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THE KOZYREV–KUIPER CONTROVERSY OVER LUNAR VOLCANISM: AN EPISODE IN SOVIET-U.S. RELATIONS[1]

Доэл Р. **Полемика Козырева–Койпера по вопросу о лунном вулканизме: эпизод в истории советско-американских отношений.**

Запуск спутника в 1957 г. драматически повысил роль и значение астрономии, в то время одной из наименее выдающихся физических наук. И в Советском Союзе, и в США под давлением холодной войны и новых программ освоения космоса повышается финансирование планетарных исследований. Для обеих сверхдержав изучение Луны быстро превращается в стратегическую задачу.

Когда в 1958 г. советский астрофизик Николай Козырев объявил, что ему удалось обнаружить на Луне действующий вулкан, это повергло в шок западных (да и многих советских) исследователей, долгое время считавших Луну геологически неактивной. В США астроном Джерард П. Койпер взялся за оценку достоверности сообщения Козырева — задачу, сильно затруднявшуюся ограниченностью научных контактов, обусловленной холодной войной. Он сделал это по многим причинам: чтобы включиться в важную научную полемику, чтобы поддержать конкурентоспособность своего научного института и чтобы оказать услугу своим покровителям из государственных и правительственных структур, включая Центральнее разведывательное управление. Полемика, не доведенная до конца в то время, возобновляется в первое десятилетие XXI века, после сообщения о том, что планетарные исследователи получили новое подтверждение недавней вулканической активности на Луне.

Sputnik's launch in 1957 dramatically increased the profile and significance of astronomy, then one of the smallest of the physical sciences. Cold war pressures and newly unveiled space programs increased funding for planetary research in both the Soviet Union and the United States: for both superpowers, exploring the Moon quickly emerged as a strategic target.

The 1958 announcement by Soviet astrophysicist Nikolai A. Kozyrev that he had discovered an active volcano on the Moon shocked Western (and indeed many Soviet) researchers, who had long accepted that the Moon was geologically inert. In the U.S., planetary astronomer Gerard P. Kuiper sought to evaluate the validity of Kozyrev's claims — a task made difficult by cold war restrictions on scientific

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communications. He did so for many reasons: to resolve an important scientific controversy, to maintain the competitiveness of his scientific institution, and to serve the state and his governmental patrons, including the Central Intelligence Agency. The controversy, never fully resolved at the time, reemerged in the first decade of the 21st century, when planetary researchers announced new evidence for recent lunar volcanism.

THE LAUNCH OF SPUTNIK in October 1957 marked a pivotal moment in the growth of American astronomy. In the mid 1950s astronomy was the smallest of the physical sciences in the United States, with under three hundred active researchers, although it was already experiencing unprecedented growth when Sputnik's ascent into orbit intensified Cold War rivalries and released a flood of new funding for science. As abundant new federal support supplanted private funds, further significant changes occurred in the practice and administration of centers of American astronomy. Observatory directors were increasingly pressured to serve as entrepreneurs as well as administrators, keenly aware that obtaining new patronage and instruments was a key strategy in the heightened competition for faculty and graduate students. They no longer expected, as could observatory directors appointed in the early twentieth century, that they held lifelong tenure in their jobs[2].

An episode that serves to bring these competitive practices into focus occurred in November 1958, when Nikolai A. Kozyrev, an astrophysicist at the Pulkovo Observatory in Leningrad, announced that he had obtained spectroscopic proof of active lunar volcanism. The claim astonished American lunar scientists, most of whom believed that the Moon had died geologically billions of years before. It also worried officials of the newly formed National Aeronautics and Space Administration (NASA), already promoting lunar exploration as the arena where

[2] Richard Berendzen and Mary Treinen Moslen, "Manpower and Employment in American Astronomy," *Annals of the New York Academy of Sciences*, 1972, 198:46–65; David H. DeVorkin, "Who Speaks for Astronomy? How Astronomers Responded to Government Funding After World War II", *Historical Studies in the Physical and Biological Sciences*, 31, part 1, 2000, 55–92; and Allan A. Needell, "The Carnegie Institution of Washington and Radio Astronomy: Prelude to an American National Observatory," *Journal for the History of Astronomy*, 1991, 22:55–67.

Americans could win the emerging race for technological superiority with the Soviet Union. It forced many observatory directors to decide whether to invest valuable telescope time in the hope of confirming the observation. In this case the problem of evaluating scientific results, a matter always involving personal and social as well as intellectual influences, was enormously complicated by Cold War tensions. The way in which this controversy was resolved holds important implications for the sociology of science, illustrating the significance of phenomenological constraints.

In this essay I examine the efforts of Gerard Peter Kuiper (1905–1973), director of the Yerkes-McDonald observatories of the University of Chicago, to evaluate Kozyrev's claims of discovery between 1958 and 1961. The available archival sources limit my treatment to American perceptions of the Kozyrev controversy, but this aspect of the episode makes an excellent case study in international relations in science during the Cold War, a theme that has received little attention from historians. Soviet science held an intrinsic fascination for American researchers at this time, in part because communications between these leading nations of science were extremely limited, in part because interpreting Soviet advances and setbacks accurately had both scientific and strategic value. Like a well-placed mirror, the Kozyrev controversy reveals problems that American scientists had with maintaining international science during the Cold War.

In his efforts to evaluate Kozyrev's claim and the work of other Soviet astronomers, Kuiper found himself simultaneously filling several roles: active scientist, leader in solar system astronomy, administrator of a major astronomical observatory, and interpreter of Soviet scientific research to his government patrons. Often these roles clashed: Kuiper's evaluations, far from reflecting an idealistic view of science, were based on political considerations as well as the need to maintain a competitive edge for his institution. The conflict between these roles was characteristic of American science in the 1950s, and it is a central theme of this essay.

I. LUNAR STUDIES

Interest in the Moon rose among military planners in the mid and late 1950s[3]. But not until the dramatic launch of Sputnik I on 4 October 1957, with its painful political repercussions, did reaching the Moon become a clear target of American officials. In late 1957 and early 1958 various agencies and aerospace corporations that were heavily invested in rocket technology, including the Jet Propulsion Laboratory (JPL) in Pasadena, California, began submitting proposals for sending rockets to the Moon. Initially members of President Dwight D. Eisenhower's cabinet and his most influential science advisors, including those in the President's Science Advisory Committee, opposed such plans, feeling that lunar probes were gimmicks rather than thoughtful responses to the Soviet challenge. Rising popular hysteria over the Sputnik launchings and the perception of U.S. vulnerability and underachievement soon softened such opposition. On 27 March 1958 Neil McElroy, the American secretary of defense, expressed the Eisenhower administration's determination not only to explore Earth from orbit, but also "to determine our capability of exploring space in the vicinity of the Moon, to obtain useful data concerning the Moon, and provide a close look at the Moon."[4]

The decision to explore the Moon meant that virtually all available data about that body's motion, surface, and environment became a matter of significance for spacecraft designers. Scientists and engineers at JPL, for example, needed to know whether the Moon retained even a tiny residual atmosphere, a possibility not entirely ruled out in studies by American and French astronomers in the mid 1940s. Even an extremely tenuous lunar atmosphere could cause an appreciable

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[3] For background on the political and social dimensions of the American space program and its forerunners, see Walter McDougall, ... the Heavens and the Earth: A Political History of the Space Age (New York: Basic Books, 1985), pp. 141–194; Clayton R. Koppes, *JPL and the American Space Program: A History of the Jet Propulsion Laboratory* (New Haven: Yale Univ. Press, 1982), pp. 62–133; and Joseph N. Tatarewicz, *Space Technology and Planetary Astronomy* (Bloomington: Indiana Univ. Press, 1990).

[4] Daniel J. Kevles, *The Physicists* (New York: Vintage, 1979). pp. 386–387; Craig B. Waff, "A History of the Deep Space Network," draft (1990), Ch. I , p. 1 (I thank Waff for providing a draft copy); and Koppes, *JPL and the American Space Program* (cit. n. 2).

drag on the movement of artificial satellites through the medium, as investigations of Earth's first artificial satellites had shown. Few astronomers believed that a lunar atmosphere of this kind would be found. Thus photographs showing what some astronomers interpreted as hazes covering the floors of lunar craters, and Kozyrev's late 1958 announcement of volcanic outgassing, aroused considerable interest and consternation among JPL scientist[5].

Various agencies began funding work in lunar and planetary science liberally. Some grants went to a small but growing number of American astronomers with interests in solar system phenomena. While other facilities specialized in research on meteors or planetary atmospheres, lunar studies were a main focus of research at the Yerkes-McDonald observatories of the University of Chicago, after 1957 under the direction of Gerard P. Kuiper.

Kuiper was not the first astronomer at Yerkes-McDonald to turn to solar system astronomy, but he was without a doubt the most influential. Born in Holland, Kuiper studied astronomy at the University of Leiden in the late 1920s under Ejnar Hertzsprung, Jan Woltjer, and Willem de Sitter. With a Ph.D. thesis in hand on the structure of binary stars, a firm command of English, and extraordinary stamina for observational work, Kuiper traveled to the Lick Observatory in California in 1933. Finding permanent appointment there blocked by resentment against foreigners, Kuiper moved to a position at Harvard University in 1935 before accepting an invitation to join the staff of the new McDonald Observatory, located in Texas. Like most members of these jointly administered observatories, Kuiper made his home in Williams Bay, Wisconsin (where the Yerkes Observatory is located), commuting once or twice a year to Texas for his scheduled time on the McDonald te1escope[6].

