Propagation Laboratory (CRPL) with institute status, all of which already existed at the Bureau. An addition was the Institute for Applied Technology (IAT), which was Hollomon's creation. Each institute would have its own director who, along with a director for administration, would report to Astin, the director of the National Bureau of Standards.[149] This name, however, would be subtitled so that the full name of the organization would become "The National Bureau of Standards Institutes for Science and Technology." The subtitle, though, would only rarely be used and was soon forgotten.

The directors of the institutes would have considerable authority. Each would be responsible for the direction, execution, and evaluation of the programs of the institute, and each would have a deputy who would assist in the direction of the institute and "perform the functions of the Director in the latter's absence." The directors of the institutes that arose directly from the Bureau's organization were long-standing members of the Bureau staff. Thus, Robert D. Huntoon became director of the Institute for Basic Standards, relinquishing his position as deputy director of the Bureau in the process. The director of the Institute for Materials Research was Irl Schoonover, in an acting capacity. He also served as deputy director of the Bureau. No change occurred in the Central Radio Propagation Laboratory under Gordon Little. The director of the

---

[147] Letter, M. J. Kelly to A. V. Astin, June 22, 1962. (NIST RHA; Director's Office file; Box 354; Folder 1962-Visiting Committee)

[148] Department Order 90 (Revised), Manual of Orders Part 1, January 30, 1964. (NIST RHA; Director's Office file; Box 383; Folder NBS Organization DO 90, 1964-1968)

[149] An organization chart is given in Appendix I.

Institute for Applied Technology was Donald A. Schon,[150] who, however, was new to the Bureau, having been brought by Hollomon to this new position from his post as director of the department's Office of Technical Services. Rounding out the new slate of the leaders of the Bureau was Astin's long-time associate, Robert S. Walleigh, who served as director of administration, and Russell B. Scott, who remained as manager of the Boulder Laboratories.

Each of the institutes contained divisions that were generally unchanged in structure from what they were before the reorganization, and there were no real surprises in their disposition. Besides the six divisions concerned with basic measurement standards, Applied Mathematics, the recently formed Physical Chemistry Division, and Laboratory Astrophysics were placed in the Institute for Basic Standards, as were two of the Boulder divisions, Radio Standards Physics and Radio Standards Engineering. IBS also contained the Office of Standard Reference Data. The Institute for Materials Research contained the three materials divisions plus Analytical Chemistry, which dropped "Inorganic" from its former title, Boulder's Cryogenic Engineering Laboratory, and the newly formed Reactor Radiations Division. It also contained the Office of Standard Reference Materials, which was in charge of utilizing the technical resources of the Bureau for the production of Standard Reference Materials, the new name for the old standard samples. The Central Radio Propagation Laboratory was unchanged but achieved institute status. As outlined in DOC Order Number 90, the mission of the Institute for Applied Technology was to "provide a variety of industry-oriented technical services to facilitate and promote the use by industry of available technology and to facilitate technology innovation." Its main functions were to identify and evaluate obstacles to technical innovation by industry; to develop a technical base for the evaluation of technological products and services; to maintain cooperation with public and private organizations producing technological standards such as codes, test methods, and engineering standards; and to disseminate technical information. It consisted of four offices, one center and six divisions. The Office of Technical Services was primarily a clearinghouse located in the department for the collection and dissemination of technical information from all sources, making the results of science and technology more readily available to industry, commerce, and the general public. The Office of Industrial Services operated the Bureau's industrial research associate program and worked with outside organizations to stimulate innovation. The Office of Weights and Measures was responsible for technical services to the states, business, and industry in the area of measurements; the design, construction, and use of standards of weights and measures and associated instruments; and the training of state and local weights and measures officials. The Office of Engineering Standards

---

[150] Born in 1930 in Boston, Donald Schon received a B.A. degree from Yale in 1951 and a Ph.D. from Harvard in 1955. He taught at the University of California and the University of Kansas City for three years, then joined the Arthur D. Little Company as director, New Products Group. He stayed in that position for six years, and in August 1963 went to the Department of Commerce as director of the Office of Technical Services, from which post he came to the Bureau. An expert on innovation, he authored a book, *Displacement of Concepts* (London: Tavistock, 1963), on invention and discovery.

worked with the voluntary standards system of the country, coordinated the Bureau's activities in it, and provided a forum in which interested parties could write standards. The Textile and Apparel Technology Center operated the residue of the CIT program at the Bureau. Three of the institute's divisions were already in existence in the Bureau. These were the Building Research Division, the Information Technology Division (formerly Data Processing Systems), and the Instrumentation Division. The new arrivals were the Industrial Equipment Technology Division, which had the ambitious mission of doing with all industrial equipment what the Data Processing Division did with information equipment. The Performance Test Development Division was an outgrowth of the Bureau's product testing and evaluation activities. Finally, the Transport Systems Division's mission was to develop "methodology and models to permit measurement and evaluation of the engineering, economic, and social factors essential to the understanding of the transportation function and to decision making on national transportation policies."[151] To many members of the Bureau staff long accustomed to an organization based on scientific laboratory work, the new institute was an enigma since it was based partly on research (most of it applied), partly on testing and test method development, partly on funding work in outside organizations, and partly on paper studies.

The new organization was not well accepted by many members of the staff. A new layer of management had been added just above the division chief, thereby removing everyone one level further from the director. Of course, reorganizations had occurred periodically in the Bureau's history, but these involved no more than reorienting, replacing, or adding divisions or sections. This was the first reorganization in the Bureau's history that involved a structural change in the institution, and it caused apprehension. But the old structure could not continue; the Bureau had simply become too big. There were twenty-three divisions just before the formation of the institutes. To have that many division chiefs reporting to the director clearly placed too great a burden on him. Indeed, this situation had been foreseen by the first Kelly Committee which recommended that associate directors be given more line responsibilities, thereby lessening the burden on the director. That end had been achieved, albeit in a then-unforeseen manner.

Considering that Astin's NIPS/NIST proposal and Hollomon's CIT program had different origins, it is not surprising that there were striking differences between the two. Astin's proposal was basically structured as a development and expansion of the Bureau program, incorporating into it the functions of other agencies of the Department of Commerce. It was thus oriented toward the support of the specific areas of science and engineering which Astin viewed as the source of industrial innovation and development, and was very much concerned with the stimulation of new industries by the support of science and technology. The resulting program was largely laboratory-based. The White House/Hollomon program, on the other hand, was directed toward specific older industries that were viewed as lacking in innovation and technological

---

[151] Department Order 90 (Revised).

development. While research in these industries would be supported, the main object was to change their scientific infrastructure. Even in the provision of information—the element common to the two proposals—there were differences. Astin speaks of "acquisition and compilation of precision data and scientific information centers," while Hollomon speaks of "dissemination of technical information in a useful form to the particular industry." That such differences would arise was inevitable. Hollomon did not have to concern himself with the maintenance of services provided by the Bureau; they would continue after CIT. Yet, despite these differences, both proposals had the same goal: to find a means by which Government and industry could interact in the realm of science and technology to stimulate economic prosperity. It was a theme that had concerned the Bureau since the end of World War I[152] and one that would arise continually for the remainder of the period covered in this history.

THE TECHNICAL WORK

In the seven years following the sputniks, administrative changes were made in response to the recommendations of the two Kelly Committees and to the changing face of science and technology. Old divisions were substantially reorganized and their directions changed. New types of organizational units—the Office of Standard Reference Data and the Joint Institute for Laboratory Astrophysics—were added. Missions were pondered and clarified. Fortunately for NBS, the sputniks triggered substantial increases in appropriated funds, and part way through the period directly appropriated funds exceeded transferred funds for the first time since World War II. All these changes culminated in the first major structural change in the Bureau organization.

But the value and efficacy of the Bureau to the Nation was not determined by its organizational structure, nor by statements of its mission. NBS was, first and foremost, a laboratory-based institution, and in the final analysis it was the output of its laboratories that determined its value. This part of the chapter is an account of the technical work during the period, emphasizing new directions and accomplishments. Again, illustration is by example rather than a complete discussion of the technical work.

STANDARDS MATTERS

Length

Frank T. Bow, congressman from the industrial state of Ohio and a member of the House Appropriations Subcommittee, was interested in the accurate measurement of length. At the Bureau's appropriations hearings for FY 1957 he asked Allen Astin, "Dr. Astin, as you know, I am interested somewhat in the measurement of tolerances, particularly in the bearing industry as they relate to their work on guided missiles and other functions. What are you doing at the Bureau now in the development of measurement to closer tolerances?"

---

[152] See, for example, Chapter 5 of Cochrane, *Measures for Progress:* "The Tide of Commerce and Industry (1920-30)."

Astin, never loath to seize an opportunity to publicize the Bureau's work, replied, "Many of our programs could be grouped in this general category. . . . I think you are specifically interested in the area of precise length measurements, particularly in the use of precision gage blocks?" to which Mr. Bow replied, "That is right."[153]

The Bureau had, in fact organized a program to decrease the calibration tolerance on the best grade of gage blocks from $\pm 2 \times 10^{-6}$ in/in to ($\pm 1 \times 10^{-7}$ or $\pm 2 \times 10^{-7}$) in/in. The reason for this new reduction was that industry was anticipating the need for measuring to ($\pm 1 \times 10^{-6}$ or 2×10^{-6}) in/in, when it was at that time measuring to $\pm 2 \times 10^{-5}$ in/in. But gage blocks cannot be used to measure to their indicated tolerance. A factor of 10 is allowed to compensate for dimensional instability caused by aging and by wear, and for errors introduced when combining two or more gage blocks. Thus the need for blocks with a length tolerance of $\pm 2 \times 10^{-7}$ in/in.

There were two principal problems in developing such ultra-precise gage blocks.[154] One was simply the problem of measuring their length, and the other was the problem of dimensional stability. Because temperature is extremely critical,[155] the thermal conductivity of the master block should be as close to that of the object being calibrated as possible. Since the latter is either a working block or a measuring instrument, both of which are usually made of steel, the master block is pretty much constrained to be made of steel. Moreover, block specifications require that the gaging surfaces of the block be quite hard, which means that the steel be a hardenable alloy or that the surfaces of the block be hardened by some type of special treatment, such as nitriding. Because alloy steels are inherently unstable materials, their dimensions may increase or decrease with time. Making them stable is a complex metallurgical problem.

To solve this problem, a joint program involving the Optics and Metrology and the Metallurgy Divisions was begun. Sixteen companies, including experienced firms such as Brown & Sharpe, General Electric, IBM, Pratt & Whitney and Timken Roller Bearing, provided consultation, materials, facilities, and personnel for lapping, as well as a portion of the operating funds.

The scheme of the work was simple in concept but not easy in execution. The metallurgy team, headed by Melvin R. Meyerson, ordered fully annealed bars of various steels with a cross section of 1.5 in × 0.5 in prepared by the manufacturer, along with one set of gage blocks made of aluminum oxide. At the Bureau the bars were cut into blanks, given heat and/or surface treatments, and manufactured into gage blocks with gaging lengths of 2 in. Fifteen different materials were used, leading to forty-one combinations of materials and treatments. All machining was done in the Bureau shops except the final lapping, which was done by several commercial firms:

---

[153] 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 on Appropriations, House of Representatives,* 84th Cong., 2d sess., National Bureau of Standards, 20 March 1956: 107. The Bureau reported progress on this problem to the Appropriations Subcommittee each year until 1962.

[154] Most of this description comes from M. R. Meyerson, T. R. Young and W. R. Ney, "Gage Blocks of Superior Stability: Initial Developments in Materials and Measurement," *Journal of Research of the National Bureau of Standards* 64C (1960): 175-207. A shorter version by the same authors is "The Development of More Stable Gage Blocks," *Materials Research and Standards* 1 (1961): 368-374.

[155] A temperature change of 0.03 °C produces a change in length of 10^{-7} in/in.

Melvin R. Meyerson of the NBS Metallurgy Division removed type 52100 tool-steel gage blocks from a chloride bath where they were immersed at 1550 °F for 15 min.

Brown & Sharpe, Dearborn Gage, DoAll, and Pratt & Whitney. Extensive metallurgical observations of microstructure and hardness were made by the Bureau staff. These blocks were then stored and their lengths measured periodically by members of the Optics and Metrology Division.

The metrology team under the leadership of Theodore R. Young had a different set of problems. They had to devise a means of measurement yielding a precision of one in ten million without going to absolute determinations, which would be impractical with this volume of work. As a result, an optical comparator was built according to a design described in the literature. Using as a reference standard an existing block—one which absolute measurements over a period of years had shown to be particularly stable—it was possible to obtain the desired precision. But even this effective technique was not sufficient to handle the measurement load for the program, for a minimum of four hours per measurement was necessary to obtain thermal equilibrium. To alleviate this problem, an electro-mechanical comparator was acquired and was used along with a system by which the master block was used only to determine the length of one of a group of blocks, all of which had received the same thermal history. Even with this complex system, special mathematical handling of the data was required. Indeed, it was found that it made a difference how the two blocks to be compared were picked up in practice and held in the gloved hands of the operator. Results were different

Grace Chaconas of the NBS Optics and Metrology Division positioned a pair of gage blocks in a mechanical comparator. Such comparisons, carried out in accordance with a specially designed statistical procedure, provided data on the stability of gage blocks being developed by the Bureau.

when the blocks were held by the narrow, rather than the broad faces. This was ascribed to the temperature effects caused by the position of the operator's palms with respect to the two blocks.

The final result of these various investigations was to show that it was possible to produce—and measure—gage blocks with a precision an order of magnitude better than was previously possible. Blocks of specially prepared type 410 stainless steel measured over a period of more than a year showed a maximum variation in length of 2×10^{-7} in per year, and in some cases even less. Blocks of specially hardened type 52100 tool steel were equally good. These results achieved the objectives set out at the beginning of the program.

At the Appropriations Committee Hearings in 1961, Deputy Director Robert D. Huntoon—Astin being absent due to illness—was able to announce: "For the first time in NBS history, the Bureau has certified the accuracy of length measurements on commercial gage blocks to better than 1 part in 5 million."[156] It was a fitting conclusion to a well-conceived and well-executed program.

At the same time that work was progressing with what might be called engineering standards for length measurement, momentous events were taking place on the primary-standard front. There had long been an effort to replace the venerable platinum-iridium meter bar that was the international prototype of length with a standard based on the wavelength of light emitted by a suitable element. Such a move would make redundant the international standard bar kept at the International Bureau of Weights and Measures (BIPM) in Sèvres, France. Any suitably equipped laboratory, with staff who had the time and the inclination, could have its own primary standard, for the prototype artifact of human construction would be replaced with a constant of nature available to all.[157]

Consequently, at the 1952 meeting of the International Committee on Weights and Measures in Paris, an advisory Committee for the Definition of the Meter was appointed. By 1954, the means of defining the meter in terms of the wavelength of light was agreed upon. By this time there had been sufficient comparisons of various wavelengths from different elements with the international prototype that it was agreed that no more would be carried out. Instead, the wavelength in a vacuum of the red line of natural cadmium was defined to be exactly 6438.4696 angstroms (10^{-10} m, or "tenthmeters") and all measurements of the wavelength of other radiations were to be made by comparison with cadmium, a relatively easy task.

There were three serious proposals for the standard radiation. The Bureau proposed mercury-198 in an electrodeless lamp; the German Physikalisch-Technische Bundesanstalt (PTB) proposed the orange-red line from krypton-84, later changed to krypton-86 because of easier availability; and the Institute of Metrology of the U.S.S.R. proposed cadmium-114. The Bureau put lamps with each of these candidates into operation.

---

[156] Appropriations Hearings for 1961: 249. Huntoon's statement is somewhat misleading. While blocks could be compared with this precision, the absolute accuracy was not that high. (John Beers, private communication.)

[157] I. C. Gardner, "Light Waves and Length Standards," *Journal of the Optical Society of America* 45 (1955): 685-690; "Wavelength of Kr^{86} Light Becomes New International Standard of Length," *Technical News Bulletin* 44 (1960): 199-200; Annual Report, 1961: 21-22.

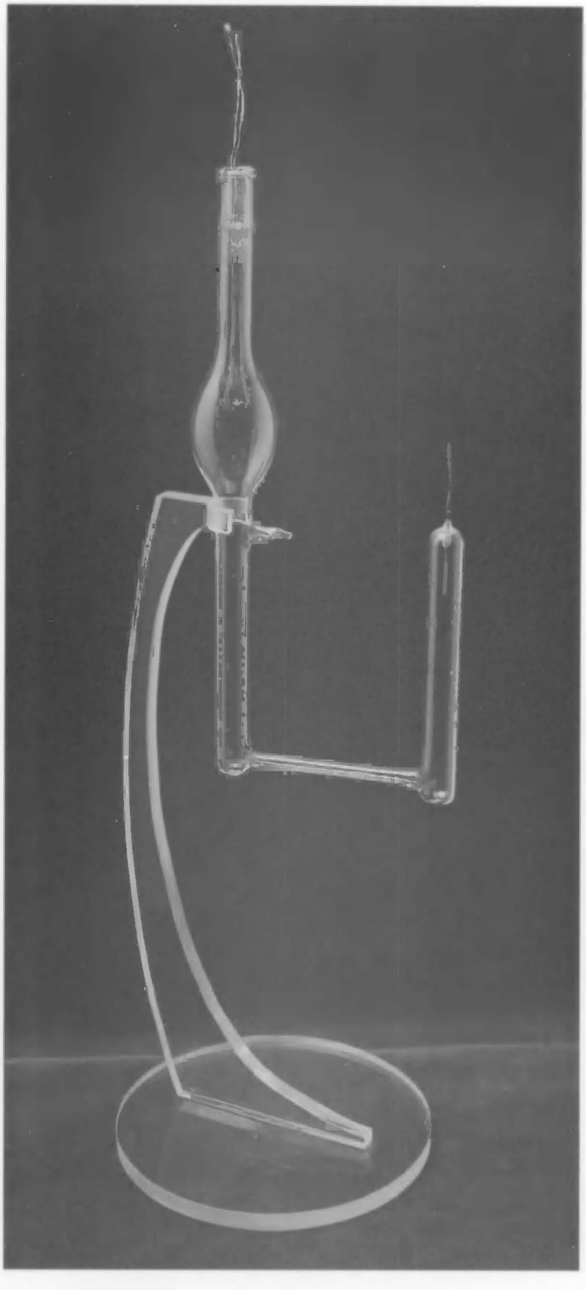
At the 1960 meeting of the General Conference for Weights and Measures in Paris, the PTB candidate was accepted, and the wavelength of the orange-red line of krypton-86 became the new international standard of length. The new definition of the meter was 1 650 763.73 wavelengths of this light. A relative uncertainty of 1 part in 100 million was now possible for the measurement of length.

The Bureau was one of only two laboratories that had carried out a direct comparison of the krypton wavelength with the standard meter bar, the other being Canada. The Bureau results were slightly different from the proposed value, but the Bureau accepted it because the difference caused no practical problems and the new value made the angstrom exactly 10^{-10} m. The old meter bars would remain as the principal means of performing calibrations. They had served the industrial world for almost 100 years as the primary standards of length. In less than twenty-five years, the new standard would be superseded by an even more precise definition of the meter based on the speed of light.

The U.S. delegation to the 1960 General Conference on Weights and Measures took a last look at the comparator on which prototype length standards from around the world were compared with the former international standard: the French platinum-iridium bar. From left to right: Louis Polk, president of The Sheffield Corporation; Allen V. Astin, NBS director; and Elmer Hutchisson, director of the American Institute of Physics.

Karl F. Nefflen of the Engineering Metrology Section assembled a krypton-86 lamp. The lamp was maintained at the triple point temperature of nitrogen, 63 K, to improve the reproducibility of the standard wavelength.

In another aspect of engineering metrology, a method of accurately measuring long light paths was being explored. High-accuracy surveyor's tapes such as those used in surveying missile tracking sites, were calibrated with a relative uncertainty of 1 part per million. The use of lasers was expected to improve the precision of those measurements. For this purpose, a helium-neon laser constructed at the Bureau was made to operate in a single mode, thereby providing a single narrow wavelength of light. The laser operated in the infrared at a wavelength of 11 523 Å, and required an image converter to make it visible. The laser light was passed through a specially designed Michelson interferometer whose two reflecting mirrors were placed 100 m apart at the



In 1960 a new definition of the meter, 1 650 763.73 wavelengths of orange-red light emitted from a krypton-86 lamp, was adopted at the General Conference for Weights and Measures in Paris. The meter, previously defined as the length between two marks on a platinum-iridium bar stored at 0 °C at the International Bureau of Weights and Measures in Paris, would in 1983 be redefined as the distance travelled by light in a vacuum during 1/299 792 458 of a second.

ends of the calibrating tape in the Bureau's tape tunnel. Fringes were obtained, and by counting the fringes in the span length it was in principle possible to relate the measurement directly to the laser wavelength. But the fringes were quite unstable because of vibrations and other disturbances, and the method required considerable development. Nevertheless, the experiment demonstrated one of the first direct applications of the laser to metrology.[158]

Temperatures High and Low

At the FY 1959 House Appropriations Hearings, Astin, as part of his presentation of Bureau achievements, announced the development of a new pyrometer for the measurement of temperatures above 2000 °C. This prompted Sidney R. Yates, congressman from Illinois, to ask Astin if it was true, as he had heard, that the Soviet Union was able to measure temperatures up to 6000 °C. Astin said that he had heard the same, but that the Bureau could measure reliably only up to 3000 °C and "by very impromptu means to 4200 °C. This is our current limit."[159] With the advent of the sputniks still fresh in everyone's mind and a technological rocket race with its need to measure high temperatures in rocket exhausts and re-entry vehicles well started, the committee was horrified that Astin had not been permitted to ask for facilities necessary for the attainment and measurement of higher temperatures. Thus, along with the approval of the full request the Bureau had made that year, came instructions from the committee in 1960 to devote $1.16 million to "activities in the field of very high temperature" for FY 1961.[160]

The Bureau was in a good position to carry out that instruction.[161] Along with the pyrometry work, it had just completed a five-year exchange of platinum resistance thermometers with six other nations to determine the reproducibility of the steam point—the reproducibility was ±0.001 °C—and with Canada of the sulfur point, 444.60 °C, where the reproducibility was ±0.002 °C. NBS was actively working to extend the range of these thermometers to the gold point (1063 °C), thereby replacing the inherently less accurate platinum vs 90 percent platinum 10 percent rhodium thermocouples. Indeed, in his presentation at the FY 1960 hearings, Astin listed thirty-eight high-temperature projects. Besides projects on the production and measurement of high temperatures, he listed such activities as the properties of materials at high temperatures, spectroscopy and atomic energy levels, phase equilibria and high-temperature chemical and physical processes.

For temperatures above 1063 °C, the measuring instrument of choice was the optical pyrometer, and the Bureau was deeply involved in improving the accuracy of pyrometer calibrations in the critical range between 2500 °C and 4000 °C. For this purpose, both a stable source of high temperatures and an accurate measuring instrument were necessary. By 1960, zirconium and carbon arcs had been developed as a source for calibrations, but their instability limited the accuracy of routine calibrations to ±40 °C at 3800 °C. Attempts to improve the stability centered on an electrically heated graphite tube in an inert gas atmosphere. The improvement in measuring instruments

---

[158] "NBS Laser Produces Interference Fringes," *Technical News Bulletin* 47 (1963): 80-82.

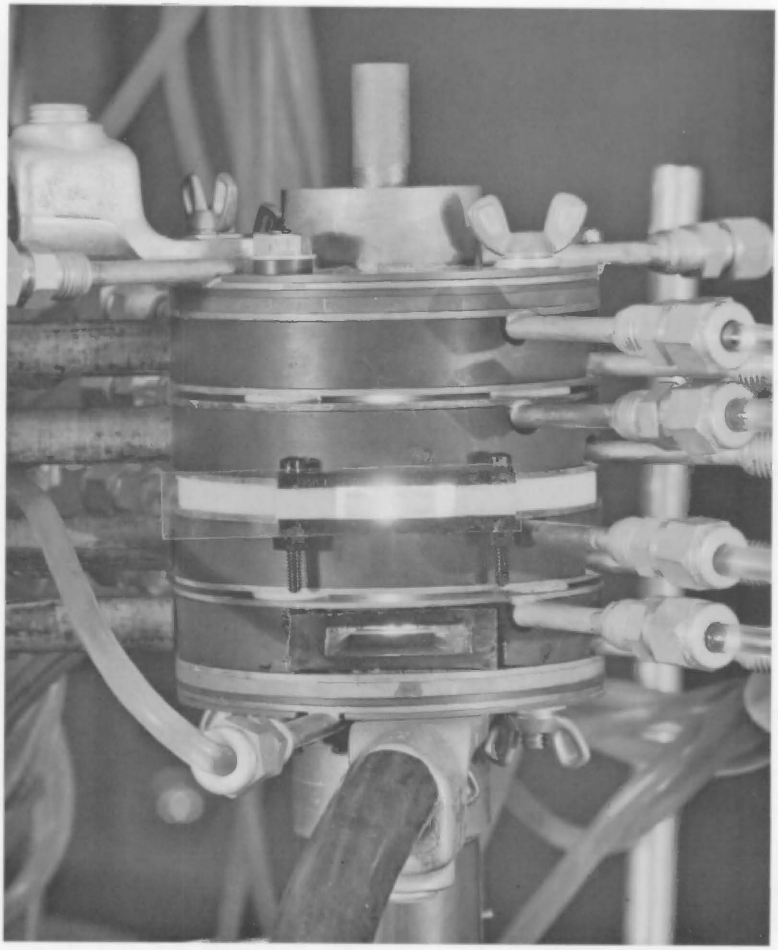
[159] Appropriations Hearings for 1959: 417-418, 431-434.

[160] Appropriations Hearings for 1961: 262.

[161] Annual Reports: 1957, 23; 1958, 23-24; 1959, 29-30; 1960, 51-52; 1961, 51-53.

came in the form of a photoelectric pyrometer, which greatly reduced the human element in calibrations. In measurements at the gold point (1063 °C) this new instrument achieved a precision of ±0.02 °C, better by a factor of 15 than a visual optical pyrometer. It was subsequently found that most of the problems with this pyrometer arose from drift in the tungsten strip lamp used for internal calibration of the instrument. More frequent calibration at the gold point corrected this problem. It was then possible to realize the International Practical Temperature Scale with reduced uncertainty. The uncertainty was now 0.07 °C at 1063 °C and only 1.9 °C at 3730 °C, a substantial improvement over previous performance.

At very high temperatures in the 10 000 °C to 20 000 °C range, a wholly new source of temperature and the means of measuring it became necessary. The source developed was a plasma arc, and the means of measurement was the width and intensity of



This 15 000 °K plasma arc apparatus was used at NBS to study methods of measuring very high temperatures.

spectral lines. This shows the clear relationship between spectroscopy and atomic physics data on energy levels and transition probabilities on the one hand, and plasma physics on the other. The new arc utilized electrodes that were shielded by argon from the gas to be studied. With nitrogen as the gas, the intensity of the spectral lines remained constant within 1 percent for an hour. The most interesting results, however, would be with arcs in hydrogen or helium, for then the radiation properties could be calculated theoretically, even though these arcs were much harder to stabilize. In any case, complex calculations were necessary, so an analog computer was developed and built so that they could be made "in real time." By the end of 1964, temperatures in the 20 000 °C range were being routinely measured, if a measurement requiring a houseful of equipment can be considered routine.

While all this work on measuring high temperatures was going on, low-temperature measurement standards were not being neglected.[162] The International Temperature Scale was defined down to 90 K, and to extend the range, the Bureau began developing a thermometer based on the velocity of sound in helium. Such an instrument would give an absolute measurement of temperature and make it competitive with the much-more-cumbersome gas thermometer. At the same time, various semiconducting resistance devices were being investigated as secondary thermometers.

In 1961, the Bureau announced its aim to provide a calibration service in the range 10 K to 20 K. The scale would be based on an acoustical interferometer with helium as the working fluid. This provided the necessary absolute measurements. "Doped" germanium resistors were to be used as precision secondary standards. When cycled between 300 K and the boiling point of helium—approximately 4.2 K—their resistances showed a reproducibility within $^1/_3$ μK. Helium-vapor-pressure thermometers would also be used.

In 1964 the first service was opened, providing calibrations in the range 2 K to 5 K. Calibrations could be made every 0.1 K against a group of germanium thermometers which in turn had been calibrated against the accepted helium-vapor-pressure temperature scale. In 1965, a facility for the calibration of germanium thermometers in the range 4 K to 20 K was opened. The calibration was based on an NBS temperature scale obtained with an acoustic thermometer and transferred to six germanium thermometers.

High Pressures

In late 1958, Allen Astin received a report he had commissioned from consultant Leason H. Adams on a survey of high-pressure research at the Bureau. Along with the survey, the forty-page report made recommendations regarding future needs in this area.[163] The report concerned itself only with high pressures, defined as those from

---

[162] Annual Reports: 1959, 31; 1961, 56; 1962, 62-63; 1963-1964, 75- 76; and 1965, 18-19.

[163] L. H. Adams, "Survey of Current High-Pressure Research Program at National Bureau of Standards and Recommendations Regarding Future Needs in this Area," Revised November 19, 1958. (NARA; RG 167; Astin file; Box 15; unfoldered)

1000 bar to 10^6 bar (approximately 15 000 psi, to 15×10^6 psi) and it recommended that first priority be given to the establishment of a pressure scale based on suitable fixed points, much as are used in the establishment of the International Practical Temperature Scale. It also recommended the study of properties of materials at high pressures to advance the attainment, maintenance, and measurement of high pressures; setting up and promoting pressure-safety regulations; the presentation of selected PVT data for liquids and gases; the development of instruments important in the pressure field; and the development of methods to extend high-pressure technology. Of all these recommendations, the only ones specifically adopted and emphasized were those on high-pressure standards and on the attainment of high pressures.[164] All the other activities recommended, save perhaps the one on safety, were already actively pursued and they apparently needed no special emphasis.

In 1959, the Bureau provided pressure calibration services over the range 5 psi to 60 000 psi. However, the Mechanics Division was hard at work expanding the scale. The main instrument of choice was the dead-weight loaded-piston gage that is not packed to prevent fluid from leaking—the so-called freely rotating piston gage. In this instrument the clearance between piston and cylinder is so small that the leak rate is a negligible 1 in^3 or 2 in^3 per month. Like the gas thermometer in temperature measurements, this is a first-principles instrument in which the pressure can be precisely calculated. It was anticipated that the working range of this type of instrument could be extended to 300 000 psi or 400 000 psi.

At the same time, the division was developing a multi-anvil cell to achieve higher pressures, but the pressure in this instrument could not be confidently and precisely calculated. The first of these instruments had four anvils in a tetrahedral arrangement and could reach pressures of approximately 2×10^6 psi.[165] Moreover, it was a compact instrument that could be easily loaded into a mechanical testing machine because it was so designed that force needed to be supplied to only one of the anvils. Later a modification with six anvils in a cubic arrangement was designed and built. For use in these presses, a sample of the material to be studied is placed in either a tetrahedron or cube of an easily sheared pressure-transmitting material (liquids would turn to solids at these very high pressures) such as pyrophyllite (hydrous aluminum silicate). This assembly, along with any leads to measure electrical resistance, is placed between the anvils and force is applied.

There were several materials for possible use in generating pressure fixed points, which are basically phase transitions or crystal-structure transitions whose onset could be determined by some physical means, such as changes in electrical resistance or volume. The primary candidates for fixed points were the pressure at the freezing point of mercury at 0 °C and three crystal-structure transitions in bismuth, two occurring near 27 kbar and a third at almost 120 kbar.

---

[164] "High-Pressure Standards Program Expanded," *Technical News Bulletin* 43 (1959): 241; Annual Report, 1959: 35.

[165] E. C. Lloyd, U. O. Hutton, and D. P. Johnson, "Compact Multi-Anvil Wedge-Type High Pressure Apparatus," *Journal of Research of the National Bureau of Standards* 63C (1959): 59-64.

Both the mercury and the bismuth points were investigated with the piston gage, which had to be carefully and tediously developed to extend its range. By 1963, pressure at the freezing point of mercury at 0 °C was determined to be 109 722 psi with an uncertainty of ± 30 psi, which was adequate for use as a calibration point. Moreover, a pressure measurement gage based on the resistance of manganin wire proved to be quite a good working gage.[166]

Development of the freely rotating piston gage continued, and by 1965 a gage with a tungsten-carbide piston diameter of only 0.080 in, and operated with a maximum load of 860 kg, reached the low-pressure bismuth transition. Believed to be the highest pressure yet achieved with this type of gage, and using volume change as an indication of the transition, the pressure obtained was 25 306 bar (approximately 367 000 psi) with an uncertainty of 60 bar.[167] It was the most accurate value so far obtained for the transition.

Thus, in seven years after announcing the pressure-standards program, a new pressure apparatus had been developed, two new piston gages had been built and, most important, two new pressures suitable for use as fixed points on the pressure scale had been determined. It was an impressive performance.

While all this "classical" work was going on, Elmer N. Bunting, Alvin Van Valkenburg, and Charles E. Weir, of the Mineral Products Division, were developing a totally new means of obtaining high pressures in collaboration with Ellis R. Lippincott, a guest scientist from the University of Maryland. Their main impetus was not the development of a pressure scale, although knowledge of the pressure was important to them, but rather the study of materials under high pressure. Specifically, they were interested in the determination of the infrared spectra of solids at high pressures as a means of studying atomic bonding. This placed an immediate and immense constraint on their apparatus, for it had to be transparent to infrared radiation. What appeared to be a great hindrance turned out to be an enormous boon.

