Establishing the Meteorology Program

Powered by solar energy equivalent to nearly seven million atomic bombs, persistent winds weave vast three-dimensional patterns of which our daily weather charts show mere eddies.

—Harry Wexler, 1955

The International Geophysical Year (IGY), which ran for the eighteen-month period spanning 1957 and 1958, is well known as the formative event of the space race that took place during the 1960s as well as that of the National Aeronautics and Space Administration.¹ The new American space agency took its initial research agenda from the IGY’s Earth satellite program. It also became the institutional home for a long-term effort to construct both a global dataset and a real-time global observation system that could feed future global operational forecast models data.

Harry Wexler, the Weather Bureau’s chief of research, had been an early advocate of satellite meteorology. In fact, he had been a literal visionary. Well prior to the first satellite, he painted a picture of what Earth might look like from space—cloudscape and all. It now belongs to the National Air and Space Museum in Washington. Wexler’s artistry represents one of the major goals of meteorologists of mid-century: a detailed understanding of the general circulation of Earth’s atmosphere.
NASA was tasked in its 1958 charter with the development of space applications. Communications and weather satellites were its two earliest effects, and it was in the applications directorate that the meteorology program was located. NASA, of course, was not itself responsible for weather forecasts. That responsibility belonged to Wexler’s U.S. Weather Bureau. Hence the aim of the NASA program was to develop the application of satellite meteorology for the Weather Bureau’s ultimate use. This made the two agencies partners, in essence, if not always comfortable ones.

The NASA meteorology program was part of a larger effort within the meteorology profession to reconstruct meteorology along the lines of the so-called hard, or physical, sciences. A handful of meteorologists had sought the ability to use the fluid dynamics equations to predict the generation and movement of weather since the early years of the twentieth century. Because the fluid dynamics equations are nonlinear, and because the atmosphere is complex, computation of the weather proved extraordinarily difficult. It could not be done successfully until the development of the stored-program digital computer after World War II. This device, even in its early, primitive form, could out-calculate rooms-full of trained human computers, as they were then known. Human computers were rapidly replaced by this new machine, which then opened new realms of computational possibility. One of the first fields of science transformed by the computer was meteorology. Global data from satellites seemed to promise a second revolution in forecasting, permitting accurate forecasts months in advance.

**Numerical Forecasting**

Prior to the twentieth century, meteorology was practiced two ways. Climatologists collected and analyzed regional datasets, generating mean and average values and finding regularities in these averages. They could also make reasonable statements about the frequency of major phenomena, such as hurricanes. The calculations necessary to do this were time consuming and did not permit prediction of short-term phenomena such as weather. At best, climatologists could identify long-term trends once they began. The second component of meteorology, forecasting, was more art than science. Beginning in the 1840s, the development of telegraph networks allowed rapid collection of surface data, which became the basis for synoptic meteorology. Forecasts placed data on maps, and based on their training and experience they could make reasonable predictions of the next day’s weather. Over many years of experience, regional forecasters developed a detailed understanding of how weather typically happened in their area of respon-
sibility, making weather prediction a matter of both data and experience. It was not, however, a modern physical science, based upon the known laws of physics and quantitative analysis.

At the end of the nineteenth century, a few individuals began promoting reconstruction of meteorology along physical lines. Cleveland Abbe in the United States and Napier Shaw in Great Britain had each argued that the time had come to make meteorology a modern physical science, and Abbe set out a system of seven equations that could permit calculation of the weather. The most effective promoter of dynamical meteorology, as the nascent discipline was called, was Vilhelm Bjerknes, founder of the Bergen School of meteorology and pioneer of the highly successful (but still not physics-based) system of air mass analysis. Bjerknes had contended that the physical laws by which the atmosphere functioned were already known, in the principles of fluid dynamics and thermodynamics. He gathered many converts, including the first person to actually try to calculate the weather, Lewis Fry Richardson.

Richardson, an Englishman, Quaker, and pacifist, carried out his effort to calculate the weather during his service as an ambulance driver during World War I. He developed a set of partial differential equations and a numerical method of solving them via approximation in order to carry out his trial. He had collected large amounts of data from throughout Europe to feed into the calculation; lastly, he had had to develop methods to fill in parts of the map where data had been unavailable. Once all this work was completed, he had then spent six weeks carrying out the calculations for a six-hour forecast for only two locations on his map. And the resulting forecast was wildly off the mark, with one of the two positions having an error significantly greater than the location’s natural variability. His results were lost for several years, then found under a coal heap in Belgium and returned to him. Richardson expanded and published the results of this first attempt to calculate the weather in 1922, in a magnum opus titled *Weather Prediction by Numerical Process.*

Richardson’s effort, despite having produced an inaccurate forecast, was not ignored. It was widely read and commented upon, and it was highly regarded within the community of research meteorologists. It was a first try at a new method of prediction, made with inadequate data. Yet Richardson’s methodology was rigorous and complete. Meteorologists chose to believe that Richardson’s methodology was the correct way to go about calculating the weather, but that the sheer enormity of the calculations had prevented success. Indeed, in his book Richardson had imagined the scope of the “weather factory” necessary to numerically predict the weather in real time. He envisioned a vast hall filled with 64,000
“human automata” using desk calculators and communicating via telegraph, engaged in calculating the weather.⁶ Hence no one followed Richardson’s lead for many years.

The development of the digital computer in the final years of World War II permitted the meteorological community to revisit numerical weather prediction. John von Neumann, an already-famous mathematician at the Institute of Advanced Studies in Princeton, had collaborated on the ENIAC computer project and in the process had developed the logical structure that eventually formed the basis of all stored-program digital computers. He sought funding for such a machine for use in scientific research. He had been introduced to Richardson’s work by Carl Gustav Rossby at the University of Chicago during a meeting in 1942, and became interested in applying the digital computer to weather prediction in early 1946. After a meeting with RCA’s Vladimir Zworykin and the head of the U.S. Weather Bureau, Francis W. Reichelderfer, and more than a little enthusiastic advocacy by Rossby and the Weather Bureau’s chief of research, Harry Wexler, von Neumann decided to establish a meteorology project associated with the computer he was trying to build, the EDVAC.⁷

The numerical meteorology project crystallized under Jule Charney in 1948, after several other project leaders had left for other tasks. Charney, who had completed his PhD work at UCLA but had been strongly influenced by Rossby at Chicago during a nine-month stay in 1946, had carried out an examination of why Richardson’s prediction had been so far off prior to joining the project; once in Princeton, Charney developed a new methodology that replaced Richardson’s seven primitive equations with a single equation. This placed the computing needs within the expected performance of the EDVAC, and in fact could be solved (if slowly) by hand. Known as the barotropic model, Charney’s model made several unrealistic assumptions, but it produced reasonable twenty-four-hour forecasts. It degraded quickly after that, though, primarily due to its assumption that all processes were adiabatic, in other words, occurred without energy exchange. In 1951, Norman Phillips joined the team after completing his dissertation research at Chicago. Phillips developed the first baroclinic model, a two-layer model that permitted energy exchange. This produced very successful twenty-four-hour forecasts in 1952.⁸

Charney’s group believed that the baroclinic model’s forecasts still had value out to forty-eight hours, a significant improvement over those achieved by state-of-the-art human forecasters most of the time. Several government organizations perceived value in the group’s models as well, and the Joint Meteorology Com-
committee, administratively under the Joint Chiefs of Staff but composed of representatives of the air force’s Air Weather Service, the civilian Weather Bureau, and the Naval Weather Service, advocated formation of an operational numerical prediction center that would serve the three organizations. Known as the Joint Numerical Weather Prediction Unit, this was established in July 1954 and became operational in 1955, using models developed by Philip Thomas and George Cressman, both of whom had experience at the Institute for Advanced Study (IAS) meteorology project. Cressman became the unit’s head, and eventually became the director of the U.S. National Weather Service.