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[5] E. P. Martz to G. P. Kuiper, 12 Nov. 1960, and Kuiper to Martz, 23 Nov. 1960, both Box 18, Gerard P. Kuiper papers, University of Arizona Library, Tucson (hereafter Kuiper papers); and Dinsmore Alter, "Scientific Aspects of the Lunar Surface," *Proceedings of the Lunar and Planetary Exploration Colloquium*, 1958, I(1):3-10.

[6] Henry Norris Russell to Armin 0. Leuschner, 25 Feb. 1935, Box 28, Department of Astronomy records, Bancroft Library, Berkeley; and Dale P. Cruikshank, "Gerard Peter Kuiper, 1905–1973", *Biographical Memoirs of the National Academy of Sciences*, 1993, 62: 258–295; I thank Cruikshank for providing an advance copy).

Virtually all of Kuiper's research between 1933 and 1945 dealt with stellar phenomena. He first moved towards solar system astronomy, an earlier interest but one difficult to pursue, in 1944, when he discovered that Titan, a large satellite of Saturn, possesses a methane-rich atmosphere. This was a surprising discovery, laden with what many scientists regarded as important implications for cosmogony. For a short time the discovery put him in a quandary: Titan's atmosphere posed enough questions to suggest a rich line of research in solar system astronomy, but pursuing them would require him to abandon other promising avenues of stellar astronomy in which he was deeply invested. The dilemma was resolved when he was offered Navy funds in 1946 to make infrared studies of the atmospheres of Earth and other planets, using instrumental advances developed during World War II. Kuiper chose the outside funds[7]. The Yerkes-McDonald observatories had come through the war weakened, particularly in contrast to physics at Chicago, whose stature and financial security were enhanced by participation in the Manhattan Project. Otto Struve, Kuiper's superior at Yerkes, had worried throughout the war that lack of funds or researchers might force the observatories to close, without sufficient war-related support. Similar fears still guided remarks he made in 1947. Declaring that the German V-2 rocket, infrared detector cells, microwave and radio detectors, and advances in electronics promised to revolutionize American astronomy, he added a caveat: "It is clear that we must reorient ourselves to take account of these changes. A new plan must be devised and a new policy must be adopted if the Yerkes Observatory is to retain its place among the leading research centers of the United States»[8]. Fortunately for Kuiper, solar system research fit the bill.

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[7] Cruikshank, "Kuiper" (cit. no. 5); Ronald E. Doel, *Solar System Astronomy in America: Communities, Patronage, and Interdisciplinary Research, 1920–1958* (New York: Cambridge University Press, 1996); and Kuiper to Subramanyan Chandrasekhar, 16 Jan. 1941, Box 28, and Gerard P. Kuiper, "Memorandum to Dean Bartky in re Navy Contracts", 20 June 1946, Box 29, both in Kuiper papers.

[8] Otto Struve, "The Story of an Observatory," *Popular Astronomy*, 1947, 55:283-294, on p. 291; and David H. DeVorkin, "The Maintenance of a Scientific Institution: Otto Struve, the Yerkes Observatory, and Its Optical Bureau during the Second World War," *Minerva*, 1980, 18:595–623.

Kuiper demonstrated both a keen understanding of physical problems related to solar system research and strong entrepreneurial skills in securing federal patronage for this work. Between 1946 and 1948, with the strong support of Struve, Kuiper built a promising program for the study of planetary atmospheres at the Yerkes-McDonald facilities, involving nearly a quarter of the staff part- or fulltime. This program collapsed in 1948, largely because the infrared spectrometer Kuiper had designed had limited sensitivity, and because Gerhard Herzberg, a talented German-born spectroscopist whom Struve had hired to revitalize the Yerkes spectroscopic laboratory, resigned. (Herzberg had identified many of the atmospheric absorption lines that Kuiper's instrument detected.) Afterwards Kuiper turned his attention to solid bodies in the solar system. With funds from the Office of Naval Research, the Air Force, and the National Science Foundation, he launched a major study of asteroids and began editing an internationally authored compendium of solar system research. He also started training graduate students in the field[9].

This research on asteroids led Kuiper to his interest in the Moon. A major question that confronted both astronomers and geochemists in the 1950s was determining the absolute abundances of the elements, since many scientists thought that the concentration of radioactive potassium, uranium, and thorium would indicate whether sufficient heat existed within planetary interiors to cause core formation and global melting. The Moon, Earth's nearest celestial neighbor, was seen as a test case for this idea. After investigating the lunar surface thoroughly with the large 82-inch telescope at McDonald, Kuiper announced in 1954 that it showed evidence of such melting. His findings drew fire from Harold C. Urey, the University of Chicago chemist and Nobel laureate, who had turned his attention, to planetary evolution in 1950. Their dispute widened into a rift that was never again bridged. Kuiper had hoped that Urey would lead collaborative, interdisciplinary work involving planetary geochemistry; their break, involving professional as well as methodological and disciplinary issues, instead escalated to one of the most painful and significant controversies in American astronomy in the mid-twentieth

[9] Doel, *Solar System Astronomy* (cit. n. 6); and "General Contract Information," Box 33, Kuiper papers.

century. Kuiper emerged from the dispute more convinced than ever that astronomical, rather than geochemical, evidence was paramount in solving the riddle of the solar system's origin, and that the lunar surface was a key piece to its solution[10].

An equally significant factor in Kuiper's interest in the Moon was the prospect of greatly increased patronage for lunar research. As director of one of the nation's largest observatories, Kuiper saw himself in a strong position to help direct such programs and to obtain funds needed to maintain the competitive standing of Yerkes-McDonald. Although not as involved in the International Geophysical. Year (IGY) as astronomers engaged in upper atmospheric research or solar physics, he was well aware before the launch of Sputnik that government and military plans were converging on the Moon. In 1955 he had persuaded members of the planetary commission of the International Astronomical Union to endorse his proposal to develop a photographic lunar atlas, explaining that such an atlas would benefit future space activities and lunar astronomy. In his letters to other astronomers Kuiper demonstrated a keen appreciation that astronomy would burgeon once planned satellites were launched during the IGY. When Sputnik I began circling the Earth in October 1957, he seemed less surprised than many of his colleagues that the public and the government clamored for space research and planetary exploration[11].

Kuiper saw two particular advantages in bringing large grants for solar system astronomy to Yerkes-McDonald. The 82-inch telescope of McDonald had been the second largest astronomical instrument in the United States after World War II, but the completion of the 200-inch Palomar telescope, operated by the California Institute of Technology, and several other major university instruments had eroded Chicago's instrumental edge. Kuiper pressed this point in communications to University of Chicago officials. Warning that "a revolutionary pace is

[10] Doel, *Solar System Astronomy*. On Urey's lunar research see, e.g., Stephen Brush, "Nickel for Your Thoughts: Urey and the Origin of the Moon," *Science*, 1988, 217:891–898.

[11] "Project: Atlas of the Moon,"22 Oct. 1956, Box 14; Kuiper to E. C Aberndanon, 20 Feb. 1956, Box 10; and Kuiper to Gerard Van Doren, 3 Oct. 1957, Box 14; all in Kuiper papers.

sweeping astronomy", he argued that "without constant additions of major and expensive equipment, astronomers get behind so fast and so far as to become rapidly obsolete and ineffective". They are watching eagerly and sometimes anxiously for breakthroughs or major progress at rival institutions". Kuiper also raised the specter of international competition. The "present race", he declared, was accelerated by the need to make progress "vis a vis the USSR"[12]. Increasingly he looked to such patrons as the National Science Foundation and the Air Force Cambridge Research Center to provide new, specialized telescopes for the Chicago astronomers. He also recognized that many of his Yerkes-McDonald colleagues were principally interested in stellar and galactic astronomy and resented the increasing devotion of shop facilities and telescope time to solar system research. Large grants-and the possibility, never realized, of building a distinct institute of planetary studies within the observatory-were the tools Kuiper sought to keep his field both competitive within the discipline and secure within the confines of his own institution[13].

Solar system astronomy flourished at Yerkes-McDonald under Kuiper's leadership in the late 1950s. An infusion of new NSF and Air Force funds (five times the amount received before the launch of Sputnik) made possible intensified lunar mapping studies, and Kuiper recruited to Yerkes a core group of cartographers and geodicists — an unprecedented interdisciplinary arrangement at American observatories-to supervise the work. The Yerkes group began working on lunar maps on a scale of 1:1,000,000, employed the Yerkes 40-inch refractor to determine the Moon's moments of inertia, and launched systematic attempts to interpret the origins of lunar surface features. Kuiper persuaded Lawrence Kimpton, chancellor of the University of Chicago, to formalize the hybrid marriage of astronomy and geology by awarding joint Ph.D.s from these departments. (Geochemistry, because of his rupture with Urey, was out.) Despite his worry that the Yerkes-McDonald programs were built on the shifting sands of temporary re-

[12] Kuiper to Lawrence Kimpton, 1 Nov. 1959; and Kuiper to R. Wendell [Pat] Harrison, 16 April 1960; both Box 18, ibid.

[13] Kuiper, "Proposal [to NSF] for a 'Center' or 'Institute' of Planetary and Lunar Studies," July 1958. Box 13, ibid.

search contracts rather than on endowed funds, Kuiper realized that his research ambitions were closely linked with the new, emerging federal and military patrons of the field[14].