From earlier infrared measurements on a large number of gem diamonds obtained from the U.S. Customs Service at no cost to the Government, Bunting and Van Valkenburg learned that certain rare types of diamonds, known as type II, were relatively transparent to infrared light, and, of course, like all diamonds, were very hard and strong.[168] These rare type II diamonds, of which they found several in the supply of gems, could be used for infrared microspectroscopy studies. Not much material was needed, so a very small portable apparatus for generating load was all that was necessary.

Their first attempt, which was to use a 7.5 carat diamond with a small hole through it containing steel pistons to compress the sample between them, was unsuccessful. A fundamentally new way to do this had to be found.

---

[166] Annual Report, 1963: 38-39.

[167] P. L. M. Heydemann, "The Bi I-II Transition Pressure Measured with a Dead-Weight Piston Gauge," *Journal of Applied Physics* 38 (1967): 2640-2644.

[168] E. N. Bunting and A. Van Valkenburg, "Some Properties of Diamond," *American Mineralist* 43 (1958): 102-106.

Percy W. Bridgman, the father of high-pressure research, had pioneered in the pressing of materials between two flat anvils as a means of applying pressure to them.[169] The Bridgman technique was applied to the new apparatus, and a device was built which made use of opposed diamond anvils. For this purpose, two gem-cut type II diamonds weighing approximately 0.036 g were selected from their abundant supply of contraband brilliantcut stones. The culet (the tip of the conical part) of each stone was ground to form a facet parallel to the table (the large flat top part of the gem-cut diamond). The diamonds, now having become anvils, were placed in a mount with the facets facing one another in an opposed orientation. The material to be studied was placed between them and the two anvils were driven together with an ingenious leverarm arrangement compressing a calibrated spring.

The two anvils were purposely selected to be slightly different in size, with the anvil area of the smaller used for calculation of the pressure. The first two devices made had anvil areas of 0.000156 in^2 and 0.000182 in^2. It is clear that a pressure of 100 000 psi required the small forces of 15.6 lb and 18.2 lb, respectively. In fact, one of the first instruments built was estimated to have reached a calculated pressure of 16 GPa. This pressure was later shown by the ruby pressure measurement method to be extremely overestimated. A pressure of only 5 GPa had been attained. The pressure cell was a small device that could be held in the palm of one's hand, and microscopic observation of the material under pressure was easy and routine.

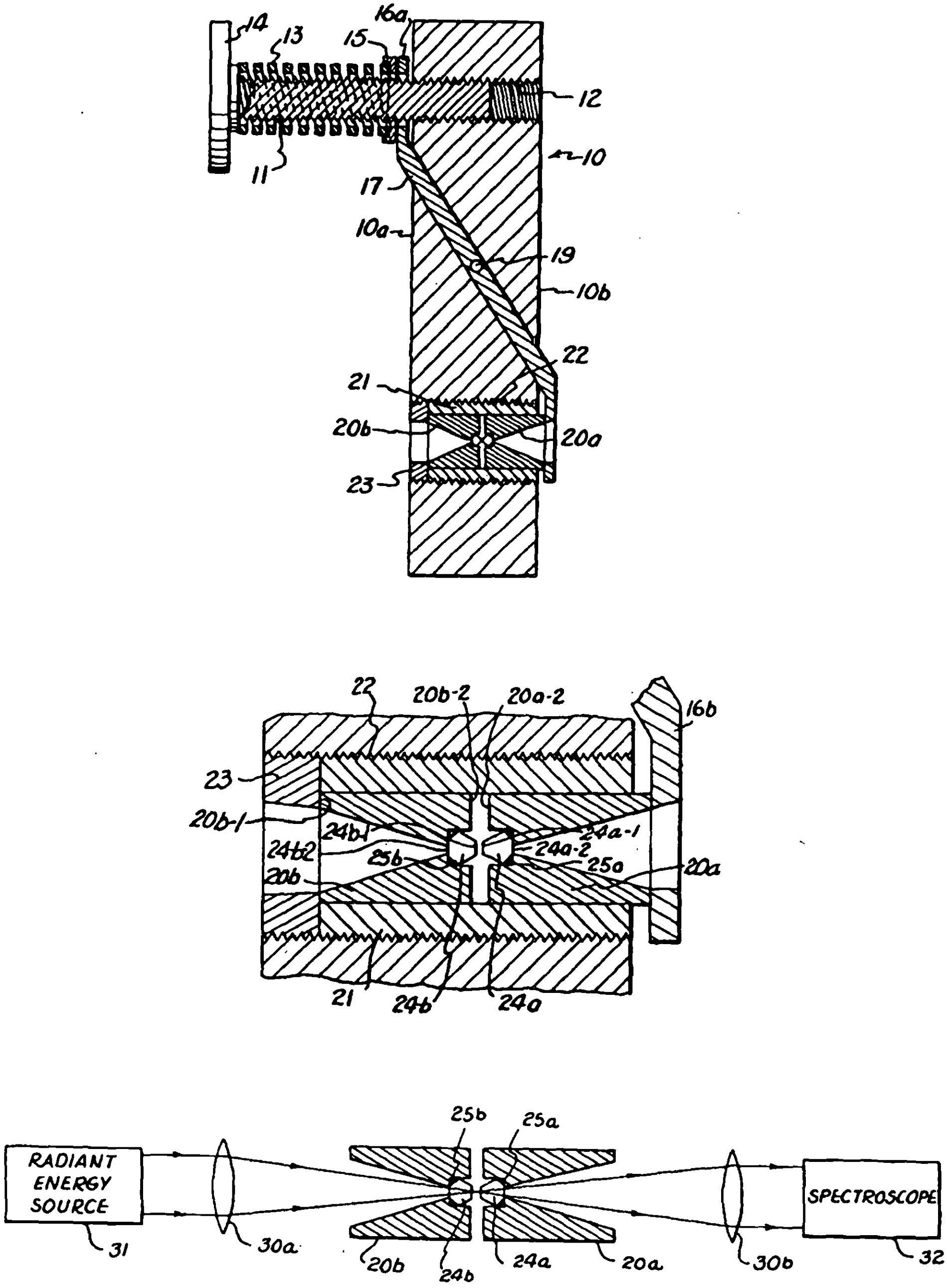
The first pressure cells were used to study infrared spectra of solids, and development of the cell was rapid.[170] Later, Van Valkenburg developed a method to confine liquids by using a thin (10 mil) sheet of Inconel 600 metal containing a tiny hole (12 mil diameter).[171] The liquid was placed in the hole and squeezed between the two anvils; it was used to observe pressure-induced phase changes in liquids and also solids immersed in liquids. A new hydraulically operated design permitted taking x-ray-diffraction powder patterns. High-pressure single-crystal studies developed as a result of a meeting convened by Howard F. McMurdie, chief of the Constitution and Microstructure Section, with Stanley Block, Gasper J. Piermarini, and Charles Weir attending. McMurdie asked the three if they thought that single-crystal x-ray diffraction studies were possible at high pressures now that the confinement of liquids in the diamond cell was possible with a metal gasket. Block replied that indeed they were possible if a pressure cell could be fabricated from beryllium metal, which is transparent to x radiation. As a result of that meeting, work on the development of the single-crystal high-pressure x-ray technique was initiated. In a tour de force of apparatus construction, Weir, in collaboration with Block and Piermarini, built one into an x-ray precession camera, thus permitting, for the first time, crystal structure determinations

---

[169] P. W. Bridgman, "The Resistance of 72 Elements, Alloys and Compounds to 100,000 kg/cm^2," *Proceedings of the American Academy of Arts and Sciences* 81 (1952): 167-251.

[170] C. E. Weir, E. R. Lippincott, A. Van Valkenburg, and E. N. Bunting, "Infrared Studies in the 1- to 15- Micron Region to 30,000 Atmospheres," *Journal of Research of the National Bureau of Standards* 63A (1959): 55-62.

[171] A. Van Valkenburg, "High Pressure Microscopy" in *High Pressure Measurements*, eds. A. A. Giardini and E. C. Lloyd (Washington, D.C.: Butterworths, 1963): 87-94.



The high-pressure optical cell invented by Charles E. Weir, Alvin Van Valkenburg, and Ellis R. Lippincott, was utilized in the spectrum analysis of solid materials. The drawings above are from Patent No. 3,079,505. Top: cross-sectional view of the high-pressure cell. Middle: detailed cross-sectional view taken through the diamond holders in the lower portion of the cell. Bottom: System incorporating the cell.

of single crystals under high pressure.[172] The first single crystal structure determination was done on a high-pressure phase of benzene at room temperature.[173]

The diamond-anvil cell was a superb instrument, but it remained useful at that time primarily for qualitative observation since it lacked a means of pressure measurement. Various fixed points had been investigated, including solid/liquid transitions and shifts in the absorption band of nickel dimethylglyoxime, but their use was extremely cumbersome and subject to considerable error. Van Valkenberg left the Bureau in 1964. Block, Piermarini, and Weir continued further development of the diamond cell and used it in pioneering studies in anisotropic compressibility measurements on numerous explosive materials by single-crystal x-ray diffraction, phase diagram determinations of sulfur, zirconia, benzene, carbon tetrachloride, and other materials. Weir retired in 1970.

In 1968, John B. Wachtman became division chief and the division had become Inorganic Materials. He realized the need to find a better method for the calibration of the pressure in the diamond cell in line with the traditional mission of the Bureau. In an effort to solve this problem, Wachtman arranged a critical meeting with Stanley Block and John D. Barnett, then on leave from Brigham Young University, to discuss possible techniques to explore to solve this calibration problem. One of these was fluorescent spectroscopy which had not yet been tested. Shortly after that meeting, spectroscopist Richard A. Forman suggested testing several materials he had on the shelf in his laboratory. These materials included: Al_2O_3 (0.5% Cr), $YAlO_3$ (0.2% Cr), YAG (0.38% Cr_2O_3), MgO (Cr) and others. In the tests it turned out that ruby was the best fluorescence material as a pressure sensor because it had the optimum characteristics for pressure shift, line sharpness, and line intensity. The idea was tried in a pressure cell in the laboratory with ruby dust excited by a filtered super-high-pressure mercury arc, and it worked. Using various materials with reasonably well-known transitions as fixed points, the shift in the wavelength of the radiation was found to be exactly proportional to the pressure.[174] Later, the shift in the ruby fluorescence line was calibrated against the compression of NaCl and linked to a pressure through its equation of state.[175] Finally, after more than a decade, a means of establishing the pressure in the diamond-anvil cell as accurately as the fixed points available was at hand.

---

[172] C. E. Weir, G. J. Piermarini, and S. Block, "Instrumentation for Single Crystal X-Ray Diffraction at High Pressures," *Review of Scientific Instrument* 40 (1969): 1133-1136.

[173] G. J. Piermarini, A. D. Mighell, C. E. Weir, and S. Block, "Crystal Structure of Benzene II at 25 Kilobars," *Science* 165 (1969): 1250-1255.

[174] R. A. Forman, G. J. Piermarini, J. D. Barnett, and S. Block, "Pressure Measurement Made by the Utilization of Ruby Sharp-Line Luminescence," *Science* 176 (1972): 284-285.

[175] G. J. Piermarini, S. Block, J. D. Barnett, and R. A. Forman, "Calibration of the Pressure Dependence of the R_1 Ruby Fluorescence Line to 195 Kbar," *Journal of Applied Physics* 46 (1975): 2774-2780.

The new ruby technique was used initially to improve the pressure capability of the cell by noting the pressures at which distortions of the metal cell components caused anvil breakage. By correcting these deficiencies, Block and Piermarini were able to extend the pressure range of the diamond cell to 0.6 Mbar, the highest static pressure achieved and measured for that time and simultaneously revise the fixed-point pressure scale, lowering it by a factor of almost two. The generally accepted value of 50 GPa for the gallium phosphide (GaP) transition had to be lowered to 22 GPa as a result of their work.[176] At this point, they decided that this pressure range was sufficient for their purposes, leaving such scientists as geologists, interested in pressures at the center of the earth, to develop the cell for higher pressures. Block and Piermarini went on to develop pioneering applications and uses not only for the pressure cell, but also for the ruby pressure measurement technique.

For example, they, along with Barnett, used the physical characteristics of the ruby R_1 lines to measure the onset of stresses in liquids by noting the pressure at which the lines broadened.[177] They used this technique to determine for the first time the hydrostatic limits in many liquids, i.e., the pressure at which a liquid no longer provides a hydrostatic pressure transmitting environment. These data were extremely valuable to the high pressure community. They also developed an elegant classical Stokes technique to measure the pressure dependence of viscosity of liquids and used a ruby sphere as the falling body in the liquid encapsulated in the diamond cell.[178] The ruby sphere also served as the pressure sensor. The viscosity data corroborated the ruby line-broadening results and was in agreement with the extrapolated glass transition pressure. The pressure cell was also designed for static heating to over 500 °C with a resistance coil furnace surrounding the anvil assembly. All of the above were pioneering developments at the NBS laboratory.

The ruby fluorescence pressure scale was subsequently extended by various scientists to 1 Mbar in 1978, and to 5.5 Mbar in observations in 1986. No longer was the diamond cell simply a qualitative or semi-quantitative instrument, but a serious quantitative research tool for carrying out studies—the quest for metallic hydrogen and the investigation of the state of matter at the center of the earth—not possible in any other way. It is not, in a general sense, a preparative device, although it has been used to synthesize tiny quantities of material and to determine the synthesis parameters for scale-up purposes. However, it is the instrument of choice as a research tool. Indeed, it is so ubiquitous and there are so many publications on its use, that its origins are all

---

[176] G. J. Piermarini and S. Block, "Ultrahigh Pressure Diamond-Anvil Cell and Several Semiconductor Phase Transition Pressures in Relation to the Fixed Point Pressure Scale," *Review of Scientific Instruments* 46 (1975): 973-979.

[177] G. J. Piermarini, S. Block, and J. D. Barnett, "Hydrostatic Limits in Liquids and Solids to 100 Kbar," *Journal of Applied Physics* 44 (1973): 5377-5382.

[178] G. J. Piermarini, R. A. Forman, and S. Block, "Viscosity Measurements in the Diamond Anvil Pressure Cell," *Review of Scientific Instruments* 49 (1978): 1061-1066; R. G. Munro, G. J. Piermarini, and S. Block, "Wall Effects in a Diamond Anvil Pressure-Cell Falling-Sphere Viscometer," *Journal of Applied Physics* 50 (1979): 3180-3184.

but forgotten. It was the result of the insight of scientists who were trying to do something that could not be done by the old, standard methods, and who had to stride off in completely different directions.

Large Forces

In 1939 the Bureau received fewer than fifty force-measuring devices for calibration. By 1959 the number was more than 900 annually. Moreover, spurred primarily by the need to measure the thrust of rocket motors and the weight of rockets, there was an immense growth in the requests for calibrations of devices to measure very large forces, some as high as 3 million pounds. In 1962, Astin told the House Appropriations Committee, "One of the most urgent programs we have at the present time is one of extending our capability for making large force measurements. This is entirely brought about by the need for calibrating the devices which measure the forces on large rockets."[179]

Along with this was the necessity to obtain higher accuracy. In the previous year Astin had told the committee, "Recently, NBS was asked to calibrate a 1.5 million pound load cell for Rocketdyne's use on its contract with NASA to develop a 1-million-pound-thrust rocket motor. In August [the] Air Force . . . released the estimate . . . that an improvement in the accuracy of thrust measurements from the present three-fourths of 1 percent to one-fourth of 1 percent would save $100 to $150 million in the static test stages of current missile and rocket programs."[180]

Before continuing, we should make very clear the distinction between force measurement and mass measurement. Every undergraduate mechanics text points out that force is equal to the product of mass times acceleration, and that "weight" as commonly used in trade, refers to a force, not to a mass. In force calibrations, the usual equation is

$$F = K\, m\, g\, (1 - \alpha/\rho),$$

where

F is force measured, for example, in newtons, dynes, pounds of force, or kilograms of force;

m is mass in grams, kilograms, or pounds of mass;

g is the acceleration due to gravity, measured in meters per second per second, centimeters per second per second, or feet per second per second;

α is the density of air; and

ρ is the density of the mass used in the measurement.

If m is thought of as the apparent mass measured against brass standards in normal air, then the appropriate values for α and ρ are 1.2 (kg/m^3) and 8400 (kg/m^3), respectively.

---

[179] 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 on Appropriations, House of Representatives,* National Bureau of Standards, 3 May 1961: 821.

[180] Appropriations Hearings for 1961: 285.

The quantity K in the equation is a constant whose value depends on the units used for the variables:

$K = 1$ if F (newtons), m (kg), and g (m/s^2);

$K = 1$ if F (dynes), m (g), and g (cm/s^2);

$K = 1/32.17405$ if F (pounds force), m (pounds mass), and g (ft/s^2);

$K = 1/980.665$ if F (pounds force), m (pounds mass), and g cm/s^2); and

$K = 1/9.80665$ if F (kg force), m (kg mass), and g (m/s^2).

Thus the *weight* of a 1 kilogram *mass* on the moon, where the force of gravity is about one-sixth of its value on earth, would be $^1/_6$ (kilogram force).

Calibration of such measuring devices as load cells or proving rings can be done quite accurately with dead weights, i.e., by hanging a known mass on the device and knowing the local gravitational acceleration and air buoyancy correction. In 1958 the Bureau had two dead-weight machines, one with a capacity of 111 000 pounds dating from 1927, and a smaller one with a capacity of 10 000 pounds. Force-measuring devices with capacities up to this level could be calibrated with an uncertainty of about 0.02 percent.[181] But in calibrating devices with higher capacities, several steps were required; combinations of proving rings or elastic devices had to be used, and accuracy was severely degraded in this process. Load cells up to 3 million pound capacity could be calibrated, but the accuracy was only 0.4 percent. Accuracies of 0.1 percent were necessary in measuring the thrust of large rocket motors.[182]

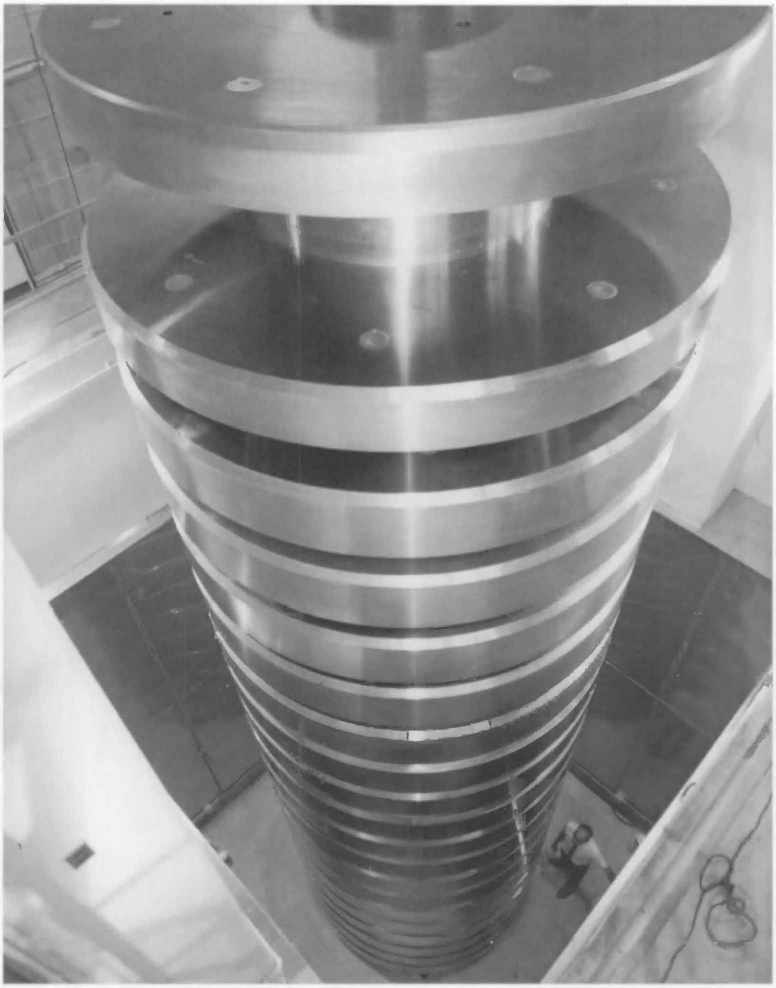
In 1958 the Bureau designed a 300 000 pound force dead-weight machine. Later, with the experience gained in this effort, the staff proceeded to design and build a 1 million pound force machine. The complement of machines was completed with a new 112 000 pound force dead-weight tester and several smaller ones. By 1964, the new machines were in operation in a special building at the Bureau's new Gaithersburg site, the first building constructed at the new site because of its urgency. They were joined by the venerable 111 000 pound force machine, which had been refurbished and brought out from the old site.

It is with the weights that accuracy begins, and they were impressive, to say the least. Thick discs of stainless steel, they weighed from 1000 to 50 000 pounds. The 1000 pound weights were 3 feet in diameter, while the heavier ones were 10 foot diameter giants. In each machine the weights could be hung incrementally from the device to be calibrated to provide a force range from zero to the full range of the machine. The machines themselves were not simple devices. Each consisted of a stack of weights, a loading frame holding the device to be calibrated at the top, and a lifting frame, actuated by a hydraulic jack, to lift the weights incrementally and thus

---

[181] Annual Report, 1960: 46-47.

[182] "Million-Pound Dead-Weight Machine Designed," *Technical News Bulletin* 43 (1959): 240.



Deadweight stack of the 1 million pound force deadweight machine that was installed in the Engineering Mechanics Building, the first building completed at the NBS site in Gaithersburg (1963). Each 50 000 lb weight was 10 feet in diameter. Most of the stack sat below the first-floor level in a 26 foot deep pit. In this photograph, James I. Price adjusted the temperature control.

load them onto the device. The million-pound force machine had twenty 50 000 pound weights, and the other two had combinations of smaller weights. And the machines were large. The million-pound force giant was 96 feet tall, the 300 000 pound force machine was 62 feet, and the old 111 000 pound force machine was 54 feet. Access was available at various levels, and temperature in the whole assembly was controlled within ±0.5 °C.

To obtain the applied force from the mass of the weights, which is directly traceable to the standard kilogram, knowledge of the value of the acceleration due to gravity, or

Arnold J. Mallinger calibrated a million-lb capacity proving ring in the 1-million-lbf capacity deadweight machine.

g, at the location of the machines is necessary. To obtain this value, an absolute determination of the acceleration was made.[183] The value obtained, 9.801018 m/s^2, was used to adjust the force to a value of g of 9.80665 m/s^2, which was the value adopted in 1913 by the General Conference for Weights and Measures for the purpose of defining such units as pound-force and kilogram-force. Adjustments were also made for buoyancy caused by the atmosphere. When all was completed, the force applied by the dead-weight machines was accurate to 0.002 percent. It was an impressive performance.

The calibration of devices to measure forces greater than 1 million pounds force, as is necessary with very large rocket motors, is simple in principle. A number of force-measuring devices with 1 million pound capacity are first calibrated. Then, to calibrate

---

[183] D. R. Tate, "Absolute Value of g at the National Bureau of Standards," *Journal of Research of the National Bureau of Standards* 70C (1966): 149; "Acceleration Due to Gravity at the National Bureau of Standards," *Journal of Research of the National Bureau of Standards* 72C (1968): 1-20.

a larger capacity device with, say, a 5 million pound capacity, five of the calibrated 1 million pound devices are coupled together in parallel, so that each carries one-fifth of the applied load. This arrangement is placed in series with the high-capacity device to be calibrated and a force is impressed across the arrangement. The force applied to the unknown device is the sum of the forces on the calibrated devices, which are easily obtained from their output indicators. The output of the unknown device is noted, and the first point of the whole calibration curve is obtained. The procedure is repeated for other loads until the whole calibration curve is established. While the procedure is direct, accuracy is inevitably degraded for a variety of reasons.

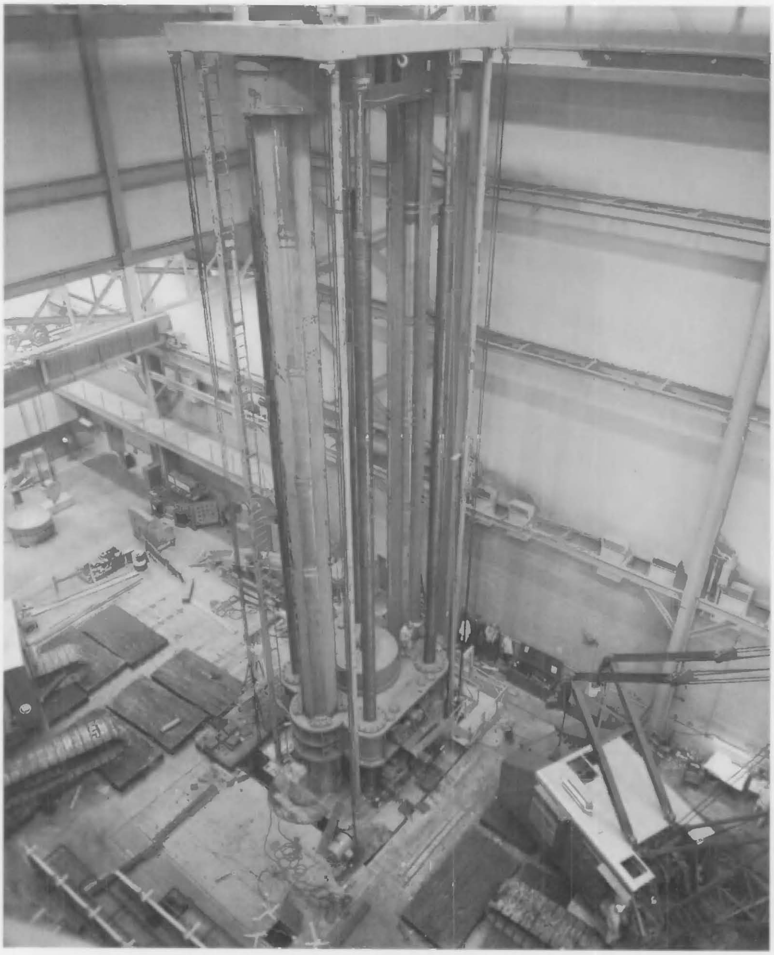
The problem with this procedure is that it requires some kind of machine capable of supplying very large forces (5 million pounds force in the above example) and holding them steady while the necessary readings are taken. For this purpose, and because of its use in measuring the mechanical properties and strength of large structural elements, such as bridge columns and beams, the Bureau procured an immense testing machine. With the capability of supplying 12 million pounds force in compression, 6 million pounds force in tension, and 4 million pounds force to a flexural specimen, it was a true giant, 101 feet tall, with its lower 23 feet in a pit below ground level.[184] It was placed in a special 97 feet tall bay near its companion dead-weight machines, and in 1999 is still the Nation's largest universal testing machine.[185] Alongside its companion dead-weight machines, the whole formed a splendid new facility for the Bureau and the Nation.[186]

---

[184] Arthur F. Kirstein, *Universal Testing Machine of 12-million-lbf Capacity at the National Bureau of Standards*, Natl. Bur. Stand. (U.S.) Special Publication 355; September 1971.

[185] The story of how the decision was made to obtain the machine is interesting. The main purpose for the machine was the calibration of the large devices used in determining the thrust of rocket motors. Although the military—the main customers—were not certain what the highest thrust would be, a manufacturer of solid propellant motors was considering a 5 million pound thrust engine. But there was also considerable interest in the engineering community on large scale structural testing. Huge skyscrapers using immense beams were being considered. Norway was planning a testing frame with a 5 million pound capacity for ship structures, and Japan was building a 5 million pound machine for testing ships and the guy system for a planned inter-island suspension bridge. The largest capacity machine in the United States, located at Lehigh University, had a 5 million pound capacity. With these currents flowing, Astin ordered a study to give him a recommendation on the type of machine to be built. In due course a presentation was made to Astin by Lafayette K. Irwin, chief of the Engineering Mechanics Section of the Mechanics Division, accompanied by Bruce L. Wilson, chief of the division. Their recommendation was for a machine with a capacity between 10 million pounds and 20 million pounds in compression, but that it should not be a universal testing machine. The machine would be used for force calibration only, although a yoke could provide some testing capability as well. Such a machine need not be very tall, and would not be terribly expensive. Astin listened to the whole presentation silently, staring off into the distance, as was his custom. At the conclusion he turned to Irwin and Wilson and, stunning his audience, said, "The Bureau is going to have the largest testing machine in the world," and thus was made the decision to obtain the immense device. (Lafayette K. Irwin, private communication.)

[186] "New NBS Force-Calibration Facilities Capacity Increased to One Million Pounds," *Technical News Bulletin* 49 (1965): 186-188.



A 12 million lbf capacity hydraulic testing machine, believed to be the world's largest, was installed in the Engineering Mechanics Building. The machine provided the force to calibrate multi-million-lbf capacity force-measuring devices for space and industrial applications and to test full-scale structural components.

Time and Frequency

Time is one of the fundamental quantities of the physical measurement system.[187] Thus the Bureau has always been concerned with time and frequency (essentially the reciprocal of repeated time interval) standards, and in this area carries out three functions: the development of frequency standards, from which clocks can be made

---

[187] More precisely, it is time interval and its unit, the second, that occur in the basic measurement system. Epochal time does not.

by direct (but not necessarily technically easy) counting; the definition of the unit of time—the second—and its relation to the standard of frequency; and the dissemination of time and frequency signals. During the period covered by this chapter, work in these areas produced a veritable cornucopia of results and added to the Bureau's capabilities and facilities.

Frequency Standards

In 1949 the Bureau announced the first "atomic clock."[188] Really a "molecular clock," it was based on the microwave absorption line of ammonia. At that time the second was still defined on the basis of the rotation of the earth. The national standard of frequency, which served as the top of a chain for calibrating frequency measuring instruments, and controlled the frequency of the Bureau's two time and frequency broadcast stations—WWV in Beltsville, Maryland, and WWVH in Maui, Hawaii—was a set of quartz crystal oscillators rigorously maintained at a constant temperature in 50 feet deep wells. Their frequency was calibrated against the rotation of our slightly wobbly earth, a long, tedious process. The growth of science, the needs of modern navigation, the explosion in communications traffic, and military and space requirements, not to mention the sheer intellectual challenge involved, made the development of ever-more-accurate standards imperative, and the ammonia clock was the first attempt to build a timepiece based on a natural atomic constant.

While the idea of using an atomic oscillator as a frequency standard had been around for a long time,[189] recent advances in radio techniques and fundamental physics had made the construction of atomic clocks a real possibility.[190] The ammonia clock was based on the control of the frequency of a quartz crystal oscillator by the absorption of radiation by ammonia, which occurs very precisely at a frequency of 23.870 GHz. In the first model, the crystal oscillator was adjusted manually to keep its frequency at the value that would provide maximum absorption by the ammonia, which was contained in a 30 foot cell. Later, a feedback loop automatically adjusted the oscillator. In either case, the ammonia resonance controlled the frequency of the oscillator, and counting the cycles produced a clock. With a stability of 1 part in 20 million, it was no better keeper of time than the rotation of the earth, but it pointed the way to better instruments.

---

[188] Annual Report, 1949: 66-69.

[189] George K. Burgess, the second director of the Bureau, wrote in the *Standards Yearbook* in 1928, "Any radiation frequency emitted by an atom is the ticking of an atomic clock...." Snyder and Bragaw, *Achievement in Radio*: p. 292, cite Lord Kelvin as suggesting "the vibrational states of hydrogen and sodium atoms as natural standards of frequency and length (wavelength)."

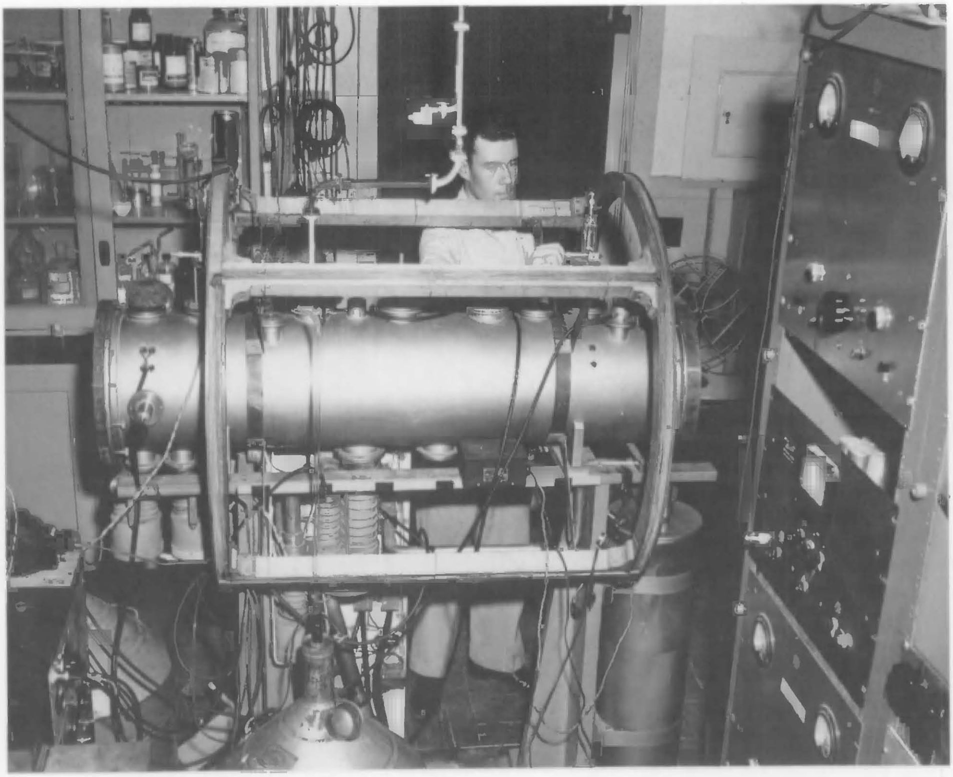
[190] C. H. Townes, "Atomic Clocks and Frequency Stabilization on Microwave Spectral Lines," *Journal of Applied Physics* 22 (1951): 1365-1372. An analysis of the various types of possible atomic clocks, this paper was stimulated by Harold A. Lyons, then chief of the Bureau's Microwave Standards Section, under whose direction the ammonia clock was produced.