The models developed at the IAS project prior to 1953 were all regional in extent. Because the weather was not regional, but traveled globally, this limited their predictive range to two days or so. Longer-range prediction required models that were at least hemispheric in extent. In 1955, Phillips demonstrated the first of a series of general circulation models that accurately replicated the seemingly permanent, large-scale structures of the global atmosphere, such as the jet stream and the prevailing winds. Researchers in several places began to conduct general circulation experiments after Phillips’s demonstration. While these were not forecast models, their success, combined with the success of the regional forecast models developed in the IAS project, indicated that global weather prediction could be accomplished.

The principal challenge facing researchers interested in developing global circulation models, and particularly those interested in extending the useful length of weather forecasts after the mid-1950s, was data. Charney’s group at IAS made use of data collected in previous years to initialize their forecast models, and could compare the model output to the actual weather as recorded by the Weather Bureau. One of the most convincing experiments had been a successful retrospective prediction of an unusual winter storm on Thanksgiving Day, 1950, by Phillips’s baroclinic model. This experiment had demonstrated the superiority of the baroclinic model over Charney’s older barotropic model, which had not generated the storm from the same initial dataset. Model researchers believed that they could only improve model performance by comparing their model’s output to real data. But there was no such dataset against which to compare the detailed performance of global models. There had never been a reporting network in the Southern Hemisphere, and from the standpoint of meteorological researchers of the early 1950s the Southern Hemisphere remained a vast terra incognita. The situation in the equatorial belt was no better. Data for the Northern Hemisphere existed and was collected routinely after World War II in support of the ongoing
military operations of the United States. This data, once centrally archived and evaluated for quality, permitted the construction of hemispheric prediction models during the late 1950s. But global models had to wait for a global dataset.

The small Weather Bureau modeling effort, finally, was not the only one in the United States during the 1950s. At UCLA, Yale Mintz and Akio Arakawa were also developing general circulation models. The armed services more generally were interested in atmospheric models, which could aid in everything from predicting weather for flight planning to understanding the effects of nuclear weapons. Will Kellogg, a UCLA doctoral graduate who had moved to RAND, worked on local-scale models to allow prediction of the dispersion of radioactive fallout from nuclear weapons tests during the 1950s, for example.\(^\text{13}\) He also co-wrote one of the earliest proposals for satellite meteorology.

**ROCKET RESEARCH, THE IGY, AND THE PROMISE OF GLOBAL DATA**

NASA’s science program of the 1960s had its roots in an informal, self-appointed group of physicists that had formed in 1946 around Germany’s V-2 rocket. Calling themselves the V-2 Panel, and later the Rocket and Satellite Research Panel, this rather casual entity used many of the hundred or so V-2s assembled from parts collected in Germany at the end of the war for upper atmosphere research. They were primarily interested in investigating the ionosphere and the regions above it using new instruments and techniques that they devised for themselves. The members of the group were employed by a variety of universities, including Princeton, Johns Hopkins, Iowa State, and Harvard, and by military agencies, particularly the Naval Research Laboratory (NRL). Their funding came through various mechanisms from all three armed services, which sought better understanding of the upper atmosphere’s radio characteristics to improve radar and radio performance.\(^\text{14}\)

When the V-2s ran out, the panel had utilized U.S.-built sounding rockets to continue their research. Simultaneously, the U.S. government spent vast sums developing longer-range liquid-fueled rockets to serve as delivery systems for the atomic bomb. These new rockets offered a tantalizing future opportunity to obtain information about the Earth from outside it—a truly global dataset. The potential was not lost on the few individuals who had clearance to know about the rocket research. The RAND Corporation, founded in 1945 to serve as an advisor to the Army Air Forces, had started looking at the possibilities inherent in orbital observation posts in 1947. Writing about his experiences in the RAND project
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during the late 1940s, meteorologist William Kellogg explained that what RAND most needed was evidence that a higher-altitude perspective would be useful to weather prediction.¹⁵ The V-2 Panel provided it in 1949, producing a series of cloud photographs from a V-2 launch.

Many scientists not involved with rocket research were dismissive of it. Historian David DeVorkin has argued that the rocket researchers were almost entirely outsiders, scientists who were conducting research in well-established scientific fields without being members of the relevant research communities.¹⁶ They were members of a new technical culture interested in producing new research capabilities, new ways of doing research. They were less interested in the results, and given the unreliability of rockets, they often achieved little but frustration for their efforts. But they were by and large not deterred either by frustration at rocket and instrumentation failures or by the skepticism (and frequently outright hostility) of more traditional scientists. Because their patrons were the armed services, and the services recognized the importance of improved understanding of the high altitudes even if the rest of the scientific community did not, their funding, while hardly infinite, was assured.

Activism by the members of the rocket research panel, and particularly by Lloyd V. Berkner, aimed at increasing the stature of rocket research helped lead to a 1950 proposal that 1957 be declared an “International Geophysical Year.”¹⁷ The idea had gotten its start in James Van Allen’s living room, where a number of the rocket researchers had gathered to meet geophysicist Sydney Chapman.¹⁸ It grew out of a discussion of the need for a “Third Polar Year” to expand scientific understanding of the complex polar atmosphere.¹⁹ The “year” was to run for eighteen months, and it was chosen to coincide with the solar sunspot maximum. During the IGY, a wide range of scientific studies would be carried out planet-wide by international teams of scientists. The organizers hoped IGY would include Antarctic studies, investigation of the airglow phenomenon that occurred at various altitudes, and deep-sea experiments, in addition to a great deal of rocket research. It would also involve extensive field expeditions, including multiyear ocean studies and Antarctic exploration. In 1952, the International Council of Scientific Unions (ICSU) accepted the American proposal, making the project international in scope.

Late in 1952, the United States started organizing its IGY effort by forming the National Committee for the International Geophysical Year (USNC). The USNC’s task was coordination of what would be a very large effort encompassing a number of government agencies and many universities. It was formed under the auspices of the National Academy of Sciences, which had been chartered by
Congress in 1863. The Academy represented the United States within the ICSU, the parent organization of the IGY, and it also served as a source of scientific advice to the government. The Academy was technically a nongovernmental organization, however, and placing the USNC outside the government permitted the civilian scientific mission of the IGY to remain paramount. The USNC’s task was coordination and selection of experiments, which it did through a series of committees that recommended which experiments should be funded. The funding agency for the IGY’s scientific effort was the National Science Foundation (NSF), which let grants and contracts to experimenters based on the USNC’s recommendations.