Each of Kuiper's programs promised to provide government and military agencies critical information needed to develop spacecraft expeditions. Yet Kuiper quickly came to understand that the government lunar effort, driven by political exigencies and the desire to score triumphs against the Soviets in the shortest possible time, required knowledgeable scientists to review and coordinate work at American institutions as much as it did hard data. Space research, including lunar science, was far larger than the capacity of any single institution to provide it. By the late 1950s the prospect of obtaining large grants inspired astronomers at many academic centers — Harvard, Caltech, and Johns Hopkins University among them — to tailor existing research programs in ways to gain grants from NASA and from the Air Force. While many of these proposals were solidly developed, a few were evidently designed to pry loose funding from generous review panels[15].

Aware that many leaders of the emerging U.S. space program were engineers by training or scientists trained in fields other than astronomy, Kuiper sought to serve as an advisor on astronomical research in support of rocket-based lunar missions. For him such advice giving was a golden opportunity to promote his scientific research. It would also help allay his anxieties about maintaining professional standards in this broadly interdisciplinary, rapidly expanding field. Increasingly Kuiper complained that the sudden flood of funds for solar system research made the field a "happy hunting ground" for researchers dissatisfied with their own areas of work[16].

Despite his eagerness to recruit new contracts, Kuiper did not wave the flag of American-Soviet competition in solar system research more than his colleagues. He disagreed with Donald H. Menzel, director of

[14] Kuiper, "Proposal" (cit. n. 12); and Kuiper to Jan Oort, 1 July 1958, Box 13, ibid.

[15] Kuiper to A. R. Hibbs, 21 Nov. 1960; and Kuiper, "Review of JPL Technical Memo 33–37," 1 Mar. 1961; both Box 18, ibid.

[16] Kuiper to Hibbs. 21 Nov. 1960, p. 2; and Kuiper to Aleksandr Mikhailov, 24 Apr. 1960, Box 11, ibid.

the Harvard College Observatory, who declared to patrons that more astronomers were engaged in lunar and planetary studies in the Soviet Union than any other country, including the United States; Kuiper put their ranks at about equal. Nevertheless he did feel that Soviet researchers had made large strides in the field. He wished to evaluate their research results in order to guide his own investigations[17].

It was in this context that Kuiper first learned of the controversial claim by Kozyrev, one of nearly 125 astronomers employed at the Pulkovo Observatory in Leningrad, that he had obtained spectrographic evidence for an active volcano on the Moon. Kuiper believed that the finding, if accurate, would have significant implications for the design and construction of lunar spacecraft, and require new kinds of systematic lunar observations. It would also serve as a window into a field of Soviet astronomy that had intense interest to scientists as well as to national policymakers. Kuiper well understood the competitive value of knowing whether to concentrate resources on this question in the hope of making further important discoveries, or to steer clear of an unfruitful path. Evaluating Kozyrev's claim became one of Kuiper's major goals.

II. EVALUATING THE EVIDENCE EMERGING CONTROVERSY

Kozyrev's claim that the Moon was volcanically active took many American astronomers by surprise. By the late 1950s most American astronomers had come to accept that most lunar craters and the far larger lunar "seas" resulted from impacts by meteorites at high velocity, abandoning the idea that they were caused by large volcanic explosions, which had been the leading view among American astronomers in the early twentieth century. This shift in consensus can largely be traced to the publication by the American astronomer Ralph B. Baldwin in 1949 of *The Face of the Moon*. In this work Baldwin gave a pains-

[17] Donald H. Menzel and Gerard de Vaucouleurs to E. R. Dyer, Jr., 2 Dec. 1959, p. 2, Box 32, Papers of the Director, Harvard College Observatory, Harvard University Archives (hereafter HCO director's papers); and Kuiper to Central Intelligence Agency, 10 April 1959, Box 33, Kuiper papers.

taking analysis of the depth-versus-diameter measurements of craters produced by bomb shells during World War II, then extrapolated the curve to large-scale features the size of lunar craters[18]. A few Western astronomers (joined by many geologists) rejected this interpretation, arguing that morphological similarities between volcanic calderas and lunar craters suggested that the lunar surface was molded principally by volcanism. Many Soviet astronomers also rejected Baldwin's arguments. At the Leningrad University the Soviet school led by the astronomers V. V. Sharonov and N. N. Sytinskaya argued that nonrandom distribution of craters was contrary to the impact hypothesis, and that polarization measurements of the Moon's surface strongly indicated lava flows. This difference in opinion was well known to American astronomers. Even so, virtually all lunar scientists had agreed that the lunar landscape was ancient; even advocates ofthe volcanic theory had not predicted contemporary eruptions[19].

What also surprised American astronomers (although the novelty of finding the news there was rapidly fading) was that first reports of Kozyrev's discovery appeared in the popular press rather than in established scientific journals. The first news of Kozyrev's reported discovery was moved out on the wires of the Soviet news agency TASS on 12 November 1958; it was subsequently carried in several American newspapers, including the *New York Times,* which featured it on page one. Initial details were sketchy. The TASS report noted only that Kozyrev, while observing at the Crimean Astrophysical Observatory

[18] Fred L. Whipple to Ralph B. Baldwin, 1 Sept. 1949, Box 1, Fred L. Whipple Collection, Harvard University Archives; Donald H. Menzel to Zdenek Kopal, 11 April 1960, Box 41, HCO director's papers; and Ralph B. Baldwin, interview by Ronald Doel, 25 Oct. 1989, pp. 34–44, Niels Bohr Library, American Institute of Physics, New York (hereafter AIP).

[19] Otto Struve to Harold C Urey, 7 Jan. 1953, Box 87, Harold C. Urey papers, Central University Library, Mandeville Department of Special Collections, University of California at San Diego (hereafter Urey papers). For American research involving impact craters see William Graves Hoyt, *Coon Mountain Controversies: Meteor Crater and the Development of Impact Theory* (Tucson: Univ. Arizona Press, 1987); and Kathleen Mark, *Meteorite Craters* (Tucson: Univ. Arizona Press, 1987). For Soviet views see A. V. Markov, ed., *The Moon: A Russian View* (Chicago: Univ. Chicago Press, 1962).

on the night of 3 November, had found the spectrographic signature of volcanic activity within the lunar crater Alphonsus, a circular depression about sixty miles in diameter near the center of the Moon's visible disk. Kozyrev was quoted as declaring that his discovery refuted the idea that the Moon was "a dead celestial body." The report also quoted Aleksandr A. Mikhailov, director of the Pulkovo Observatory and well known by reputation to many American astronomers, to the effect that Kozyrev's observation was of great importance in showing that the impact theory was "entirely erroneous", and that volcanism remained an active geologic process on the Moon[20].

Direct communications between American and Soviet astronomers were constrained by the Cold War, not to mention linguistic barriers and very real limitations on mail and telephone calls — both direct regulations and the less overt fear of governmental notice. American astronomers thus tried to gauge the substance and significance of the report much as they had news of the launch of Sputnik I one year earlier, through informal contacts with one another. The first substantial details came from a Czechoslovakian astronomer then working in Manchester, England: Zdenek Kopal. Kopal succeeded in placing a long-distance telephone call to Mikhailov. Reporting on the conversation in the British journal *New Scientist,* Kopal wrote that Kozyrev, using the Crimean 50-inch telescope, had spotted a reddish glow in Alphonsus while making spectrographic studies of the Moon and had immediately begun a new plate, then exposed a third plate once the visual activity subsided. The second plate appeared to show intense emissions at 4,737 angstroms, characteristic of the Swan bands of molecular carbon, at the point where the slit of the spectrograph had intersected the crater's central peak. Within two months additional details were presented in an article Kozyrev submitted to *Sky and Telescope,* a semipopular magazine widely read by American astronomers[21].

[20] "Eruption of a Volcano on Moon Reported by Russian Scientist", *New York Times*, 13 Nov. 1958. pp. A-I, A-12.

[21] Zdenek Kopal, "Volcano on the Moon?" *New Scientist*, 1958, 4:1362–1364; and N. A. Kozyrev, "Observation of a Volcanic Process on the Moon," *Sky and Telescope*, 1959, 18:184–186. One measure of the significance of the controversy is the number of publications it inspired: thirty-six articles devoted to lunar vol-

Although most American astronomers believed the impact theory of lunar craters to be correct, a number of them thought that contemporary volcanic eruptions were possible. By early 1959 several American and European researchers, including Kopal, voiced support for Kozyrev's evidence and interpretation. Urey's new model of the lunar interior, which postulated inhomogeneous composition of the lunar interior as the most promising means of explaining the Moon's earth-facing bulge, could accommodate local volcanic eruptions, and Urey pointed to Kozyrev's observation as important evidence for his theory in a 1959 paper[22]. Dinsmore Alter, director of the Griffith Observatory and Planetarium in Los Angeles, who had initiated lunar surface studies in the early 1950s, also supported Kozyrev's interpretation and wanted to use the large telescopes at Mount Wilson, where he enjoyed guest observer privileges, to search for additional instances. Other astronomers considered launching similar programs. Although many wished to evaluate a full-length journal publication describing the discovery (Kozyrev's article included a photograph copy of the plate but few technical details), there was general awareness that the intense competition of the space race brought great pressures on researchers to announce preliminary results, while a full report might be delayed for months, possibly for reasons of national advantage[23].