There was already a better candidate at the time the ammonia clock was announced. In 1945, I. I. Rabi of Columbia University had suggested that a magnetic resonance atomic beam technique could be used to produce a highly accurate atomic clock. In such a technique, the various line-broadening mechanisms, such as Doppler shift and pressure broadening, would be largely absent. This suggestion led to the proposal to use cesium beams where, in the absence of an external field, the interaction of the nuclear magnetic moment with the field at the nucleus produced by the spin of the valence electron splits the ground state of the atom into two states—the so-called hyperfine splitting. In one of the states, the electron spin is parallel to the nuclear moment; in the other it is antiparallel. The transition from one state to the other involves a very precise frequency. In an isolated atom, this is an invariant frequency, perfect for a standard. In the cesium beam device, a beam of atoms is produced by evaporation in an oven in which molten cesium is kept. A finely collimated beam is produced and passed through a short inhomogeneous magnetic field (a Stern-Gerlach magnet) which splits the beam into two, one in which all the atoms have their magnetic moment up, and one in which it is down. A slit selects one of the beams so that it passes into a long cavity in which it is subjected to a radio frequency (rf) field. From there it passes into another Stern-Gerlach magnet. If the frequency of the rf field is the same as the atomic moment reorientation frequency, then during the passage of the atoms through the cavity the magnetic moment of the atoms is flipped in orientation and the second Stern-Gerlach magnet directs them into a detector, which then produces a signal. If the frequency is not the same, most of the atoms are lost, and no signal comes from the detector. The result is an exceedingly sharp resonance curve with a center frequency equal to the reorientation frequency, a constant of nature approximately equal to 9.192 GHz for a cesium beam. As with the ammonia clock, the observed signal from the detector may be used with a servomechanism to control the frequency of the rf field, which is provided by a quartz oscillator with an appropriate frequency-multiplier chain.[191]

Research on the first NBS atomic beam clock, with help on the design by Polykarp Kusch, who had worked with Rabi, was begun in the same year as the announcement of the ammonia clock. Finished in 1952, it was labelled NBS I and had a stability of

---

[191] There is more to the matter than this simple explanation gives. First, in the cavity the atoms are not subjected only to an oscillating rf field. There is, in this region, a small uniform constant field "applied in order to preserve the state identity of the atom" while passing through the cavity (Beehler, Mockler, Richardson, "Cesium Beam Time and Frequency Standards," 118). Since in the general case the reorientation frequency of the atomic moment is dependent on this constant field, a bad situation for a frequency standard would arise, for its accuracy would depend on an accurate knowledge of the strength and uniformity of this constant field. For this reason, the transition ($F = 4$, $m_F = 0$) to ($F = 3$, $m_F = 0$) is chosen for the standard since its frequency has only a second order dependence on the field, and this can be made negligibly small. Second, the atoms are not subjected to an oscillating field throughout the whole region of the cavity. Norman F. Ramsey showed that it was necessary only to subject the atoms to the oscillating field over a small region at the beginning and end of the cavity, provided only that the rf field in these two regions be accurately in phase. This technique also makes the resonance peaks narrower; makes their sharpness independent of nonuniformities in the constant field; and is more convenient to use. (N. F. Ramsey, "A Molecular Beam Resonance Method with Separated Oscillating Fields," *Physical Review* 78 (1950): 695-699). Professor Ramsey shared the 1989 Nobel Prize for his discovery.



The clock known as NBS I, fully operational by 1952, used a beam of cesium atoms to control electronic and microwave circuits to better than 1 part in 100 million. A conventional electric clock connected to the atomic clock would vary no more than one second in three hundred years.

1 part in 10^{10} and, after modification, it attained a relative uncertainty of 2 parts in 10^{10}. Built in the Bureau's Washington laboratories, it was disassembled in 1954 and shipped to the Bureau's new site in Boulder, where it was reassembled. A relatively primitive instrument, it had no servomechanism, and the frequency of the driving quartz oscillators had to be adjusted manually, as was necessary with the ammonia clock. This feature made its use as a frequency standard or clock impractical.

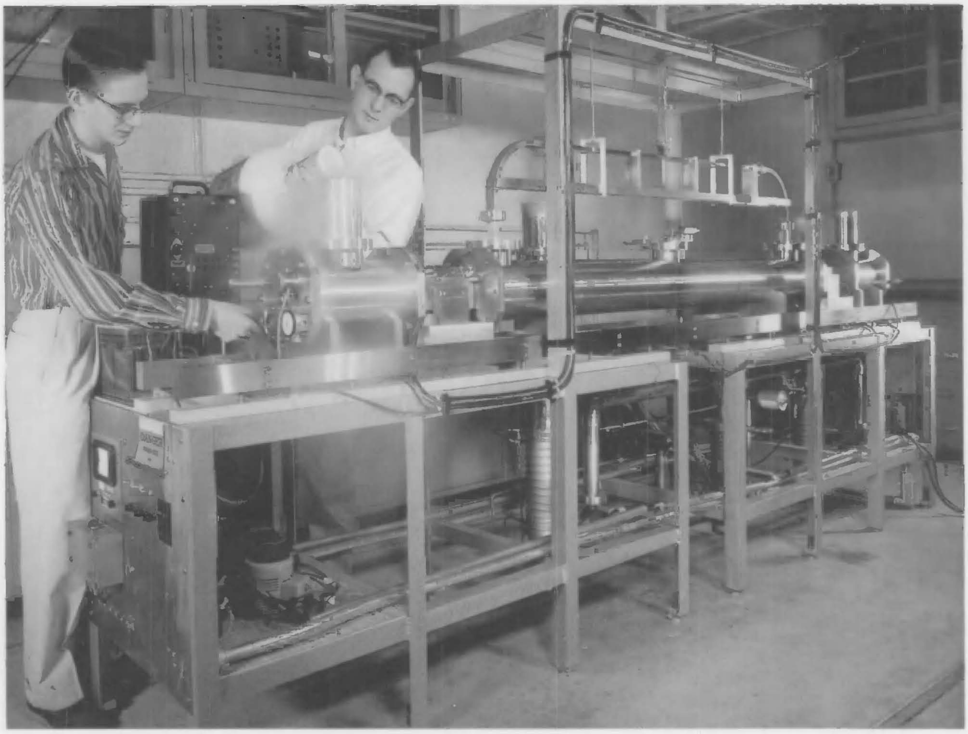
Construction began on another cesium beam clock that same year . With a longer cavity,[192] and with servomechanism control, this was expected to be a far more advanced clock. It was completed in 1958 and compared with NBS I, which had been rebuilt and had attained "a precision of 2 parts in 10^{10}."[193] A careful evaluation of the two clocks "enabled the United States frequency standard to be referred to in terms of atomic resonance with an accuracy of 1.5 parts in 10^{11}."[194] On January 1, 1960, NBS II

---

[192] The longer the cavity, the sharper the resonance. Townes, "Atomic Clocks," 1371-1372.

[193] Annual Report, 1959: 123.

[194] Annual Report, 1960: 38; R. E. Beehler, R. C. Mockler, C. S. Snider, "A Comparison of Atomic Beam Frequency Standards," *Nature* 187 (1960): 681-682.



NBS II measured frequency and time intervals to an accuracy equivalent to the loss of one second in three thousand years. In 1960, it became the National Frequency Standard. As shown in this photograph, Roger Beehler adjusted the atomic beam detector while Charles Snider poured liquid nitrogen into a cold trap at one end of the instrument. The nitrogen helped to form a vacuum so that cesium atoms could be beamed through the machine without being deflected by molecules of air.

became the national standard of frequency, supplanting the quartz oscillators in their cool wells. From that day to this, the frequency of a cesium clock has been the frequency standard of the United States.

In 1959, before NBS II became the national standard of frequency, construction was begun on NBS III. It was finished in 1963, whereupon it was designated as the national standard. It had a resonant cavity 366 cm long—twice the length of NBS II. It served until 1969, at which time it had achieved a stability of 1 part in 10^{13}, and a relative uncertainty of 5 parts in 10^{13}. Another clock, NBS-4, a short 50 cm cavity clock built to test new ideas for improving the stability of cesium clocks, took over from NBS III in 1969. It was extremely stable, achieving a stability of 7×10^{-15}. To complete the roster, NBS-5 was completed in 1972 and served until 1974, when the next model, NBS-6, took over with a relative uncertainty of 9 parts in 10^{14}, or 1 second in 300 000 years. NIST-7 officially replaced NBS-6 in January 1993. It was put into service with a relative uncertainty of 4×10^{-14} that was later improved to 5×10^{-15}.

Atomic clocks spawned a new industry. Small, portable cesium-beam and rubidium-beam clocks are made commercially and sold to organizations that require high-precision clocks, such as broadcasters, the military, and telephone systems. The National Institute of Standards and Technology uses them as well.

To leave the matter here would not do justice to all the effort in time and frequency. Many other paths had been travelled before selecting the cesium clock as the national standard. The original ammonia clock had been highly developed and supplanted by the much more precise ammonia maser.[195] In turn the ammonia maser was followed by the hydrogen maser, still under investigation at the end of the period. A hydrogen cyanide maser had been tried. Oxygen had been explored as a replacement for ammonia in an absorption-controlled clock. Thallium-beam and rubidium optical-microwave double resonances had been investigated, but had not supplanted the cesium clock. Quartz crystals at the temperature of liquid helium had been studied. But the cesium beam clock became the standard, in large part because it was extensively investigated internationally, hence was best understood.

The Definition of the Second

Since the unit of time is the second—one of the basic units for quantified characterization of physical standards—NIST has the central responsibility in the United States for its definition. To carry out this obligation, NIST serves as the Nation's representative to the international bodies—the International Committee for Weights and Measures (CIPM) and the General Conference for Weights and Measures (CGPM)—that promulgate this definition. When the second is compared to units of other quantities, however, it has a unique peculiarity. Like the krypton-86 unit of length, it is not a unit that can be stored in a vault and periodically pulled out for calibration purposes; it must be generated. In either case, of course, the apparatus may be stored in a vault—at least conceptually. But the second, or time, has a further peculiarity. If a clock is to be built from a frequency apparatus that generates seconds by counting repeated events, then the apparatus must operate continuously, for time constantly passes even when the clock is stopped. Without this requirement for continuity of operation, a frequency apparatus is useful for the generation of time interval, but not for time nor, its more formal description, epoch.[196]

Before the advent of atomic clocks, the definition of the second was an astronomical matter, for the second was defined on the basis of the motion of the earth with respect to other astronomical bodies. This provided a good definition of the second and had complete continuity. The earth was a good clock but its use was cumbersome for determining time interval. Thus, in those days the function of the Bureau was primarily as custodian of the national standard of frequency with the assurance that this standard was in accord with the astronomical definition of the second.

---

[195] Gordon, Zeiger, and Townes, "The Maser," 1264-1274.

[196] The word epoch is used rather than the word time, because time in its common usage can refer either to a time/date point along some timekeeping continuum or to a time interval. Epoch can only be interpreted as the time/date point (the reading and logging of the day, date, hour, and minute), and is difficult to confuse with time interval. (Donald B. Sullivan, private communication)

Before 1956 the second was defined as $^1/_{86400}$ of a mean solar day. This definition was not adequate since its basis, the mean solar day, was subject to temporal variations in the rotation of the earth. Nevertheless, it was the basis for various time scales which denoted Universal Time, or UT. In an attempt to remedy the non-constant nature of the second as thus defined, a new definition based on the more constant yearly revolution of the earth, was adopted in 1956.[197] Called the ephemeris second, this new unit was based on an invariant occurance, but this did not make its realization any easier. Even as the new unit was adopted, it was clear that its realization was not as accurate as the second generated by atomic-frequency devices, although by its definition it was a constant unit of time.

In a three-year cooperation between the National Physical Laboratory and the U.S. Naval Observatory, the frequency of a cesium frequency generator was determined in terms of the ephemeris second with the finding that the cesium frequency was 9 192 631 770 ± 20 Hz, with most of the uncertainty coming from the astronomical observations.[198] By 1964, national laboratories were achieving relative uncertainties of about 1 part in 10^{11} with their cesium-beam devices, and the Twelfth General Conference for Weights and Measures authorized the CIPM to designate an atomic or molecular frequency to be used temporarily for the physical measurement of time. The International Committee declared the cesium-133 frequency to be 9 192 631 770 Hz. The Bureau had adopted this frequency on January 1, 1960.[199] The final step in the shift from an astronomical to an atomic definition of the second was taken in 1967 when the 13th CGPM adopted the unit of time as the second, defined as the duration of 9 192 631 770 periods of a well-defined resonance in the cesium-133 atom.[200] All previous definitions were abrogated.

The various national laboratories did not wait for this formal action to construct atomic clocks and time scales. Even before the adoption of the value of the cesium frequency by the CIPM, it was clear what the choice would be, and various time scales were developed.[201] All that was necessary was a starting point to define the epoch and an arrangement to ensure that the system operated continuously. The most direct way to make a clock to generate a time scale would be to divide down the frequency of the quartz oscillator that is locked to the cesium frequency standard, and drive a mechanical or electronic clock directly. But that would require that the cesium standard work continuously, and this is unrealistic. In fact, in the Bureau system during the period, a free-running quartz oscillator drove the clock. Every day the frequency of the oscillator was calibrated in terms of the National Frequency Standard and, assuming that any observed difference was caused by a linear drift, corrections were made to the time as

---

[197] *Onzième Conférence Général des Poids et Mesures* (Paris: Gauthier-Villars, 1960): 15-16.

[198] W. Markowitz, R. G. Hall, L. Essen, and J. V. L. Parry, "Frequency of Cesium in Terms of Ephemeris Time," *Physical Review Letters* 1 (1958): 105-107.

[199] Annual Report, 1960: 38.

[200] The official, more-precise definition of the second is given in Barry N. Taylor, *The International System of Units*, Natl. Inst. Stand. Technol. (U.S.) Special Publication 330; August 1991: 29.

[201] R. E. Beehler, R. C. Mockler, and J. M. Richardson, "Cesium Beam Atomic Time and Frequency Standards," *Metrologia* 1 (1965): 127-128.

kept by the quartz clock. This provided a time scale called NBS-A. Its epoch was set to be "approximately equal to that of UT2 at 0h0m0s on 1 January, 1958." Other time scales based on the cesium standard were also developed, differing mainly in epoch and calibration scheme. The Naval Observatory developed a similar scale, called A_1, based on the weighted average of nine laboratory and commercial cesium standards throughout the world. The Swiss TA_1 scale, also set to coincide with Ephemeris Time at 0h0m0s on January 1, 1958, was based on the cesium standard at the Laboratoire Suisse de Recherches Horologeres (LSRH).

Astronomical time was, however, important for navigation and astronomical purposes, so it was important to reconcile any differences that might arise between time kept by the various atomic time scales and that kept by the earth, with its relatively erratic motion. As a result various corrections were made periodically to the atomic scales to keep them consistent with the "earth clock." Adjustments were made either in the broadcast frequency or in the phase of time signals. A typical adjustment of the latter kind reads,[202]

> An adjustment in the transmission of time signals has been announced jointly by the U.S. Naval Observatory and the National Bureau of Standards. . . . The transmitting clocks at the radio stations were retarded 100 milliseconds 1 November 1963 at zero hours Universal Time (7 p.m. EST of 31 October).
>
> The adjustment becomes necessary because of changes in speed of rotation of the earth, as determined by astronomical observation. Such adjustments are made by international agreement, according to a plan whereby the times of emission of time signals are synchronized to about 1 millisecond. The last previous adjustment in phase of time signal pulses was made 1 August 1961.
>
> The countries participating in the coordination of time signal transmissions are Argentina, Australia, Canada, Italy, Japan, South Africa, Switzerland, the United Kingdom, and the United States.

In later years "leap seconds" would be either inserted or removed from the time scale to maintain consistency.

Frequency and Time Dissemination

The Bureau began its radio dissemination of standard frequencies in 1923 with broadcasts from its Van Ness site via radio station WWV, which it operated for this purpose.[203] Throughout its history, improvements were continually made to this service: in the accuracy of the signals, in the number of frequencies broadcast, by the broadcast of 1 second time pulses, by the broadcast of time in cooperation with the Naval Observatory, and by the broadcast of 1000 Hz and 440 Hz (musical A) signals.

---

[202] "Adjustment in Phase of Time Signals," *Technical News Bulletin* 47 (1963): 219.

[203] Snyder and Bragaw, *Achievement in Radio*: 260-288, give a complete and detailed chronology of the Bureau's time and frequency dissemination activities via radio transmission.

In 1931, the transmitter was moved to College Park, Maryland, for a brief one year stay, and then to the Agricultural Research Station of the USDA in Beltsville, Maryland. The location name of WWV was changed to Greenbelt, Maryland, in 1961. The frequencies were always controlled by the national standard of frequency (later called the USFS). In 1948 another station, WWVH, was established on the island of Maui in Hawaii, to better serve the Pacific Ocean area. In 1957 WWV broadcast at 2.5 MHz, 5 MHz, 10 MHz, 15 MHz, 20 MHz and 25 MHz, and WWVH at 5 MHz, 10 MHz, and 15 Mhz. The broadcasts covered the United States and the Pacific Ocean, and were widely received throughout the world.

While the frequency of the carrier wave as broadcast—controlled as it was by the national standard of frequency—was as accurate as possible, the frequency of the received signal was far different. Studies carried out in Boulder in the early fifties showed that the frequency of the received signal was subject to change, leading at times to a relative uncertainty as great as ±3 parts in 10^7, while the relative uncertainty of the transmitted signal was approximately 1 part in 10^9. It happens that the relatively high frequency waves broadcast—so called "sky waves"—are propagated by reflection from the ionosphere, which is a variable, movable mirror subject to solar and other disturbances. This causes Doppler shifts and other interferences which degrade the signal and reduce its accuracy. Comparisons of a given frequency with this varying signal can be made more accurate by time averaging over one to ten days, but this was too long for many calibration and research purposes.

It was thought that the best way to solve this problem was to broadcast via a ground wave which would travel within the duct formed by the ionosphere and the earth's surface, rather than relying on reflection from the ionosphere for propagation. Such a signal would not be subject to error-producing reflections. It was well known that such propagation occurred at low transmission frequencies. Indeed it had been shown by John A. Pierce of Harvard that, for frequencies below 100 kHz and for distances up to 5000 km, it took only about ten minutes to compare local frequencies with the broadcast standard to 1 part in 10^9. As a result, William D. George, acting chief of the Radio Standards Laboratory, began experimental low-power broadcasts from Boulder at 60 kHz, using the call letters KK2XEI (later WWVB). By June 1956, with the help of Pierce, the Bureau broadcasts were compared with British low frequency transmission at 60 kHz and 16 kHz to better than 1 part in 10^9, a lower uncertainty than could be obtained by comparison with the high-frequency broadcasts with ten-day averaging.[204]

The 60 kHz broadcasts were designed to cover the forty-eight contiguous states. To serve a greater area, and possibly the whole world, required even lower frequencies, and in 1958 experiments were begun on broadcasts at 20 kHz. To provide such broadcasts required international cooperation, for radio waves do not respect national boundaries. In April 1959, acting on a Bureau recommendation, the U.S. group of the International Radio Consultative Committee (CCIR) adopted 20 kHz as the most efficient frequency for the transmission of standard frequency on a global scale. In December 1959, this frequency was also adopted by the International Radio Conference. Finally,

---

[204] "Experimental Standard Frequency Broadcast on 60 Kilocycles," *Technical News Bulletin* 41 (1957): 99-100.

in January 1960, the U.S. Government's Interdepartmental Radio Advisory Committee (IRAC) approved a Bureau application to operate a 20 kHz standard frequency broadcast. By international agreement on such requests, the Bureau had ninety days to get on the air.[205]

The price of transmitting low frequencies is antennas. At 20 kHz the wavelength is 15 km, and antennas have to be immense if they are to efficiently broadcast this frequency. Fortunately, in Four-Mile Canyon near Sunset, Colorado, the Bureau had an existing antenna, previously used for low-frequency ionospheric research and other purposes. This was modified for the 20 kHz broadcasts. It was an impressive array. A copper-coated steel cable stretched 3400 feet between two peaks 900 feet above the canyon floor. A feed line from its center dropped down to the 20 kW transmitter; the call letters were WWVL. Transmission began in the spring of 1960, and it was gratifying that the signal was received at least as far away as New Zealand.

It had always been planned that neither the WWVB site in Boulder nor the WWVL site near Sunset would be permanent; they were for experimental purposes. The permanent site would be on a carefully chosen 380 acre tract near Fort Collins, Colorado,

Facilities near Fort Collins, Colorado, for stations WWVL and WWVB. The antenna for each station consisted of four steel towers arranged in a diamond 1900 feet long and 750 feet wide.

---

[205] "New Standard Frequency Broadcasts," *Technical News Bulletin* 44 (1960): 120-122.

some fifty miles north of Boulder. The primary assets of the new site were high soil conductivity, the availability of electric power, relative freedom from both weather extremes and man-made noise, and ease of access. Here 50 kW transmitters and more efficient antennas would greatly increase the radiated power, which for WWVL would jump from 15 W to 1 kW, and for WWVB from 1 kW to 5 kW. A fifty mile cable would provide a direct connection, hence frequency control of the transmitters with the national standard of frequency at Boulder. When WWVB began broadcasting on July 5, 1963, and WWVL in August of the same year, the relocation of the Bureau's low-frequency transmitters was complete. The Bureau had a new facility for expanding a service it had historically provided.[206]

Calibrations

The late fifties were a time of ever-increasing demand for calibration services. Spurred by the needs of the electrical industry and the military services, the demands did not lessen with the turn of the decade. In 1960 the Annual Report states: "The demand for improved calibration services based on new or more accurate measurement standards continued to grow during the year. Calibration needs were most evident in electronics, but requests for greater accuracy and wider range of measurement were received in virtually all fields of measurement."[207] By that time the Bureau was limiting itself insofar as possible to the calibration of reference standards for other standards laboratories, which then calibrated working standards and, finally, the instruments of industry, science, and the military. "To an increasing extent, the Bureau was called upon for assistance to the other standards laboratories that have been set up," states the Annual Report for 1959.[208]

The need for a wider range of calibration services was well documented. In 1959 the Aerospace Industries Association queried seventy member companies in its field and found increased needs in microwave, temperature, vibration, and shock measurements. More than one hundred of the needs were for services not already provided by the Bureau. "Either the Bureau did not provide any service for the particular physical quantity involved, or the range of measurement or accuracy required was not available." More basic research was needed.[209]

In no field were these demands for services stronger than in radio standards. In response to this need, the Bureau began the construction of an Electronic Calibration Center on the Boulder site in 1956 with support from the military services. Built particularly to provide services to the military standards laboratories, it began operation in 1958, and FY 1959 was its first full year of operation. This new facility permitted an explosion in the number of calibrations of radio-related equipment. Thus, 508 items of radio equipment were calibrated in 1958. The number jumped dramatically in 1959 to

---

[206] "Frequency and Time Transmission: New LF and VLF Antennas and Transmitters Under Construction," *Technical News Bulletin* 46 (1962): 185-186.

[207] Annual Report, 1960: 15.

[208] Annual Report, 1959: 7.

[209] Annual Report, 1961: 9.

The Electronic Calibration Center, a new wing of the NBS Boulder Laboratories, was completed in 1958. Its 27 000 square feet of floor space accommodated $2 million worth of equipment for calibration service to the electronics industry and the military.

8594. Through 1964 calibrations averaged 3600 radio instruments per year. In the other divisions demand held steady, totalling about 75 000 calibrations yearly. Yet despite this disparity in the number of items, the total fee value of the two groups of items was about the same, averaging $969 000 for radio standards and $1.05 million for all other types. Clearly, the value per item was much higher for the complex radio equipment, being of the order of $275 per item as contrasted with about $15 per item in other calibrations. And business boomed, increasing from a total of $1.58 million in 1960 to $2.36 million in 1964, an increase that was more than twice the rate of inflation.[210]

The Bureau took several actions to improve the Nation's calibration system. In 1960 two new advisory committees were formed, one on Engineering and Related Standards, and the other on Calibration and Measurement Services. Unlike the other advisory committees, which served individual organizational units, these were concerned with the Bureau's entire operation in their assigned fields. Then, as part of its effort to divest itself of routine calibrations and limit itself to the calibration of reference standards for commercial, private, and military standards laboratories, NBS formed and sponsored the National Conference of Standards Laboratories. The brainchild of Harvey W. Lance, head of the Electronic Calibration Center, this organization provided

---

[210] Annual Reports: 1958: 100; 1959: 9; 1960: 16; 1961: 11; 1962: 14; 1963: 3; 1964: 6.

a means by which standards laboratories could cooperate and share information on calibration techniques and the operation of standards laboratories. The first meeting of this new institution was held at the Boulder site in 1962. For three days, 600 persons discussed technical and administrative matters of importance to standards laboratories. A record of the proceedings was published.[211] Yearly conferences became the norm.[212]

Other, more specialized conferences were held, particularly in the radio field, and new specialized publications were issued, culminating in 1962 in the massive three-volume Handbook 77, *Precision Measurement and Calibration*.[213] A special column in the monthly *Technical News Bulletin* was initiated in the same year. In 1963 the Bureau also began publishing the schedule of services and fees for calibrations that had been published for many years in the *Federal Register*.[214] The catalog was initially issued in sections prepared by the various NIST calibration services, but is now a single volume and has replaced the notices in the *Federal Register*.

Standard Samples Become Standard Reference Materials

The history of standard samples during the period generally mirrored that of calibration services. Starting with a large jump in sales of items from 38 800 in 1957 to 70 500 in 1959, sales leveled off between 66 000 and 81 000 items annually. But, as with calibrations, the value of sales increased, rising from $188 200 in 1957 to $573 600 in 1964—a factor of three, and far greater than the rate of inflation—attesting to the increasing sophistication of the items.[215] New samples were added periodically and old ones retired as their utility and need decreased. Thus, in 1957 580 samples were for sale, while in 1964 more than 600 were available. But more illustrative is the change in kinds of available samples. In 1957 a wide range of pure hydrocarbons and organic sulfur compounds of interest in automotive fuels research were available. All had been discontinued by 1964 following the demise of the Bureau's automotive research program. To the venerable steel-composition standards, which had been the first of the Bureau's standard samples, dating back to 1905, zirconium-based alloys had been added because of their utility in nuclear power development, and titanium alloys had been added due to aerospace concerns. In

---

[211] *Proceedings of the 1962 Standards Laboratory Conference*, Natl. Bur. Stand (U.S.) Miscellaneous Publication 248; August 1963.

[212] In 1985, this eminently successful organization became independent, operating without Bureau sponsorship but maintaining close liaison with it. By that time it had expanded to include foreign members, and over 900 organizations belonged. Its scope had grown to include all areas of measurement.

[213] *Precision Measurement and Calibration*, Volumes I-III, Natl. Bur. Stand. (U.S.) Handbook 77; February 1961. This handbook was expanded in the late 1960s and early 1970s to a 10 volume set, Special Publication 300, of the same name.

[214] *Calibration and Test Services of the National Bureau of Standards*, Natl. Bur. Stand. (U.S.) Miscellaneous Publication 250; December 1970.

[215] The cost of the background research on which standard samples were based could not be charged to the customer. Only the costs of preparation and handling were permitted.

radioactivity standards, new nuclides were offered, among them americium-241, promethium-147, cobalt-60, a series of point source gamma ray standards, and, of course, maintaining carbon-14 samples for standardization of radio carbon dating. All but three of twelve rock samples certified for radium content were removed. The changes reflected a moving snapshot of the development of nuclear technology.

In symmetry with the National Standard Reference Data System, standard samples, which had been variously labeled as standard samples, standard materials, and reference materials, were finally labeled Standard Reference Materials (SRMs) in 1963. At the same time, an Office of Standard Reference Materials was formed under Wayne Meinke, new chief of the Chemistry Division. This office had the responsibility of administering the production and sale of SRMs along with the function of working with the outside scientific and industrial community to plan the future directions of the activity and the development of new SRMs. With this move the Bureau's SRM program was organized and stabilized.

The End of Product Testing

In an agreement with the General Services Administration (GSA), the Bureau, as a result of the AD-X2 affair and the resulting report of the first Kelly Committee, clarified its position with respect to routine testing of Government purchases. Under that agreement, it limited itself to the preparation of general test methods and specifications for products where it had specific knowledge and interest, to areas where it had unique capabilities, and routine testing in areas where it had developed specifications. Even with these qualifications, while showing a decrease during the period, testing remained a significant activity, averaging 54 400 samples tested per year at an average total fee value of $1.2 million. As was historically the case, cement testing accounted for approximately 70 percent of this activity. In addition, a few new specifications were prepared annually, giving the Bureau custody of approximately 200 such standards, and for other agencies it reviewed on the order of several hundred specifications per year to determine their suitability as Government standards. There was even an administrative change. Cement testing, historically in the Mineral Products Division, was taken over by the Building Technology Division in 1960.

With the move to Gaithersburg coming ever closer, a more permanent resolution of the Bureau's position in acceptance testing had to be achieved. In 1965, Astin wrote to J. Herbert Hollomon:

> The . . . Bureau . . . expects to maintain a facility for the actual testing of items purchased by the General Services Administration and other agencies of the Federal Government. This will be operated by the Bureau with the understanding that the GSA, which will pay for the facility, will take over its operation by the end of 1969.[216]

---

[216] Memorandum, A. V. Astin to J. H. Hollomon, "Engineering and Commodity Standards Activities," May 3, 1965. (DOC; Assistant Secretary for Science and Technology; Accession 40-70A-6988; Box 27; Folder NBS-Commodity Standards)

Thus, in 1966, an organizational unit first called the NBS-GSA Laboratory and later the NBS/GSA Test Development Division was formed and headed by Philip J. Franklin. As promised, the unit was transferred to the GSA in 1969. "The transfer of the laboratory is appropriate," read the Annual Report, "since GSA has responsibility for qualified product lists and has the legislative authority to run a laboratory for testing products against standards."[217] Started in 1903 with the testing of light bulbs, this program, which had indirectly caused the major furor of the AD-X2 affair, quietly left the Bureau in 1969 with only a few members of the staff aware of its demise. Its departure was a testament to the changing nature of the Bureau.

Commodity Standards and a Reversal of Position

On December 17, 1962, Allen Astin, responding to a request from Assistant Secretary Hollomon, gave his views on the Bureau's role in engineering and commodity standards.[218] By "engineering standards" Astin meant "codes, specifications, and standards of quality or performance pertaining to technological materials or devices. A brief background review is desirable since my recommendations involve a reversal of recent trends." Astin pointed out that, spurred by then-Secretary of Commerce Herbert Hoover, the Bureau's involvement in such standards reached a peak in the mid-twenties. However, with the emergence of the American Standards Association (now the American National Standards Institute (ANSI)) in 1928, and a "firming-up of the concept that the development of engineering standards should be left primarily to private initiative," the Bureau's activities began to decline.[219] In the postwar period, a report by a committee headed by Charles E. Wilson, president of General Electric, recommended that the Government look to the private sector for engineering and commodity standards, and that the Bureau devote itself to data taking and test method development. In 1949, the GSA was created and given statutory responsibility for specifications and product evaluation for Federal purchases. The Bureau was put into a position of providing cooperation, not leadership. Its non-technical work, which was principally in providing procedures and a secretariat within which industry could develop standards (the same type of services provided by ASA and ASTM), was transferred to the DOC's Office of Science and Technology in 1950. The Bureau was no longer in charge of developing commodity standards.

But things did not work out well in this new arrangement. Many desirable standards had not been developed; ASA had trouble getting support from its member institutions; and U.S. participation in international standards activities was weak. The commodity standards work of the DOC declined in both quality and quantity.

---

[217] Annual Report, 1969: 3-4.

[218] Memorandum, A. V. Astin to J. H. Hollomon, "Engineering Standards," December 6, 1962. (NIST RHA; Director's Office file; Box 381; Folder Chrono 1962); Memorandum, J. H. Hollomon to A. V. Astin, "National Bureau of Standards and Commercial Standards," October 17, 1962. (DOC; Assistant Secretary for Science and Technology; Accession 40-70A-6988; Box 11; Folder October 1962)

[219] See MFP, 258, 260n, 370, 450 for a good history of these developments. An excellent account of the history of commodity standards extending to 1972 is in an unpublished report, "Voluntary Industry Standards," by Joan Koenig. (History Project File; Chapter 4; Folder Commodity Standards)

After giving this history, Astin noted that engineering standards needed to be developed in a technical environment, and because of their importance to the Nation, he recommended that the commodity standards program be transferred back to the Bureau, and that a competent man be found to operate it.