The chairman of the committee was Joseph Kaplan and his executive secretary was Hugh Odishaw. Kaplan was a physicist at the University of California, Los Angeles, who had specialized in the spectra of diatomic molecules commonly found in the upper atmosphere. He lent strong support to the idea of orbiting a satellite during the IGY, and when President Dwight D. Eisenhower approved the satellite idea in July 1955, formed a Technical Panel on the Earth Satellite Program. The Technical Panel’s function was to solicit scientific proposals and select the experiments that would fly aboard the nation’s first satellites. Its members were Kaplan and Odishaw, Homer E. Newell, Jr. of NRL, William H. Pickering of the Jet Propulsion Laboratory (JPL), Athelstan Spilhaus of the University of Minnesota, Lyman Spitzer, Jr. of Princeton University, James A. Van Allen of the State University of Iowa, and Fred Whipple of the Smithsonian Astrophysical Laboratory. Newell, Pickering, Spitzer, and Van Allen were also members of the Rocket and Satellite Research Panel. This network of close affiliations and overlapping committee memberships would eventually help smooth space science’s transition from the Technical Panel to NASA three years later. In 1955, this group held both the scientific knowledge to recognize valuable experiments and the rocket engineering experience to select experiments that might be possible within the IGY’s time horizon.

The Technical Panel hoped to orbit twelve satellites during the IGY—actually, they were more realistic in hoping for six successes out of twelve tries. By the end of 1955, the group already had five proposals in hand. The first had been Van Allen’s proposal for a cosmic ray experiment, received shortly before the panel had even been appointed. Others included S. Fred Singer’s proposal to measure erosion of the satellite’s skin by micrometeoroids and Herbert Friedman’s proposal to measure variation in the intensity of solar Lyman alpha radiation. These were proposals by insiders, people already well-connected to the IGY program, but the panel also took steps to recruit new experimenters. They began the search
for more proposals by holding a symposium in Ann Arbor, Michigan, in January 1956 on “The Scientific Aspects of Earth Satellites.” The symposium was organized by Van Allen and Odishaw, and was attended by about fifty geophysical scientists invited by the panel. The prospective researchers heard briefings on both the potential of satellite research and the technical constraints that satellites would impose on experiments, in order to help them plan valid experiments (and reduce the number of infeasible proposals received by the Technical Panel).

The thirty-three papers presented at the symposium reflected the breadth of possible atmospheric research programs, with a strong bias toward the upper atmosphere and magnetosphere. Three experiments were aimed at determining atmospheric pressure and density by various means, while five addressed measurements related to the Earth’s magnetic field. Radiation at high altitudes drew six papers, and in two papers, experimenters proposed meteorology experiments. There were also three papers on micrometeoroids and erosion of the vehicle’s surface and three more oriented at the ionosphere. The balance of the papers covered technical aspects of satellite research, including tracking and telemetry requirements.

The actual selection of experiments was made by a subset of the Technical Panel, the Working Group on Internal Instrumentation. This proved to be a dynamic process, with no single decision date. Instead, the Working Group met when it had a batch of new proposals to evaluate. They also met to assess the progress of already chosen instruments. They prioritized experiments, evaluated which experiments could be packaged with others in the very small Vanguard satellite (50.8 centimeters in diameter) without causing interference, and provided an ongoing source of encouragement and advice. They did not limit their support to experiments that could be carried out during the IGY; they also recommended funding of experiments that almost certainly would not fly during IGY but would be scientifically worthwhile in some future program. One experiment that was a relative latecomer to the program is worth following to detail how experiments (and perhaps more important, experimenters) moved from the IGY effort into the NASA era.

On 31 May 1956, Harry Wexler wrote to Joseph Kaplan to propose an experiment designed to determine the Earth’s heat balance—the difference between the incoming solar radiation and the outgoing infrared energy radiated by the Earth itself. Wexler, who had helped push John von Neumann into setting up Charney’s meteorology group at the Institute for Advanced Study (IAS) in 1946, was involved in research on weather control and modification, and he was also heavily involved in the IGY’s Antarctic research efforts. He proposed this experi-
ment because the Earth’s global, short-term heat balance was of great value to meteorologists. While incoming and outgoing radiation had to match over the long term (or the Earth would heat or cool very rapidly), it might vary substantially over the short run or in localized areas, which would affect weather patterns. For the same reason, the energy balance was useful to numerical modelers, who used an average value developed by Julius London in the early 1950s from ground-based measurements as an input. London’s value was widely believed to be flawed simply because he had possessed no way of determining the amount of energy that reached the top of the atmosphere from the Sun. Better predictions required a better understanding of the Earth’s energy flows, and these could only be measured adequately from space.

Wexler did not provide any technical detail in this letter, but a few days later he presented more detail to the Technical Panel. At the University of Wisconsin, Madison, a group of researchers in the meteorology department headed by the department chairman, Verner Suomi, had sketched out a lightweight device that could measure the three parameters necessary to calculate the heat balance. Suomi had done his doctoral dissertation at the University of Chicago on the odd subject of measuring the heat balance of a cornfield. To measure the Earth’s heat balance, he proposed using a set of three sensors. One sensor would measure the incoming solar radiation. A second sensor would detect the energy reflected off cloud tops, which had to be subtracted from the incoming radiation. Earlier ground and balloon experiments had estimated this reflected energy could be up to 35 percent of the total. A third sensor would detect the Earth’s outgoing infrared energy. Wexler still did not present a proper technical proposal, which he told the group would be submitted by Suomi.

Suomi’s detailed proposal was finally forwarded to the Technical Panel for evaluation in October 1958, and was promptly recommended for approval by the Working Group and assigned the number ESP-30. Because it was a meteorology experiment, the National Academy of Science’s Committee on Meteorology (chaired by the ubiquitous Lloyd Berkner) weighed in with a recommendation of its own, that “at the earliest feasible moment” a heat balance experiment be flown. At its 5 December meeting, the USNC approved both Berkner’s recommendation and Suomi’s experiment, and on 16 December Suomi submitted his proposal to NSF for funding. His proposed budget was $75,000, a number that would become $131,000 as the experiment played out.

The Wexler-Suomi experiment was assigned to experiment group IV, which included another meteorological experiment designed by William G. Stroud at
the U.S. Army’s Signal Engineering Laboratories at Fort Monmouth, New Jersey. This sensor was designed to produce images of the Earth in the near infrared using a pair of lead sulfide detectors. But the Technical Panel did not believe that both experiments could be supported all the way through flight primarily due to funding limitations. The Technical Panel could not decide which experiment was of greater value to meteorology, however, and to help it make what it termed an “agonizing” decision, it sought the opinions of research meteorologists in several universities and air force laboratories. At a meeting to be held 5 November 1957, the panel would use their recommendations to choose between the two.²⁶

Suomi’s experiment was chosen, but that proved not to matter. In the very short run, Stroud’s experiment continued under army funding. It would not be part of the Vanguard satellite program, but Vanguard very quickly ceased to be the only satellite program. The Soviet Union’s orbiting of Sputnik 1 on 4 October 1957 disarrayed the entire IGY satellite program. As the next two years played out, both experiments were able to fly as more and more resources were devoted to finishing the IGY program and demonstrating that U.S. science was not inferior to Soviet science.