Kuiper perceived Kozyrev's announced discovery as a matter of considerable professional importance, with clear implications for his lunar research programs at Yerkes-McDonald. Unlike Urey and Alter, Kuiper soon took a dim view of the accuracy of the evidence that Kozyrev had provided. Kuiper's impressions were partly shaped by his commitment to the molten-moon hypothesis he had developed in the

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canism were reported in the *Astronomischer Jahresbericht* in 1959, more than three times the number devoted to the topic between 1956 and 1958.

[22] H. C Urey, W. M. Elsasser, and M. G. Rochester, "Note on the Internal Structure of the Moon," *Astrophysics Journal*, 1959, 129:842–848.

[23] Dinsmore Alter to Ira S. Bowen, 17 Nov. 1958, Alter to Bowen, 24 Nov. 1958, and Alter, "Proposal for Lunar Photography", cover letter 23 Dec. 1958, all in Box 39, Ira S. Bowen papers, Huntington Library, San Marino, California; and Fred L. Whipple to Harold C Urey, 8 July 1959, and Urey, "Report of Commission I," Space Science Board, National Academy of Sciences, ca. Aug. 1958, Box 67, Folder 1, both in Urey papers.

mid 1950s, which argued that the Moon had become molten through radioactive heating early in its history and then rapidly cooled, making instances of contemporary active volcanism most unlikely. But he also distrusted photographic observations that Alter had made of the Moon at different wavelengths, which Alter believed showed the existence of lunar hazes. To Kuiper, Alter's plates indicated no more than changed conditions in Earth's atmosphere during their exposure. Particularly irritating to Kuiper was that Kozyrev had acknowledged the influence of Alter's observations on his own work[24]. Kuiper worried that blind acceptance of Kozyrev's findings (if unsubstantiated) would lead American astronomers on a wild goose chase and tarnish the reputations and institutional ambitions of solar system astronomers in the eyes of their chief patrons. By laying his claims before the general public, Kozyrev, in Kuiper's view, had committed a breach of disciplinary standards.

Kuiper confided his doubts about the accuracy of Kozyrev's report in a series of confidential letters to Joseph Ashbrook, an editor of *Sky and Telescope* and a professional astronomer by training. In late January 1959 Ashbrook sent Kuiper the photographic print of the spectrum that Kozyrev had provided to accompany his article, prepared from his original plate. Shortly afterward, Kuiper used one of his assigned nights on the 82-inch telescope at McDonald to expose twenty-five plates of Alphonsus with a spectrographic resolution of 50 angstroms per millimeter, the same that Kozyrev had reported. The results were frustratingly inconclusive: Kuiper believed that the apparent emission lines might have been caused by faulty guiding of the Soviet telescope, but the resolution was too low for certainty. Related spectral lines that Kuiper expected to find — assuming that the bright feature near 4,700 angstroms was indeed the Swan bands of carbon — did not appear. But perhaps they were simply buried in the noise of the copy. "Only inspection of the original plate will tell," Kuiper wrote. Other astronomers who examined the copy came to share this view[25].

[24] G. P. Kuiper, "The Moon," *Journal of Geophysical Research*, 1959, 64:1713–1719. p.186

[25] Kuiper to Joseph Ashbrook, 31 Jan., 12 Feb. 1959, "Departmental Communications, 1957–59" folder, W. W. Morgan papers, unprocessed collection, Yerkes Observatory; and Dinsmore Alter, "The Kozyrev Observation of Alphonsus,"

Kozyrev's plate was not only a singular event (the observation could not be repeated) but geographically isolated as well.

How then were American astronomers to judge its value? For Kuiper, the matter involved a number of considerations, including the character of the observer. He believed that statistical probability did not support Kozyrev's claim that by good fortune he had caught an active lunar eruption in the slit of his spectrograph, since a century of intense visual inspection had yielded few reports of transient activity. Kuiper put the odds at no better than one in a thousand. The problem thus became to determine the likelihood that Kozyrev had erred or falsified his evidence. Complicating the picture were the intense popular and scientific interest in the discovery, and the rapid elevation of the Moon as a target for scientific exploration and national prestige. As an administrator Kuiper recognized that decisions made about the discovery's significance would affect which institutions would control lunar research.

Moreover, the strong support that Urey voiced for Kozyrev's work left Kuiper wondering whether new resources for lunar research would go principally to astronomers or instead to scientists in other disciplines. By the late 1950s Urey was actively promoting his own blueprint for scientific explorations of the Moon and had forged strong links with aerospace contracting firms, the Space Science Board of the National Academy of Sciences, and NASA (whose Working Group on Lunar Exploration was composed entirely of geochemists and geophysicists). Kuiper was acutely aware that Urey, after their intense controversy in 1955, had attempted to disrupt his access to NSF patronage[26]. Personal, institutional, professional, and disciplinary factors were all tightly interwoven in the issue of evaluating Kozyrev's evidence.

By early 1959 Kuiper had reached no firm conclusions about Kozyrev's finding. From his inspection of Ashbrook's plate copy, Kuiper

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draft ca. 1959, Box 3, Folder 6, Urey papers. I thank Judy Bausch for facilitating my access to the Morgan papers.

[26] Tatarewicz, *Space Technology* (cit. n. 2). p. 29; Doel, *Solar System Astronomy* (cit. n. 6); and W. P. Bidelman to Harold C. Urey, 8 July 1959, Box 10, Urey papers.

was fairly certain that it showed no unambiguous evidence of emission. But he was not ready to rule it out, and the significance of the plate, if genuine, meant that a survey program of the Moon could yield a breakthrough for Yerkes-McDonald[27]. What complicated the picture for Kuiper was his worry that Kozyrev, whose previous work was not well known but seemed to him marginal in quality, might have released preliminary or even misleading results to gain standing within the Soviet astronomical community. Short of visiting the Pulkovo Observatory directly to examine the original plate — then an unlikely proposition — Kuiper judged that he needed advice from Soviet colleagues on Kozyrev's character as an observer and his standing in Soviet astronomy. Between 1959 and 1960 Kuiper devoted considerable energy to reviewing Soviet astronomy.

III. EVALUATING SOVIET SCIENCE

The problem of evaluating Soviet scientific results confronted many American scientists during the height of the Cold War. While some researchers worried about the extent of Lysenko-style intrusions into Soviet research communities, of more general concern were the limited number of Soviet periodicals available in translation (few American astronomers read Russian) and the equally limited opportunities for informal interactions with Soviet colleagues. Contact between American and Soviet astronomers became virtually nonexistent after the purge of Soviet intellectuals during the Great Terror ordered by Joseph Stalin in the mid 1930s. Many Soviet astronomers, particularly at Leningrad — including Kozyrev, then a young astrophysicist at Pulkovo — subsequently found themselves either in concentration camps or before execution squads[28]. Although a brief thaw in 1945 and 1946 allowed delegations of Soviet astronomers to visit observatories

[27] Kuiper to Ashbrook, 31 Jan. 1959 (cit. n. 24). p. 3.

[28] On the history of Soviet astronomy during the 1930s see, e.g., Loren R. Graham, *Science, Philosophy, and Human Behavior in the Soviet Union* (New York: Columbia Univ. Press, 1987), pp. 380–403; Robert A. McCutcheon, "The 1936–1937 Purge of Soviet Astronomers," *Slavic Review*, Spring 1991, 50(1):100–117; and McCutcheon, "The Purge of Soviet Astronomy, 1936–37" (M.A. thesis, Georgetown University, 1985).

in the United States (Kuiper had helped sponsor one such group at the McDonald Observatory), by 1947 the deepening Cold War was limiting personal contacts and disrupting plans for international meetings. The general meetings of the International Astronomical Union, or IAU (the only international scientific union to which the Soviet Union belonged in the 1950s) were twice postponed because of superpower conflicts, including the outbreak of the Korean War. American scientists attending the meetings actually held frequently complained that they allowed too few opportunities for extended conversations with Soviet researchers. Kuiper's predecessor at Yerkes-McDonald, Struve, and other leading American astronomers were angered, tormented, and baffled by polemical attacks in Soviet publications on their character and scientific theories, written by equally prominent Soviet astronomers such as Viktor Ambartsumian and Pavel Parenago[29]. Fresh barriers were raised in the late 1950s as officials in Moscow and Washington wrapped space science and lunar research in the mantles of national security and national prestige. Although scientists found wider personal contacts vital in evaluating new or controversial scientific results from outside their own intimate circle, such networks were difficult to establish with counterparts in the Soviet Union.

By the 1950s Kuiper was deeply involved, indeed obsessed, with the problem of evaluating foreign science. As an adult immigrant to the United States who had absorbed much of the internationalist cultural traditions that Holland had to offer, Kuiper was clearly more familiar with the personalities, structures, and styles of European science than were many of his American-born colleagues. But his desire to communicate his views to colleagues and to outside patrons tapped deeper roots. During 1944 and 1945 Kuiper served as a member of America's ALSOS mission, a civilian-scientific team deployed behind advancing Allied troops to interview Axis scientists and engineers about their progress in weapons research, particularly atomic bombs. Kuiper had been assigned the task of interviewing astrophysicists in liberated Holland, France, and Germany about German advances in solar astronomy, a field critical to long-range military

[29] Otto Struve, "Comments and Communications: Astronomy in the Manner of 1984," *Science*, 1952, 116:206–207.

communications. Appalled by the destruction wrought by the German war machine in his homeland, Kuiper began a personal crusade against Nazi collaborators after World War II, mixing credible intelligence with hearsay reports in the occasional condemnations that he provided, often on request, to colleagues and to Allied occupation officials[30]. Kuiper's role in evaluating European science was viewed positively by many colleagues. It was in part his extensive contacts with scientists in Europe that gained him the presidency of the planetary sciences commission of the IAU, and once he was in the post (which he held for two terms, from 1952 to 1958), his dealings with scientists outside the United States increased substantially. As director of the Yerkes-McDonald Observatory in the late 1950s Kuiper remained in contact, by letter and through occasional personal visits, with a large number of European astronomers.