This decision to use scarce resources to get more involved in this highly applied activity was, in at least a small way, a reversal of the Bureau's postwar emphasis on basic science. As stated by William A. Wildhack, associate director for engineering, "The center of gravity for NBS must be closer to the forefront of science—to help extend the limits of knowledge and the competence of other laboratories by extending the precision of measurement and providing improved measurement techniques."[220] But Astin clearly saw a national need that was not being met. In July 1963, the commodity standards activity, with its head Alfred S. Best, was moved from the DOC back to the Bureau. First placed in the Polymers Division, upon the reorganization of the Bureau in 1964 it became the Office of Engineering Standards in the Institute for Applied Technology.

Weights and Measures

In the United States, the fixing of standards of weights and measures is a Constitutional responsibility of the Congress, which legislatively delegated it to the secretary of commerce and thence to the National Bureau of Standards. The actual control over weighing and measuring in the buying and selling of goods and services is, however, left to the states, which have the legal responsibility for enforcing those standards. From its earliest days, the Bureau has maintained an organizational unit to assist the states in carrying out this function and to provide for uniformity in weights and measures activities and laws among the states.

From 1958 to 1964, that unit was the Office of Weights and Measures (OWM). It operated a seven-part program to carry out its functions. It provided technical assistance to the states, business, and industry in the technical aspects of weights and measures; information on the design, construction and use of standards and associated measuring instruments; training of standards officials on technical matters; technical assistance with special measurement problems; help in developing laws and regulations; assistance with conference organization; and information dissemination on measurement units and systems.

Perhaps the best known of these activities was the provision of standards for mass, length, and volume to the states, along with instruction in their use. During the period, two new states joined the union, Alaska and Hawaii, both admitted in 1959. With suitable ceremony, sets of standards were provided for the new states. Research in this

---

[220] "Minutes of Meeting of NBS Advisory Committee on Engineering and Related Standards, May 18, 1962." (DOC; Assistant Secretary for Science and Technology; Accession 40-68B-6087; Box 28; Folder National Bureau of Standards)

Senator Edward L. Bartlett, Secretary of Commerce Lewis L. Strauss, Representative Ralph J. Rivers, and Senator Ernest Gruening inspect the measurement standards presented to Alaska at the National Bureau of Standards on March 3, 1959. The banner in the background commemorates National Weights and Measures Week.

area was directed at developing a new stainless-steel alloy suitable for the construction of new sets of standards for all the states. It was used for standard weights for Latin American countries under a program to provide those countries with sets of standards. Another advance in this area of physical standards was the design and construction of a new length-bench, along with yard and meter end standards, and appropriate stainless steel tapes. This new set would make available to the state laboratories for the first time sets of various precise-length measuring instruments. A continuing problem was the handling of pre-packaged goods, particularly aerosol containers. These required a great deal of instruction by the Bureau on how to determine the contents of such containers.

NBS designed and established a metrology training center for Latin American countries as part of the U.S. State Department's Agency for International Development program. Located at the National University, Bogota, Columbia, the laboratory was equipped with length, mass, and volume standards calibrated at NBS. At left, mass standards ranging from 1 mg to 30 kg; on the right, a 30 kg precision one-arm balance, developed for the laboratory.

In the provision of model legislation, completely new laws based on the model were enacted in Washington, New Mexico, and Missouri. For Mississippi, which in 1963 had no comprehensive weights and measures law, the Bureau carried out an intensive survey of the weights and measures activity in the state, and in March 1964, the state legislature enacted the model legislation. The model had, in fact, been developed by the National Conference on Weights and Measures, a yearly conference arranged by the Bureau and attended by most of the states, the District of Columbia, the territories, various foreign countries, equipment manufacturers and other interested parties. However, only state and local weights and measures officials are permitted to vote on issues at the conference. Others present are afforded a good way to communicate with the whole weights and measures community.

Training was another continuing function. Indeed, a laboratory for this purpose was constructed, and beginning in 1961 formal courses were given periodically. Other training occurred at the state institutions, and requests for training taxed the facilities and personnel available. The office was only a small part of the Bureau's activities but an important one in providing for uniformity in the Nation's measurement system.

In 1964, Governor Paul B. Johnson of Mississippi signed the state's first weights and measures law. The law was based on model legislation that had been provided to the state by NBS. The year before, the Bureau carried out an intensive survey of weights and measures conditions in Mississippi. (Courtesy of Mississippi Department of Agriculture and Commerce, Weights and Measures Division)

The International Yard and Pound; SI

Since it was established in 1901, the Bureau, following the action set in 1893 by its predecessor the Office of Weights and Measures of the Coast and Geodetic Survey, defined the yard and the pound from the meter and kilogram international standards respectively.[221] The values adopted by the Bureau's predecessor and thence by the Bureau were 3600/3937 meters for the yard, and 0.453 592 4277 kilograms for the pound. The former fraction is approximately 0.914 401 83. These values were consistently adopted by the nations using the English foot-pound system. In the intervening years, however, the various users had drifted apart, particularly in the later years, and some significant discrepancies appeared. After "study and negotiation, it has developed

---

[221] National Bureau of Standards, Notice, "Refinement of Values for the Yard and the Pound," *Federal Register* 24, no. 128 (1 July 1959): 5348, microfilm. The notice is signed by A. V. Astin, Director, National Bureau of Standards, H. Arnold Karo, Director, Coast and Geodetic Survey, and F. H. Mueller, Under Secretary of Commerce.

that most of the discrepancies can be resolved . . . " by only slight changes in the definitions. As a result, on July 1, 1959, the Bureau announced that the exact definitions

1 yard = 0.914 4 meter

and

1 pound = 0.453 592 37 kilogram

would be adopted. These units would be called the International Yard and the International Pound, and would be used by Australia, Canada, New Zealand, South Africa, and the United Kingdom. The new yard made the inch exactly 25.4 mm.

The new units caused only minor changes, for the new yard was smaller by 2 parts per million, and the new pound smaller by about 1 part in 10 million. Such changes were insignificant except for very precise determinations in the machine tool and instrument industries, in some scientific activities, and in surveying. As a result of the last fact, the old definition for the survey measure would continue to be used by the U.S. Coast and Geodetic Survey "until such a time as it becomes desirable and expedient to readjust the basic geodetic survey networks in the United States. . . . " By 1997 this had still not been done.[222]

***

The metric system is the basis for the international system of units. This system is not static, but evolves as science and technology progress. Thus, the International Committee for Weights and Measures (CIPM) was instructed by the 9th General Conference for Weights and Measures (CGPM) in 1948 "to study the establishment of a complete set of rules for units of measurement . . . and to make recommendations on the establishment of a *practical system of units of measurement. . . .* "[223] In 1954 the 10th CGPM adopted the units of mass, length, time, electric current, thermodynamic temperature, and luminous intensity as the six base units for the "practical system." Then, in 1960, the 11th CGPM adopted the whole practical system and the name "International System of Units," with the abbreviation SI (from the French Système International d'Unités), and thus was the SI system born.[224]

The system contains base units, supplementary units and derived units, with stipulation of names and abbreviations. The units of the six base quantities were well known: kilogram (kg), meter (m), second (s), ampere (A), kelvin (K), and candela (cd). Later, the unit for the amount of substance—the mole (mol)—was added, making seven base quantities. Added to these were two supplementary units: plane angle (radian; rad) and solid angle (steradian; sr). A third set was comprised of twenty-seven

---

[222] Barry N. Taylor, *Guide for the Use of the International System of Units (SI)*, Natl. Inst. Stand. Technol. (U.S.) Special Publication 811; April 1995; 2d printing January 1996: 43-44.

[223] *The International System of Units (SI)* ed. Chester H. Page and Paul Vigoreux, Natl. Bur. Stand. (U.S.) Special Publication 330; January 1971: 1. This translation was approved by the International Bureau of Weights and Measures of its publication "Le Système International d'Unités."

[224] Comptes Rendus des Séances de la Onzième Conférence Générale des Poids et Mesures, Paris, 14-20 Octobre, 1960: 65-70.

derived units, many of which have special names and are formed by combinations of the base units. Examples are area (m^2), velocity (m/s), force (newton; N, $kg \cdot m \cdot s^{-2}$), energy (joule; J, $N \cdot m$, $m^2 \cdot kg \cdot s^{-2}$), power (watt; W, J/s), and potential difference (volt, V, W/A). Also stipulated in the 1960 CGPM ruling were the names and abbreviations for powers of 10, ranging from pico (p) for 10^{-12} to tera (T) for 10^{12}. The conference also agreed upon rules and style conventions for printing that were designed to bring order to scientific notation.

The number of base units has remained at seven but the number of derived units has grown. The 1995 edition of SP 330, the official U.S. document on the SI system, lists fifty-four derived units which form the "coherent system of SI units." Eleven derived units have no special names (e.g., current density, A/m^2) and are expressed in terms of the base units; twenty-one derived units have special names and symbols (e.g., frequency, hertz, Hz, s^{-1}; pressure, pascal, Pa, N/m^2); and twenty-two derived units are expressed by means of SI derived units with special names (e.g., surface tension, newton per meter, N/m, kg/s^2). The two supplementary units, the plane angle (radian, rad, $m \cdot m^{-1} = 1$) and the solid angle (steradian, sr, $m^2 \cdot m^{-2} = 1$), were eliminated as a separate group and are now included with the other derived units with special names and symbols. The powers of 10 were expanded to 10^{-24} (yocto, y) and 10^{24} (yotta, Y).[225]

The seeming complexity of the SI system is in the details. In fact, it has brought considerable order to the expression of quantities in the physical sciences as well as engineering and chemistry.

A TOUR OF THE DIVISIONS

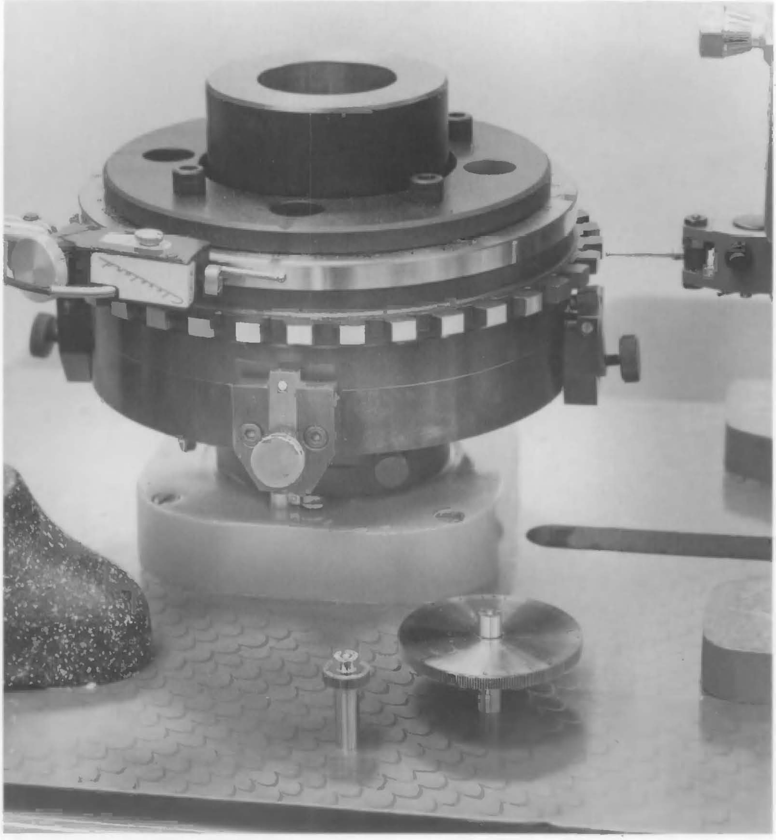
Optics and Metrology

The big news in the work of this division was in basic and engineering length standards, as already detailed in the section on Standards Matters. But in a related activity in response to industry needs as expressed in a series of conferences held at the request of the Aerospace Industries Association, the Bureau established a Gear Metrology Laboratory in the Metrology Division in 1962. In work designed to standardize gear measurement, equipment was acquired for calibrating involute profile masters, concentricity, and spacing of gear teeth, much of it on loan from industry and interested Government agencies. It was believed to be the only laboratory of its kind in the world.

The division also had responsibility for photometric standards, and in 1958 entered into a program proposed by the BIPM for international adoption of uniform units of luminous intensity and luminous flux for the carbon-filament lamp, the vacuum-tungsten lamp, and the gasfilled-tungsten lamp. The units would be based on their average magnitudes found by applying the Bureau-developed primary standard of a blackbody at the freezing point of platinum. Because of the difficulty of operating

---

[225] Taylor, *Guide for the Use of the International System of Units (SI)*: iv, 3-8.



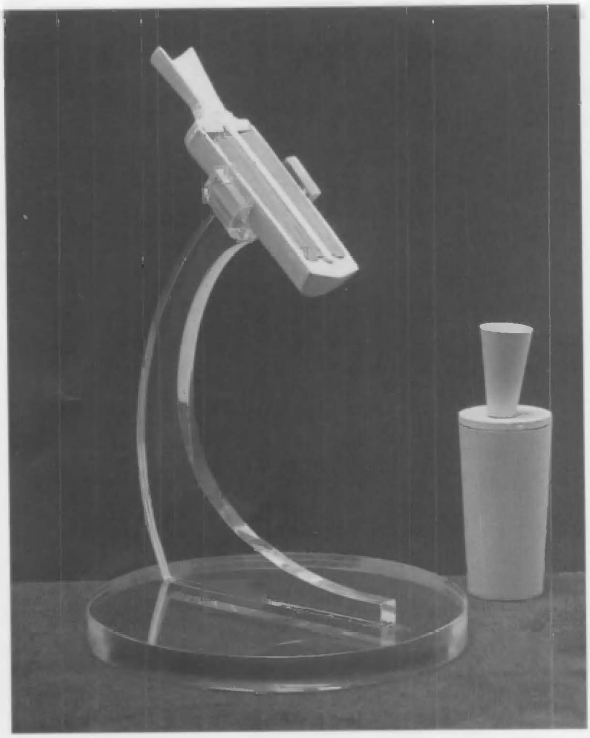
A master index plate in position for calibration in the Gear Metrology Laboratory at NBS. This plate was used, in turn, to calibrate industrial instruments that checked gear indexing and deviation from concentricity. The two master gears in the foreground could also be calibrated with the equipment shown.

a high-temperature blackbody standard and of assigning uncertainties to it, it was never used for calibrations at the Bureau or anywhere else. Nevertheless, it formed an important conceptual step in relating photometric units to the base units of the measurement system.[226]

Other activities carried out in the division included color measurement and standards, mass calibrations (transferred in 1960 from the Mechanics Division), various aspects of aircraft and landing field lighting (all on transferred funds), lens design, and research in various parts of the photographic process.

---

[226] Bruce W. Steiner, private communication.



In 1908 Charles W. Waidner and George K. Burgess suggested the use of a blackbody (one which completely absorbs all radiation incident upon it), totally immersed in a bath of freezing platinum, as a primary standard of luminance. This was experimentally realized 20 years later using fused thoria placed at the bottom of a sight tube. Molten platinum was contained in a thoria crucible. By 1933 the International Committee of Weights and Measures had accepted both the blackbody concept and the freezing platinum reference. This model of the Waidner-Burgess standard of light shows in cross-section the thoria crucible, metal in the space occupied by the freezing platinum, and a thoria tube (the blackbody) in the center.

Mechanics

Charged with the custody of the measurement standards for mechanical quantities, the work of the Mechanics Division needed to develop measurement techniques and standards for sound pressure and intensity, shock, vibration, force, strain, pressure, volume, and rate of flow. It provided services for the calibration of measuring instruments for these quantities.

The most notable accomplishments during the period were in pressure and force standards. Due to the urgency of calibrating large force-measuring devices essential for the Nation's space and military programs, it was the first division to move to the Gaithersburg site. In the main, the program for the present period was a continuation of work previously discussed. Here we give only a few noteworthy advances to give a flavor of the activities.

With the Nation very much concerned with jet engines, it is not surprising that work continued in this area. One of the concerns was the measurement of temperature in the jet engine. Work throughout the period on thermocouples of various kinds led to the 1964 publication of reference tables for several types (primarily 60 percent iridium versus 40 percent rhodium-iridium alloy of several compositions) that covered the temperature range from –100 °C to 2150 °C.[227]

---

[227] L. O. Olsen and P. D. Freeze, "Reference Tables for the Platinel II Thermocouple," *Journal of Research of the National Bureau of Standards* 68C (1964): 263-281; G. F. Blackburn and F. R. Caldwell, "Reference Tables for Thermocouples of Iridium-Rhodium Alloys Versus Iridium," *Journal of Research of the National Bureau of Standards* 68C (1964): 41-59.

In work more in keeping with its scientific disciplines, the division maintained reference flow-rate facilities for the calibration of devices that metered the flow of fuel to jet engines. Fuel metering must be very precise for optimum engine performance, and metering components require precise adjustment and test prior to installation. First operated under the sponsorship of the Navy Bureau of Aeronautics, sponsorship of these facilities shifted to the Bureau of Naval Weapons in 1963. It was an arrangement similar to that of the military in the Electronic Calibration Center.

A continuing area of study throughout the whole period was infrasonic waves in the atmosphere. Practically always present, these waves have periods ranging from one second to 30 seconds and are approximately the same intensity as speech, but of course, they cannot be heard. Some types were correlated with tornadoes more than 1000 miles distant, with earthquakes, and with magnetic storms.[228] Others, with periods of about 6 seconds, arrived from the east and were thought to be caused by waves breaking on the Atlantic shore. The infrasonic waves were complementary to seismic waves discovered in the study of earthquakes.

In the large reorganization of 1960, the Heat Division's Rheology Section was transferred to the Mechanics Division, where it found a more natural home. The section was concerned with the properties of viscoelastic substances—polymers are the prototypical materials—which it studied by both experimental and theoretical work. Concerned with the explanation of the results of stress-relaxation experiments at high extensions, three of the investigators formulated a nonlinear theory of viscoelastic substances which worked remarkably well.[229] It was a significant advance in a recondite area.

The Electricity Division Stimulates Polymer Science

One of the divisions most concerned with basic measurement standards and calibrations was the Electricity Division, which started the period as Electricity and Electronics, but reverted to Electricity upon transferring personnel to the Instrumentation Division. During the period it continued its work on basic measurement standards, notably undertaking another absolute determination of the ampere occasioned by the measurement of the gyromagnetic ratio of the proton. Equally notable was the determination by NBS of the unit of electrical resistance based on an approximately 1 picofarad capacitor invented by A. M. Thompson of the Australian National Measurement Laboratory. The capacitance of this unit could be calculated accurately simply from its dimensions. This capacitor was used to establish the value of a 0.01 microfarad capacitor which, when used with a frequency-dependent bridge, established the value of a 10 000 ohm resistor. When compared with the Bureau's prevailing 1 ohm standard resistors, the results showed that the standard resistors used to maintain the NBS unit of resistance did so with an estimated accuracy of 2 parts per million. Another effort

---

[228] "Unusual Recordings of Infrasonic Disturbances in the Atmosphere," *Technical News Bulletin* 44 (1960): 206-208.

[229] B. Bernstein, E. A. Kearsley, and L. J. Zapas, "A Study of Stress Relaxation with Finite Strain," *Transactions of the Society of Rheology* 7 (1963): 391-410.

was the redetermination of the Faraday constant by the electrolytic dissolution of metallic silver. The new determination gave a value of 96516.5 ± 2.1 coulombs per gram-equivalent weight on the physical scale, better by 0.008 percent than the previous value.

In a joint venture with the Atomic Physics Division, the measurement of another fundamental constant was carried out when Raymond L. Driscoll of Electricity and Peter L. Bender of Atomic Physics made an absolute determination of the gyromagnetic ratio of the proton. This quantity is important both for practical reasons and for basic nuclear physics. Since it is the constant of proportionality that connects the precession frequency of the proton to the value of the magnetic field in which it is placed, its knowledge reduces the determination of magnetic field strength to the measurement of a frequency, and since protons are readily available in water, the experimental material is always at hand. On a basic-science level, theory gives the quantity as the ratio of the magnetic moment of the nucleus to its angular momentum.

Raymond L. Driscoll with the apparatus he and Peter L. Bender used to redetermine the gyromagnetic ratio of the proton in 1958. The solenoid (center) whose magnetic field could be precisely calculated from its known dimensions, caused the protons in a water sample to precess with an accurately measurable frequency. The proton gyromagnetic ratio was then calculated from the precession frequency and the precisely known field strength. The large coils which surrounded the apparatus counterbalanced the earth's magnetic field.

Thus the knowledge of the gyromagnetic ratio in absolute units for the simplest nucleus, the proton, permits the determination of the magnetic moment of any other nucleus, provided only that its spin is known. Moreover, the gyromagnetic ratio is given by an expression containing only fundamental constants, so its knowledge and accurate measure is a check on their values.

While the Bureau was not the first to measure the gyromagnetic ratio of the proton, it did carry out a determination in 1950,[230] but an iron-core magnet had to be used because measurements could not, at that time, be made at low field strengths. As a result the uncertain measurement of the magnetic field of the magnet somewhat degraded the accuracy. In the intervening years, however, science had progressed to the point where measurements at low fields could be made. Hence Driscoll and Bender joined forces to make another absolute measurement, this time using as a source of magnetic field the same solenoid that had been used by Driscoll for the realization of the absolute ampere; it provided the most accurate and best-known field available. The value obtained was only slightly smaller than the previous measurement, $(2.67513 \pm 0.00002) \times 10^4$ gauss^{-1} sec^{-1} as compared to $(2.67523 \pm 0.00006) \times 10^4$ gauss^{-1} sec^{-1}.[231] With the value related directly to the fundamental unit of current, it was an experiment that only the Bureau could carry out. Basic science had a new, more accurate fundamental quantity whose value, interacting with other fundamental quantities would improve all of them. Driscoll and Bender received the first Stratton Award for their work.

But perhaps more than any other, the division's activities in the study of magnetic and dielectric properties of matter led to new directions for the division and the Bureau. The former study developed almost into atomic physics when investigations were begun on the nuclear magnetic resonance of various ferromagnetic alloys. The latter study, however, was to have an enormous effect on materials science both at the Bureau and in the scientific community at large.

It began with the hiring of John D. Hoffman, whose father, James I. Hoffman, a renowned analytical chemist, had been a senior scientist in the Bureau's Chemistry Division for many years. An expert in the dielectric properties of the normal paraffins, the younger Hoffman was hired in 1954 to begin a study of dielectric materials in the Polymer Structure Section of the Organic and Fibrous Materials Division. He assembled a group in the Polymer Structure Section and began an intensive investigation of polymers as prototype dielectric materials. In 1956, in order to consolidate in a single group much of the research on dielectric materials and associated standardization work at frequencies below 30 kHz, Hoffman's group was moved to the Electricity Division as the Dielectric Section. The first polymer chosen for study was polychlorotrifluoroethylene (PCTFE) because it had a dipole moment at right angles to the polymer chain, but no side groups capable of reorientation independent of the backbone polymer chain. Thus, any observed relaxation processes could be unambiguously

---

[230] H. A. Thomas, R. L. Driscoll and J. A. Hipple, "Measurement of the Proton Moment in Absolute Units," *Physical Review* 78 (1950): 787-790.

[231] R. L. Driscoll and P. L. Bender, "Proton Gyromagnetic Ratio," *Physical Review Letters* 1 (1958): 413-414.

attributed to motions of the polymer chain itself. These ideas led to a thorough study of this polymer, for which data over the temperature range from –50 °C to +250 °C and the frequency range from 0.1 Hz to 8.6 GHz were obtained.[232]

The most interesting aspect of the study—and the one that led to interest from a community far broader than that simply concerned with dielectrics—is the fact the PCTFE is a crystalline polymer and, like practically all such polymers, is a mixture of amorphous and crystalline regions, with the same chain generally existing in both phases. By applying appropriate quenching techniques during sample preparation, the degree of crystallinity could be varied from essentially pure amorphous to about 80 percent crystalline. It was the attempt to relate the bulk properties of the polymer to this variable degree of crystallinity that led the study beyond purely dielectrics and into broader aspects of materials science.

To understand how this came about, a few words have to be said about other events in polymer science at the time. In 1957, Andrew Keller at Bristol University discovered the then amazing fact that, in crystallization from dilute solution, single crystals of polymers could be formed. They were strange objects indeed: thin lamellae of the order of 10 nm in thickness, but with lateral dimensions of several micrometers. But most astounding was the fact that the chain axis appeared to be approximately normal to the surfaces of the lamellae, and since the polymer chains were several hundreds of nanometers long, this meant that the polymer chains folded back on themselves at the broad surfaces of the lamellae. A crucial result was the demonstration that the thickness of the lamellae depended on the crystallization temperature: the higher the temperature, the thicker the lamellae.

An important problem Hoffman and his associate James J. Weeks addressed concerned the melting of the bulk polymer. It was known that the melting point as determined, for example, by volume change, increased as the rate of heating decreased, but no limit seemed to be approached. And, for polyethylene, the observed melting point was much lower than the melting point of the n-paraffins extrapolated to very long chain lengths. Hoffman and Weeks, adopting a model that the crystalline regions of the polymer in the solid state also consisted of lamellae, reasoned that such thin crystals would have a melting point that depended on their thickness, hence the crystallization temperature, and that this melting point would be much lower than that of a bulk crystal composed of fully extended polymers chains. Following this reasoning, they produced samples of polymers crystallized at different temperatures.[233] They determined the melting point of these samples under rapid heating since polymers require a substantial undercooling in order to crystallize (an easy task under the polarizing microscope since the polymer crystals are highly birefringent). This procedure

---

[232] A. H. Scott, D. J. Scheiber, A. J. Curtis, J. I. Lauritzen, Jr., and J. D. Hoffman, "Dielectric Properties of Semicrystalline Polychlorotrifluoroethylene," *Journal of Research of the National Bureau of Standards* 66A (1962): 269-305.

[233] J. D. Hoffman and J. J. Weeks, "Melting Process and the Equilibrium Melting Temperature of Polychlorotrifluoroethylene," *Journal of Research of the National Bureau of Standards* 66A (1962): 13-28.

precluded structural rearrangement as much as possible. They then plotted these observed melting points against the crystallization temperatures. This plot gave them a straight line with positive slope, which they extrapolated to the (unobservable) line for which the melting point and the crystallization temperature were equal. This gave them the equilibrium melting point: a value of 224 °C, compared to 218.2 °C for the highest melting point obtained directly. This procedure was widely copied and is now the standard means for determining polymer melting points.

Important as that development was, it was by no means the most important one for polymer science. In a purely theoretical study, Hoffman and John I. Lauritzen, Jr., his long-term associate, proposed a theory of polymer crystallization.[234] Far too complex to record in detail here, suffice it to say that it was a nucleation theory. In this model, the lamellae grow laterally by deposition along the lateral edges of the crystal. The nucleation barrier is the deposition of a straight segment of polymer chain (called a "stem" in the argot of the science) on the lateral edge of the crystal, followed by the subsequent folding of the chain at the surface of the lamella. These processes increase the free energy of the crystal and form a barrier to crystallization. More of the chain then deposits adjacent to the first-laid stem, folds at the other lamella surface, and continues the process. Each of these subsequent events lowers the free energy of the crystal, and the rate constant for each of these steps can be determined by standard kinetic theory. The full set of kinetic equations is easily written and, with somewhat more effort, solved. The rate of growth is obtained, with the lamella thickness as a parameter. An ensemble averaging of the growth process yields a value for the thickness, and this value can be interpreted as the optimum thickness for the most rapid growth. Both the growth rate and the thickness are given by the theory as functions of the undercooling: the growth rate as a negative exponential of the reciprocal of the undercooling, and the thickness as inversely dependent on it. This theory and its subsequent elaborations stimulated an immense amount of theoretical and experimental activity in the field of polymer science. Its predictions, with subsequent clarifications, proved largely correct. It has become one of the mainstays of polymer science, and its concepts are still the subjects of investigation.

In June 1964, with the retirement of Gordon Kline, chief of the Polymers Division, Hoffman assumed that position. His dielectrics/polymer-science group also was moved to the Polymers Division. Thus, in carrying out research on its basic measurement mission, the Electricity Division stimulated the birth of a new branch of polymer science.

---

[234] J. I. Lauritzen, Jr., and J. D. Hoffman, "Theory of Formation of Polymer Crystals With Folded Chains in Dilute Solution," *Journal of Research of the National Bureau of Standards* 64A (1960): 73-102. Two other theoretical treatments of this phenomenon followed the Lauritzen-Hoffman paper: F. P Price gave a quasi-equilibrium treatment in "Markoff Chain Model for Growth of Polymer Single Crystals," *Journal of Chemical Physics* 35 (1961): 1884-1892, and F. C. Frank and M. Tosi in "On the Theory of Polymer Crystallization," *Proceedings of the Royal Society (London)* A263 (1961): 323-339 presented a variation of the Lauritzen-Hoffman work. The latter paper gives an elegant solution of the kinetic equations and an important discussion of the rate constants.

Radio Standards

The most far-reaching activity of the Radio Standards Division was the development of the atomic frequency standards discussed earlier in this chapter. But the division (later a Laboratory with two divisions) was concerned with a plethora of other standards as well.[235] There were standards, measurement methods, and calibrations for attenuation, impedance, power, voltage, current, noise, field strength, pulses, and phase shifts. The ever-expanding frequency range made certain that the division did not run out of urgent work. In addition, there was research on materials of importance to high-frequency radio up to millimeter waves at about 100 GHz. While still assiduously developing measurement methods and calibrations, the background work became ever more basic and fundamental. In 1962 the division was split into two: Radio Standards Physics, where most of the background research was carried out, and Radio Standards Engineering, where the standards and calibrations work was done.

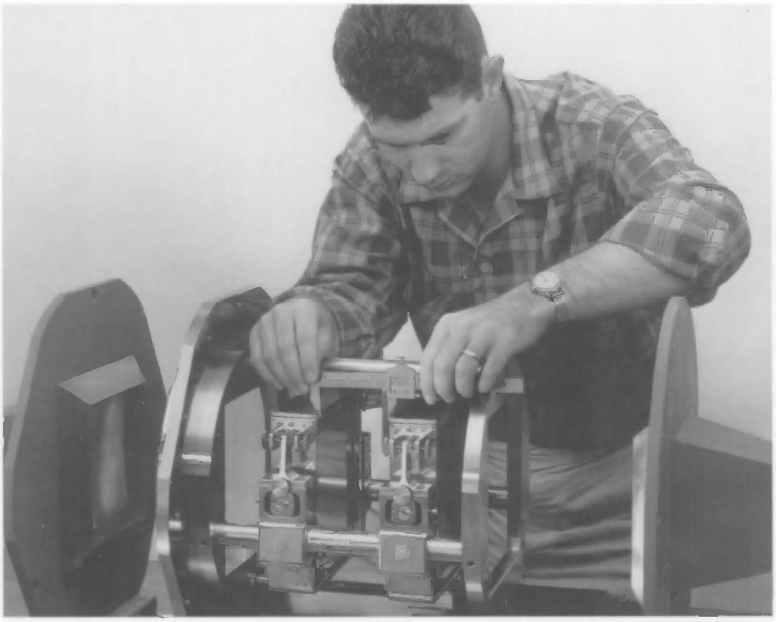
A Bureau scientist makes attenuator and linearity calibrations on a high-frequency field-strength meter.

---

[235] Snyder and Bragaw, *Achievement in Radio*: 360-402, 619-622.

Much of the work was concerned with microwave frequencies and millimeter waves. The latter formed a relatively unexplored region of the electromagnetic spectrum. With such short wavelengths, optical techniques were applicable, and both Michelson and Fabry-Perot interferometers were built for use in the millimeter-wave range. The latter instrument used ingenious reflecting plates made from highly polished metal or silvered glass flats with accurately spaced and dimensioned holes. In radio parlance, the reflecting plates, placed face-to-face as in a Fabry-Perot interferometer, formed a resonant cavity which, for a given wavelength, could be brought into resonance when the spacing between the plates corresponded to an integral number of half wavelengths. At this point the output through the holes is a maximum. Thus the device could be used both as a source of millimeter waves and as a means of measuring their wavelength. In fact, this was done with a relative uncertainty of 0.04 percent.

With a new means of measuring wavelength, and a world-leading capability in measuring frequency, it was only natural that a redetermination of two fundamental constants be made. The speed of electromagnetic radiation (the same as the speed of light) was determined utilizing the Michelson, rather than the Fabry-Perot, interferometer. Using 6 mm waves and a klystron of very well-known frequency, the new determination produced no surprises. Another measurement was made using gamma rays and the Mössbauer effect. And in a microwave study of the spectrum of singly ionized helium, the fine-structure constant was determined.



A Fabry-Perot interferometer, designed to operate at 3 mm to 4 mm, was used as the cavity resonator for a hydrogen cyanide gaseous maser. G. L. Strine was photographed in 1961 adjusting the parallelism of the perforated reflectors.

In an investigation that was not directed at a fundamental constant, but borrowed from spectroscopy, the Fabry-Perot millimeter wave interferometer was used to measure the Stark shift in a rotational transition in methyl cyanide at a frequency of 37 GHz. This led to a method for the measurement of high voltages with a high precision of 1 part in 10^8, albeit with a relative uncertainty of 1 part in 10^4.