Major changes occurred around the Earth satellite program in Sputnik’s vast wake and the subsequent explosion of the first Vanguard shot in December. The Army Ballistic Missile Agency (ABMA) in Huntsville, Alabama, where Wernher von Braun’s rocket engineers had carried out a series of successful suborbital reentry tests during 1957, received permission to enter the satellite race using its relatively (compared to Vanguard, anyway) mature Jupiter rocket. ABMA’s Project Orbiter, proposed in conjunction with JPL in Pasadena, had been Vanguard’s chief competitor in 1955 but had been shelved after President Eisenhower had chosen Vanguard. Orbiter had never been far from von Braun’s mind, however, and after Sputnik he was able to convince his superior, General John B. Medaris, to let him prepare for a January 1958 launch. Medaris assigned the actual satellite to JPL, giving that organization its entry into the satellite business.²⁷

The January 1958 launch gave JPL less than ninety days to produce a satellite and deliver it to Huntsville, which was possible only because one of the IGY’s instruments had already been prepared. In his memoir, James Van Allen reported that Ernst Stuhlinger of ABMA had told him about the Jupiter’s progress in early 1957 and in April 1957 an ABMA group had visited him in Iowa and given his instrument team specifications for a Jupiter C payload. He had used the information to produce a version of his cosmic ray instrument compatible with the Jupiter just in case Vanguard failed. Because Medaris had decided on his own authority
to prepare for a Jupiter shot even before Vanguard’s explosion, JPL’s Pickering (who was also a member of the Vanguard Technical Panel) had Van Allen’s instrument transferred to Pasadena in November 1957.28

JPL’s satellite, Explorer 1, went into orbit successfully on 31 January 1958. In addition to being the first American satellite, it also produced radiation data that was entirely unexpected and caused JPL to put several more copies of Van Allen’s instrument into orbit to verify and expand upon its results. Explorer 1’s data showed much higher levels of charged particles than current theory predicted, but the dataset was very incomplete. Explorer 1 did not have the ability to store data taken during its orbit for retransmission when it passed over the ground station, limiting the utility of its data. Subsequent Explorers launched in February (which did not reach orbit) and March had tape recorders to permit capture of a full orbit’s data. Explorer 3’s data allowed Van Allen to determine that the high radiation levels were real and theorize that they were a product of the Earth’s magnetic field, which was trapping charged particles at certain energy levels and confining them to belts around the planet.

Van Allen’s radiation belts, as they quickly became known, were the first major scientific return from the IGY’s Earth satellite program, and the Explorer series of satellites made JPL a major center for space research. JPL’s sudden ascent out of obscurity challenged NRL’s previous dominance of the budding field of space science while not yet really changing the scientific goals of the overall space program. ABMA’s challenge to Vanguard also had no effect on the science agenda in the short term because Medaris had chosen to give JPL the satellite task. Because JPL director Pickering was one of the V-2 Panel veterans on the Technical Panel for the Earth satellite program, this group continued to set the scientific agenda for the nation.

That began to change in June, however. On 4 June 1958, the National Academy of Science’s president, Detlev Bronk, met with Alan Waterman of NSF; Hugh Dryden, chairman of the National Advisory Committee on Aeronautics (NACA); Lloyd Berkner; and Herbert York, chief scientist of the Defense Department’s Advanced Research Projects Agency (ARPA). They agreed to establish a new panel within the Academy to permanently supervise the nation’s space science effort. They chose Berkner to be the chairman of the new Space Science Board (SSB), carved the amorphous term space science into seven scientific disciplines, and assigned SSB a set of tasks, including completion of the IGY satellite program and coordination of the nation’s space program.29 Unlike the Technical Panel, however, SSB was not to be an operating agency. Once its IGY role was over, it would not continue to actually run the science program—it would advise.
Hugh Odishaw became SSB’s executive director when it began operating in June. The new organization very rapidly absorbed the satellite effort from the IGY’s Technical Panel, and it began planning for an expanded post-IGY effort. On 11 June, George Derbyshire sent Odishaw a list of the instruments that needed to be completed and a handful of technical problems that needed to be solved to carry out an expanded space science program, including on-orbit stabilization, de-spinning of the satellite after launch, and recovery of instrumentation or samples dropped by satellites. Carrying over the rest of the instruments into whatever future program emerged was going to cost about $6 million, and the next day Odishaw wrote to Alan Waterman at NSF to request $2 million of the sum. He also put in a plug for money for the Weather Bureau, which had been denied a supplemental request for $75,000 to fund processing of the data it expected from Suomi’s instrument.30

Lloyd Berkner called together the first meeting of SSB’s members on 27 June. The members were Leo Goldberg, H. Keffer Hartline, Donald F. Hornig, Richard Porter, Bruno B. Rossi, Alan H. Shapley, John A. Simpson, Harold C. Urey, James A. Van Allen, O. G. Villard, Jr., Harry Wexler, Harrison S. Brown, W. A. Noyes, Jr., and S. S. Stevens. NACA’s Hugh Dryden attended as an invited guest, because his organization would become the nation’s new space agency within a few months. Of this group, only four were actively involved in space research already, and two were members of the self-appointed Rocket and Satellite Research Panel (Van Allen was its chair) and of the Technical Panel on the Earth Satellite Project.31 This reflected a deliberate attempt to broaden the constituency for space research and reach out to a scientific community that was quite skeptical about the endeavor. From this meeting came a decision to immediately solicit more experiments from a wider variety of researchers and to establish subcommittees representing each of the seven newly defined space science subdisciplines plus five more devoted to various non-disciplinary technical issues. The disciplinary subcommittees would evaluate the research proposals received and recommend funding over the next several months, and they would also craft longer-range research plans that would be handed to NASA when it opened its doors.

In soliciting experiments, Berkner continued to seek expansion of the constituency devoted to space research. Whereas the Technical Panel on the Earth Satellite Project had deliberately restricted its call for experiments to those with preexisting experience, Berkner decided to cast a very wide net. He dispatched a telegram on 4 July to 150 university science departments and private research institutions asking for proposals “within a week.” SSB received more than two hundred proposals and requests for more information in response. As retired
NASA scientist John Naugle pointed out, Berkner’s telegram was inspirational, particularly among young scientists looking for new fields in which to make names for themselves. While most of these proposals could not be funded immediately—at its second meeting on 19 July, SSB approved six for recommendation to the three extant funding agencies, NSF, ARPA, and NACA—they reflected a groundswell of interest in space science that was a product of Berkner’s activism in the context of Cold War competition with the Soviet Union.

On 1 October 1958, NASA replaced NACA, initiating a period of uncertainty about who would establish the goals of the nation’s space science program and choose the experiments conducted in it. The new agency had no space scientists yet. But President Eisenhower intended NASA to be a space science agency. He and his scientific advisors did not think that the engineering effort involved in putting men in space would be particularly useful. Robotic satellites could provide scientific and intelligence information much less expensively. And he clearly didn’t yet perceive the propaganda value of Men in Space. While authorizing the Mercury program, he kept it small, and he eschewed the Moon. So NASA, in his intended incarnation, was to develop space science and space applications. It needed scientists.