Although Kuiper became somewhat more conservative as he aged, he was a liberal by American political standards, with no apparent political ax to grind with the Soviet Union. In 1950 he distanced himself from the pro-Communist writings of his former instructor at Leiden, Hertzsprung, complaining that Hertzsprung seemed "annoy[ed] with the Western world around him."[31] He made little reference in his correspondence to the difficulties of American astronomers. ensnared in the web of the House Un-American Activities Committee in the 1950s, whose victims included Harlow Shapley, until 1952 the director of the Harvard College Observatory, and the circle of astronomers who had joined Shapley in supporting internationalist causes during World War II. On the other hand, he made no effort, as did Ira S. Bowen, director of the Mount Wilson and Palomar Observatories, to alert prospective

[30] Karl Hufbauer, *Exploring the Sun: Solar Science Since Galileo* (Baltimore: Johns Hopkins Univ. Press, 1991), pp. 119–159; and Gerard P. Kuiper, "German Astronomy during the War," *Popular Astronomy*, 1946, 54:263–283. On Kuiper's role in ALSOS see Mark Walker, *German National Socialism and the Quest for Atomic Power, 1939–1945* (New York: Cambridge Univ. Press, 1989), pp. 151–160; David H. DeVorkin, *Science with a Vengeance: The Military Origins of Space Science* (New York: Springer, 1993), Ch. 3 (I thank DeVorkin for providing a draft in advance); and Kuiper to Zentralspruchkammer Nordbaden, 3 March 1950, Box 28, Kuiper papers.

[31] Kuiper to Oort, 31 July 1950, Kuiper papers.

academic or institutional employers to the names of astronomers called to Washington to testify to the committee[32]. The issue that appeared to trouble him most deeply was the influence of the McCarran-Walters Act, passed by Congress in 1952, which served to limit meetings and informal contacts among international scientists. The law prevented blacklisted foreign scientists from visiting the United States and outspokenly liberal American scientists from using their passports to travel abroad. "Perhaps the McCarran Act will be changed when all prominent Europeans have been in Russia", thus preventing them all from traveling to the United States, Kuiper lamented to Struve. "We need more, not less, contact with the Russians, because the virtues which all human beings possess, at least to some degree, will help our side"[33].

The lack of routine contacts between Soviet and American astronomers clearly hindered Kuiper's attempts to evaluate Kozyrev's standing within his local community. Kozyrev was not entirely unknown to Kuiper at the time the discovery announcement was made. He had apparently learned details of Kozyrev's arrest and exile in Siberia from 1936 to 1948, as well as his subsequent restoration to the staff of Pulkovo, from Subramanyan Chandrasekhar, a Yerkes colleague who had met with Kozyrev and other Soviet astronomers at Pulkovo in 1934[34]. Kuiper had himself met Kozyrev, albeit briefly, in a corridor encounter at the 1958 meeting of the IAU in Moscow. Their conversation did not leave a positive impression in Kuiper's mind. Kozyrev had seemed "a nervous and broken man" as a result of his years in the gulag, Kuiper advised colleagues. This impression probably contributed to his distrust of Kozyrev's earlier planetary findings, including a claim — supported by Urey and several other American researchers — to have detected aurorae in the atmosphere of Venus. Moreover, the spectacular nature of the new discoveries disturbed Kuiper, as did reports that Kozyrev

[32] For an instance of Bowen's acting thus see Ronald E. Doel, "Defining a Mission: The Smithsonian Astrophysical Observatory on the Move," *J. Hist. Astron.*, 1990, 21:137–153, on p. 41.

[33] Kuiper to Otto Struve, 27 May 1954, Box 28. Kuiper papers.

[34] McCutcheon, "The 1936–37 Purge»(cit. n. 27); and Robert A. McCutcheon, "Interview with A. A. Kozyrev concerning the Early Career, Arrest, and Imprisonment of His Brother, the Astrophysicist N. A. Kozyrev,"1989, AIP.

was at work on a physical theory of time[35]. When he raised the issue of probable error in his long letter to *Sky and Telescope's* Ashbrook, he also listed violations of research ethics in the history of twentieth-century astronomy, including false claims about the canals of Mars during the early twentieth century and "fake spectrograms of fifth magnitude stars made at the Chile Station of the Lick Observatory by an assistant who discovered that a night's work could be compressed into less than an hour by observing bright stars of the same spectral type."Kozyrev's data appeared no less suspect to Kuiper[36].

But were Kozyrev's spectra genuine or forged? Kuiper may well have felt less secure about his negative conclusion after receiving a detailed letter from Struve. Through the 1950s Struve was the most well-informed American astronomer on Soviet astronomy. Russian by birth and early education, he had fled before the advancing Red Army after World War I; he nevertheless remained preoccupied with Soviet astronomy, and in 1947 he began editing an informal newsletter that offered American astronomers translated abstracts of Soviet astronomical research. Struve provided no ammunition against Kozyrev's finding. He advised Kuiper that the telescope that Kozyrev had almost certainly used to expose his plates was the Crimean Observatory's 50-inch reflector, which had been confiscated by Soviet astronomers from the Berlin-Babelsberg Observatory after World War II in retaliation for the Germans' destruction of the Seimeis Observatory's main instrument. The Crimean telescope was then the Soviet Union's largest, but more important, its spectrograph was a high-quality instrument manufactured by the German astronomer Paul Guthnick. Kozyrev had

[35] Kuiper to C. S. Beals, 28 Nov. 1958, and Kuiper to Beals, 19 Dec. 1958, both Box 10, Kuiper papers; Walter Sullivan, "Aurora Believed Sighted on Venus," *New York Times*, 17 Feb. 1958, p. 23; and Harold C. Urey to E. Opik, 5 April 1957, Box 72, Urey papers. There is no evidence that Kuiper then understood the extent to which the theory was under debate within the Soviet Union; see, e.g., M. Kitaev, "Kozyrev's Controversial Theory of the Nature of Time," *Bulletin of the Institute for the Study of the U.S.S.R.*, 1960, 7:39–47.

[36] Kuiper to Ashbrook, 31 Jan. 1959 (cit. n. 24), p. 2. Kuiper implied that Kozyrev had forged his observations to make his reputation; for an analogous case see Jan Sapp, *Where the Truth Lies: Franz Moewus and the Origins of Molecular Biology* (New York: Cambridge Univ. Press, 1990), p. 9. p.192.

thus employed a detector of known standards. Whether this was news to Kuiper is not clear. Like Kuiper, Struve was a skilled spectroscopist, and after examining a print of the Alphonsus plate mailed to Alter by Kozyrev, he advised colleagues that it appeared genuine to him. Struve's opinion probably influenced Bowen's decision to permit Alter to use the Mount Wilson 60-inch telescope for stepped-up lunar reconnaissance, hoping, as Bowen put it, "to catch the volcano in the act."[37] None of this pleased Kuiper, who still wanted to use the Mount Wilson telescope in slack times to photograph the Moon for the lunar atlas his team at Yerkes was preparing.

Kuiper quickly became convinced that his best hopes for evaluating the plate lay in canvassing Soviet astronomers. In February 1959, after completing his analysis of the spectroscopic test plates of Alphonsus exposed at the McDonald Observatory, Kuiper wrote for the first time to astronomers in the Soviet Union for advice on Kozyrev and his lunar spectra. (Kuiper apparently did not write Kozyrev directly, perhaps believing that little useful intelligence would come of it.) The astronomers he addressed had traveled to the United States and visited with him at Yerkes; his personal relations with them gave him confidence that he could trust, or at least evaluate objectively, what they had to say. Among them were Kyril Ogorodnikov, an astronomer at the Leningrad University well known to American and European astronomers, and Alla Massevitch, a Soviet theoretical astrophysicist highly regarded by American astronomer[38].

Kuiper discovered that no consensus existed among Soviet astronomers on the accuracy of Kozyrev's volcano report. In early February Ogorodnikov informed Kuiper that solar system astronomers in the Soviet Union appeared to accept Kozyrev's claim. He reported attending a Leningrad meeting at which V. V. Sharonov and A. V. Markov, leading figures in the field, were present. Kozyrev's spectrum had been shown at the gathering; while Sharonov and Markov had disagreed

[37] Ira S. Bowen to Dinsmore Alter, 19 Nov. 1958, Box 39, Bowen papers; and Struve to Kuiper, 31 Dec. 1958, Box 14, Kuiper papers.

[38] Alla Massevitch to Fred L. Whipple, 10 Sept. 1959, Box 10, Fred L. Whipple papers, Smithsonian Institution Archives, Washington, D.C.; and Massevitch, interview by Spencer R. Weart, 1 Sept. 1976, AIP.

over details of Kozyrev's interpretation, "all agreed it was due to some kind of eruption of gases." At Kuiper's request Ogorodnikov had also "quite privately" visited Mikhailov, Kozyrev's director at Pulkovo and a prominent Soviet astronomer. While Mikhailov expressed some misgivings over Kozyrev's previous research, Ogorodnikov reported, he corroborated the value and significance of the volcano announcement[39].