There were other areas of basic research. Plasmas and their interaction with radiation were studied as part of the Bureau's plasma program. Work in materials was "primarily directed toward advancing the present understanding of solid state phenomena as well as toward improving and developing standards and measuring techniques for determining material properties."[236] In 1963 work on magnetics was concerned with the change in elastic modulus of ferrites upon magnetization. Other efforts included time dependent magnetization effects; improvement in permeability measurement technologies; shape effects in the determination of gyromagnetic ratios; studies of ferrimagnetic resonance and the losses due to spin waves; and the investigation of electron paramagnetic resonance in iron-doped synthetic quartz.

In dielectrics, the work was more directly concerned with standards. An international comparison with Canada and England of the dielectric constant of fused silica, glass, and alumina showed close agreement for the real part, but a disagreement on the imaginary part, which is much more difficult to measure accurately. Nevertheless the results were used to improve knowledge of the dielectric constants of glass and silica, and standard samples were made available.

Along with efforts in applied mathematics and numerical analysis, the division conducted research in mathematical physics—almost a small version of the applied mathematics division. The studies were viewed as end products in themselves, as well as providing theoretical foundations on which continued work could be based and providing the background for mathematical and physics consultation. One example of the work was the application of a new theory on the quantum statistics of multi-component systems to the calculations of the properties of a plasma model which was far more realistic than the familiar electron-gas model.

Lest we leave the impression that the division failed to carry out its main mission of providing standards and calibrations in the radio field, we list four topics from the 1962 report:

1. The provision of a method for the measurement of pulse power. With a range from 0.25 W to 10 kW, a limit of error of ±3 percent, pulse widths as small as 5 μs, and a frequency range of 3 KHz to 1 GHz, the method formed the basis for a new calibration service.

2. Radio-frequency voltage-calibration services were expanded to include 500 MHz, 700 MHz, and 1000 MHz.

3. The high-frequency impedance calibration service was improved by the addition of new instrumentation and hardware.

---

[236] Annual Report, 1962: 51.

4. The calibration uncertainty of attenuators for field strength meters was reduced from ±0.8 db to ±0.2 db over the frequency range of 400 MHz to 1000 MHz, and the attenuation range was increased to 150 db.

Heat

With responsibility for the temperature scale, the Heat Division was deeply involved in basic thermometry research, with activities in extending the range of temperature attainment and measurement. In addition, the division did a great deal on the acquisition, analysis, and methods for the measurement of thermodynamic data. During the period its work became more basic, and it remained one of the leading scientific divisions in the Bureau.

Perhaps the activity that best illustrates the basic nature of its program is statistical thermodynamics.[237] Throughout the whole period there was continuous work on the development of calculation programs for the extension of thermodynamic data to an ever-wider range of temperature and density. But the effort was deeper than one limited to methods of calculation. Plasmas were as important theoretically as they were experimentally in the attainment of high temperatures, with studies of the equilibrium behavior of a fully ionized plasma in a magnetic field, and the necessary modifications of standard treatments of high-temperature gases to take into account the presence of long-range coulomb interactions. Theoretical calculations of dense gases were a continuing interest, with an analysis of the underlying assumptions made in traditional calculations. A two-year calculation of the pair distribution function in dense gases and liquids was carried out, but the application to a hard-sphere model showed no evidence of a gas-liquid phase transition—that nirvana of the theory of liquids. Continued work in this field would make the Bureau a center of statistical thermodynamics research.

Two programs illustrate that this emphasis on more basic work did not lessen the Bureau's concern with national problems. Both were concerned with the space race and military considerations and both were supported by the military, but now the work was basic research, not ordnance development as it had primarily been in pre-AD-X2 days.

One program was concerned with data on thermodynamic properties. In 1959 the recently formed Advanced Research Projects Agency (ARPA) of the DOD began to support the Bureau for a program to determine the thermodynamic properties of the light elements and their compounds, using the argument that "one of the most effective means of developing more efficient chemically propelled rockets is to select particular fuel materials which generate more power per unit mass and lose less power in the exhaust products." In principle, such fuel materials should be composed of light elements and their compounds, but more data were required before selection for top performance could be made.[238] The resulting effort was an interdivisional, multi-disciplinary program coordinated by Charles W. Beckett of the Heat Division. Thermodynamic data on lithium, beryllium, aluminum, and zirconium, and their compounds

---

[237] Annual Report, 1958: 21-22.

[238] Annual Report, 1960: 53-54.

with hydrogen, oxygen, fluorine, and chlorine, were determined and disseminated. Many properties were ascertained. For example, the entropy of lithium aluminum hydride by low-temperature thermal measurements was determined to help in predicting under what conditions this new compound would form or decompose, and the very-high-temperature aluminum-oxygen gaseous system was investigated. But most important were heats of formation. Measurements accurately determined these heats for the alkali-metal perchlorates and ammonium perchlorate, an oxidizer used in many solid-fueled rockets. Similar data were obtained on compounds of beryllium, aluminum, and lithium with oxygen, fluorine, and chlorine. That none of these exotic compounds achieved the status of rocket fuels illustrates on the one hand the tenor of the times in the military space race and, on the other, the various by-ways that need to be explored in a crucial research effort.

Far better known, and with important effects on the Bureau's development, was a complementary, but considerably more exploratory, program on free radicals.

A Program on Free Radicals

In 1954, Herbert P. Broida, physicist in the Temperature Measurement Section of the Heat and Power Division, and John R. Pellam, chief of the Cryogenics Section of the same division, published a paper on certain new and strange phenomena they had observed.[239] They had passed a stream of nitrogen through an electrical discharge tube and had frozen the resulting gas on the walls of a chamber kept at the temperature of liquid helium. They were surprised to see that the resulting deposit gave off a strong green glow and, occasionally, small patches erupted with a blue flash, like a miniature explosion. They hypothesized that during the discharge, nitrogen atoms—the most elementary form of free radicals—had formed and then became trapped in the solid molecular nitrogen frozen on the chamber walls. The green glow and blue flashes must be connected to the presence of these atoms. The green glow persisted for several minutes after the discharge tube was turned off. The experiments were continued with hydrogen, oxygen, and water vapor and led to similar conclusions about the coexistence of radicals and molecules at these low temperatures.

The interest in rocketry, hence energetic fuels, in the mid-fifties had made free radicals into subjects of interest.[240] Calculations showed that radicals of such gases as hydrogen, nitrogen, and oxygen would far surpass any other fuels for rocketry in terms of the immense energy released upon their recombination. Therefore, despite the difficulty in forming and storing them, they were under active study. The great importance of the Broida-Pellam result was that it indicated a possible means of storing atomic free radicals, i.e., of stabilizing them, something that was essential before they could be used.

---

[239] H. P. Broida and J. R. Pellam, "Phosphorescence of Atoms and Molecules of Solid Nitrogen at 4.2 °K," *Physical Review* 95 (1954): 845-846.

[240] Maurice W. Windsor, "Trapped Radicals in Propulsion," in *Formation and Trapping of Free Radicals*, Arnold M. Bass and Herbert P. Broida, eds. (New York: Academic Press, 1960), 387-409.

With advice from technical committees, and with the results of the Broida-Pellam experiment, the Department of Defense—specifically the Army Ordnance Department, with its long history of support of work at the Bureau—in September 1956 began a national program on the study of free radicals, concentrating on means of stabilizing them. The program involved increased support of existing work at industrial and university laboratories and the designation of the Bureau as a central research laboratory, due to its experience and its wide variety of staff and facilities.[241] A special section, called Free Radicals Research, was formed for the program and Broida was named its chief, with spectroscopist Arnold M. Bass as assistant chief. Work was also carried out in other sections where there was relevant expertise.

From the start there were unique aspects to the program. There were thirty to thirty-five senior scientists, about half of them from—and supported by—industry, eight from universities (six of which were foreign institutions), and the remainder from the Bureau. Three American universities provided consultants. Most interesting was the method of choosing projects. Illustrating the support of basic research and the scientific freedom of the times—and the personality and philosophy of Broida—the senior scientists were given complete freedom in their research activities, although lack of results would lead to a conversation with Broida. The style was not direction from above, but choice from below. All the work was done at the Bureau and the maximum effort took place in the latter half of 1958, when sixty-nine persons worked on the program. Designed as a three-year program, work began in September 1956 and ended in July 1959.

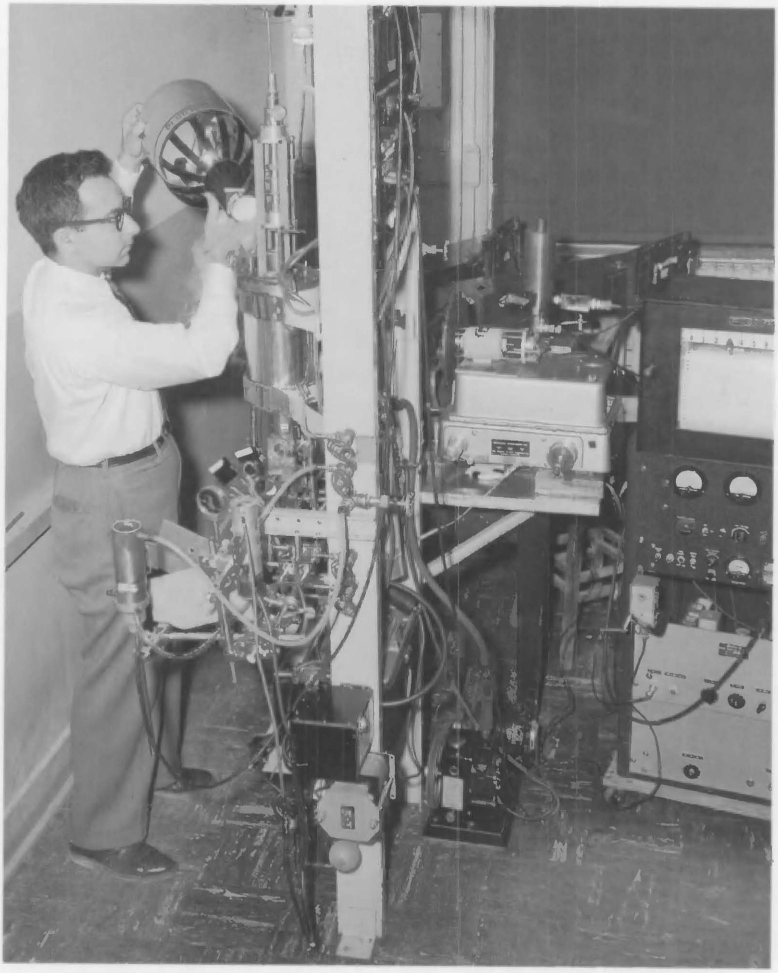
Cohesiveness in this free atmosphere was provided by almost constant interaction among specialists from all relevant disciplines at coffee breaks, luncheons, and weekly seminars. Many members of the Bureau staff not directly associated with the program were active participants in these discussions. Guests and visitors were welcome and attended in abundance. A bimonthly newsletter was instituted. It began with a circulation of 90 and ended with 550. The program attracted important and capable scientists, a number of whom subsequently joined the Bureau staff and achieved leadership positions. In Madison Avenue parlance, it provided advertising for the Bureau that could not have been purchased.

At the end of its three-year life, the program ended; the Free Radicals Section was abolished and the guest workers went home to their own laboratories. But the work did not cease; it continued in the home laboratories of the scientists, just the way Army Ordnance had designed the program.

---

[241] James W. Moyer, "Organization and Rationale of the NBS Free Radicals Program," in *Stabilization of Free Radicals at Low Temperatures*, A. M. Bass and H. P. Broida, eds., Natl. Bur. Stand. (U.S.) Monograph 12; August 1960: 1-4. This volume contains papers that give an excellent account of the scientific results of the program. Hereafter it will be referred to as Monograph 12. *Administrative Bulletin* 56-66, October 29, 1956, also gives an account of the rationale and organization of the program.

There was a very substantial scientific output from the program, although no "breakthrough" in rocket fuels. At the time the program was closed, eighty-nine scientific papers had appeared in the scientific literature or were in preparation.[242] With the freedom given to the senior scientists for project choice, it is not surprising to find



View of the equipment used at NBS in the low-temperature stabilization of free radicals. Arnold M. Bass added liquid nitrogen to the outer Dewar of the cryostat in which the free radicals were frozen into solid form. The spectroscopic equipment at right was used to study and analyze the visible and ultraviolet radiation given off by the frozen atoms.

---

[242] H. P. Broida, "La Recherche sur les Radicaux Libres au National Bureau of Standards," *Journal de Chimie Physique* 56 (1959): 813-816.

that many scientific areas and methods were explored.[243] Thus, in the production of radicals, besides the original method of electric discharge pioneered by Broida and Pellam, thermal techniques, electron impact, photolysis, and gamma radiation were explored. And again, while nitrogen, oxygen, hydrogen, and water vapor were the most intensively studied substances, a number of others were investigated, including diborane, ammonia, ozone, hydrogen peroxide, methane, hydrazoic acid, and hydroxylamine.

The large number of investigators on the program provided specialized knowledge in an abundance of disciplines, so that a whole litany of techniques in various scientific areas were brought to bear on the studies. Spectroscopy was an important area, with a number of studies in absorption and emission spectra. X-ray diffraction studies on various radical-containing solids at the temperature of liquid helium were carried out by Bureau specialists, necessitating the development of specialized equipment in the process. Thermometric energy-release studies were carried out on a number of species and for a variety of processes. A number of studies of chemical reactions and their kinetics were performed. The uniquely applicable technique of electron spin resonance was applied with illuminating results, particularly with respect to the demonstration that the ground state of nitrogen atoms in the matrix of nitrogen molecules is stabilized at the low temperatures used. Theoretical studies formed a significant part of the program.[244] They were concerned with the deposition of the radical-molecule mixture in the gas on to the cold substrate, with the kinetics of the process, with the emission and absorption of energy, with the nature of the forces and structures that trap atoms in the lattice, with the thermodynamics of radicals, and with the mechanisms and kinetics of the disappearance of trapped radicals. Not the least of the results of the program were the development of methods for handling large quantities of liquid helium, the development of new designs of dewars and their commercial availability, and the development of more specialized techniques.

Thus, while no radical rocket propellant ever reached the stage of contemplation, the science of molecular stability was given a significant boost, the utility of low temperatures in research was made known beyond the confines of "classical" low temperature physics, and the Bureau substantially enhanced its reputation in basic experimental and theoretical chemistry.

Atomic and Molecular Physics

As one of the Bureau's elite scientific units, the Atomic Physics Division continued its work on various aspects of spectroscopy, electron physics, solid state physics, and other more specialized topics, including the determination of the gyromagnetic ratio of

---

[243] Milton D. Scheer, "Experimental Aspects of the National Bureau of Standards Free Radicals Program," in *Stabilization of Free Radicals at Low Temperature*, A. M. Bass and H. P. Broida, eds., Natl. Bur. Stand. (U.S.) Monograph 12; August 1960: 7-20.

[244] C. M. Herzfeld, "A Survey of Theoretical Work on Trapped Radicals at the National Bureau of Standards," in *Stabilization of Free Radicals at Low Temperature*, A. M. Bass and H. P. Broida, eds., Natl. Bur. Stand. (U.S.) Monograph 12; August 1960: 21-36.

the proton (described under the Electricity Division), the precision determination of the Rydberg constant,[245] and work on a portable rubidium-based atomic-frequency standard. From its enormous outpouring of scientific work, we select three topics to give a flavor of the division's work: laboratory astrophysics, synchrotron radiation, and electron physics.

Not surprising for the birthplace of JILA, the division carried out a large effort related to astrophysics. Indeed, astrophysics was one of the main applications of the work done on atomic energy levels, transition probabilities and collision cross-sections. In 1957, at the very beginning of the period, a High Altitude Observatory was established on Mauna Loa in Hawaii, where the oxygen content of the Martian atmosphere was measured and the presence of molecular hydrogen in the atmosphere of Jupiter was verified.

Practically all the work was laboratory based and concerned with spectroscopy and related studies—hardly surprising considering the division's history. Thus, one of the areas of study was in low-energy collision cross sections for "selected processes of critical importance to astrophysics and plasma physics." This work was concerned with electron collisions with neutral atoms and negative ions, a process thought to "play a leading role in determining properties of the solar atmosphere."[246] The photo-detachment of electrons from negative ions, particularly H^-, O^-, and C^-, was featured. The experimental work was buttressed with theoretical studies.

The work in atomic spectroscopy, where NBS had held a predominant position for many years, was centered on the rare earth elements. The three volumes of Charlotte Moore Sitterly's widely used compilation, *Atomic Energy Levels*, which were published in 1949-1958, covered all the periodic table except for the lanthanides (rare earths) and actinides.[247] Wavelength measurements of spectral lines from improved light sources and the use of powerful new computer methods were necessary for successful energy-level analyses of some of the most complex rare-earth spectra. Such analyses of several of these spectra were first successfully carried out at NBS by the early 1960s, and work on these elements continued until the 1978 publication of the compilation *Atomic Energy Levels—The Rare-Earth Elements.*[248]

---

[245] W. C. Martin, "Value of the Rydberg Constant," *Physical Review* 116, no. 3 (1959): 654.

[246] Annual Report, 1960: 61.

[247] Charlotte E. Moore, *Atomic Energy Levels As Derived from the Analysis of Optical Spectra. Vol. I–Vol. III.* Natl. Bur. Stand. (U.S.) Circular 467; 1949-1958.

[248] W. C. Martin, R. Zalubas, L. Hagan, *Atomic Energy Levels—The Rare-Earth Elements. The Spectra of Lanthanum, Cerium, Praseodymium, Neodymium, Promethium, Samarium, Europium, Gadolinium, Terbium, Dysprosium, Holmium, Erbium, Thulium, Ytterbium, and Lutetium.* Natl. Bur. Stand. (U.S.) NSRDS 60; April 1978.

Lewis M. Branscomb inserted a filter into the optical system of the apparatus developed at NBS for studying the photodetachment of electrons from negative atomic and molecular ions (1961). The studies had direct bearing on ionospheric theory and research and on the study of plasmas.

Atomic transition probabilities were important for the evaluation of the temperature and state of equilibrium of stellar atmospheres, and for determining the relative abundance of elements in stars. Thus, with support from ONR and ARPA, a data center to gather and index all published information on atomic transition probabilities was formed in 1961. By 1962, a bibliography of all known publications on these probabilities was produced and published as NBS Monograph 50. At almost the same time, a 562-page monograph on the transition probabilities of seventy elements was issued.[249]

---

[249] B. M. Glennon and W. L. Wiese, ***Bibliography on Atomic Transition Probabilities***, Natl. Bur. Stand. (U.S.) Monograph 50; August 1962; C. H. Corliss and W. R. Bozman, *Experimental Transition Probabilities for Spectral Lines of Seventy Elements*, Natl. Bur. Stand. (U.S.) Monograph 53; July 1962. The latter monograph was the first Bureau publication produced by completely automatic methods. The computer-produced tape of the calculations was converted to a binary tape in the proper format to drive an automatic phototypesetting machine, which produced film positives of the pages ready for producing the final printing plates.

Unquestionably, the most exciting development of the period was the use of radiation from the electron synchrotron for spectroscopy in the far, or vacuum, ultraviolet. This research would uncover a host of exciting new phenomena and would lead to the nationwide development of new scientific facilities that, at this writing, is still continuing.

A New Light Source

When the Bureau placed its new 180-MeV synchrotron into operation in the 1950s, it was already known theoretically that the circulating electrons in the machine emitted light.[250] It was expected that the wavelength of the light would form a pure continuum with the peak power emitted at a wavelength that would become shorter the higher the energy of the electrons. Due to relativistic effects, the light was predicted to be emitted in a narrow beam tangent to the electron orbit, and since the beam sweeps across a point on the orbital plane as the electrons sweep around their orbit, the radiation on an experimental device like a spectrometer slit would be spatially uniform in the lateral direction. Moreover, exactly on the orbit plane, the light would be completely polarized with the electric vector in the orbital plane, while in directions making small angles above and below the orbital plane there would be a small admixture of light with the perpendicular polarization, the amount depending on the angle. Experiments in 1948 on the General Electric 70 MeV synchrotron confirmed many of these expectations, and a much more complete set of experiments in 1956 on the Cornell 300 MeV synchrotron confirmed them in greater detail.[251] This analysis, however, was photographic, with consequent limitations in determining intensity profiles. The later work, particularly at the Bureau and DESY (an electron synchrotron in Hamburg, Germany), was photoelectric and supplied the really detailed confirmation. The wavelength of the Bureau machine was expected to extend into the vacuum ultraviolet, and it was a perfect source for absorption spectroscopy in this difficult wavelength range.

While interested in such spectroscopy, the Bureau was also interested in synchrotron light because it was at least conceivable to develop it into an absolute radiometric standard. Thus in 1961 Lewis Branscomb, then chief of the Atomic Physics Division, was encouraged by Karl Kessler, chief of the division's Spectroscopy Section, to hire Robert P. Madden, a young spectroscopist trained at Johns Hopkins and then working at the Engineering Research and Development Laboratories, Fort Belvoir, Virginia, to form a new Far Ultraviolet Physics Section, with the charge to develop radiation standards in the far ultraviolet spectral region and instrumentation for the study of this region. He arrived in 1961 on the same day and at the same hour as another young spectroscopist, Keith Codling from Imperial College, London, who had already published some related work. A short time later their collaboration would make the

---

[250] Robert P. Madden, private communication.

[251] F. R. Elder, R. V. Langmuir, and H. C. Pollock, "Radiation from Electrons Accelerated in a Synchrotron," *Physical Review* 74 (1948): 52-56; D. H. Tomboulian and P. L. Hartman, "Spectral and Angular Distribution of Ultraviolet Radiation from the 300-Mev Cornell Synchrotron," *Physical Review* 102 (1956): 1423-1447. The latter paper gives a full list of references on the theoretical work.

The very short wavelength ultraviolet radiation from the NBS 180 MeV synchrotron was utilized as a source of photons in the energy range from 5 eV to 165 eV. These photons interacted with the samples under study to give scientists previously unavailable information on the atomic structures of these substances. Robert P. Madden (left) adjusted the automatic pressure controller on advice from Keith Codling, who read the pressure gage that connected directly with the interaction chamber. After the pressure had reached equilibrium, the synchrotron was operated remotely. At upper right under the dark cover was the 3 m vacuum spectrograph specially constructed at the Bureau for these studies.

two of them famous. The two worked together from the start until five years later when Codling, his visa having expired and not wanting to apply for U.S. citizenship, returned to England.

They began by studying their instrument. A tangent section had already been attached to the glass "doughnut" of the synchrotron by the Bureau's high-energy physics staff to sample the radiation. To this Madden and Codling attached first a photometer for studying the radiation, and later a grating spectrograph for spectroscopic work, and thus was born SURF I (Synchrotron Ultraviolet Radiation Facility). In operation the synchrotron vibrated horribly, and this drove them to design and construct a special spectrograph to obtain high resolution in this mechanically hostile environment. The spectrograph was aligned and worked perfectly after a focal adjustment of only a thousandth of an inch. The usable radiation range extended down to 10 nm.[252]

Madden and Codling chose first to study the absorption spectrum of helium. But they were not acting willy-nilly. There were results from electron-impact studies which indicated that the helium atom could be excited to an energy far above the single-electron-ionization energy, which meant that both electrons were excited. This state could then "autoionize" by losing one of the electrons into the ionization continuum, with the lifetime of the state determined by the strength of the interaction between the doubly excited state and the continuum.[253] Two-electron states of this type had been considered by theorists, but they had never been studied by absorption spectroscopy in the noble gases.[254] Knowing that they were entering unexplored territory, Madden and Codling consulted with Bureau theoreticians, and received mild encouragement. The doubly excited resonance had already been detected by electron spectroscopy, but its shape would be better determined with the higher resolution of the absorption experiment. It would be a nice experiment, and some higher members of the series might also be observed.

So Madden and Codling continued and carried out the experiments.[255] The results were shocking. While theory predicted two series of absorption lines, only one was seen.[256] This led to an exciting and intensive period, with the theorists and experimentalists in continuous interaction at seminars, luncheons, and ad hoc meetings. The work was extended to other noble gases: neon, argon, and later krypton and xenon, where two-electron and subshell single-electron excitations were discovered. The proliferation and visibility of the two-electron resonances were a surprise to all. The Electron Physics Section, under the leadership of John Simpson, began electron-impact studies, and found many optically forbidden transitions in all these noble gases.[257] New results

---

[252] K. Codling and R. P. Madden, "Characteristics of the 'Synchrotron Light' from the NBS 180-MeV Machine," *Journal of Applied Physics* 36 (1965): 380-387; R. P. Madden, D. L. Ederer, and K. Codling, "Instrumental Aspects of Synchrotron XUV Spectroscopy," *Applied Optics* 6 (1967): 31-38.

[253] R. Whiddington and H. Priestley, "Note on a New Transition Produced by Electron Impact in Helium," *Proceedings of the Royal Society of London, Series A* 145 (1934): 462-464.

[254] Early on, Ugo Fano studied the spectrum of the noble gases near the ionization limit: Ugo Fano, "Sullo Spettro di Assorbimento dei Gas Nobili Presso il Limite Dello Spettro D'arco," *Nuovo Cimento,* Nuova Serie 12 (1935): 154-161. Fano further studied the problem in 1961: "Effects of Configuration Interaction on Intensities and Phase Shifts," *Physical Review* 124 (1961): 1866-1878.

[255] R. P. Madden and K. Codling, "New Autoionizing Atomic Energy Levels in He, Ne, and Ar," *Physical Review Letters* 10 (1963): 516-518. A companion paper by the Bureau theorists J. W. Cooper, U. Fano, and F. Prats, "Classification of Two-Electron Excitation Levels of Helium," *Physical Review Letters* 10 (1963): 518-521, appears adjacent to the Madden-Codling paper.

[256] Actually the lines were resonances, as indeed they were expected to be.

[257] J. A. Simpson, S. R. Mielczarek, and J. Cooper, "Observation of Optically Forbidden Transitions in the Continuum of the Rare Gases by Electron Energy Loss Measurements," *Journal of the Optical Society of America* 54 (1964): 269-270.

seemed to be coming forth daily. In due course, the observations were explained theoretically,[258] but they remained so startling that they were still questioned good naturedly at meetings and symposia.

What had been discovered was a new series of interaction mechanisms between the noble gases, radiation, and free electrons, later including the process inverse to autoionization, or dielectronic recombination. Clearly these results were of considerable interest in astrophysics, for these new processes had to be taken into account in any stellar models that involved the interaction of atoms, ions, radiation, and electrons. Equally important, this synchrotron was the first to use synchrotron radiation for experimental purposes on a regular basis. The Nation now has, in addition, synchrotron radiation facilities at the Brookhaven National Laboratory, Stanford University, the University of Wisconsin, Cornell University, Louisiana State University, and Lawrence Berkeley Laboratory, and a large electron storage ring at Argonne National Laboratory. There are also many facilities around the world. Moreover, the technology of using synchrotron radiation in the soft x-ray region (0.8 nm to 4 nm) for lithography in the production of ultra-large-scale integrated circuits is on the verge of becoming commercial.[259] The Bureau work was a successful pioneering effort that proved the usefulness of synchrotron radiation for research.

The synchrotron SURF I was converted into a 240 MeV storage ring, called SURF II, in 1974. Subsequently its electron energy was increased to 300 MeV and with over 300 mA of injected current, SURF II remains a useful source for far UV physics and radiometry.

***

One of the division's major programs was carried out in its Electron Physics Section under John A. Simpson. With the ostensible aim of doing everything with electrons that could be done with light, the section's work to achieve that aim led it into new and interesting directions. The program was many-faceted. It involved polarization studies, with attempts to generate polarized beams by photo-emission from magnetic materials; elastic and inelastic scattering from both solids and gases—a

---

[258] While we cannot enter too greatly into details, the lines formed a series, with the lowest member being the state $2s2p$. This state is degenerate with $2p2s$. The series converges to the $n=2$ level of He^+. However, the probability of excitation to $2skp$ or $2pks$, where k is a running integer, was expected to be about the same, and thus two separate series were expected. But since only one was observed, it was clear that $2skp$ or $2pks$ were not an appropriate classification of the lines of the series. Now, these states are nearly degenerate, and, with electron-electron interaction sufficiently strong, the wave function should be $[u(2skp) \pm u(2pks)]/\sqrt{2}$. Further analysis showed that the + wave function should be the more dominant. Indeed, in due course Madden and Codling were able to find the very weak lines corresponding to the minus sign. Since excitations to $2pkd$ for $k=3$ and above are also allowed by the selection rules, a third mixed series was also expected. This series has subsequently been observed and, as predicted, is very much weaker than the negative series. (M. Domke, G. Remmers, and G. Kaindl, "Observations of the $(2p, nd)^1P^0$ Double-Excitation Rydberg Series of Helium," *Physical Review Letters* 69 (1992): 1171-1174)

[259] "Industry Gambles on Electron Synchrotrons for X-Ray Lithography," *Physics Today* 44(10) (October 1991): 17-21.

continuing activity; and an improved way of studying multiple scattering events by measurement of total energy loss. This last-named technique permitted the calculation of the mean free path of the electron within a few percent. And a study of scattering from aluminum showed that the angular distribution of the scattered electrons was independent of the thickness of the scattering foil, indicating a type of diffraction. This result supported a theory of electron scattering proposed by Gregor Wentzel at the University of Munich in 1921. Inelastic scattering in the same material proceeded by multiple collisions of a type involving long-range interactions with the electrons in the foil supported a model proposed by Ugo Fano in 1956.[260] Scattering from vapors was also studied and applied to such problems as measuring the velocity distribution in beams of metal atoms.

But the activity that was to provide exciting and unforseen results arose from a combination of scattering from gases and instrument development. Always interested in instrument improvement, the section staff was constantly concerned with electron monochromators, electron diffraction apparatus, energy analyzers, and similar instruments. Beginning in 1961, they began a concerted study of electron optics. The payoff was quick and rewarding. From this study there developed a superb electron monochromator and energy analyzer. With electron beams monochromatic within 0.02 eV in the range 1 eV to 100 eV, it was superior to anything available elsewhere.

At the time of the analyzer's completion, the first autoionization results from the synchrotron radiation experiments were being obtained and, under Simpson's personal leadership, the instrument was immediately turned to studies of the same systems. Results were immediate, and they completely confirmed and complemented the spectroscopic work. Indeed, they had an advantage in that, with electrons, optically forbidden transitions could be observed.[261] In addition, a measurement of electron transmission in helium showed a sharp increase in transmission at 19.3 eV,[262] a window which eventually would be used for constructing another type of monochromator.[263] In addition, negative ion states could be observed when the instrument was operated to display total scattering cross-section. Such states—previously unknown—were found in all the rare gases, in molecular hydrogen and deuterium, and in mercury.[264] It was a startling discovery.

---

[260] U. Fano, "Atomic Theory of Electromagnetic Interactions in Dense Materials," *Physical Review* 103 (1956): 1202-1218.

[261] Simpson, Mielczarek, and Cooper, "Observation of Optically Forbidden Transitions."

[262] J. A. Simpson and U. Fano, "Classification of Resonances in the Electron Scattering Cross-Section of Ne and He," *Physical Review Letters* 11 (1963): 158-159.

[263] J. A. Simpson, C. E. Kuyatt, and S. R. Mielczarek, "Electron Monochromator Utilizing the Scattering Resonance in Helium," *Review of Scientific Instruments* 34 (1963): 1454-1455.

[264] "NBS Develops New Tool for Exploring Atomic Structure," *Technical News Bulletin* 48 (1964): 64-68.

It was a time of intense coordination among the spectroscopists, the electron people, and the theoreticians. New results seemed to come almost daily. Dozens of new—and new kinds of—energy levels were observed, studied, and classified. The Bureau had become a world center for this area of intermediate-energy physics. And in instruments, beyond synchrotron light, the Bureau had developed a new precision electron monochromator-analyzer that opened up this new field of experimental physics to the whole world.

Molecular Spectroscopy

Research on molecular spectroscopy dates from the very early days of NBS when William W. Coblentz measured the infrared absorption of various chemical compounds and noted the characteristic wavelengths which provided a "fingerprint" for each. While it would take twenty-five years for the theory to be developed to the point that this characteristic spectrum could be correlated with the structure and chemical bonding in a molecule, Coblentz recognized the potential of infrared spectroscopy for identification of chemical substances. In 1945 Director Condon brought Earle K. Plyler to NBS to establish a laboratory for high-resolution infrared spectroscopy. By utilizing the new solid state infrared detectors, which were much more sensitive that the thermal detectors previously used, he built an instrument of unsurpassed resolving power. In the late 1950s he recruited younger spectroscopists, such as Arthur Maki and Walter Lafferty, who used this spectrometer to elucidate the structural details of a host of small molecules. They were also among the first to use computers for processing the voluminous data produced by high-resolution spectrometers.