The new administrator, T. Keith Glennan, had kept ex-NACA chairman Hugh Dryden as his deputy and appointed the Lewis Flight Propulsion Laboratory’s assistant director Abraham Silverstein head of the agency’s Office of Space Flight Development. Silverstein knew he needed a chief scientist but hadn’t yet offered the job to anyone. He hired NRL’s Homer Newell after Newell and his NRL colleagues John W. Townsend, Jr. and John F. Clark came to see him to discuss the future of space science in the agency. Newell became assistant director for space science on 20 October, reporting to Silverstein. Newell put Clark in charge of ionospheric research and Townsend in charge of space research. Newell told SSB at its third meeting on 24–26 October that he intended to bring his entire NRL staff with him to NASA; as his efforts played out, NRL allowed him “only” fifty people. Newell’s fifty NRL veterans, along with others recruited from the Air Force Cambridge Research Laboratory and the Army Signals Engineering Laboratory and all of Project Vanguard, formed the core of a new research center on the site of the Beltsville Agricultural Research Center near Greenbelt, Maryland. Formally named the Goddard Space Flight Center on 1 May 1959 in honor of the American rocket pioneer Robert Goddard, this center became the focus of NASA’s meteorology program.

The third meeting of SSB marked the beginning of what Newell called in his memoir his “love-hate relationship” with SSB. NASA’s charter made it, not SSB,
responsible for the nation’s space science program, but SSB expected to continue its prior role of soliciting and selecting proposals. NASA, in SSB’s view, would provide engineering services and launch vehicles to outside experimenters, but it would not have scientists or a science program of its own. This led to a brief skirmish between NASA and SSB that NASA easily won. In December, the new NASA administrator, T. Keith Glennan, approved a policy document that reserved to NASA the right to establish the research priorities of its program and choose the specific experiments and schedules for them. But it would use the recommendations of SSB and independent proposals in formulating its overall research program. It did not end SSB’s attempt to retain for itself a larger role, which continued for almost another year. The new policy, though, did permit SSB to remain a strong influence on NASA’s plans.

Writing to NASA Administrator Glennan, Alan Waterman at NSF, and the director of ARPA on 1 February 1959, Hugh Odishaw forwarded SSB’s recommendations for the future program. This document established long- and short-range goals, chose experiments, recommended experimenters, and suggested satellite packages and schedules. It reflected Berkner and Odishaw’s ongoing attempt to secure control of the space science program, but it also was an invaluable aid to NASA in establishing its own program. The program Homer Newell described in his “National Space Sciences Program,” circulated internally and to SSB on 16 April and presented to Congressman Albert Thomas’s Appropriations Subcommittee on Independent Offices on 29 April, drew heavily on SSB’s language.

Newell cast the NASA space sciences program into the subdisciplines SSB had created: atmospheres, ionospheres, energetic particles, magnetic and electric fields, gravity, astronomy, and biosciences. He enumerated a very similar set of existing problems scientists faced in understanding the nature and functioning of the Earth’s atmosphere, including the structure, circulation, and dynamics of the high atmosphere; sources of energy within it; the relationship between it and the Van Allen radiation belts and between it and the lower atmosphere; and the high atmosphere’s detailed chemical composition. He did not exclude the lower atmosphere from his presentation, noting the major problems as its radiation budget and how it affected circulation and weather, the same scientific concerns SSB had stated.

In defending his request for $45 million in fiscal year 1959 to carry out his space science agenda, Newell had to repeatedly emphasize potential practical benefits from space science, an ongoing need that he knew from several years of personally defending the IGY program before Thomas’s subcommittee—when Newell was out of the room, Thomas congratulated Glennan and Dryden for hiring such a
“topper” away from NRL. Newell also linked the NASA research program directly to the IGY effort, telling Thomas that

our immediate program is to carry on with the momentum developed during the IGY, and to study the atmosphere at even higher altitudes. Before proceeding to show you the program, I would like to point out that in the process we have had this practical application: we have developed an engineering standard atmosphere from the rocket data obtained to date. This standard atmosphere is used in the design of aircraft and vehicles that would fly at these levels. . . . In the future we can hope to find out the relations between the upper atmosphere in this region and the lower atmospheric weather phenomena. This is the sort of thing that will be fundamental to a truly universal application of, say, meteorological satellites.\(^\text{40}\)

In linking the NASA program to the IGY, Newell was arguing that the agency was expanding upon a successful scientific research effort, not engaging in the “wasteful duplication” that congressional funders did not like. And by pointing to a pair of economically valuable applications of the research, he drew on the notion that scientific advance produced economic gains. Finally, he cast the NASA science program as an orderly, essentially linear transition from the IGY’s, ignoring the still somewhat messy relationship between SSB and NASA.

In the short run, nearly all of the IGY experiments were carried out between 1958 and 1960 using either the Vanguard rocket or Jupiters. The SSB had gotten funds from NSF after Sputnik to repackage most of its instruments for the JPL Explorer satellite; Vern Suomi’s heat balance instrument was built in flight-worthy form for both Vanguards and Explorers. It ultimately flew aboard Explorer 7 on 13 October 1959, and was celebrated by Wexler as the first completely successful meteorological experiment in space. Stroud’s cloud-cover experiment, which had been launched aboard Vanguard 2 on 17 February 1959, had worked as designed but the satellite’s motion in space was not what the experiment required. Its data could not be resolved into images as intended.\(^\text{41}\)

The IGY itself concluded in 1960. The data generated by the event’s scientists was deposited in three World Data Centers for safekeeping and public access. Beyond its data and the space race, the IGY left a legacy of more formalized international scientific cooperation through a series of new committees attached to ICSU. One of these concerned space cooperation, the Committee on Space Research (COSPAR).\(^\text{42}\) The IGY also left a permanent imprint on the Earth’s most inaccessible region, Antarctica, where several nations, including the United States, established permanent research facilities. The continent itself was set
aside as a research laboratory for any nation via treaty concluded in December 1959. The U.S. site, McMurdo Station, would eventually host NASA research expeditions.

THE NASA METEOROLOGY PROGRAM

In addition to the atmospheric science research program NASA had adopted from the IGY effort, it gained an important space application that also came to include extensive meteorological research from ARPA: a weather satellite project known as TIROS (Television-Infrared Observations Satellite). ARPA, in turn, had acquired it from the army. NASA leaders had pursued transfer of TIROS because it would provide an early, highly visible success, thus helping to validate the new agency’s existence. But the public (and congressional) reaction to TIROS was so powerful that the agency expanded its efforts, becoming involved in a long-term collaboration with the Weather Bureau to advance weather satellite technology and to demonstrate the value of that technology by undertaking meteorological research.

TIROS was a moniker that accurately reflected the project’s origin in a surveillance satellite program. In the late 1940s, at the RAND Corporation in Santa Monica, researchers had started thinking about the uses of space, despite the current technological inability to reach it. Their first report on the subject, “Preliminary Design of an Experimental World-Circling Spaceship,” in May 1946, had discussed at a general level the potential economic and military value of space. In May 1947, another report had analyzed the possibilities of space for military surveillance and intelligence collection. In 1950, RAND had pointed out that space surveillance could produce an unimaginably vast amount of data, and that planners would have to find ways of handling the torrent. The following year, RAND scientist William Kellogg had published a study of a meteorological satellite that helped inform the later IGY effort.