Massevitch's response, which Kuiper received shortly thereafter, was far more negative. In a crisply worded letter she reported that she had obtained a copy of the plate and showed it to astronomers in Moscow interested in lunar studies. No one outside Kozyrev's close circle of professional acquaintances, she claimed, would endorse the accuracy of Kozyrev's interpretation, or even the authenticity of the plate. Massevitch did not speculate on why Ogorodnikov had offered a favorable review, or why others in Kozyrev's circle had defended his work. She summed it thus: the whole matter was "really puzzling, but of course there can be no questions /*sic*/ of the spectrum being faked"[40].

Kuiper replied to Massevitch that he was «still uncertain what to think about the lunar spectrum," and wished that at least one Soviet astronomer would attest to the authenticity of the plate: "If the results were not so difficult to believe, one would never raise the question of the true nature of the spectrum at all." But even without further word from Massevitch, Kuiper returned to his hard-line position on the observation and increasingly used her argument to justify warning American colleagues against initiating searches for lunar events. He passed over Ogorodnikov's favorable review, possibly because Ogorodnikov, as he had reminded Kuiper, was not an expert in lunar or planetary science, while Massevitch had made occasional contributions to the field. One reason that Kuiper embraced Massevitch's charge of fakery was that it resonated with his own doubts about the spectrum. By early spring 1959 Kuiper's criticisms of the spectral evidence (including published critiques for the first time) fairly bristled with contempt. He argued, for instance, that the apparent bright emission band near 4,700 ang-

[39] Kyril Ogorodnikov to Kuiper, 4 Feb. 1959, Box 13, Kuiper papers.

[40] Alla Massevitch to Kuiper, 26 March 1959, Box 12, and "Massevitch" folder, Box 18, both ibid.

stroms seemed to contradict the physical environment expected of an active eruption[41].

More important, perhaps, Massevitch's arguments resonated with Kuiper's emerging view of how the Soviet community of astronomers operated and of Kozyrev's standing within this community. Although Kuiper had, through his commitments to the IAU and his work at Yerkes-McDonald, been in contact with individual Soviet astronomers, his first visit to the Soviet Union had not come until August 1958, when he had attended the Moscow meeting of the IAU. Like many American astronomers he was intensely curious about this vast, scientifically advanced, yet relatively mysterious land that had become the focus of U.S. political, military, and cultural anxiety; and, like so many, he revealed his thoughts in a trip diary.

Kuiper was not reassured by what he saw that August. His diary is filled with impressions of a bleak and threatening culture; he noted the absence of paved roads in Soviet villages, the likelihood that bugging devices were present in his hotel room, and his feeling of isolation from all news from the West. He had particularly wanted to visit nearby astronomical facilities to assess the quality of instrumentation and to gain firsthand impressions of scientific work; he had been prevented from doing so. Like other American astronomers, he complained that the scientific meetings had been deliberately planned to provide little opportunity for personal discussions, and that the scheduling of evening talks made it nearly impossible to visit informally with Soviet colleagues. What Kuiper did gain from his Moscow visit was the impression that Soviet astronomers remained vulnerable to political pressure, and that the boundaries between science and the state remained subject to incursions by political authorities (although the situation did not seem to him nearly as grim as that in Soviet genetics during the height of Lysenko's power). In short, the Soviet astronomical community could not be evaluated by the same standards that applied in the West. "Rus-

[41] Kuiper to Alla Massevitch, 13 April 1959, Box 12, ibid.; and Kuiper, letter on Kozyrev's observation of volcanic activity, *Sky Telesc.*, 1959, 18:307. Other American astronomers sought Massevitch's opinions on the reliability of other Soviet astronomers; see, e.g., Donald H. Menzel to Massevitch, 30 April 1960, Box 42, HCO director's papers.

sian scientists are sufficiently intelligent to know when they are compromising reality," Kuiper had written to George and Priscilla Polyani, editors of the bulletin *Science and Freedom*. "The real problem is to bring them and their countrymen to a condition where they can afford to be objective in official statements and articles[42].

Kuiper came to accept the view that Soviet scientific results could be properly evaluated only through extensive local knowledge, which only Soviet contacts could supply. Although Massevitch's response did not directly address the issue of Kozyrev's place within Soviet astronomy, it reinforced Kuiper's impression that Kozyrev was a marginal actor within the Pulkovo community, perhaps tolerated only because of his past nightmarish persecution. Kuiper had long had suspicions about the reliability of Kozyrev's planetary studies, but now his doubts assumed a more cynical cast. He expressed his new thinking in letters to astronomers, federal patrons, and administrators of the burgeoning space agencies, intending to forestall further attention to Kozyrev's claims. But it is clear that he also believed he was communicating to them important information about the evaluation of all Soviet research, and thus providing a service that few American astronomers were capable of offering. His unguarded thoughts were nowhere as clearly expressed as in a letter to a political scientist who wrote him in the early fall of 1959 to ask why he had criticized the work of another Soviet astronomer, I. S. Shklovsky. Such cases, Kuiper responded, were "part of the baffling picture which scientists in the West are facing" when attempting to evaluate the work of their Russian colleagues:

Kozyrev's publication on the eruption of the central peak of the Crater Alphonsus on the Moon is a somewhat related case. The best informed opinion on such cases appears to be that men who are mildly (Shklovsky) or severely (Kozyrev) persecuted by the Communist party and who have not been protected by their colleagues, use that sort of comic opera performances to attract attention to themselves and to

[42] G. P. Kuiper, trip diary, and Kuiper to Raymond Mitchell, 20 Sept. 1965, both Box 18; and Kuiper to G. and P. Polyani, 28 Oct. 1958, Box 28; all in Kuiper papers. On Lysenko's influence in Soviet genetics, including foreign perceptions of the controversy, see David Joravsky, *The Lysenko Affair* (Cambridge, Mass.: Harvard Univ. Press, 1970).

embarrass their colleagues. We in the West would make a mistake if we would take these mental acrobatics seriously[43].

Kuiper seemed to believe that the judgments of astronomers who were in favor with the Communist Party — individuals like Massevitch and Ambartsumian, who were at the zenith of the Soviet astronomical community and enjoyed the freedom to travel to the West — were more trustworthy and objective than those out of favor with the Party. If Kuiper recognized the inapplicability of this reasoning to Soviet genetics, he committed none of his doubts to paper.

By mid 1959 Kuiper stressed scientific arguments for rejecting Kozyrev's claim in publications and to the press, but in private he made clear that the confidential assessments of Kozyrev's character he had received from Soviet contacts had influenced his judgment more heavily. Kuiper usually refused to name Massevitch and other Soviet astronomers who provided him with information, claiming that to expose them would jeopardize his continued access to scientific results in Soviet astronomy and space science. As he pointedly reminded a NASA leader, such information was as important to American research in lunar science as new research results developed at home[44].

Kuiper's attacks on Kozyrev's work succeeded in stalling possible U.S. and Canadian plans to search for similar variable phenomena on the Moon. That they did had much to do with technical aspects of the matter: the belief of American scientists that the event was indeed improbable, the degree of telescope time such searches would require, and Kuiper's solid reputation as a spectroscopist and lunar astronomer. But these decisions also reflected broader worries about Lysenkoist influences and the ability of Soviet astronomers to speak freely, leaving a residue of doubt regarding controversial claims by Soviet researchers.

One sign of American scientists' sensitivity to the issue of political interference with Soviet research was provided, ironically, by Urey.

[43] Kuiper to Albert Parry, 10 Oct. 1959, Box 18, Kuiper papers. Kuiper had criticized Shklovsky's suggestion that unusual properties then attributed to the orbits of the two small moons of Mars might be accounted for by assuming the moons were artifacts of an ancient Martian civilization; see Parry to Kuiper, 30 Sept. 1959, ibid.

[44] Kuiper to Robert Jastrow, 2 Dec. 1959, ibid.

Urey was certainly the most adamant of Kozyrev's supporters in the United States, and he repeatedly argued that he saw no reason to reject Kozyrev's spectrum or his interpretation of it. He regarded Kuiper's attacks on Kozyrev's integrity as little short of scandalous. Yet Urey was himself worried that Soviet work in planetary geochemistry suffered from intrusions by political factors, and perhaps for this reason he appeared to mute his criticisms of Kuiper's approach[45]. Kuiper's rejection of lunar volcanism prevailed within U.S. astronomical institutions and their patrons until detente allowed Soviet and American scientists new opportunities for direct interaction.

IV. COMPETITION AND NATIONAL SECURITY

Kuiper's views of Kozyrev were influenced not only by his theoretical, institutional, and professional commitments, but also by his views of the relation of science and national security, and by his anxieties over retaining access to new federal patrons and patronage. By 1959 the rapid expansion of space research, and the continued identification of lunar rockets with technological superiority in the Cold War, placed NASA officials under still greater pressure to learn more about the lunar environment. In December 1959 officials of the Jet Propulsion Laboratory received authorization to develop what became known as Project Ranger, a series of spacecraft designed to explore the Moon through hard-landed instrument packages and later through television images. The first Rangers were planned for launch in 1961. While American plans coalesced, two Soviet craft, Lunik I and Lunik III, scored impressive scientific and political victories by sweeping past the Moon. Lunik I relayed information indicating that the Moon possessed no significant magnetic field, while Lunik III photographed the previously hidden lunar far side[46]. (Lunik II hit the Moon on the eve of Soviet Premier Nikita Khrushchev's visit to the United States, compounding American angst over inferiority in the space race.) Respond-

[45] H. C Urey to B. J. Levin, 1 Aug. 1956, Box 52, Folder 35, Urey papers.