In the 1955-1958 period David R. Lide built up a microwave spectroscopy laboratory, introducing to the Bureau a technique for studying pure rotational spectra of molecules. This work was initially located in the Heat Division because of its applicability to predicting thermodynamic properties. New techniques were developed for using microwave spectroscopy to study internal rotation in molecules and to characterize transient free radicals and molecules present in high-temperature vapors. The microwave and high-resolution infrared groups were merged into one section in the Atomic Physics Division in 1963. The joint application of these techniques led to significant advances in the analysis of complex molecular interactions; for example, NBS spectroscopic data were a key to understanding the early infrared gas lasers.[265] Other groups involved in high-resolution spectroscopy in the visible and ultraviolet regions were added later. Thus by the mid-1960s NBS had one of the world's premier molecular spectroscopy laboratories, which attracted many guest scientists from the United States and abroad for sabbaticals and postdoctoral stays.

---

[265] D. R. Lide and A. G. Maki, "On the Explanation of the So-Called CN Laser," *Applied Physics Letters* 11 (1967): 62.

Radiation Physics

The first Kelly Committee found the Radiation Physics Division (at that time combined with Atomic Physics) one of the stellar scientific organizations of the Bureau. The division maintained this position during the period of the present chapter. With activities in radiation protection, dosimetry, radioactivity standards, neutron physics, high energy radiation, x rays and gamma rays, nuclear physics, and theoretical physics, the division was nicely balanced between basic research and directly useful results. It provided Standard Reference Materials for radioactivity standards, calibrations of x-ray measuring equipment, data for radiation protection, research on dose measuring methods, and research in radiation and nuclear physics as support for all these activities and because of its fundamental importance. Four areas of work—nuclear structure, dosimetry, radioactivity standards, and theoretical studies—illustrate well the breadth of the work.

One of the constant themes of the division research program was nuclear structure, where the effort was directed at determining the size and shape of the nucleus. The single example we describe arose from the capability to align nuclear moments. This capability was exploited in the Heat Division for the parity experiment. For several years, studies of the polarizability of the nucleus, as determined by the scattering and absorption of photons, indicated that some nuclei do not have spherical symmetry but are different in three orthogonal directions, which indicated that the nuclear polarizability was a tensor quantity. To check this result, photoneutron production was studied in a single crystal of holmium ethyl sulfate, holmium being one of the elements with an asymmetric nucleus. The yield of photoneutrons was measured as a function of the crystal orientation with respect to the photon beam. At 4.2 K and above, where the nuclei are not aligned, only slight asymmetry was measured, but at 0.3 K, at which temperature the nuclei are aligned, considerably larger asymmetries were found, an indication that the nucleus indeed has a tensor polarizability. Its nuclear matter was not symmetrically distributed.

Dosimetry and the study of various ways to measure radiation dose also were continuing activities. Three main areas were covered: ionization chambers, photographic films, and solid state devices. In the ionization chamber method—the standard calibration means for x rays—a large chamber was constructed for determining the total energy transported by x-ray beams with energies between 6 MeV and 70 MeV, with an uncertainty of 2 percent. A complete description was published as NBS Monograph 48, and the calibration was transferred to laboratories in France, Germany, Switzerland, and Yugoslavia.[266]

Photographic dosimetry was actively pursued, concentrating on the extension and refinement of the photographic process and broadening the range of its applicability in the measurement of radiation. And in solid state dosimetry, it was shown in one investigation that it is possible to use silicon radiation cells as photodiodes for x rays, particularly at high dose rates.

---

[266] John S. Pruitt and Steve R. Domen, *Determination of Total X-Ray Beam Energy With a Calibrated Ionization Chamber*, Natl. Bur. Stand. (U.S.) Monograph 48; June 1962.

A unique and vigorous aspect of the division's program was the production and sale of Standard Reference Materials as radioactivity standards. Some twenty new nuclei were offered, from tritium-labelled toluene to americium-241. Featured were two redeterminations of the half life of carbon-14, with a final value of 5745±50 years.

The program that perhaps best illustrates the balance of basic research and applied output was that in radiation theory. As the division that first emphasized the importance of theoretical research, it always maintained a strong program in theoretical studies, with a careful balance between pure and applied research. For example, the theoretical analysis of cross sections was a continuing activity, with a study of bremsstrahlung and pair production, and a quantitative theory for calculating the polarization of gamma rays from the measured angular distribution of electron-positron pairs was developed.

Another continuing program was in the computer calculation of multiple scattering of various radiations, and hence their penetration into various media, both bounded and unbounded. Illustrating the public concern of the time, with support from the Office of Civil and Defense Mobilization and the Defense Atomic Support Agency, a study was carried out on civil-defense related shielding problems, and published as NBS Monograph 42.[267]

Up to 1963, the work of the division was built around nuclear physics and what might be called the "classical" fundamental particles—protons, neutrons, electrons, positrons, gamma rays and, to a lesser extent, pions, or pi mesons as they were then called. Particle physics, with its myriad of baryons and mesons, was mostly left to laboratories that had machines of sufficient energy to produce and study this bewildering (to the average scientist) array of particles. But the so-called "eight-fold way" that was developed by Murray Gell-Mann of the California Institute of Technology and Yu'val Ne'eman of Israel promised to be a system that would bring some sense of order. In the very next year, the Bureau initiated a small program in particle physics, an important step which expanded its interest beyond nuclear physics to fundamental particle physics and true high-energy. The step was apparently too big, and the new field brought only a slight connection to the Bureau's traditional measurement standards role. No experimental program was ever initiated; the program was purely theoretical and even that was haltingly supported. Thus, while other aspects of theoretical nuclear physics were pursued vigorously, particle physics was not. Yet the program lasted for almost twenty years, and during its continuance provided the Bureau with a tiny window to one of the most challenging fields of modern science.

Perhaps the most important events that occurred during the period were not scientific developments, but the procurement of two new radiation sources: a linear accelerator, or LINAC, and a nuclear reactor, both of which added a whole new dimension to the Bureau's radiation capabilities.

---

[267] L. V. Spencer, *Structure Shielding Against Fallout Radiation from Nuclear Weapons*, Nat. Bur. Stand. (U.S.) Monograph 42; June 1962.

Two New Radiation Sources

At the House Appropriations Committee Hearings for FY 1960, Astin announced to the committee that he had formed a nuclear reactor study group in late FY 1958 to "undertake initial planning for an NBS research reactor."[268] The group was surveying the NBS present and future programs and external scientific organizations as to their needs for a research reactor. The areas of study were already known to be neutron standards and dosimetry at high flux levels, development of activation analysis as a basic and highly sensitive tool in analytical chemistry, neutron diffraction, particularly in magnetic and organic materials, and neutron irradiation to produce short-lived isotopes.

In fact, by the time Astin presented this plan, the decision had essentially been made to proceed with a nuclear reactor facility. In a long 1957 memorandum to Astin, Nicholas Golovin had proposed the formation of a group to "launch the reactor program,"[269] and Bureau management decided to accept this proposal. It was an unusual decision. There was no strong clamor for such a facility from the Bureau staff, as would normally be expected,[270] but this was ascribed to the lack of a facility. If staff enthusiasm were to be used as a criterion for the facility, support was not likely, and would not be found until the facility existed. It was a chicken-and-egg problem. To correct this situation, Golovin recommended that, during the planning and construction phase of the reactor facility, appropriate NBS staff work at other institutions, such as the Argonne National Laboratory, to begin an NBS research program. Carl O. Muehlhause, a nuclear physicist with reactor and research experience at both the Argonne and Brookhaven National Laboratories, was hired in 1958 to head the group that Golovin had proposed. Muehlhause was assigned to the Director's Office. Thus, reminiscent of the way Astin made the decision to acquire the large testing machine, Bureau management made the decision to obtain a nuclear reactor facility despite the absence of enthusiastic support from the Bureau staff. It proved to be a wise decision.[271]

At the FY 1961 hearings, Astin asked for $700 000 for design work for the reactor, and in 1962 a further $8.15 million for construction of the reactor building and the reactor itself.[272] The reactor was meant to be used by the whole Washington scientific community. In fact, users were to come from much farther away. The reactor was

---

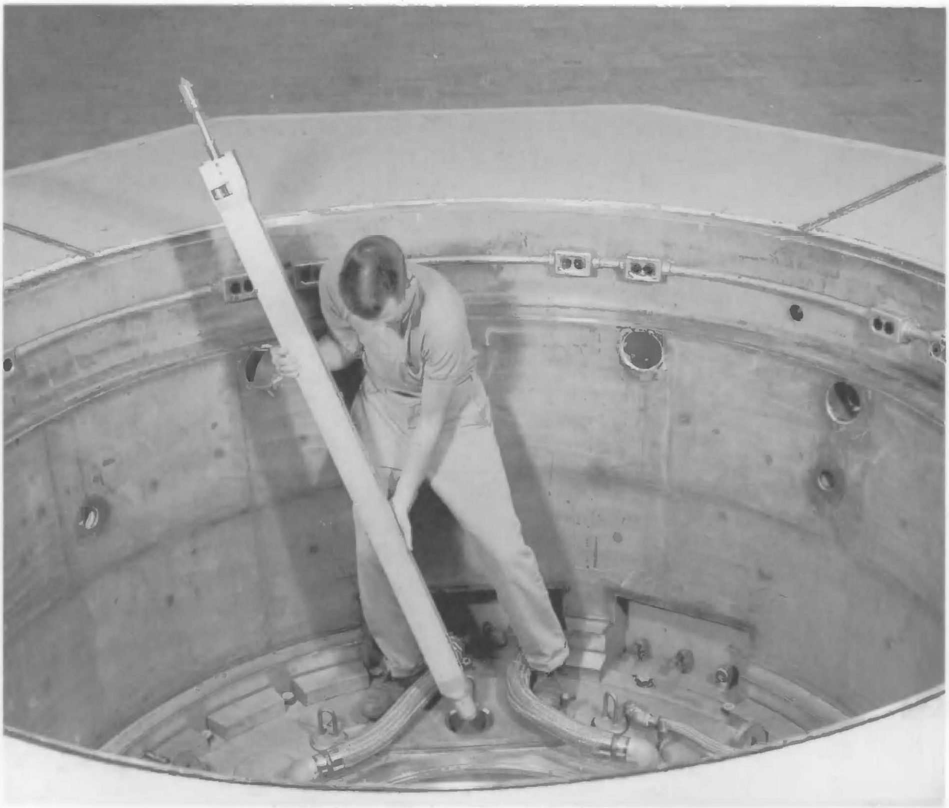
[268] House Committee on Appropriations, Subcommittee on Department of Commerce and Related Agencies, *Department of Commerce and Related Agencies Appropriations for 1960: Hearings Before a Subcommittee of the Committee on Appropriations, House of Representatives,* 86th Cong., 1st sess., National Bureau of Standards, 1 May 1959: 305.

[269] Memorandum, N. E. Golovin to A. V. Astin, "NBS Requirements and Plans for a Nuclear Research Reactor," July 8, 1957. (NARA; RG167; Astin file; Box 18; Folder Reactor Accelerator)

[270] Ibid.; also Memorandum, I. C. Schoonover to A. V. Astin, "NBS Reactor," February 13, 1958. (NARA; RG 167; Astin File; Box 18; Folder Reactor Accelerator)

[271] This was not the first time the Bureau considered a nuclear reactor. In 1946, during Condon's tenure as director, there was a "Proposal for a National Radiation Institute of the NIH and the NBS." (NARA; RG167; Directors file; Box 16; Folder NIH-NBS (Project on Atomic Pile)). Nothing came of it.

[272] Appropriations Hearings for 1961: 238; Appropriations Hearings for 1962: 828-829.



Charles Ware inserted a fuel element containing uranium-235 into the loading port of the NBS 10 MW nuclear reactor just before it became operational (1966).

designed by Muehlhause, Robert S. Carter, and Harry H. Landon, a small group compared to the huge teams used to design reactors in later years. Building construction began in 1963 and was completed in 1966. The reactor achieved criticality December 7, 1967. The Bureau had a new radiation source.

There had been considerable discussion about the type of reactor that should be built, specifically on its size. In an analysis of this question, Randall S. Caswell came to the conclusion that the reactor should be either a small machine that would provide only a few irradiations, but quite cheap, or a large, "really first rate reactor."[273] An intermediate size reactor would be neither fish nor fowl, and there would be difficulty in supporting a solid research program. He estimated that a large reactor with a flux of 10^{14} neutrons/$\text{cm}^2 \cdot$ s would cost about $10 million and require a crew of perhaps

---

[273] Memorandum, R. S. Caswell to L. S. Taylor, "Nuclear Reactor Planning," March 5, 1957. (NARA; RG 167; Astin file; Box 18; Folder Reactor Accelerator)

fifteen persons. Bureau management readily opted for the large, deluxe reactor.[274] They selected a 10 megawatt machine with a flux of 1.7×10^{14} neutrons/cm$^2 \cdot$ s, almost twice Caswell's planning criterion. Its fuel, enriched uranium-235 and uranium-238, was alloyed with aluminum and then clad with aluminum to prevent contamination of the coolant. The moderator was heavy water. Cooling water flowed at the rate of 19 m^3/min. Thirteen beam tubes, or ports, running from the heavily shielded reactor core transported the neutrons to the working areas for experimental purposes. In addition, there were facilities for in-pile radiation studies. A space was also provided for a "cold source," in which neutrons could be cooled to cryogenic temperatures. Such a source would be added in the late 1980s, putting the Bureau at the forefront of the use of neutrons for experimental studies. The total cost at the time of construction was somewhat over $9 million, very close to Caswell's prophetic $10 million estimate.

In 1970 the reactor had its first refueling. By that time four neutron spectrometers were in operation for crystal and liquid structure work, and a time-of-flight instrument for inelastic scattering. Under development were a triple-axis spectrometer for crystal dynamics studies, and a high-resolution cold neutron facility, which was not completed until 1990, by which time the reactor power had been doubled to 20 MW. Bureau studies in 1970 were concerned with ternary fission in uranium-235 and plutonium-239 α-accompanied fission. A technique for neutron-flux measurements using boron-10 and cobalt as absorbers was under development. The facilities were used extensively for activation analysis, hydrogen-bonding studies in crystals, and phase transitions in long chain hydrocarbons. There were collaborative programs with other agencies, notably with Los Alamos for the calibration of a uranium-235 fission neutron source. Local agencies, such as the Naval Ordnance Laboratory and the Naval Research Laboratory, carried out structure determinations on a variety of magnetic materials and amorphous silica. An extensive activation analysis program was being initiated, and considerable work was being performed in collaboration with university scientists in solid state and nuclear physics studies.

There was never any doubt about enthusiastic staff support for a linear electron accelerator, or "LINAC," for short. Such a machine was a natural outgrowth of the Bureau's high-energy physics program, in which the main available machines were the 50 MeV betatron and the 180 MeV synchrotron. But there were other reasons for wanting a LINAC. Near the end of the 1959 appropriations hearings, where much of the discussion had been on high temperatures, the following exchange took place:

---

[274] "High-Flux Research Reactor for New NBS Site," *Technical News Bulletin* 45 (1961): 127-128; "Work Begun on NBS Reactor Buildings," *Technical News Bulletin* 47 (1963): 154-155; "NBS Completes High-Flux Reactor," *Technical News Bulletin* 50 (1966): 212-214; "NBS Reactor Achieves Criticality," *Technical News Bulletin* 52 (1968): 50; "Research at NBS Reactor," *Technical News Bulletin* 54 (1970): 174-176.

> MR. YATES: Are there other such fields that you think we should know about?
> DR. ASTIN: Yes there are. Actually, in the things I submitted to the Secretary, the high temperature studies were third priority. My first priority was high intensity radiation-producing devices which we need for precise measurements in the nuclear and atomic radiation field.
> MR. FLOOD: Off the record.

The remainder of the discussion is not available, but upon coming back on the record, the discussion continued:

> MR. YATES: With what you have asked for—and apparently you will get—with \$1.5 million can you . . . make an . . . attack upon the problems you refer to in your justifications as your basic research program?
> DR. ASTIN: Yes.
> MR. YATES: I would like the record to show that that would be earmarked and pigeonholed and nailed down as a line item, if necessary, despite what the glorified clerks masquerading under three stars might have to say.[275]

The line item was the LINAC. Clearly, along with its own specific needs, there were classified military reasons for NBS to obtain such a machine, and it was being awarded \$1.5 million to get the process started. Thus, when funds were appropriated in September 1958, the staff of the High Energy Radiation Section began a study on the proposed new instrument.[276] The purpose of the study was "the examination of the Bureau's responsibilities in research areas made possible by recent accelerator developments, the specification of a research program, and accelerator and a building design."

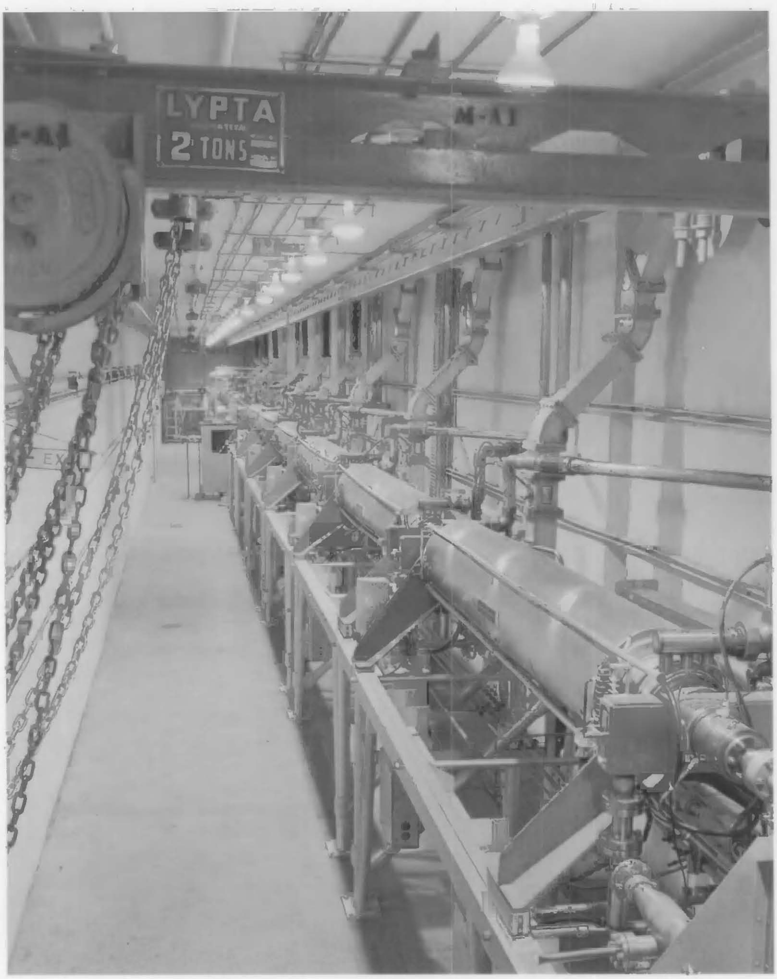
The principal difference between the proposed accelerator and the Bureau's existing machines was beam power, not particle energy. Recent advances in accelerator technology had made possible beam powers of 10 MW at electron energies of 10 MeV and, the report states, these were being investigated commercially for the sterilization of pharmaceuticals and foods, for the polymerization of polymers and the vulcanization of rubber, and the curing of tobacco. The Bureau program would provide the scientific basis for this emerging technology. However, these applications would probably "not be studied directly by NBS," but "the assembly of basic data and measurement techniques that make such applications possible would be the goal."

None of this could be achieved with the Bureau's existing accelerators. The power output of the 50 MeV betatron was 20 mW, and for the 180 MeV synchrotron it was 100 mW. Commercial practice was absolutely impossible with such low beam-power machines. Thus what the Bureau proposed was a linear accelerator which, operating at an electron energy of 40 MeV would produce a 40 kW beam. This represented an increase in power by a factor of about 100 000. And not only would the instrument produce the primary electron beam, x rays, positrons, and neutrons could be produced with the beam impinging on various targets.

---

[275] Appropriations Hearings for 1959: 440.

[276] "A Proposal for a High-Intensity Electron Accelerator," NBS Report 6555 (June 1959).



Electrons were accelerated through LINAC's 100 ft nine-section pipe at a speed that approached the speed of light. In the gap after the third section, the electron beam could be changed to a positron beam by means of a target interaction. The LINAC could generate a 100 MeV electron beam.

The research would involve studies in nuclear and atomic physics, and the development of dosimetry standards and measurement techniques. The reason for the high beam power requirement in nuclear physics was that some of the processes of interest occur very infrequently, and some experiments, if possible at all on the betatron and synchrotron, would require months of exposure time, whereas they could be done in a few hours with the proposed instrument. And the presence of the other radiations permitted studies not possible at all with the older instruments.

In 1966, the Bureau's powerful linear accelerator (LINAC) went into operation. It was located entirely underground in a wing that projected from the Radiation Physics Laboratory at the NBS site near Gaithersburg, Maryland.

Dosimetry standards, always a major part of the Bureau's responsibilities in the radiation field, were carried out at relatively low radiation doses on the order of 100 rad, and dose rates of thousands of rad per hour. But new advances required standards at much higher dose rates for nuclear research, personnel protection, and science and industry generally. Indeed, the military projected a need for standards in the 10 billion rad range. Related to dosimetry were shielding studies on such materials as concrete and lead, and control methods for such intense beams. The addition of the proposed LINAC to the Bureau's array of other radiation sources—cobalt, radium, x-ray, and electron beam devices—would give the Bureau an impressive array of tools for such studies.

Funds were appropriated for the construction and the High Voltage Engineering Corporation built the LINAC using detailed specifications developed by the study group. What resulted was a linear accelerator that produced a beam of 150 MeV electrons at low beam power, and 100 MeV electrons at a beam power of 50 kW. It consisted of nine 10 ft sections of waveguide powered by radar klystrons operating at a frequency of 1.3 GHz. This accelerator delivered a 2 mm diameter beam to a beam-control room where iron magnets directed it to any of three 45 ft × 35 ft experimental rooms. For personnel protection, the whole assembly was 32 ft underground, and the experimental rooms were separated from one another by 12 ft of concrete. Along with all the other Bureau radiation research devices (except, of course, the reactor), it was housed in a special building on the Gaithersburg site.

The LINAC was accepted from the contractor on October 7, 1965. It had always been planned that it would be used by the whole scientific community, and a preliminary meeting of LINAC users was held on October 11, 1965.[277] The Bureau had a new high energy radiation source.

Chemistry

A comparison of the annual reports on chemistry in 1964 with those in 1958 shows both continuity and striking changes. This development was only partly occasioned by the 1961 reorganization which split the original Chemistry Division into two: one called Analytical and Inorganic Chemistry (the "Inorganic" was dropped at the 1964 reorganization into Institutes) and the other, Physical Chemistry. At the same time, all organic coating work was stopped and the Electrodeposition Section was transferred to the Metallurgy Division.

The changes went much further than simple changes in the organization chart. During the seven years of the period, there were many substantive changes. Consider first the Analytical Chemistry Division. Where it previously had ten sections, it was now left with five: Pure Substances, Spectrochemistry, Solution Chemistry, Standard Reference Materials, and Applied Analytical Research.

In 1958 the Annual Report lists the following topics as being investigated during the year:

- Analysis of small amounts of tungsten in steel.
- Use of flame photometry for the determination of sodium, potassium, calcium, and strontium in the presence of other alkali and alkaline-earth ions.
- Purification of titanium halides.
- Automatically controlled equipment for the evaluation of purity.
- Simultaneous spectroscopic determination of eighteen elements.
- Development of an integrating electrochemical coulometer.
- Synthesis of tritium-labeled carbohydrates.
- Preparation of a series of oil-soluble metal analytical standards to be used in assessing engine wear.
- Experimental and theoretical studies of boron compounds as potential high-energy fuels for rocketry.
- Development of an apparatus for the controlled sublimation and separation of substances at very low temperatures, potentially useful for free radicals.
- Instrument for estimating night vision.

---

[277] *Radiation Physics Division Information Handbook,* February 1, 1965. A copy of the handbook was provided to the attendees of this meeting. It contains NBS Report 6555 and is the source for most of the material used in this section.

- The use of a plasma jet for the analysis of an alloy.
- Spectrometric identification of trace constituents in complicated gas mixtures such as smog.[278]

By contrast, the 1964 Annual Report listing for the Analytical Chemistry Division reads:

- Activation analysis improved.
- Improved Mössbauer spectrometer.
- Radioisotopic tracers used for analysis.
- Clean room constructed.
- Spark source mass spectrograph acquired.
- Single reproducible pulse obtained with laser microprobe.
- X-ray spectrographic analysis of solutions.
- Electron probe microanalyzer extends capabilities.
- Spectra excited in controlled atmospheres.
- Special cast iron standards prepared for the Malleable Research Foundation.
- Errors in the microscopical measurement of spheres studied.
- Thermodynamics of solutions in heavy water.
- pH standards in partially aqueous media investigated.
- Preconcentration polarography.
- Magnetic densimeter constructed.
- Copper-base and white iron standards issued.
- New uranium and plutonium isotopic standards issued.
- High-precision coulometry.
- Analysis and purification services provided.[279]

A short perusal of these lists shows that in both years the division was working in its traditional areas of the analysis of metal alloys and the provision of standard materials, the production of pure materials and the development of methods of assessing purity, and the development of analytical methods. In 1964, however, the focus was a little sharper, the emphasis had changed, and totally new items appeared. Activation analysis, spurred by the forthcoming nuclear reactor, was a new area. Trace analysis, arising from the division's history in evaluation of purity, had a whole new clean room constructed for its study. But perhaps most striking was the number of new, sophisticated, and costly instruments: a spark source mass spectrometer, a laser microprobe, an electron microprobe, and a Mössbauer spectrometer. This was part of a concerted effort, announced in 1961, to increase emphasis on instrumental methods of analysis. In that year alone seven major pieces of analytical apparatus were purchased. These new trends would continue. Under the aggressive leadership of its new chief, W. Wayne Meinke, the Analytical Chemistry Division would become a world leader in activation, trace, and instrumental analysis.

---

[278] Annual Report, 1958: 35-39.

[279] Annual Report, 1964: vi, 125-131.

It is nearly impossible to make the same kind of comparison between the 1964 Physical Chemistry Division and what existed earlier. Four of its seven sections were led by, and consisted of pieces of, sections other than the original Chemistry Division.[280] It was as if Bureau management had taken some of its best people in physical chemistry and related areas and thrown them together. The Annual Report for 1962 gives a good summary of the research. With the stated aim "to develop an understanding of the molecular basis of bulk properties and macroscopic processes," the division carried out an extensive set of "studies on synthesis of labeled carbohydrates, isotope effects, and conformations of sugars; spectroscopic determinations of structural constants of free radicals and simple organic molecules; analysis of elementary processes in flames; investigations of radiolysis and vacuum ultraviolet photolysis of organic molecules; precise measurements of heats of reaction and formation; studies of chemical reactions and ionization processes at crystal surfaces; determinations of atomic weights; and measurements of nuclear spin-spin interactions."[281]

Cognizant of the need for reference data, in 1962 the division was actively setting up a document retrieval system for physico-chemical data, and in 1963 undertook a complete revision of Circular 500, *Selected Values of Chemical Thermodynamic Properties.*[282] Groups were also established to compile and evaluate data on gas-phase chemical kinetics and ionization energies (appearance potentials) of molecules. The division would eventually have four data centers in the NSRDS, and become a world leader in the dissemination of thermodynamic and other physico-chemical data.

Materials

The three materials divisions—Inorganic Solids, Polymers, and Metallurgy—all showed significant changes in the character of the work performed during the period. Indeed, a few years later the Annual Report was to declare, "Even in specifically technological areas such as glass, paper, ceramics, metallurgy, and corrosion, NBS programs have changed in the last few years from a craft orientation to a science-based orientation. As a result, the output of data, standards, and methodology have much

---

[280] Specifically, Ralph Klein came from the Electron Physics Section of Atomic and Radiation Physics to head the Surface Chemistry Section, along with Vernon H. Dibeler and his Mass Spectrometry Section. From the Heat Division's Free Radical Program came Robert E. Ferguson to head the Molecular Kinetics Section, along with David E. Mann and his Molecular Spectroscopy Section. The able theorist Robert W. Zwanzig from Heat's Statistical Physics Section came into the Office of the Chief of Physical Chemistry. Thermochemistry under Edward J. Prosen, Organic Chemistry under Horace S. Isbell, and Molecular Structure and Properties of Gases came from the Chemistry Division, as did Merrill B. Wallenstein, the chief of the new Physical Chemistry Division.

[281] Annual Report, 1962: 84-85.

[282] Frederick D. Rossini, Donald D. Wagman, William H. Evans, Samuel Levine, and Irving Jaffe, *Selected Values of Chemical Thermodynamic Properties*, Natl. Bur. Stand. (U.S.) Circular 500; February 1952. W. Reeves Tilley, chief of the Office of Technical Publications, relates that Circular 500 was so important that after it was out of print and no longer available, an industry official asked Tilley to "name your own price" for a copy.

Hideo Okabe prepared equipment prior to the photolysis of deuterated ethane.

greater precision, meaning and usefulness."[283] By the end of 1964, this transformation was largely complete, although it was more evident in the Polymers Division than in the other two.

There was no revolutionary change in the program of the Inorganic Solids Division (named the Mineral Products Division until 1962) but, as indicated by the transfer of the Concreting Materials Section to the Building Technology Division, management wanted the work to become more basic. The division would no longer have the responsibility for the huge cement testing activity, and the cement and concrete supporting research would henceforth be done in Building Technology. But, while there was no

---

[283] Annual Report, 1967: 5.

revolutionary change, there were an increasing number of examples of basic research carried out during the period. There was, first of all, the diamond anvil (produced, incidentally, with little or no encouragement from management) and its use to study the effect of pressure on the infrared spectra of solids, and in pressure-induced phase transformations. Other noteworthy efforts were:

- The direct observation of dislocations in ceramics by transmission electron microscopy, now common and routine, but at that time an exciting new development, especially for ceramics.
- The new analysis of experimental data which showed that configurational entropy is a criterion for glass transformation.
- The study of single crystals, including methane as the simplest solid that shows a phase change; crystals of the rare gases, because they are the simplest solids known; and a program supported by ARPA on the methods of growth of crystals of inorganic materials.
- The theoretical development of a computer-generated "random-network" model to get at the "statistical topology and geometry" of such materials as silica glass and water.[284]
- The production of glasses of unique morphology by bringing to critical opalescence glass compositions that show liquid-liquid separation, followed by rapid quenching, forming a random two-phase solid with both phases interconnected.

Unquestionably the most elegant scientific development resulted from a study of internal friction by John B. Wachtman, who would, in 1968, become chief of the Inorganic Materials Division. In a material demonstrating this phenomenon, the application of a sinusoidal stress (or strain) results in a sinusoidal strain (or stress) response that is out of phase with the applied excitation. This behavior results from the fact that some mechanical energy is converted to heat, hence the friction in the name. Similar behavior is exhibited by dielectrics excited by a time-varying electric field, and the behavior is generally undesirable.

For some materials, measurement of the internal friction over a wide temperature range at a constant frequency indicates a characteristic peak in the internal friction at a specific temperature, with this temperature being higher the higher the frequency of the excitation. In such materials, a relaxation time for the molecular process responsible for the internal friction may be defined as the reciprocal of the frequency at the peak. Numerous experiments show that the relationship between the temperature of the peak and the relaxation time follows an Arrhenius relationship, suggesting that whatever the molecular process, it is an activated one. The question in any given situation is, "What specifically is the molecular process?"

---

[284] Annual Report, 1963: 104.

Wachtman carried out a complete series of experiments on thorium oxide containing 1.5 percent calcium oxide, a system that shows an easily observable internal friction peak. Now, in this material, the calcium ion replaces a thorium ion substitutionally, and because it has only one oxygen compared to thorium's two, an oxygen vacancy must perforce exist around the calcium ion. Indeed, there are eight lattice sites the vacancy can occupy. Without going into more detail, we can state that Wachtman showed that the molecular process responsible for the internal friction was the stress- or electric field-induced motion of oxygen vacancies from one site to another.[285] Since in this movement the vacancy has to surmount an energy barrier, this is the necessary activated process. He was able to show that the process occurred with a single relaxation time. In effect, the application of stress or an electric field changed the population distribution of vacancies between sites, and when the frequency of the rearrangement equalled the applied frequency of the applied excitation, a peak in the internal friction occurred.[286]

As thorough as these investigations were, they did not answer the general question, "What is necessary in a crystal containing defects for it to show internal friction?" In a series of papers, Wachtman and his associate H. Steffen Peiser were able to give a broadly applicable answer to this question.[287] They considered a perfect crystal and asked what would happen to the symmetry of various sites in the crystal upon the application of a homogeneous strain, and what would happen if a point defect were placed at these various sites. If such a defect were placed at either of two positions which were equivalent before straining, and were still equivalent in the strained crystal, no internal friction would result. If, upon straining, the two sites were no longer equivalent, then internal friction would at least be possible, for then it could be advantageous for the defect to jump from one position to the other, thereby changing the distribution of vacancies. It is possible, of course, that internal friction might not be seen because its magnitude might be too small. What had been derived was a necessary condition, not a sufficient one.

From this simple, almost self-evident consideration of the change of symmetry of perfect crystals on straining came a set of rules for the occurrence of internal friction, and in a number of publications all 232 space groups were analyzed. The geometry, if not the physics, of the requirements for internal friction was firmly established.

The Organic and Fibrous Materials Division (to become simply the Polymers Division in 1962) began to change its emphasis from products formed from natural polymers (cotton, wool, silk, cellulose, natural rubber) to synthetic polymers. This trend continued so that by 1964 the principal focus of the division was on synthetic

---

[285] J. B. Wachtman, Jr., "Mechanical and Electrical Relaxation in ThO_2 Containing CaO," *Physical Review* 131 (1963): 517- 527.