In 1956, the U.S. Air Force had initiated a program known as WS-117L to develop a reconnaissance satellite. Lockheed won the air force’s contract; RCA, the unsuccessful bidder, turned to the U.S. Army for funding. The army accepted RCA’s proposal and initiated a reconnaissance satellite program of its own, named JANUS. This was administered by ABMA. When ARPA was formed in 1958, JANUS became an ARPA project, although it remained under the administration of ABMA. At the same time, responsibility for reconnaissance satellites was removed from the army and JANUS had its television cameras detuned to a resolution that would serve for meteorology but not for intelligence gathering.
name was also changed to TIROS. Kellogg became chairman of the TIROS project’s science advisory committee, helping define the vehicle’s observing capabilities.48

When Dryden and Silverstein had begun organizing NASA in 1958, they had assigned Edgar Cortright to ARPA’s Ad Hoc Committee on Meteorology, which oversaw TIROS. ARPA sponsored a two-day meeting on the subject of meteorological observations from space on 18 and 19 June 1958, after which Cortright summarized the technical possibilities. In addition to TIROS, the air force was proposing use of the WS-117L vehicle (ultimately known as Agena) for a WS-117W, a three-axis stabilized, 1360-kilogram polar-orbiting Orbital Meteorological System.49 This WS-117W was to measure cloud cover, cloud definition, cloud layers and thickness, moisture content, ozone content, wind direction and velocity, albedo, the spectra of incoming radiation, heat balance, and lightning location—a tall order for a system that was to be operational by 1964.

The day before signing NASA’s founding legislation, President Eisenhower had decided that meteorological satellite development should go to the new agency, and Cortright arranged for it to transfer on 13 April 1959. Cortright had also sought to bring researchers with relevant expertise into the agency, and he recruited William Stroud from Fort Monmouth to head the NASA meteorological program. Cortright, whose title was chief of advanced technology programs, assigned TIROS to the Goddard Space Flight Center.50 He arranged for the creation of a Joint Meteorological Satellite Advisory Committee in May 1959 to coordinate meteorological satellite research and development efforts, composed of members from NASA, the Defense Department, and the Weather Bureau.51 And finally, he hired Morris Tepper, a researcher at the Weather Bureau, to direct the agency’s meteorology program at the headquarters level.

TIROS was initially a three-satellite project, and each satellite was to have three instruments. The largest would be the television system for transmitting visible light images to the ground. The television system consisted of two lenses, one wide-angle lens imaging a square area about 1287 kilometers on a side, and a narrow-angle lens that photographed a much smaller area inside the same region. The second instrument intended for it was an improved version of Stroud’s infrared sensor that had flown on Vanguard 2. It could detect five different wavelength bands, allowing it to sense reflected solar radiation, water vapor absorption in the atmosphere, outgoing long-wave infrared radiation, and visible light. The third instrument was a lightweight version of Suomi’s heat balance device. The first TIROS, however, flew with only the television camera.

TIROS 1 was launched on 1 April 1960 into an equatorial orbit, from which it
could photograph the Earth between 50 degrees north and south latitude. It began sending back images almost immediately, and over its 76-day life it transmitted 22,952 useable photographs. The images revealed structural features in clouds that were entirely unexpected. It provided the first photographs of oceanic storms, revealing a spiral-banded structure like that of hurricanes and photographing an unreported typhoon near New Zealand. TIROS also showed that mountain-wave cloud structures were much larger in scale than previously believed, extending from the Andes mountains across the entire width of South America and displaying short- and long-wave structure that was also unexpected. Finally, it showed that unexpectedly rapid changes in the cloud patterns of vortexes occurred in the early phases of storm system formations. At the third meeting of SSB’s Committee
on the Meteorological Aspects of Satellites in June, Wexler told the group that the TIROS images had triggered a review of convective cell research to try to explain why cloud structure was so much more variable than previously believed and assumed clear organization over such different scales.\(^5\)

The first TIROS also revealed that the Earth’s magnetic and gravitational fields could affect spacecraft in low-Earth orbits in ways that could be used to help stabilize future spacecraft. TIROS 1, which had the shape of a short but wide cylinder, was spin-stabilized around its short axis to keep it from tumbling in orbit. Because of this, the satellite did not maintain a constant orientation with respect to the Earth below it; instead, its orientation changed continuously, with the cameras actually pointed at space more than 90 percent of the time. For the same reason, when they were pointed at Earth, the cameras also photographed the Earth at different angles. Using the photographs, William Bandeen and Warren Manger found that TIROS’s orientation to the Earth was varying in a way that was unexpected.\(^5\) It was experiencing precession, or wobbling, imposed by an outside source. The gravitational torque exerted on it by the Earth could not explain this.\(^5\) Instead, it was the product of two forces. An interaction between the Earth’s magnetic field and the magnetic field of the satellite exerted the primary force, while gravity imposed a lesser one. The magnetic field of a satellite could be varied deliberately by adding a loop of wire and controlling the magnitude of the electrical current within it, permitting a simple means of improving the stability of future satellites.

TIROS 1 was also enormously popular in the public arena and the more important political realm. While Van Allen’s radiation belt discovery was probably more famous, TIROS provided results that were far more visible to average people (and politicians). Its photographs of the Earth’s cloud cover placed a phenomenon of everyday life into a new context. The photographs of oceanic clouds suggested to anyone interested in the weather that TIROS could photograph storms at sea before they reached land, significantly lengthening storm warning times. The satellite received a four-column, front-page article in the *New York Times*, which also reproduced two of the satellite’s first photographs.\(^5\) Walter Sullivan, the paper’s science writer, pointed out that TIROS presaged an “era when such vehicles [would] produce a constant stream of information of great, immediate value in our daily lives.”\(^5\) Other outlets followed the *Times*’ lead, making TIROS and its instantly recognizable cloud photographs famous.

The Weather Bureau’s Wexler, who had been involved in attempts to produce cloud photographs using rockets in the late 1940s, had foreseen the potential of meteorological satellites well before TIROS moved to NASA. Late in 1958, he
had arranged for the establishment of a Meteorological Satellite Division in the Weather Bureau to begin looking at ways to link cloud-cover photographs to the standard weather maps that forecasters used, and he had also gotten NASA’s assurance that while it would produce the early experimental weather satellites, it would not seek to make itself the operator of whatever permanent operational
weather satellite system emerged. NASA also did not intend to carry out meteorological research using them, and when it adopted TIROS from ARPA in early 1959, it transferred $2 million to the Weather Bureau to fund its research into weather satellite operations and applications.\textsuperscript{58}

Late in June 1960, SSB reviewed the satellite meteorology programs at NASA and the Weather Bureau. Summarizing the results for President Eisenhower’s science advisor, Lloyd Berkner stated that the TIROS 1 had been “immediately useful,” but there were significant challenges in interpreting the data. He continued that SSB believed that the Weather Bureau should have responsibility for the “basic design of the satellite observational systems, data analysis, and research,” and therefore it should receive the funding for those efforts. NASA was currently paying for this, but it did not intend to support “exploitation of meteorological satellite data for either research or operational purposes subsequent to fiscal year 1961.”\textsuperscript{59} Berkner was endorsing an arrangement already made between Francis Reichelderfer, chief of the Weather Bureau, and NASA Deputy Administrator Dryden during April and May.\textsuperscript{60}