[46] Koppes, *JPL and the American Space Program* (cit. n. 2), p. 106; R. Cargill Hall, *Lunar Impact: A History of Project Ranger* (Washington, D.C.: NASA, 1977); and Tatarewicz, *Space Technology* (cit. n. 2), p. 28.

ing to the heightened demand for scientists experienced in solar system astronomy, including celestial mechanics, such schools as Yale and Cincinnati began expanding their graduate programs or offered summer schools, while Harvard, Colorado, and Caltech initiated bold interdisciplinary programs designed to appeal to the broadening range of patrons in this field, including the NSF, the Air Force, and NASA. Although new funds were provided for these efforts, competition for them remained fierce[47].

Kuiper remained an active competitor for these funds, and increasingly willing to use all available means to sustain his programs in solar system research. Demands on his entrepreneurial skills increased in early 1960, when he made the decision to transfer his research contracts, supporting staff, and graduate students (ten people in all) from Chicago to the University of Arizona. The shift came about after serious departmental conflicts at Yerkes derailed his plans to expand his research further at that facility. The institutional difficulties that torpedoed his efforts to enlarge the institutional base for solar system astronomy at Chicago reflected the rapid changes then affecting American astronomy. During 1959 Kuiper had continued to attract new federal and military patronage for research projects and new instruments at Yerkes-McDonald, including a 28-foot infrared-microwave telescope for the McDonald site, intended for studying planets and cool stars. But by the fall of that year Kuiper faced increasing opposition from Yerkes-McDonald staff members whose principal research interests involved stars and galaxies. Rising controversy over the allocation of telescope time between these fields of research, as well as Kuiper's handling of a large Air Force grant to build a telescope for galactic research in Chile, caused university officials to intervene and, ultimately, to force Kuiper's resignation as director of the observatories. Kuiper's gruff, autocratic directorship was clearly a major cause of the conflict. But so too was the increasing ability of the NSF and other patrons to fund research in stellar and galactic astronomy, giving astronomers with interest in these fields ever greater influence in setting observa-

[47] Donald H. Menzel and Gerard de Vaucouleurs to Edward R. Dyer, 2 Dec. 1959, Box 32, HCO Director's papers; and Tatarewicz, *Space Technology*, pp. 111, 114.

tory policy. Although Kuiper came to welcome the move to Arizona, he had exclusive access there to just one major telescope, a 36-inch reflector. To perform the kind of research needed to fulfill his lunar contracts, as well as the more ambitious programs for lunar and planetary research he envisioned, he found himself more dependent than ever on obtaining major funding from federal agencies such as NASA to finance the building of entirely new instruments[48].

Kuiper continued to seek new consulting opportunities, aware that they gave him a chance to impose his professional standards on new work in the field and to increase his usefulness to major patrons. He saw the heightened national competition between the United States and the Soviet Union as an opportunity for advising federal patrons on the burgeoning Soviet achievements in space and lunar science, often hidden behind veils of secrecy[49]. After 1958 Kuiper took on new consulting work with such agencies as General Electric and the Armour Research Foundation of the Illinois Institute of Technology (for which "secret" clearance was required) and secured new opportunities for advising officials at the Jet Propulsion Laboratory, fast becoming the lead center for NASA in studies of the Moon and planets. Kuiper used such occasions to criticize Kozyrev's reported observations as unfounded, and to promote his interpretation of what could and could not be trusted in the publications of Soviet astronomers[50].

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[48] Kuiper to W. Gordon Whaley, 19 Aug. 1960, Box 14, Kuiper papers; G. and E. M. Burbidge, memo, ca. Oct. 1959, Burbidge file, Morgan papers; Kuiper, "Proposal for the Development of an Infrared and Microwave Facility at the McDonald Observatory of Texas," Univ. of Texas 1960 file, Yerkes Observatory director's files, Williams Bay, Wisconsin; Cruikshank, "Kuiper" (cit. n. 5); and Ewen A. Whitaker, *The University of Arizona's Lunar and Planetary Laboratory: Its Founding and Early Years* (Tucson: Univ. Arizona, 1985).

[49] Kuiper, "Report on Symposium, 'The Moon'", confidential CIA draft, ca. Dec. 1960, p. 2, Box 14, Kuiper papers. Limited communications by Soviet authorities made it difficult for astronomers to obtain even the most elementary information, such as the orbital characteristics of the Sputnik satellites; see Wolfgang Priester, interview by Ronald Doel, 19 Jan. 1987, AIP; and Walter Sullivan, *Assault on the Unknown: The International Geophysical Year* (New York: McGraw-Hill, 1961), pp. 72–73

[50] See Kuiper to R. N. Dyruff, 7 June 1958, and R. O. Buchanan to E V. Kelly, 15 May 1958, both Box 11, Kuiper papers.

Kuiper was not the only American astronomer in this period to find his dedication to scientific pursuits strained by loyalties to national aims. Many scientists discerned a genuine Soviet political threat during the Cold War years, and those who filled out mandatory "trip reports" for the State Department after attending the International Astronomical Union meeting in Moscow in 1958 were aware that their information would serve political rather than scientific ends. What distinguished Kuiper from his colleagues was his willingness, even eagerness, to provide the American intelligence community with such information. On 10 April 1959 Kuiper successfully proposed to the Central Intelligence Agency that he "review and evaluat[e] current Soviet astronomical literature" to aid American astronomers and government policymakers. Kuiper subsequently made clear the limits of his involvement: he would not deliberately deceive Soviet colleagues for political gain, as he was apparently requested to do. But he saw no harm in supplying the government with information of strategic or possibly military importance, and believed that such an evaluation would prove helpful in planning future research[51].

To assist him on the project, Kuiper hired a Yugoslavian astronomer, Leo Randic. Randic was fluent in Russian and had visited many Soviet astronomical institutions but was "entirely western in outlook" since he was "raised in Roman Catholic surroundings" (Randic had also spent a year in Scotland). The project ran from November 1959 to October 1960, with Kuiper officially devoting to it 5 percent of his professional time and Randic 75 percent of his time. Together they produced two lengthy confidential reports, including a detailed commentary on Soviet astronomers, their work, and their institutions. The reports assessed the relative strengths and weaknesses of American and Soviet solar system astronomy and compared the number of American and Soviet workers in the field.

In his proposal to the CIA Kuiper declared that his review would be based principally on published documents. His view of that literature (including Kozyrev's work) remained strongly influenced by his occasional personal contacts with Soviet astronomers. For example,

[51] Kuiper to Herman L. Croom, 18 Oct. 1960, Box 33, ibid., 52; Kuiper to CIA, 10 April 1959, Box 33, ibid.

in private letters to American researchers in the fall of 1959, Kuiper sharply criticized the far-side lunar images attributed to the Lunik III mission, declaring that they appeared to be gross extrapolations from marginal data or even outright forgeries. Yet by October 1960, in his final CIA contract report, Kuiper praised the Lunik program as one of "great competence" whose data had not been "over-interpreted," on account of the "calm competence" of the Lunik III science team[52] Kuiper's change of view owed much to the visit of a Soviet astronomer, V. I. Krassovsky, to the Yerkes Observatory in late November 1959. Krassovsky was one of the few Soviet astronomers to visit this facility in the late 1950s, as exchange visits increased only gradually following the death of Stalin. An atmospheric specialist, a high-ranking official in the emerging Soviet space program, and a member of the Lunik mission science team, Krassovsky had been invited to the United States as part of a program organized by the American Rocket Society; he apparently had asked to visit Kuiper on account of the Yerkes lunar-mapping program. The two men, joined by Randic, gathered for a U.S. Thanksgiving Day meal at the nearby Lake Lawn Lodge. Krassovsky and Kuiper then retreated to Kuiper's home where, over refreshments served by Mrs. Kuiper, the two men conversed long into the night[53].

The meeting made a strong impression on Kuiper's views of Soviet work in solar system astronomy, and, to judge by his notes on the meeting, played a large role in shaping the tone and content of his reports to the CIA as well as to colleagues and patrons. The force and assurance of Krassovsky's character, as well as his detailed explanations of the Lunik III imaging system, Kuiper reported, were what persuaded him that the lunar far-side photographs were indeed genuine. (To what

[52] Kuiper, "Memorandum on Russian Astronomy: Report No. 1,"ca. Oct. 1959, p. 10, Box 33; Kuiper to J. J. Raimond, ca. summer or fall 1959, Box 28; and Kuiper to Lawrence Kimpton, I Nov. 1959, p. 3, Box 18; ibid. Translated excerpts from the Lunik III science reports circulated in the United States prior to the preparation of official translations; see, e.g., Leo Goldberg, "Translation from Russian Astronomical Circular No. 206, December 12, 1959, 'First Results of Investigation of Photographs of the Other Side of the Surface of the Moon'", Dec. 1959, copy in Box 11, ibid.

[53] Kuiper to Andrew G. Haley, 8 Dec. 1959, Box 31, ibid.

degree Kuiper's views persuaded other American astronomers is uncertain, although a consensus along those lines had emerged by early 1960.)[54] Kuiper also valued Krassovsky's corroborating testimony that Kozyrev, in his view, was personally unstable, and furthermore, that he had seen the critical spectrum of Alphonsus and found it "defective". Kuiper repeated his question about Kozyrev a second time, uncertain whether Krassovsky, speaking through an interpreter, had understood; he was delighted when Krassovsky reiterated his criticism. What Kuiper appeared to value most from the meeting was not merely the specific information he received, but that their long, private conversation seemed to pierce through the political tensions of the Cold War and allowed them to speak as scientists, one to another: "The visit was very worthwhile and cleared up a number of questions on which we could not have obtained good answers except by such personal discussions." Throughout much of 1960 Kuiper cited the authority of his meeting with Krassovsky in expressing opinions on the nature of Soviet solar system research[55].