[286] The relaxation time for dielectric relaxation was predicted—and found—to be twice the value of the mechanical relaxation time because only half the relaxation modes are operative.

[287] J. B. Wachtman, Jr., and H. S Peiser, "Symmetry Conditions for Internal Friction Caused by Jumping of Point Defects in Crystals," *Applied Physics Letters* 1 (1962): 20-22.

polymers. The actual research directions were toward crystallization phenomena, solution properties as background research for the preparation of two badly needed molecular-weight standards, adsorption and the study of polymers at surfaces, and the degradation of polymers.

As already mentioned in the discussion of the Electricity Division, polymer science was revolutionized by the discovery that lamellar single crystals of polymers could be grown, and by evidence that such single crystals, along with an amorphous component, could exist in the solid state of those polymers that had the necessary regularity to crystallize at all. In the division program, these facts were used in attempting an explanation of bulk properties, led to studies of the kinetics of crystallization, and undergirded the study of single crystal forms and habits of different polymers. For example, studies were carried out on the internal friction of polypropylene, as the degree and character of the crystallinity were systematically changed. A study of the lattice spacings in crystallization of copolymers showed that segments of the minority component were incorporated in crystals of the majority component, a result that was hotly contested at the time.[288] A comprehensive study of the crystallization of polypropylene from solution led to the determination of exceptional crystal growth propagation characteristics exhibited by this polymer that were still the object of considerable research long after the Bureau had stopped working on them.[289] Of course, the Polymers Division scientists were in constant contact with the work going on in the Electricity Division.

In the area of Standard Reference Materials, the effort was devoted to the provision of standards for molecular weight—aside from chemical composition, the single most important property of polymers. Because synthetic polymers are mixtures of molecules of different lengths, their molecular weights are only averages, and for a fuller material specification, not only must the molecular weight be specified, the distribution of molecular weights must be given as well. There are three principal methods for the determination of polymer molecular weights, all of them based on the behavior of polymers in solution: the determination of the concentration distribution of a polymer solution in the enormous gravity forces obtained in an ultracentrifuge, the scattering of light by polymer molecules in solution, and the osmotic pressure of polymer solutions. Each of these methods can be analyzed from first principles and yield scientifically accurate results (when performed properly). The problem is that they are exceedingly tedious to carry out. Practically all molecular weight determinations in industry are made via the viscosity of polymer solutions, which is an easy and rapid measurement, but one with only an empirical relationship to molecular weight. Because of this state of affairs, industry had long requested that the Bureau provide a Standard Reference Material that could be used to calibrate these viscosity methods. Thus, in 1957 an ultracentrifuge was obtained and studies in sedimentation equilibrium were begun, along

---

[288] J. P. Colson, R. K. Eby, "Melting Temperatures of Popolymers," *Journal of Applied Physics* 37 (1966): 3511-3514.

[289] F. Khoury, "The Spherulitic Crystallization of Isotactic Polypropylene From Solution: On the Evolution of Monoclinic Spherulites From Dendritic Chain-Folded Crystal Precursors," *Journal of Research of the National Bureau of Standards* 70A (1966): 29-61.

with studies in light scattering and osmotic pressure. By 1964, two polystyrene SRMs were available, one with a narrow molecular weight distribution, and another with moderately broad distribution, and with average molecular weights of 180 000 and 217 000 respectively. In later years, with advances in gel-permeation chromatography, samples with certified molecular weight distributions would be offered.

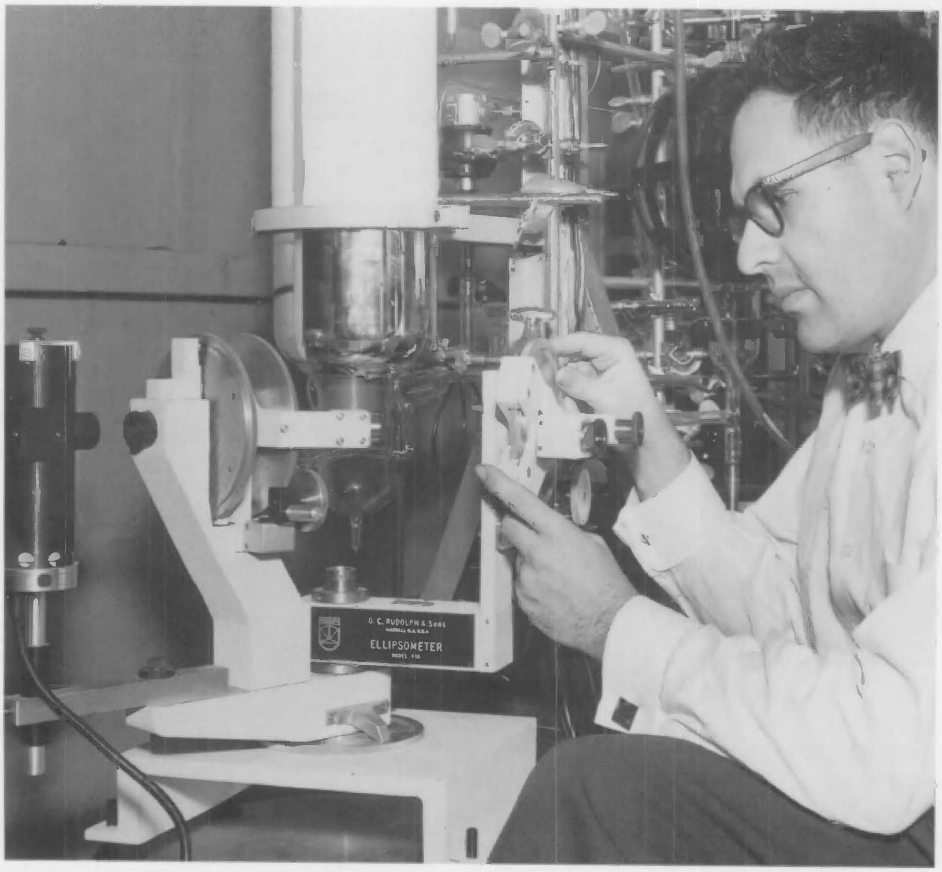
In a different area, with support from the Navy Bureau of Aeronautics, and spurred by the concern for the adhesion between glass fibers and the matrix in polymer-glass composites, the division in 1958 began a study of the adsorption of polymer molecules from solution. The results indicated that far more polymer was absorbed than could be explained by the polymer molecules lying flat on the surface. This result could be explained if polymer segments separated by bridges of dangling polymer chains were the units adsorbed. In effect, this meant that a solvent-swollen film of polymer lay on the surface, and the study of this film became important. To carry out this study, an instrument called an ellipsometer was obtained. The instrument measures the change in the state of polarization of light upon reflection from a surface, and from the measurement the optical constants of a film-free surface may be calculated. Further, if the optical constants of the surface are known, and if the surface is overlain by a dielectric film, the refractive index and thickness of the film can be determined—just what was needed for the polymer experiments. These did indeed show that the polymer was in the form of a swollen film, but the details of the situation depended on the polymer-surface and polymer-solvent interactions. Nevertheless the results elicited quite a bit of attention, and a significant theoretical effort supported the experiments. For a number of years, the Bureau was a world center for this type of work.

Equally important, the use of the ellipsometer was a significant part of other work. Jerome Kruger in the Metallurgy Division also used the ellipsometer in investigations on passive films in corrosion, and Theodore R. Young used it in work on the role of adsorbed films on the measuring surfaces of gage blocks used related techniques. The Bureau had become a center for such measurements, and a symposium with international participation on the use of the ellipsometer was held. The resulting proceedings volume was for many years the bible of ellipsometry for any scientist interested in the properties of surfaces.[290]

The program on degradation in the Organic and Fibrous Materials Division had many facets, and was mainly supported by NASA and various agencies of the DOD. The basic aim of the program was to determine the mechanisms of degradation and with this information try to synthesize more resistant polymers, or to design additives that would prevent degradation. Thermal degradation and degradation by radiation were of particular concern, the latter because of the space environment. In addition, radiation was used to try to synthesize high temperature elastomers based on fluorinated hydrocarbons.

---

[290] *Ellipsometry in the Measurement of Surfaces and Thin Films*, E. Passaglia, R. R. Stromberg, and J. Kruger, eds., Natl. Bur. Stand. (U.S.) Miscellaneous Publication 256; September 1964.



Jerome Kruger, metallurgist on the Bureau staff, demonstrated an ellipsometer to study corrosion reactions on the surfaces of metal single crystals. The instrument was on display during a two-day symposium on ellipsometry held at the Bureau in September 1963.

One particular study, concerned with materials for missile and rocket nose cones, illustrates one of the more practical outputs. In this study, three different groups of polymers were subjected to pyrolysis up to 1200 °C. One group consisted of highly cross-linked materials such as epoxy resins, another of polymers that develop cross-links by this temperature treatment, and a third which did not cross-link. The first two groups gave carbonaceous residues and low-molecular-weight volatile products, while the third group yielded only volatile products. The analysis of the results gave proof that the energy-absorbing capacity in thermal degradation is inversely proportional to the molecular size of the volatile products, and also demonstrated that polymers that leave a carbonaceous residue and liberate gas at elevated temperatures generally give superior ablation resistance to the heat generated by re-entry to the earth's atmosphere in missile nose cones.

If the changes that occurred in the other materials divisions could be classified as evolutionary, then the changes in the Metallurgy Division were revolutionary, as befits a division that received a quite critical report from the first Kelly Committee. Simply, the division became one based on metal physics rather than one based on traditional metallurgy. The change began in 1957 when James I. Hoffman, himself a superb analytical chemist who had spent his career in the Chemistry Division, but was now acting as chief of Metallurgy, brought physical chemist Lawrence M. Kushner to Metallurgy from Chemistry to form a new section called Metal Physics. As explained in the Annual Report for 1957: "The objective of the [metallurgy] program has always been to increase both theoretical and practical knowledge of metals. . . . A new metal physics laboratory has been instituted to augment the theoretical work carried on" in the division.[291] It is clear from the designation of the new unit as a "laboratory" rather than a section that it was expected to grow beyond section size, and it is also clear from the subsequent history that "theoretical" meant "basic," and not only strictly theory, although that also flowered.

A comparison of the sections in 1957 and 1964 shows the change in the nature of the work. In 1957 there were five sections: Thermal Metallurgy, Chemical Metallurgy, Mechanical Metallurgy, Corrosion, and Metal Physics. By 1964, the three metallurgy sections had coalesced into one called Engineering Metallurgy, and the Metal Physics Section had spawned three new sections: Crystallization of Metals, Alloy Physics, and Lattice Defects and Microstructure. Corrosion was still a section, but its program had changed dramatically, becoming far more basic than previously. The new division had a strong theoretical component, particularly in the areas of crystallization, diffusion, and dislocation theory.

Organizationally the new section structure arrived with the major Bureau reorganization into institutes, but the programmatic changes occurred continuously from the beginning of the period. Corrosion research became primarily concerned with the growth of passive films. A great deal of the work was concerned with the growth and breakdown of these films. A long series of studies was concerned with single crystals—primarily copper and iron—and the difference in film behavior on the various prismatic surfaces of the crystal. One of the principal aims of the studies was the development of techniques for the study of passive films and, as already mentioned in the discussion of the Polymers Division, the corrosion scientists, under the leadership of Jerome Kruger, joined the polymer group to make the Bureau a leader in the use of ellipsometry for measurements on film-covered surfaces.

Next to be implemented was the study of the growth of metal crystals from the vapor. Here the aim was to check existing theories of crystal growth. Experimentally, both crystals of normal habit and the perfect but microscopic needle-like crystals known as "whiskers" were studied. The reason for studying growth from the vapor was, of course, the ease of theoretical treatment, and because of the simple experimental situation. In vapor-phase growth the crystals are visible, whereas in growth from the melt they are not. But in 1964, experiments on the growth of high purity aluminum

---

[291] Annual Report, 1957: 51.

crystals by pulling from the melt were begun. Later this work would lead to important theoretical advances in studies of crystal form, known as morphological stability theory.

In 1959 the division began a program on nuclear magnetic resonance of metals and alloys. This was the division's entry into quantum mechanics and the electronic structure of metals and how it changed on alloying. Measurements were concerned with the Knight shift, which is the normalized difference in nuclear resonance frequency between a given metal nucleus in a reference compound and in a metal or alloy. Caused by the paramagnetic susceptibility of the s-like electrons at the Fermi surface of the metal, which cause a magnetic field at the nucleus, it gives information about the electronic density of states at the Fermi surface in the metal, and can be used as a complement to traditional methods in the study of crystallographic structure and phase transformations.

The study of electronic structure was reinforced in 1961 with the construction of a soft x-ray spectrometer, used to determine the density of electronic states throughout the whole conduction band and, in 1962, with a low temperature calorimeter, used to give the density of states at the Fermi level. The avowed ambitious aim of this whole effort was to "contribute critical data for the eventual formation of a quantitative theory of bonding in metals and alloys."[292] To further this provision of data, the division in 1966, with the help of the OSRD, began a center called simply the Alloy Data Center. In 1977 a massive four-volume compendium on Knight shifts, which gave a critical analysis of all the data in the literature at the time, was published.[293]

A third new effort, in diffusion, began in 1959 with the hiring of John R. Manning. The work was mostly theoretical. It was concerned with diffusion by vacancies and investigated such matters as vacancy flow in the presence of external driving forces, and correlations between atom motions. Experimental requirements led to the construction of an electron microprobe to measure the composition gradients in experimental diffusion samples. Due to the importance of diffusion to the science of metallurgy, an NSRDS data center, called simply the Diffusion Data Center, was opened in 1965. Its first publications were on diffusion in copper alloys, prepared with help from the International Copper Research Association.[294]

The final new effort was essentially electron microscopy coupled with x-ray diffraction. A great deal of the work was concerned with the direct electron microscopical observation of dislocations, a new capability eagerly pursued at the time in many laboratories. Beginning with the effect of dislocations on second phase precipitation

---

[292] Annual Report, 1961: 94.

[293] Gesina C. Carter, Lawrence H. Bennett, and Daniel J. Kahan, *Metallic Shifts in NMR* (Oxford: Pergamon Press, 1977).

[294] Daniel B. Butrymowicz, John R. Manning, and Michael E. Read, *Diffusion Rate Data and Mass Transport Phenomena for Copper Systems*, Part I (New York: International Copper Research Assoc., 1977), and Part II, 1981, with the same title but with Butrymowicz as the sole author.

(wherein the precipitate particles were initiated on dislocations) and the production of dislocations by plastic flow, the work developed into a study of stacking faults. These are caused in face centered cubic (FCC) metals by the splitting of a dislocation into two partial dislocations connected by a "stacking fault." This is a region of misfit between two crystal planes. The ultimate configuration is a strip of stacking fault with partial dislocations at its lateral edges, much like a street between curbs. The width of the fault increases as the energy of misfit between the atomic planes, or simply the stacking fault energy, decreases. Stacking faults are important since they have important effects on mechanical properties.

In an FCC metal, the stacking fault corresponds to a layer of close packed hexagonal (HCP) crystal. Now, some alloys of copper, silver, and gold show a phase transition from FCC to HCP at a given concentration of the alloying constituent. It follows that at the phase boundary the stacking fault energy should go to zero. Electron microscope studies in silver-tin alloys, extending well beyond the period, were consistent with this reasoning.[295] Begun during a period when direct observation of dislocations was a new science, it was a forward-looking piece of work.

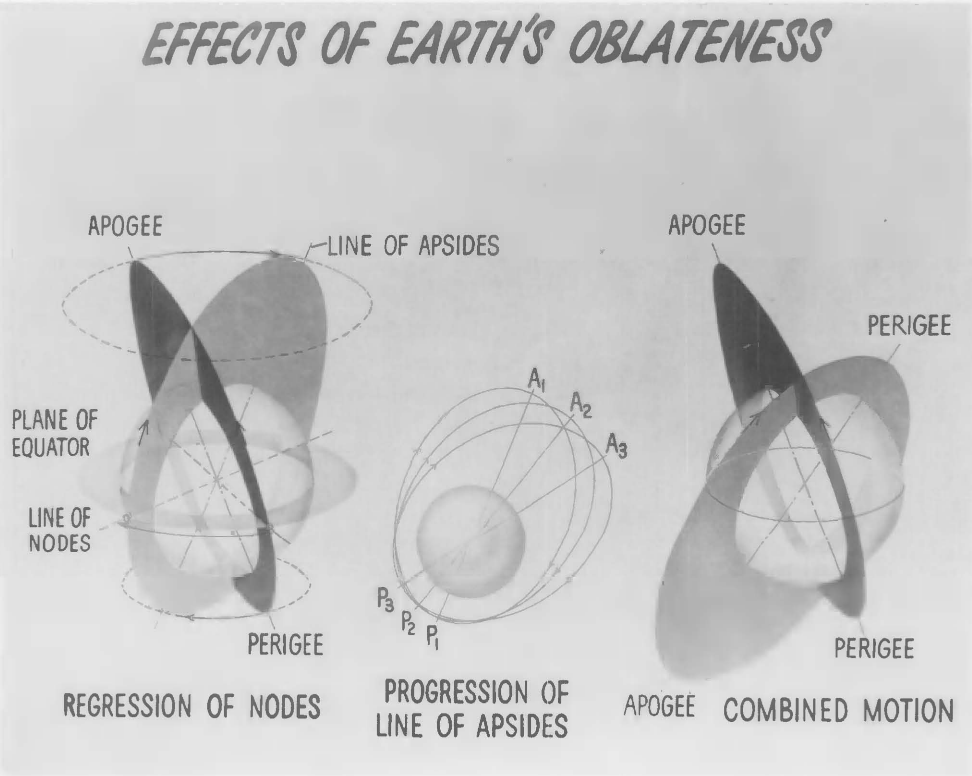
Applied Mathematics

The Applied Mathematics Division continued with its mission to carry out basic and applied research and to provide assistance and advisory services to the Bureau staff and other Government agencies. Much of the work was built around the use of computers, and the division had responsibility for the Bureau's computation laboratory, which provided computation service and technical assistance. This phase of its operation expanded during the period. Its IBM 704, obtained in 1957, was replaced in 1962 with a 7090-1401 system, with the 7090 in turn replaced by a 7094 in 1964.

While most of its scientific work continued in the areas of numerical analysis, statistical engineering, and mathematical physics, the actual problems undertaken reflected the changing nature of science and Government programs. Thus, in mathematical physics for example, a considerable amount of work sponsored by NASA was done on the theory of satellite orbits. This project was concerned with the motion of a satellite around an axially symmetric, but oblate planet, an approximation to the actual figure of Earth. Ramifications included taking into account equatorial asymmetry. Plasma physics was another mathematical physics area where the Bureau had a larger program. Part of this work was supported by the Bureau and was concerned with the transport properties of plasmas interacting with a magnetic field. Another aspect, supported by NASA, was concerned with the use of plasmas for rocket propulsion, and included plasma dynamics and the study of plasma oscillations.

---

[295] L. K. Ives and A. W. Ruff, Jr., "Extended Dislocation Nodes in a Silver-Tin Alloy," *Journal of Applied Physics* 37 (1966): 1831-1837; A. W. Ruff and L. K. Ives, "Stacking Fault Energy Determinations in HCP Silver-Tin Alloys," *Acta Metallurgica* 17 (1969): 1045-1055.



Previous efforts to calculate the effects of the earth's oblateness had used perturbation theory. In 1959, the Applied Mathematics Division at NBS devised a more direct approach to the problem which took into account the full oblateness of the earth at the very start of the calculation. (Courtesy of the National Aeronautics and Space Administration)

New areas came not only from mathematical physics, but also from mathematics and from the existence of the computer. These new areas were operations research, combinatorial analysis, and machine translation. Both basic and applied research were carried out in operations research. In the basic effort, there were studies in game theory, graph theory, weapons simulation, Boolean functions, and mathematical models of distribution networks. U.S. Post Office operations was a continuing area of application where, for example, studies of distribution networks had the aim of optimizing the location of mail distribution centers. A considerable amount of work was devoted to defense problems, such as the analysis and simulation of missile system operation, of electronic countermeasures, and optimal radar site distribution.

In mathematics, a new area of investigation was combinatorial analysis, the branch of mathematics concerned with the arrangement of finite sets of objects. It is essential in probability theory and in statistical mechanics. Its use was applied to the selection of the best pattern of linkages in transportation and distribution networks, in the determination of the most efficient method of encoding messages to provide automatic correction of transmission errors, and in the design of experiments to yield the maximum amount of information with the least effort. On a more theoretical note, the concept of abstract spaces was introduced into combinatorial analysis.

The other new area, carried on from 1959 to the end of the period, was machine translation. The project was concerned with the translation of Russian into English. Featuring a Bureau developed system called "predictive analysis," in which a Russian word in a sentence predicts "certain other grammatical forms, as transitive verb predicts an object," and a procedure called "profiling," by which clause or phase boundaries are recognized, the system was tested in 1964 with success as far as the limited amount of storage in the computer permitted. Some progress had been made on this very difficult problem, but the project ended before complete success.

At the very end of the period, the division acquired a new function. An Interagency Committee on Data Processing had been formed in 1957 to serve as a forum for departments to discuss ADP problems of mutual concern and to serve as a medium for the exchange of information. In 1962, the committee recommended the formation of an Advisory Council to advise the Bureau of the Budget (BOB) on "plans, policies, and guidelines for the ADP program of the executive branch."[296] Astin, asked to appoint a member, named Samuel N. Alexander to the post.[297] In 1963, the Council recommended the formation of a Computer Sharing Exchange (the Exchange) and Computer Service Center (the Center). In addition, the Bureau was recommended as the logical place to house the activity since it was already carrying out many functions of the same type. With some minor provisos, Astin accepted the assignment.[298] Acting quickly by means of Bulletin 64-9, January 2, 1964, the BOB announced the establishment of such an operation at NBS.[299] Astin followed immediately with an announcement to heads of all executive departments and establishments on "Plans for Operation of an Experimental Computer Sharing Exchange and Computer Service Center."[300] Essentially, the Exchange part of the experiment was to facilitate the sharing of computers between Government organizations, and the Center part was the provision of actual computation. For the latter, the Bureau provided the facilities of the Computation Laboratory in the Applied Mathematics Division, placing the Exchange and Center in that division, which had been carrying out nearly the same function for many years. It was nothing completely new, but Astin now had solid justification for it.

---

[296] Letter, D. E. Bell, Director, BOB, to A. V. Astin, October 1, 1962. (RHA; Director's Office file; Box 381; Folder Chrono. 1962)

[297] Letter, A. V. Astin to D. E. Bell, October 18, 1962. (RHA; Director's Office file; Box 381; Folder Chrono. 1962)

[298] Letter, A. V. Astin to H. Seidman, August 21, 1963. (RHA; Director's Office file; Box 381; Folder CHRONO 7/63-12/63)

[299] Executive Office of the President, Bureau of the Budget, *Bulletin* No. 64-9, January 2, 1964.

[300] Memorandum, A. V. Astin to the Heads of Executive Departments and Establishments, "Plans for Operation of Experimental Computer Sharing Exchange and Computer Service Center," January 17, 1964. (RHA; Director's Office file; Box 381; Folder CHRONO 1/64-4/64)

Data Processing

One of the Bureau's "special central missions" that accrued to it during and after World War II, the Data Processing Systems Division (renamed Information Technology in 1964) was a direct outgrowth of the Bureau's computer program. It served "both as a central research and development agency and as a readily available source of technical information for other Government agencies. The advisory services strengthen the basic program, which ranges from research in components and systems to advanced work in new computer applications."[301] Its work could be classified into four categories: the design and construction of new computers for special purposes; the study, analysis, and development of new components; the development of both general purpose computer programs and programs for specific purposes; and advice and assistance to other Government agencies.

The computer construction work was a direct result of the experience of designing and constructing the SEAC and SWAC computers. By the beginning of the period SEAC was seven years old, still going strong, and used for research on computer components. Nevertheless, this phase of the division's work would gradually lessen because of the explosive growth of the computer industry. New computers were constructed, particularly PILOT—a fast computer built specifically for data processing problems encountered in such Government operations as air-traffic control and patent searching. Another such special computer, AMOS IV, was constructed for the Weather Bureau to use in processing sensor data in an experimental automatic weather station. It was the fourth in a line of computers built for special Weather Bureau programs. A third computer was a portable analog device built for the Army Signal Corps that could predict radioactive fallout for a selected locality after a nuclear explosion.

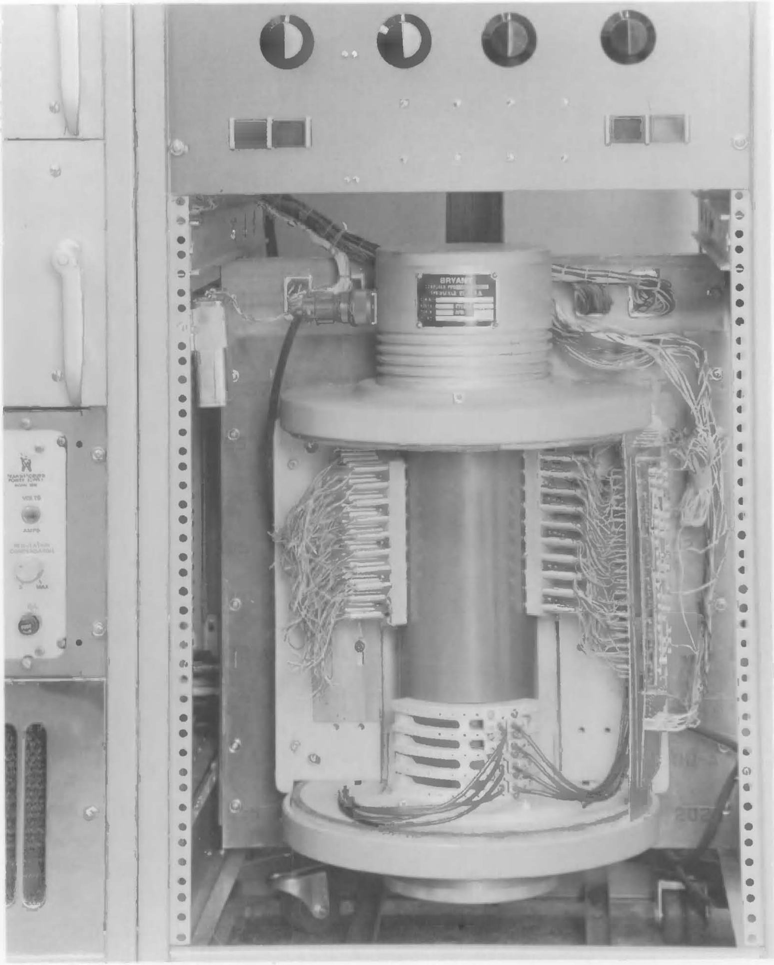
The evaluation and development of components was a continuing activity. As early as 1957, transistors and solid state diodes were under active investigation for switching and other circuits. Memories were a constant source of study, and in a 1959 example of the work, the need for large random-access memories with read-write times of less than one microsecond led to the investigation of thin magnetic films as storage elements. By 1964, the computer field had exploded to the point where two staff members "were directed to maintaining current awareness of the latest components, devices, and circuit techniques."[302] The work on components had developed into a full scale research effort, with studies on ultra thin ferromagnetic films, the quantitative analysis of thin film by x-ray fluorescence, and the simulation and characterization of tunnel diodes and epitaxial transistors. It was solid state physics applied to computer components.

Two important general purpose computer programs—OMNIFORM I and OMNITAB—were produced during the period. The former, written by Joseph Hilsenrath of the Thermodynamics Group and Gerald M. Galler of the Computation Laboratory, was based on a general program for generating sixteen types of functions, and could generate tables of such calculations as anharmonic corrections for diatomic

---

[301] Annual Report, 1960: 14.

[302] Annual Report, 1964: 184.

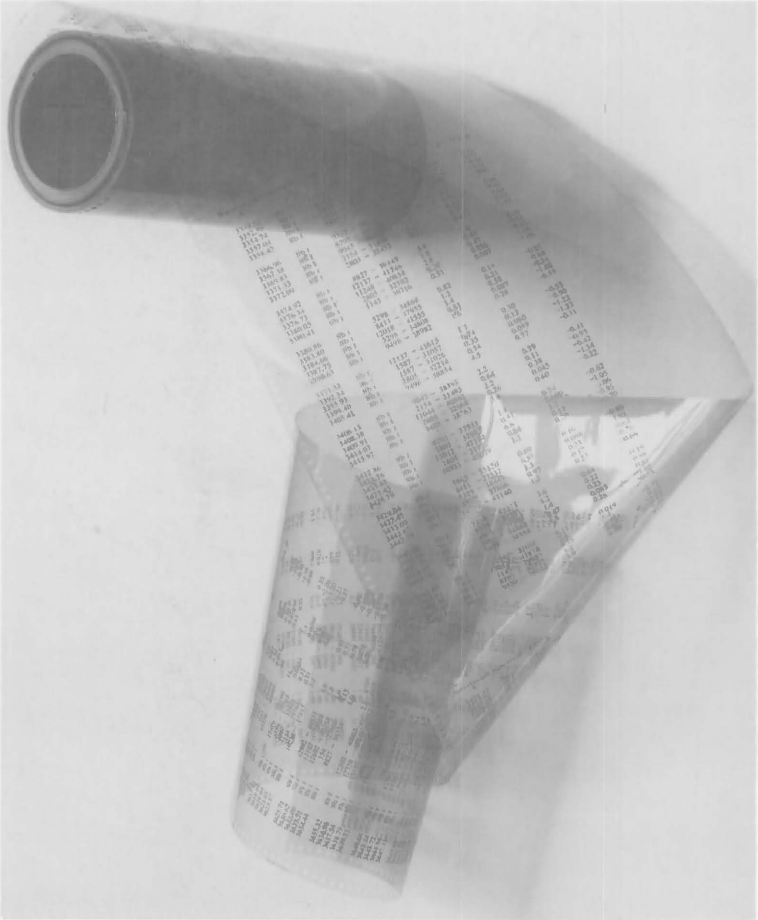


A view of the rotating magnetic drum, the main storage unit of the AMOS IV computer.

molecules virial coefficients for nitrogen gas, and negative exponentials corresponding to certain atomic energy levels. The related OMNITAB, developed by Hilsenrath, and Guy C. Ziegler and Philip J. Walsh of the Applied Mathematics Division, was a second generation general purpose program that permitted the user to instruct the computer by using simple sentences with familiar English words, such as "multiply *a* by *b*." It was written primarily for persons who would normally carry out computations on a desk calculator and a multicolumned worksheet, but who did not use Fortran. The Statistical Engineering Laboratory adopted OMNITAB and produced a number of publications.

Other special programs, generally developed with other Bureau scientists or for other agencies, included:

- Rapid calculation of color differences.
- Collaboration with the Patent Office to develop programs for the comprehensive search of chemical patent literature, and for general searching, but with emphasis on the mechanical arts.
- Analysis of data on human heartbeats for the Veterans Administration.
- A program to produce a magnetic tape which could instruct a photocomposition machine under current development by the Merganthaler Linotype Company and IBM. Developed because of the the need to publish extensive tables of atomic transition probabilities, this program brought the division into contact with the emerging field of computer-driven typesetting.



One of the rolls of 8 in wide photographic film positive from which printing plates were made for the Bureau's tables of experimental atomic transition probabilities.

A host of other agency projects were carried out, a number having already been mentioned. One of the most interesting was the formation with the National Science Foundation of a Research Information Center and Advisory Service on Information Processing in 1959. Located at the Bureau, the service was "designed to bring together research and development data on methods and equipment for the automatic processing of scientific information."[303] By 1964 it had collected 15 000 bibliographic references on information storage, selection, and retrieval. A few additional other-agency-funded projects in 1961 give a flavor of the work: assistance to the Bureau of Naval Weapons on problems of weapons-systems evaluation and test-range instrumentation of the Pacific Missile Range, studies of a future airtraffic control system for the Federal Aviation Administration (FAA), research for the Navy on computer methods for translating aerial photographic information into elevation profiles, and the development of a program for simulating municipal traffic flow by high speed computers for the Bureau of Public Roads. Unquestionably the biggest other-agency program was a continuing one with the Post Office Department on the development of methods for the most efficient handling of mail.

Instrumentation

What began in 1950 as a small office operating a program on instrumentation became a full scale division with five sections in 1960. This change was prompted by the transfer of the electronics experts from the Electricity and Electronics Division to form the technical staff of an Instrumentation Division. The Electricity and Electronics Division again became the Electricity Division, as it had been previous to the merger in 1953. The old Office of Basic Instrumentation became a true technical research division.

As one phase of its operation, the new division developed instruments for specific purposes, primarily for other agencies. A few examples illustrate the nature of this work:

- The development of automatic weather stations, under Navy sponsorship, to measure and broadcast meteorological information, and the installation of a chain of such stations in Antarctica.
- The study of hygrometry and the development of new hygrometers.
- The study of pressure and displacement transducers. A calibration service for these devices was provided.
- An electronic scanning microscope for automatically scanning spectrographic plates.
- FOSDIC (Film Optical Scanning Device for Input to Computers), the most famous of the devices invented by the division, was developed for the Census Bureau's 1960 decennial index. FOSDIC read responses collected during the census and converted them into electronic signals for direct input to the computer. Later versions were developed for the Weather Bureau,

---

[303] Annual Report, 1959: 6.