The next step toward formalizing the relationship between the two agencies came in September, when Administrator Glennan invited Frederick Mueller, the secretary of commerce, and Elwood Quesada, administrator of the Federal Aviation Agency, to a lunch meeting to discuss the operational meteorological satellite program that Wexler and Reichelderfer sought. Glennan wrote that the three needed to clarify the responsibilities for carrying out satellite development, systems integration, and data processing so each agency could prepare its funding requests to the Budget Bureau.\textsuperscript{61} At their 10 October meeting, they agreed to establish a new Interagency Meteorological Satellite Planning Committee that was to be chaired by a NASA official and that was to produce a development plan. Wexler protested, however, that NASA should have no role in planning operation of the system, and instead he wanted the system planning to be carried out within the existing National Coordinating Committee for Aviation Meteorology, which the Weather Bureau controlled.\textsuperscript{62}

Reichelderfer took the issue to Glennan again in late November, gaining Glennan’s agreement to use the existing National Coordinating Committee, with the addition of NASA members and a Panel on Operational Meteorological Satellites that would do the planning.\textsuperscript{63} Reichelderfer also received assurance that NASA’s leaders would tell their staffs to stay out of the operation of operational satellites. This was probably necessary because no one in NASA’s meteorological satellite office, now run by Morris Tepper, or in NASA’s science program more generally, believed that the weather satellite technology was really ready to be
declared operational.\textsuperscript{64} Nor did they believe that the Weather Bureau had created the capability to effectively process and disseminate the vast flood of routine data that even the very simple TIROS would produce. Nonetheless, political pressure to produce an operational weather satellite system quickly led to the formulation of a plan for a National Operational Meteorological System.

The agreement that evolved was built around a preexisting program at NASA Goddard to develop an experimental, fully stabilized polar-orbiting weather satellite that would follow the TIROS series. It was to have a much different set of instruments providing data that research-oriented meteorologists believed had great potential over the long term for providing a better understanding of the lower atmosphere’s processes, but that no one as yet really knew how to make use of. Called Nimbus, it had been the result of advocacy by Cortright and William Stroud for research into new instrumentation. TIROS’s small size and spin-stabilization limited its utility for carrying out instrument research, and initially Cortright had wanted to build a very large, multidisciplinary research satellite based on the WS-117L. Stroud, however, had argued that integration of the large, complex payload represented a huge risk. A simpler satellite, larger than TIROS but a good deal smaller than the Agena vehicle, yet still having a three-axis stabilization system that could keep its instruments aimed toward Earth continuously, made more sense.\textsuperscript{65} Cortright had accepted Stroud’s argument, and he successfully advocated this mid-sized Nimbus satellite program for the fiscal year 1960 budget.

Nimbus was not originally intended to be an operational weather satellite; instead, it was to be a research tool toward an eventual operational satellite. There were a good many ideas floating around the new space science community about what might be done to improve weather prediction with space technologies. Cloud pictures were only somewhat useful; they clearly improved hurricane warning, but they would not help much with forecasting outside the coastal regions. They also could not help advance the Weather Bureau’s numerical prediction effort. To produce hemispheric and global forecasts, the models needed wind and temperature data. Radio astronomers had devised techniques to infer atmospheric and surface temperatures from the electromagnetic emissions of the other planets, and a handful of meteorologists thought this form of remote sensing might be useful for their research as well. Others believed that satellite-tracked balloons could provide the global-scale datasets needed by numerical prediction researchers. The Nimbus program was aimed at finding out which of many possible techniques would work out.

Nonetheless, in early 1961, the Panel on Operational Meteorological Satellites
recommended basing the National Operational Meteorological Satellites system on Nimbus. The Weather Bureau’s Harry Wexler and David S. Johnson had initially made this proposal to Morris Tepper at a meeting the previous November, and the deal was concluded easily due to a longstanding friendship between Reichelderfer and Hugh Dryden.\(^6^6\) NASA agreed to have the first Nimbus ready for launch in 1962, and the TIROS series would be extended until then. The Weather Bureau would receive the appropriations for both TIROS and Nimbus, ensuring its control of the program, and transfer the money to NASA. President John F. Kennedy, in his special message to Congress of 25 May 1961 requesting the increased funding necessary for the Apollo project to reach the Moon, also requested additional funding for the Weather Bureau to pay for the operational meteorological satellite system. Congress approved the money that October, and in January 1962 NASA and the Department of Commerce, to which the Weather Bureau belonged, signed an interagency agreement to carry it out.\(^6^7\) The resulting Nimbus Operational System (NOMS) was projected to cost about $60 million per year to operate.

The agreement between NASA and the Department of Commerce did not last eighteen months. There were several overlapping reasons for its failure, all of which were sufficient to justify breaking it. The first was simply that the technology was not ready for operational use, and it needed a good deal more research and development. The second, of course, was that NASA was a research-oriented agency, even within the Office of Applications that was host to the meteorological satellite program. Stroud had not wanted Nimbus to be an operational satellite, since its operational nature would substantially reduce its value for instrument research. Third, the technology’s non-readiness led to substantial cost overruns that were beyond what the Weather Bureau could justify to the Budget Bureau. Fourth, the Defense Department decided that Nimbus was not what it wanted and it actively advocated for the Weather Bureau’s defection. And fifth, both Wexler and Reichelderfer left the Weather Bureau, Wexler dying unexpectedly in August 1962. The two men that replaced them as leading meteorological satellite advocates, J. Herbert Holloman, assistant secretary of commerce for science and technology, and S. Fred Singer, who became director of the National Weather Satellite Center in June 1962, wanted a significantly different operational system than Nimbus. They sought to remove NASA from the weather satellite business completely so that they could pursue their own agenda.

The Department of Commerce began its efforts to break the agreement in April 1963 in a meeting between Holloman and Robert Seamans, the associate administrator of NASA. The projected cost of the Nimbus satellite had doubled.
at this point, and the first launch had been delayed to 1964 due to the need to replace the original solar cells with ones more resistant to radiation damage. Singer’s National Weather Satellite Center also believed that the lifetime of the satellites would be too short to justify the cost. Holloman and Seamans agreed to review the program over the next few months. Singer hired an outside contractor, the Aerospace Corporation, to produce an analysis of the program. Delivered in early September, the Aerospace Corporation’s analysis told the Department of Commerce what it wanted to hear: the highly complex Nimbus satellite would have a lifespan of only about three months, leading to a program cost of $80 to $100 million per year, vice the $56 million that the Weather Bureau had anticipated. The value of the data received was not worth this cost, in Holloman’s judgment. In the requirements he had forwarded to Seamans in June, Holloman placed a reasonable value on weather satellite data of $26.7 million per year.

In its own analysis of the flap, the Budget Bureau’s staff pointed to two real issues. There was a technical issue at stake, but it was not the projected life of the satellite or the reasonable value of the data. Holloman and Singer wanted to adopt gravity stabilization for their operational satellite, a technique being developed within the Defense Department. Nimbus was to use active thrusters to maintain its orientation, while gravity stabilization utilized the gravity and magnetic torques discovered by TIROS 1 to achieve the same effect. Thrusters needed fuel that would run out eventually, while the gravity gradient system would not. They also sought nuclear power to bring about a three-year lifespan, which solar power could not (yet) achieve. There was also, the Budget Bureau’s analyst pointed out, a powerful “ad hominem” issue. Nimbus had been “plagued by bad feeling, bad interagency communications, and charges of bad management” for the past two years. This reflected both strong and incompatible personalities on all sides as well as the divergent goals of the NASA program manager and his Weather Bureau counterpart.