This ocean-based weather station developed by NBS automatically reported local weather data by radio. It could be anchored in a remote location and left unattended for as long as six months.

the Office of Emergency Planning, the Department of Defense for its National Fallout Shelter Survey, again for the Census Bureau for its monthly collection of employment and unemployment statistics, and for NASA, for its conversion of personnel records to magnetic tape storage. This diversity in applications required new versions of the device, and by 1964 FOSDIC IV had been developed.

Interesting and useful as these instruments were, perhaps more important was the more general research. In 1960 the new division recognized that modern instrumentation relied on electronic techniques, even when the initial measurement was not electrical but converted to electrical signals by specialized sensors. The division announced an electronics program which included the investigation of materials used in "vacuum and semiconductor electron devices," and the "characteristics and capabilities of electron devices themselves."[304] A large part of this program was the study of semiconductor materials, leading the division into applied solid state physics. Characterized by working with actual industrial materials, one of the first projects was a new

---

[304] Annual Report, 1960: 100.

method for the measurement of minority carrier lifetimes in silicon, silicon carbide, and boron phosphide. The method was applicable in the millisecond lifetime range in small samples of arbitrary dimensions. This was followed by a study of the important problem of second breakdown in transistors, a failure mode of great concern to industry. No specific cause was found, but guidelines were developed for the mode of operation where such failure would not occur.

Other studies were concerned with the contacts to semiconductors and, under sponsorship of the Bureau of Naval Weapons, standards and measurement methods for Hall effect devices. Such a standard developed by the Bureau was proposed to the International Electrotechnical Commission as an American standard.

Perhaps the most important program in this area was begun in 1964 to improve the fundamental accuracy with which the electrical properties of semiconductors could be measured. Sponsored by ARPA, the program concentrated chiefly on the measurement of resistivity at room temperature, which industry indicated was their most important need. Various types of probes were investigated, concentrating on the effects of surface finish and on probe materials. The investigation was continuing. Indeed, it and its derivatives would continue for the remainder of this history. By contrast, the effort in basic semiconductor research carried out in the Atomic Physics and Analytical and Inorganic Chemistry divisions, which at this time was concerned with TiO_2, would eventually be curtailed.

The whole effort in this area of electronic materials was characterized by a close interaction between the Bureau, industry, and the voluntary standards system, particularly ASTM. The Bureau carried out its mission of providing measurement methods and data, and of working with the standards societies in ensuring the technical soundness of their standards. This phase of its work would have increasing importance.

Central Radio Propagation Laboratory

As befits an organization whose work was described by the second Kelly Committee as being "of the highest quality,"[305] there was no need for the character of the work of the CRPL to change. Nicely balanced between basic and applied work, between science and engineering, it changed as science developed, and it grew. In 1957 appropriated funds were $2.12 million,[306] but increased to $4.28 million in 1964[307] plus an approximately equal amount transferred from other agencies, primarily the military. Organizationally, in 1958 the laboratory had two divisions, Radio Propagation Physics and Radio Propagation Engineering. By 1964 it had grown to four, with field stations spanning the planet either directly operated, on contract, or cooperatively manned with other countries.

The most famous program the laboratory was associated with in the period was the International Geophysical Year, of which we now give a brief account.

---

[305] Second Kelly Committee Report: 90.

[306] Appropriations Hearings for 1959: 412.

[307] Appropriations Hearings for 1965: 600.

The International Geophysical Year

On July 1, 1957, as many as 20 000 scientists from 67 nations began the International Geophysical Year (IGY), a concerted and coordinated study of the earth and its atmosphere. For eighteen months, until December 31, 1958, they were to make observations and measurements of the earth's interior, its crust, its oceans, its atmosphere, and its sun, in an effort to achieve a better understanding of mankind's home. Earlier concerted studies of this kind had occurred during the Polar Year of 1882-83 and the Second Polar Year, which began on August 1, 1932, they were dwarfed by the magnitude of the IGY effort.[308]

The idea for such a year appears to have occurred spontaneously in April 1950. British geophysicist Sydney Chapman, who had participated in the discovery of various layers of the ionosphere during the Second Polar Year, visited Washington. He and a number of other scientists visited the Silver Spring home of James A. Van Allen of the Carnegie Institution of Washington on what started out to be a social occasion. The group came to the conclusion that further advances in the earth sciences required an intensive coordinated worldwide effort, and that new instrumentation and techniques and the advent of rocketry made such an effort propitious. Their ideas were submitted to Lloyd V. Berkner, then president of the International Conference of Scientific Unions (ICSU), who warmly embraced the concept and pressed the effort. Seven years later the IGY began.

The choice of the period July 1, 1957, to December 31, 1958, was not arbitrary. A maximum in the eleven-year sunspot cycle was expected during this period, offering an unexcelled opportunity to study the effects of the Sun on the earth's atmosphere and climate with modern instruments and techniques. The ICSU invited all nations to participate, and forty-six accepted the invitation, twenty-one more would join later. Illustrating the growing technological capabilities of the period, the ICSU passed a resolution "urging that the participating nations study the possibility of launching artificial satellites which would [give] scientists their first sustained view of this planet from outside its atmosphere. . . . Both of the leading world powers had indicated their intention to launch such satellites."[309] In fact, the United States planned on twelve satellites to be launched in conjunction with the IGY.[310] It will be recalled that the sputniks were launched only a few months after the beginning of the IGY, and the U.S. satellite *Explorer I* shortly thereafter.

Following a request from the ICSU, and with the concurrence of Congress, in March 1955 the NAS appointed a committee to guide the national effort for the IGY, and to maintain liaison with the NSF, which administered the $39 million appropriated by

---

[308] An excellent account of the International Geophysical Year is given in three papers by Hugh Odishaw, all entitled "International Geophysical Year," (1) *Science* 127 (1958): 115-128; (2) *Science* 128 (1958): 1599-1609; (3) *Science* 129 (1959): 14-25.

[309] Walter Sullivan, "The IGY—Scientific Alliance in a Divided World," *Bulletin of the Atomic Scientists* 14 (1958): 69.

[310] Senate Committee on Appropriations, *International Geophysical Year: A Special Report Prepared by the National Academy of Sciences for the Committee on Appropriations of the United States Senate*, 84th Cong., 2d sess., 1956, S. Doc. 124: 17.

Congress for the effort. Called the United States National Committee, it was chaired by Joseph Kaplan, Professor of Physics from UCLA, and it had many parts: an Executive Committee, Committees on the Antarctic and the Arctic, Data Processing, a Continental Committee, and an Equatorial Committee. In addition, there were thirteen technical panels, ranging alphabetically from Aurora and Airglow to World Days. The Bureau was well represented on the committee. Alan H. Shapley of the Radio Propagation Physics Division was vice chairman of the main committee and also served on three subcommittees, chairing two technical panels (Solar Activity and World Days). Hugh Odishaw was executive director of the main committee, having taken a leave of absence from the Bureau to join the NRC staff specifically for this purpose. Allen Astin and Director Emeritus Lyman Briggs were members of the main committee. A number of other Bureau staff members served on subcommittees and technical panels.[311]

While the Bureau's effort in the IGY was only a small fraction of its activities, involving only about 100 staff members, primarily from the Radio Propagation Physics Division of the CRPL, it nevertheless played a significant role in the whole IGY effort.[312] Its two aspects might be called support, or staff functions and, of course, research functions in those areas where it had special competence. In the support functions, it operated as a World Warning Agency. Since the IGY had been chosen to coincide with a period of maximum sunspot activity, the Sun was under visual, optical, photographic, photometric, and radio observation for every minute of the IGY. The World Warning Agency, located at the Bureau's Fort Belvoir, Virginia, radio forecasting center, received all these data from all over the world, and processed them to maintain a constant account of the state of the Sun. During any periods of unusual activity, alerts were sent to scientists all over the world. In particularly interesting cases, "Special World Intervals" were declared during which scientific observations throughout the whole world were accelerated. Indeed, one such interval was declared even before the IGY formally began. A major solar flare was observed on June 28, 1957, two days before the official start of the IGY. The arrival on Earth of the particles associated with solar flares creates immense electrical disturbances in the earth's ionosphere, with radio signals being absorbed rather than reflected, thereby blacking out long range radio communication. Immediately on receipt of the news of the flare, an Alert was announced, and eight hours later, a World Interval was declared. Observations on the ionosphere, the Northern and Southern Lights, and the earth's magnetic field were intensified, and balloons and rockets were sent up to measure ultraviolet emissions, x rays, and cosmic rays. Observations, some by rocket, showed that a new layer of ionization was produced some twelve miles below the normally lowest point of the ionosphere, which remained otherwise undisturbed.[313]

---

[311] Ibid. This document gives a complete listing of all the parts of the committee and their membership. "The International Geophysical Year," *Technical News Bulletin* 39 (1955): 139, gives a listing of all Bureau representatives on the various parts of the committee.

[312] D. M. Gates, "Preliminary Results of the National Bureau of Standards Radio and Iononspheric Observations During the International Geophysical Year," *Journal of Research of the National Bureau of Standards* 63D (1959): 1-14.

[313] Odishaw, "International Geophysical Year," *Science* 127 (1958): 115.

Kent D. Boggs of NBS, one of the IGY World Warning Agency forecasters, entered solar data on a globe representing the Sun. The white chalk circles showed the location and size of sunspots.

The other support function was the operation of a data center. For the storage of the raw data accumulated during the IGY, three data centers had been instituted, labeled A, B, and C. Data center A was operated by the United States, with eleven sub-centers; center B was in the Soviet Union, with two sub-centers; and center C, with nine sub-centers, was operated by several European and Pacific Ocean nations. The Bureau operated a sub-center devoted to airglow and the ionosphere. The principal function of the centers was to store and index the data they received from all over the world, and to make them accessible to research workers.

The Bureau's scientific efforts were naturally concentrated in those areas in which it had the greatest research interests and competence. The most intensive effort was in the study of the ionosphere. The Bureau normally operated twenty sounding stations,

Reels of 35 mm film were kept in a humidified storage vault at the NBS Boulder Laboratories, forming a large part of the data cataloged and stored as the IGY World Data Center A for Airglow and the Ionosphere. Frances Stryker was photographed returning a reel to the rack.

but during the IGY it operated thirty-four, either directly or in cooperation with other nations.[314] The interest was in the study of the ionosphere in the region of the equator, for which a closely spaced chain of sounding stations (operating on a fifteen-minute

---

[314] The customary way of studying the ionosphere is by a process called "sounding," in which radio signals are sent vertically upward from a pulsed transmitter while slowly sweeping the radio frequency. A signal is reflected from the ionosphere when the frequency of the signal and the electron density in the ionosphere are appropriately related. Thus by observing the echoes from the ionosphere, it is possible to map out the electron density of the ionosphere as a function of the height. Based on such studies, the ionosphere is customarily divided into four layers: D (the lowest), E, F_1, and F_2 (the highest), although the layers overlap. It is this reflecting ability that makes long-range radio communication possible.

cycle) was instituted; the measurement of electron density profiles along the seventy-five degree meridian, from essentially the equator to Fort Monmouth in New Jersey; and vertical incidence sounding at five stations in Antarctica, where for the first time studies were made over either of the two poles. Surprisingly, these showed that there was a weak diurnal variation during the polar winter.

Another area of interest was the propagation of VHF signals in the Far East. It was known that propagation by the sporadic E layer[315] of the ionosphere was enhanced more in this area of the world then in comparable regions in the Western Hemisphere. Hence comparable propagation circuits were established in both hemispheres. Studies showed that the sporadic E enhancement was indeed greater in the eastern circuit, but that the F region also contributed.

Other areas of interest to the Bureau workers were the comparison of the propagation of VHF signals in the Far East and those in the Western Hemisphere; VHF forward scatter in the equatorial regions, which had never been studied systematically; the study of radio noise by a special network of IGY stations which showed that local conditions were important in determining the noise level; and airglow observations, where one of the attempts was to determine if there was any relation between this phenomenon and the aurora, with undecided results. Finally, there were some satellite observations. Within twelve hours of the announcement of *Sputnik I,* workers for the CRPL had modified existing pieces of equipment to receive the 20 MHz and 40 MHz signals from the satellite. By setting up interferometers at these frequencies, the "radio" direction of the satellite could be obtained and compared to the visual direction. Differences in these two directions could be attributed to refractive index gradients in the ionosphere at the radio frequencies, and thereby giving an idea of the structure of the ionosphere which could be compared to the sounding data. Similar and more extensive studies of this type continued with other satellites.

The CRPL participation in this type of international program did not end with the IGY. In 1964-1965, the IGY was followed by the International Year of the Quiet Sun (IQSY), chosen because the Sun was expected to be in a quiet phase. The interaction of the CRPL with this new program was much the same as it had been for the IGY, and consisted of coordination and direction of ionosphere and airglow projects, operation of a Western Hemisphere Radio Warning Service, and operation of the World Data Center-A for Airglow and Ionosphere.

***

There is perhaps no better way to illustrate the development of the CRPL program than its interaction with the Nation's space activities. The interaction was of two types: the use of rocket and satellite capabilities to carry out CRPL's own programs, and the provision of data and consultation for other parts of the Nation's program.

The utilization of space capabilities employed rockets and satellites to study the ionosphere from above—for sounding in the downward direction. The problem is that ground-based sounding cannot measure the electron density above the height of its maximum (approximately 300 km). Pulses that penetrate this region are not reflected,

---

[315] This is a portion of the E layer that appears and disappears intermittently.

Four-stage Javelin rockets were used to carry instrumentation above the ionosphere in suborbital tests of topside-sounding satellite payloads.

but are lost to space. To study the ionosphere above this height, soundings need to be taken from above, hence the name "topside sounder."[316]

Both rockets and satellites were used for this purpose. All launchings and flights were, of course, under the control of NASA. First, two Javelin rockets were launched from Wallops Island to a height of approximately 1000 km, the first on June 24, 1961, and the second on October 3 of the same year. With the aim of testing the system

---

[316] W. Calvert, R. W. Knecht, and T. E. Van Zandt, "Ionosphere Explorer I Satellite: First Observations from the Fixed-Frequency Topside Sounder," *Science* 146 (1964): 391-395.

that would be used later on satellites, the first rocket obtained pulse reflection for thirteen minutes. The second also worked flawlessly, and it detected local ionization irregularities about 20 km in diameter and an east-west spacing of 1 km to 30 km. The flight data also yielded information that led to a modification of the instrumentation.

The next experiment was not really a Bureau effort, but a Canadian one. This was a top-sounding satellite named *Alouette I,* launched September 29, 1962, which gave a considerable amount of new data, with the CRPL participating in its analysis. But the experiment was not completely satisfactory. The sounder was a swept-frequency device which required eighteen seconds per sweep, during which time the satellite moved 130 km, hence the technique was unsuited to studying irregularities in the ionosphere.

The final experiment was *Ionosphere Explorer I* (also called Ionosphere Explorer A, S-48, Fixed Frequency Topside Sounder Satellite, TOPSI, 1964 51 A, and Explorer XX), which took soundings at six fixed frequencies. With a time of only 0.105 seconds between successive soundings at the same frequency, the satellite moved only 800 meters between readings, making it much more suitable for studying lateral ionospheric irregularities. Like *Alouette I,* it was in a nearly circular orbit approximately 1000 km high with an inclination of 79.9°, so that it could study the ionosphere from the tropics to the arctic regions. It began yielding information from the start.

Not only did the CRPL use space technology to further its own program, it provided NASA with advice and consultation on communications problems. In a 1962 study begun before President Kennedy's announcement of a planned lunar landing, it provided information to help plan communications between points on the lunar surface, predicted the power required, and analyzed ground-proximity losses and noise sources. During the Project Mercury flights of astronauts Walter M. Schirra, Jr., and L. Gordon Cooper, special hourly forecasts of radio propagation conditions were issued for the high frequency circuits that comprised the ground communications network. And during the period between orbital flights, forecasts were issued weekly with daily updates as necessary. All of this was done to ensure reliability of communications in obtaining data from the astronauts and spacecraft equipment, and telemetering it to tracking stations and the Mercury Control Center at Cape Canaveral, Florida. The CRPL was in a central position to assure reliable communications for the space effort.

Cryogenics

As its name implies, the Cryogenic Engineering Division was a fully integrated engineering laboratory. Begun as a means of producing liquid deuterium for the hydrogen bomb, it became an essential part of the Nation's missile and space programs, concerned with all the problems involved in the production, transport, and use of the cryogenic liquids—primarily hydrogen and oxygen—that were crucial for those programs. Indeed, it was the sole supplier of those liquids in the early days. But it also provided measurement methods, data, and equipment design for science and the Nation's fledgling cryogenics industry. As an engineering laboratory, it was interested in producing things, not merely studying them, and as an integrated laboratory it developed the scientific and engineering background information necessary for the effort. Its work can be classified into four categories: materials, processes, equipment and instrumentation, and production of cryogenic liquids.

Materials studies were concerned with materials of construction, materials for sensing elements, and with the cryogenic liquids themselves. A long series of studies addressed the mechanical properties of stainless steels at temperatures as low as that of liquid helium, approximately 4 K. Chosen for their ductility at these low temperatures, some alloys showed the formation of the embrittling martensite phase upon storage and cycling to the low temperatures. However, detailed studies showed that the resulting two phase structure was quite well behaved mechanically.

One set of studies on sensor materials was concerned with thermocouples. Because thermocouples used at these low temperatures can be affected by heat conduction, the thermal conductivity of a gold-cobalt alloy widely used in such thermocouples was measured down to 4 K, which permitted the errors caused by conduction to be estimated. To round out the project, the temperature-emf relationship for this same alloy against copper and silver-gold alloys was determined down to 20 K for these widely used thermocouples. While no substitute for the platinum resistance thermometer, these thermocouples were much easier to use and provided an uncertainty of about 0.1 K, which was sufficient for most engineering purposes.

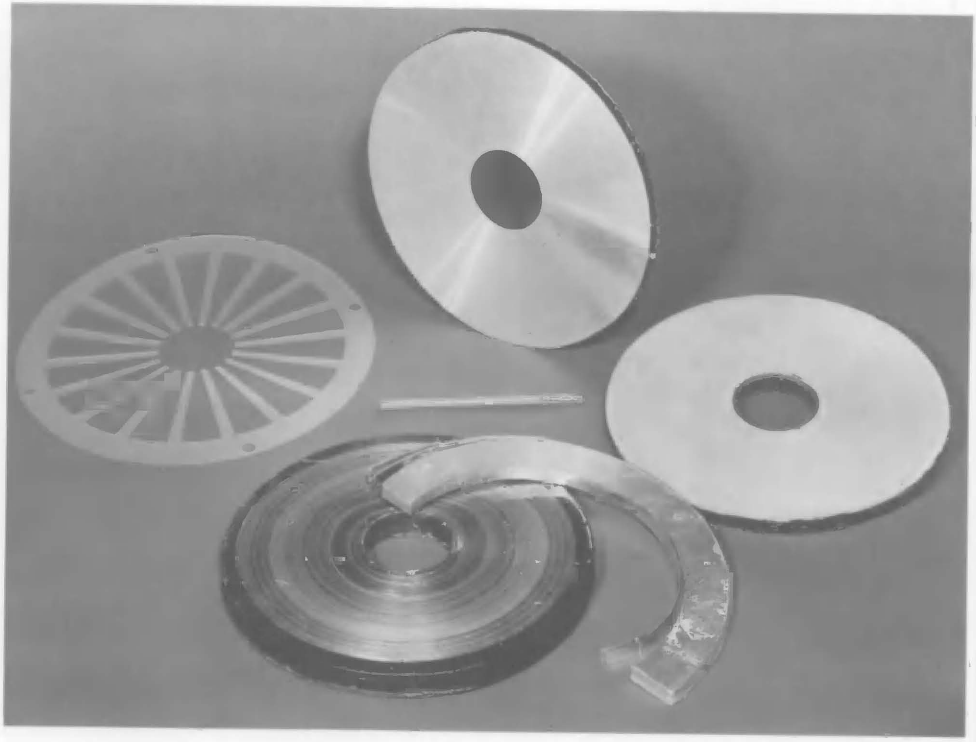
Cryogenic insulation was, of course, a constant concern. In 1959 a new insulation consisting of a laminate of alternate layers of aluminum foil and thin fiberglass batt or paper, provided a ten-fold improvement over the customary powdered insulation when properly handled and applied. Cryogenic liquids also received considerable attention. The PVT relationships for *para*-hydrogen over the temperature range 20 K to 100 K at pressures up to 350 atmospheres were determined, tables of thermodynamic functions computed, and dielectric constants and viscosity determined—all essential design properties for the use of liquid hydrogen in rocket propulsion systems.

Later in the period some of the work changed from this intensively applied character and became very basic. Thus, investigations were carried out on the superconducting energy gap in thin films, and experiments were underway in 1964 on the the Fermi surface of niobium which, alloyed with tin, forms a high-field, type II superconductor.

A number of processes peculiar to cryogenics were under constant investigation. Fluid flow, important for the design of flow meters, received considerable attention. For cryogenic liquids, flow is complicated by the fact that both liquid and vapor are generally present. This causes "choking" (or, in automotive parlance, "vapor lock") at constrictions such as valves, which greatly limits the flow rate. Idealized theoretical solutions for predicting the upper and lower limits for such flow were obtained and could be used as broad design guides. Refrigeration was another process under continuous investigation, and notably led to the development of an expansion engine rotating at 9000 revolutions per second to 10 000 revolutions per second. The 0.3116 in diameter turbine, which could be held in the palm of the hand, could produce 200 W at 21 K to 30 K, and 8 W at 4 K. In heat transfer, a problem develops when a very cold surface is exposed to the atmosphere, as occurs in vaporizers, uninsulated transfer lines, air dryers, and missile tanks. In such cases, frost forms, and studies on the frost formation and heat and mass transfer on such surfaces were carried out. Wind velocity, temperature, and humidity of the atmosphere on a surface at a temperature of 77 K were investigated. An interesting conclusion was that condensation which takes place in the boundary layer, but does not contribute to frost formation, is important in heat transfer.

It is not surprising to find an engineering division producing equipment. In fact, the division worked on pumps, bearings, transfer systems, containers, magnets, couplings, pressure transducers, flow meters, storage and transport equipment, and a liquid hydrogen bubble chamber. This equipment was produced primarily for the military missile programs and the civilian rocket and satellite programs, and included such items as a truck transport vehicle for 6000 gallons of *para*-hydrogen, and a distillation column for separating liquid hydrogen and deuterium. Perhaps most notable was the assistance provided to the University of California Radiation Laboratory in the construction of a liquid hydrogen bubble chamber to be used in fundamental particle high-energy physics. Successfully operated in 1958, the 72 in chamber contained 550 L of liquid

The 72 in liquid hydrogen bubble chamber at Lawrence Berkeley Laboratory. (Courtesy of Lawrence Berkeley National Laboratory) hydrogen, and a Bureau-designed hydrogen refrigeration system provided continuous cooling and temperature control. Unlike the rest of the division's output, this was a direct service to fundamental science, rather than to engineering programs. Another notable program somewhat out of the mainstream for the division at that time was in magnets. Here the aim was to produce very-high-field magnets for use in particle accelerators in high-energy physics, in magnetohydrodynamics studies, and for plasma containment in nuclear fusion reactors. Two routes were available, one via high-field superconductors, and the other via cryogenically cooled normal-metal magnets. In the former area, in collaboration with the Metallurgy Division, a magnet wound with niobium-clad superconducting Nb_3Sn wire produced in the Metallurgy Division was built and produced fields up to 190 000 gauss (19 tesla). It was a precursor of what would become standard magnet technology. The other route



In 1963, scientists at NBS helically wound alternate layers of aluminum foil and capacitor paper to form the coil of a high-field cryogenic magnet. This photograph shows slabs (center front) that were cut from the cylindrical coil, machined to a smooth surface (center rear), and etched with sodium hydroxide (right) to prevent metallic short-circuiting at the edges of the turns. Fourteen such slabs, stacked alternately with polyester separators (left), were connected in series by copper wires. The separators provided electrical insulation and channels for liquid hydrogen cooling medium.

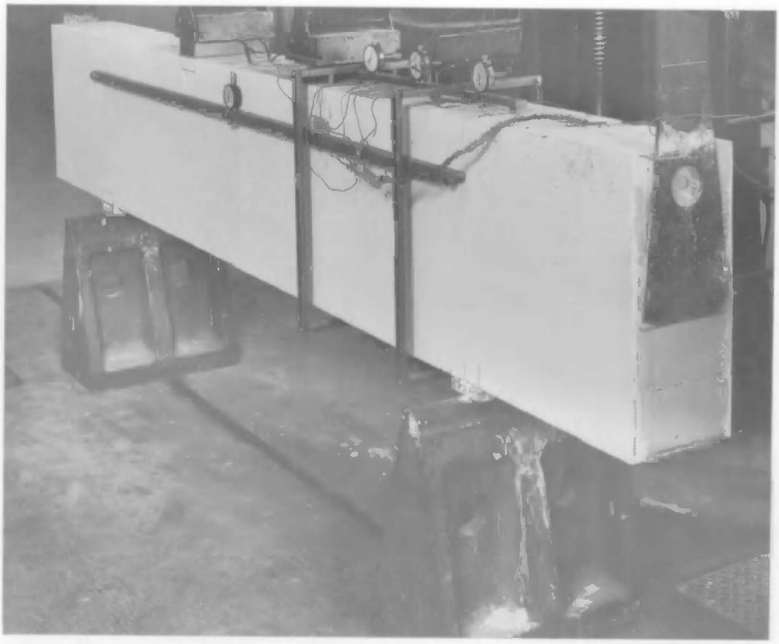
led to the production of a solenoid magnet using aluminum as the conductor. Made up of a stack of "doughnuts" formed of aluminum foil separated by paper, the magnet was cooled by liquid hydrogen and produced a field of 70 000 gauss (7 tesla) in a hole cavity 3 in in diameter and 8 in long. It was an impressive achievement, but such technology would be superseded by the superconducting magnet.

The division operated a production plant that produced cryogenic liquids shipped throughout the whole Nation. The peak production was in 1959 when the division produced 280 000 L of liquid *para*-hydrogen, 1 200 000 L of liquid nitrogen, and 4800 L of liquid helium, about half of which was air-shipped to Washington for use in the free-radicals program. With the ending of that program and the expansion of commercial production, this phase of the division's activities began to decrease. By 1963 all the liquified gases used in the Boulder Laboratories were procured from commercial sources; none was produced by the division. However, the helium liquefier was maintained in a standby mode in view of the uncertainty of commercial availability. The liquid hydrogen plant was still used for research purposes, but the liquid nitrogen plant was declared surplus. While any ending is sad, there was the consolation that it was the success of the division's program that ran it out of the cryogenic liquid business.

Finally, we should mention another important part of the division's activities. In 1958 it began a Cryogenic Engineering Data Center, which periodically published compendia of data and bibliographies. In 1962 it began emphasizing thermophysical properties, and in 1964 it became part of the recently-formed NSRDS. From input to output, the division was a complete operation.

Building Research

As a central agency in the Government for the advancement of knowledge in the building sciences, the Building Technology Division, renamed the Building Research division in 1961, carried out a broad program on building systems, components, and materials. From plumbing systems to roofs, from concrete beams to heat pumps, from thermal insulation to asphalt, there was hardly a portion of building science not covered by its work. During the period most of the work was a continuation of that done from 1950 to 1957, but there was one important new phase. In 1960, the cement testing program was moved from the Mineral Products Division to the Building Technology Division, along with the associated research on cement. There concrete was added to the testing function, and the study of cement and concrete became the single most intensively studied area in the division's total program. Some of the work was, indeed, on the basic side. For example, in a study of how the cement-aggregate bond in concrete was affected by shape and chemical factors, polished aggregate specimens of various materials were partly imbedded in cement and the shear strength of the bond determined. In another area, the study of crack growth in concrete beams indicated that fracture mechanics could be applied to the problem.



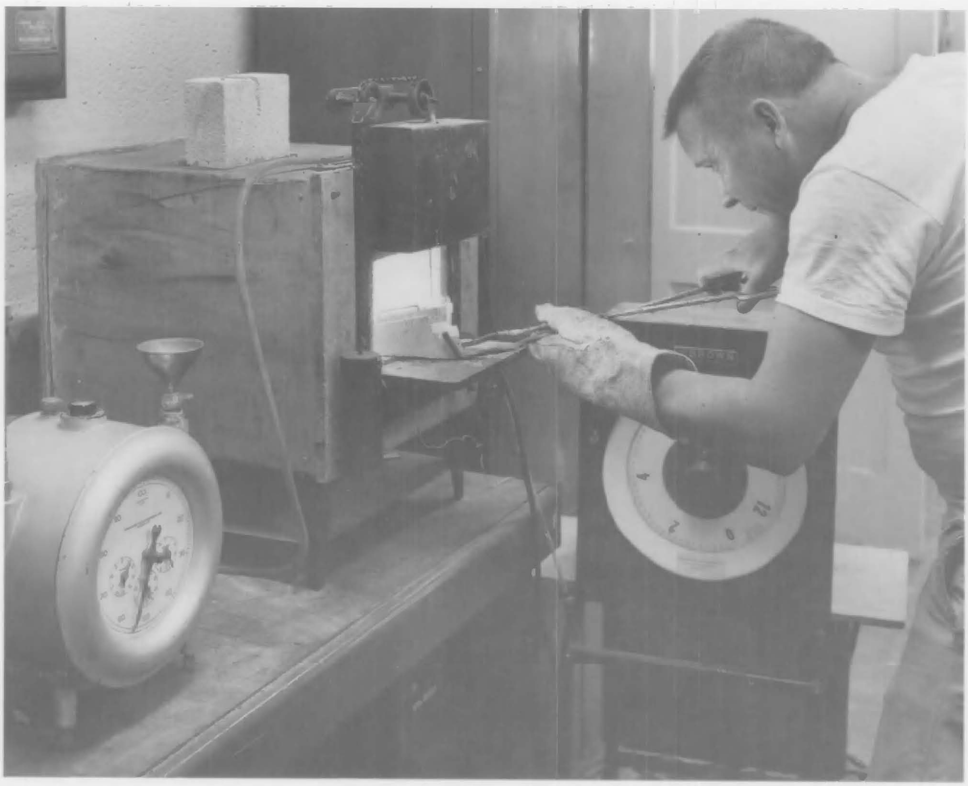
A test setup at NBS for loading reinforced concrete beams. A number of beams were loaded to failure to study the redistribution of stress as the beams cracked.

Another area that received considerable attention was the measurement of thermal conductivity in thermoelectric materials used for direct conversion of heat energy to electricity. Here the work was concerned with developing new methods of measurement and providing specimens of the right shape and size to be used as thermal conductivity reference standards for investigations of solid semiconductor materials. Thus, in 1960, an apparatus using small specimens (0.5 in by 1 in diameter discs) was developed for determining the steady-state thermal conductivity of semiconducting materials. In 1963 a new method of measurement using radial heat flow was developed. Thermal conductivity reference standards over a range of conductivities and temperatures were made available through the Standard Reference Material activities.

As in the previous period, asphalt was a material of continuing study. A particularly interesting result showed that the asphaltene fraction (the dispersed phase) of asphalt carries a positive electric charge and shows electrophoretic mobility when suspended in nitromethane. However, attempts to correlate electrophoretic mobility with durability were unsuccessful, indicating that the polar bonding forces had little effect on this important property.[317]

---

[317] Annual Report, 1963: 189; J. R. Wright, R. R. Minesinger, "Velocity-Depth Relationship in Microelectrophoresis Cell for Asphaltenes in Nitromethane," *Journal of Colloid Science* 18 (1963): 802-804.



Joseph J. Loftus placed specimen container and firebrick support in a muffle furnace for firing. This was one step in the procedure developed at NBS for measuring the potential heat of materials in building fires.

Fire was, of course, an important part of the division's program, with studies of the fire hazards of common combustible materials, of fire retardants for fabrics, of the mechanisms of fire extinguishment, of surface flammability, and of the potential heat of materials in building fires.

Two other projects show directions in the development of fire research. In the first, the electron attachment coefficient of gaseous flame-inhibiting agents was studied. Coefficients considerably greater than that of oxygen were observed for twenty-three halogenated hydrocarbon agents. A qualitative correlation with flame inhibition was found. but electron attachment was not a sufficient condition for flame inhibition since some substances with high coefficients showed no flame inhibition.[318]

---

[318] T. G. Lee, "Electron Attachment Coefficients of Some Hydrocarbon Flame Inhibitors," *Journal of Physical Chemistry* 67 (1963): 360-366.

Finally, there was a project in the area of structural endurance in fires. In it, James V. Ryan and Alexander F. Robertson attacked the problem of finding a criterion for the onset of collapse of a structure in a fire, i.e., the condition at which a test assembly exposed to a fire fails to sustain the applied structural load. In a concise and straightforward way, they provided an analysis and a simple load-failure criterion for floors and beams based on both a limiting deflection and a limiting rate of deflection.[319] While not extensively used in the United States, this criterion has been adopted rather widely elsewhere.[320]

---

[319] J. V. Ryan and A. F. Robertson, "Proposed Criteria for Defining Load Failure of Beams, Floors, and Roof Constructions During Fire Tests," *Journal of Research of the National Bureau of Standards* 63C (1959): 121-124.

[320] Daniel Gross, private conversation.