It also reflected active efforts within the Defense Department to undermine NASA. The Department of Defense had been forced to give up its space projects by White House fiat in 1958, leading to the creation of NASA, and had never accepted the justice of that decision. It sought control over its own space destiny. Despite the joint Department of Defense–NASA–Weather Bureau agreement to place all operational weather satellites in the Weather Bureau, it had established its own operational weather satellite program in 1961. It wanted imaging satellites in a different orbit than the Weather Bureau did, preferring a polar orbit that provided early morning imagery to the noon orbit that the Weather Bureau sought in order to more effectively schedule reconnaissance flights and airborne refuel-
ing operations. This clandestine project had many different names over its lifetime, but it is generally known as the Defense Meteorological Satellite Program (DMSP). Its first satellite was a shrunken version of TIROS, utilized the magnetic-loop stabilization suggested by TIROS 1 and tested late in 1960 aboard TIROS 2, and could be placed into polar orbit using an existing booster.

More important to the Weather Bureau, its cameras were remounted to point out the satellite’s side, not its base. By changing the orientation of the spin axis, the side-mounting would permit the cameras to photograph the Earth on each spin cycle, while the original TIROS’s cameras spent 90 percent of each orbit pointed at space. This meant, National Reconnaissance Office historian Cargill Hall eventually wrote, 100 percent coverage of the Northern Hemisphere each day above 60 degrees latitude and 55 percent at the equator. The first successful launch of this wheel-mode TIROS was 23 August 1962. The Department of Defense then actively recruited Singer, telling him about the classified DMSP program via a series of briefings. Singer recognized the short-term cost advantage of adopting the already-developed wheel-mode TIROS, and getting out of Nimbus had the benefit of permitting him to pursue the nuclear-powered, long-lived, gravity-stabilized satellite that he believed should be the ultimate operational system.

On 12 September, Holloman informed the Budget Bureau by telephone that he intended to end the Commerce Department’s participation in Nimbus in favor of the wheel-mode TIROS as an interim system. He also stated he planned to seek outright cancellation of Nimbus, in which, the Budget Bureau’s official recorded, further investment was not “justified because the cost-performance potentialities (with heavy emphasis on cost) of the next generation of surveillance satellites now offers a so much greater potential as to justify initiating a new development program and taking the additional delay.” The Budget officer also recorded cautioning Holloman not to place too much stock in any promise of an operational satellite that had not been matured to the point of “giving real confidence that multiple spacecraft procurements were justified.” That was good advice, as the Interim Operational System that emerged out of the Commerce Department’s rebellion was still interim in 1970.

On 27 September 1963, Holloman drafted a letter to NASA’s deputy administrator canceling the Department of Commerce’s participation in Nimbus, although by this time he had backed away from seeking Nimbus’s outright cancellation. He instead wished to immediately adopt an interim system “based on TIROS technology,” the DMSP satellite he could not mention in an unclassified letter, and establish a new program “to meet the coordinated meteorological
requirements and leading to a spacecraft lifetime such that the system operating costs are commensurate with its meteorological value.” In formally telling the Budget Bureau its intentions on 2 October, the Commerce Department wrote that it expected to save $180 million between fiscal year 1964 and fiscal year 1968. A prototype of the new operational system could be initiated in fiscal year 1964, and in fiscal year 1968 the new operational system could be procured with an annual operating cost of $36 million, vice the $58 million previously allocated and the $80 million expected for Nimbus.74

On 3 October, NASA’s Seamans responded with a memo warning that the Department of Commerce’s action, while clearly within its rights, would “defer the date at which a fully operational meteorological satellite could be available.” He also stated that NASA would continue the Nimbus program regardless, at least through the launch of the first two vehicles and probably the spare as well. They would provide the in-space test of the sensors for whatever operational system emerged, and they would also provide “unique and important observations needed for research.”75

A Wall Street Journal article leaking news of the Commerce Department’s revolt forced a relatively rapid settlement of the dispute. On 4 October, the two agencies gave the broad outlines of the agreement that would emerge, and a meeting between the budget director, the secretary of commerce, and the NASA administrator, the divorce was finalized. The Department of Commerce would get its interim TIROS system, and Nimbus would continue through the first two launches on NASA funds. The two agencies would study an operational system that would cost no more than $40 million per year to procure and operate. The meeting also caused the Budget Bureau staff to comment that no compelling justification for proceeding with development of an operational system in fiscal year 1965 was given by the Department of Commerce at the meeting. The Budget Bureau’s staff was also unconvinced by the Commerce Department’s basic argument that its not-yet-designed Operational Meteorological Satellite would be less expensive than Nimbus. Finally, the anonymous drafter also opined that Nimbus would provide the basis for whatever system the Department of Commerce got anyway, and that the actual technical disagreements were minor.76

On 2 January 1964, NASA and the Department of Commerce reached a new agreement adopting the DMSP’s wheel-mode TIROS as the TIROS Operational System (TOS). The first of these was launched as TIROS 9 in January 1965; slightly more than a year later, a larger version carrying a navy-developed direct readout system that provided instant, lower-resolution images to inexpensive ground stations was launched as Environmental Science Services Agency (ESSA)
1, reflecting a name change for the Commerce Department’s Weather Bureau. This series of TIROS-based satellites continued through 1970, alternating between the original high-resolution and the newer direct readout satellite so that one of each was always in orbit.\textsuperscript{77} In 1970, the first of the Improved TIROS Operational System (ITOS) satellites replaced the TOS satellites. As the first three-axis stabilized operational weather satellites, they reflected a delay of several years over Nimbus, whose second “90 day satellite” flew in May 1966 and operated until January 1969.

NASA’s Nimbus program, freed from the operational mission its leaders had never wanted, carried out the instrument research and meteorological science that had been their goal and is the subject of chapter 2. As the Budget Bureau had suspected, Nimbus-originated instruments formed the basis of ITOS. The Weather Bureau, on the other hand, never received the funds necessary to make the vast flood of images it received from its weather satellites fully useful. At an intellectual level, both NASA and the Weather Bureau had known in 1958 that data handling would be the bane of satellite research, but neither managed to come to grips with the problem. And because the Weather Bureau had rushed to declare TIROS and its successor operational, it could not justify to the Budget Bureau money to carry out research in data processing systems. Hence in her 1991 dissertation, ex-meteorologist Margaret Ellen Courain could cite Lee M. Mace, the Weather Bureau researcher responsible for turning the TIROS and ESSA imagery into useful products, as believing that forecasters did not really accept the imagery for fifteen years—the cloud photographs did not present information in a form they could use.\textsuperscript{78} The operational satellite series provided an effective storm patrol during the 1960s, permitting early warning of approaching oceanic storms, but they did not assist routine forecasting.

The IGY’s satellite program nevertheless set off a slow-motion transformation of atmospheric science. As the NASA meteorology program proceeded over the next decade, it developed new observing technologies that finally permitted demonstration of the generation circulation of the Earth’s atmosphere, Harry Wexler’s long-sought goal. It also eventually enabled the emergence of a global meteorology.