Kuiper was in effect operating an intelligence operation through the offices of Yerkes-McDonald; the Cold War permitted him to resume practices familiar to him from World War II. The secret nature of his reports to the CIA allowed him to criticize Soviet astronomers in ways not otherwise possible except through private letters, another vehicle Kuiper showed little hesitation in using to praise or to attack the work of Soviet researchers. Because of the secrecy imposed by the contract, he did not (at least in correspondence) discuss this work, and thus it is difficult to discern the reactions to it of astronomers at Yerkes-McDonald or elsewhere. But it is clear that Kuiper regarded his network of contacts as essential for evaluating new knowledge, and contracts of this kind as a legitimate, even essential means of ensuring that astronomical institutions engaged in highly competitive fields of research would remain viable.

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[54] Leo Goldberg to Ellis Mott, 5 April 1960, Box 12; and Kuiper to Rick Riley, 21 Mar. 1960, Box 13, ibid.

[55] Kuiper to James J. Harford, 8 Dec. 1959, Box 31, ibid.; Kuiper to Hibbs, 21 Nov. 1960 (cit. n. 14); and Kuiper, "Memorandum on Russian Astronomy", (cit. n. 52). p. 10.

V. DETENTE, SCIENCE, AND COMMUNICATIONS: THE LENINGRAD CONFERENCE

Individual encounters of the kind Kuiper valued soon lost their singular influence, however, as contacts between American and Soviet astronomers increased. Political relations between the United States and the Soviet Union were hardly warm in 1960: the U-2 surveillance plane piloted by Francis Gary Powers was shot down on 1 May of that year, causing the planned Paris summit meetings between Eisenhower and Khrushchev to be canceled. Nevertheless, plans to hold the general meeting of the International Astronomical Union in the United States in 1961 went forward, and funds for American scientists to attend international meetings abroad were increasingly available. One consequence was that Mikhailov was successful in proposing that a special meeting of the IAU's lunar commission, devoted entirely to lunar research, be held at Leningrad in December 1960. Mikhailov named Kuiper as one of six members of the program committee.

Kuiper welcomed the meeting. It would allow him to gain new firsthand information about the progress of Soviet research in lunar, planetary, and space science and to deepen his acquaintances with Soviet colleagues. Pleased with Mikhailov's desire to limit the conference to fifty or sixty participants, Kuiper also realized he could clean house by excluding those he considered "substandard" American researchers, particularly those, like Alter, who accepted Kozyrev's evidence and interpretation of lunar volcanism. His gambit failed when Kopal, who had also been appointed to the Leningrad program committee and was a close associate of Urey, learned of the omissions. Kopal promptly issued invitations to lunar astronomers ignored by Kuiper[56]. By the time the meeting convened on 6 December 1960, the number of invited guests had climbed to over one hundred. Of the fifty-three invited presentations, twenty-two were offered by individuals from the United States; representatives of at least six universities and various government agencies were

[56] Kuiper to Zdensk Kopal. 30 Nov. 1960, Box 11, Kuiper papers; and Kopal to Harold C Urey. 4 Aug. 1960, and Urey to Kopal, 21 Sept. 1960, Box 50, Urey papers.

present[57]. The meeting was the first major scientific conference devoted entirely to Earth's nearest neighbor.

Kuiper did not keep a trip diary, as he had on his visit to Moscow in the summer of 1958, two and a half years before. His responses to the meeting must be gleaned from his subsequent letters and from comments he jotted in the margins of his program. What seems clear is that he expected no major surprises. Shortly before the meeting he had written Mikhailov again. In language that recalled the efforts of George Ellery Hale, Wallace W. Campbell, and other leaders of early twentieth-century American astronomy to police discussions of Percival Lowell's unorthodox claims of canals on Mars, Kuiper protested that discussions of lunar volcanism were reducing the stature of the field. Rising amateur reports of transient lunar phenomena threatened the growth of solar system astronomy at his own facility and elsewhere. Pulkovo's failure to address the "internal contradictions" of the spectrum, or to deny its authenticity, he declared, perpetuated a problem of international relations[58]. When Mikhailov did not respond, Kuiper's confidence in his convictions about Soviet astronomy soared.

The Leningrad meeting nevertheless produced surprises. None of the five sessions, devoted to such broad topics as radio observations and rocket explorations of the Moon, focused solely on activity on the lunar surface. But a session on the "origin, internal structure, and surface" of the Moon, held on 8 December, included two talks devoted to Kozyrev's 1958 spectrographic investigation of Alphonsus. Kozyrev spoke first. Kuiper listened intently as Kozyrev reiterated his claim that his plate gave unambiguous evidence of active lunar volcanism, including thermal emissions. In the second talk the Leningrad astronomers A. A. Kalinyak and L. A. Kamionko offered a somewhat different interpretation. They subjected the original plate to a microphotometric analysis, a standard procedure used to determine precise line identities

[57] Zdenek Kopal and Zdenka Kadla Mikhailov, eds., *The Moon* (I.A.U. Symposium 14) (New York: Academic Press, 1962), pp. v–ix.

[58] Kuiper to A. A. Mikhailov, 24 Aug. 1960, Box 11, and Kuiper to Bart Bok, 22 Sept. 1959, Box 10, both in Kuiper papers; and David H. DeVorkin, "W. W. Campbell's Spectroscopic Study of the Martian Atmosphere", *Quarterly Journal of the Royal Astronomical Society*, 1977, 18:37–53.

as well as quantitative values for them. Their judgment was that the spectrum indeed revealed emission, that, for example, Kozyrev's identification of the Swan bands seemed virtually certain, as faint carbon bands (which Kuiper had not discerned on his copy prints) were evident under microphotometric scrutiny. Only on one point — although a substantial one — did these researchers differ from Kozyrev: they argued that the spectral evidence was more consistent with cold gases, possibly released as a discrete gas cloud, than with hot, volcanic outpouring. Such an event, quiescent in nature, did not require that the Moon be volcanically active[59].

Kuiper's notes unfortunately do not record when his impressions of the spectrum and Kozyrev began to change. The pivotal moment seemed to come later in the conference when, peering through a high-powered magnifying glass, he personally examined the original slides. This private confrontation with Kozyrev's evidence had great effect and marked an unmistakable critical moment in the controversy. Again and again in letters to colleagues following the conference Kuiper declared that he "had no doubt left that the spectra are genuine." Even American researchers who supported Kozyrev before the conference were impressed: Urey advised Philip Abelson of *Science* that "unanimous" agreement developed at Leningrad that Kozyrev's plates were authentic[60].

For Kuiper to admit his mistake, after his forceful assessments to the contrary, required no small amount of courage. Certainly his conversion was aided by his relief that the Kalinyak-Kamionko analysis re-

[59] Versions of these papers were subsequently published: see N. A. Kozyrev, "Spectroscopic Proofs for Existence of Volcanic Processes on the Moon," and A. A. Kalinyak and L. A. Kamionko, "Microphotometric Analysis of the Emission Flare in the Region of the Central Peak of the Crater Alphonsus on 3 November 1958," both in Kopal and Mikhailov, *The Moon* (cit. n. 57). pp. 263–272, 273–287. For a Soviet perspective on the controversy see A. N. Dadaev, "Pervootkryvatel' lunnogo Vulkanizma (K 75-letiiu so dnia rozhdeniia N. A. Kozyreva)", *Fizicheskie aspekty sovremennoi astronom* (Sbornik nauchnykh trudov) (Leningrad: Akademiia Nauk SSSR, 1985), pp. 8–24. I thank Robert McCutcheon for calling my attention to this work.

[60] Kuiper to Lloyd V. Berkner, 27 Dec. 1960, Box 11, and Kuiper to Homer E. Newell, 7 Nov. 1963, Box 8, both in Kuiper papers; and Harold C. Urey to Philip Abelson, 9 Jan. 1961, Box 2, Urey papers.

quired no volcanic activity: it meant only "that occasionally some cold gases escape" and thus "no drastic change in the model for the interior of the Moon is needed"[61]. The result, in other words, posed little threat to Kuiper's evolutionary model of the Moon, and the microphotometric analysis answered objections he had first raised against Kozyrev's claim that the spectra demonstrated gaseous emission.

Important as Kuiper's confrontation with the evidence was, it was not alone responsible for causing his views to shift. In early 1959 Kuiper was aware that several American and British scientists had suggested that Kozyrev's spectra might be explained by nonthermal leakage of gases from the Moon; he had rejected this alternative on the strength of arguments by such visiting Soviet astronomers as Massevitch and Krassovsky (neither of whom attended the Leningrad conference). Moreover, in his private communications to patrons, Kuiper had emphasized social and professional reasons for rejecting Kozyrev's work, stressing his own qualifications for evaluating the research results of Soviet scientists.

In subsequent letters to his most influential patrons and contacts, including Homer Newell of NASA, the geophysicist and administrator Lloyd Berkner, and his handlers at the CIA, Kuiper paid comparatively little attention to Kozyrev's scientific findings. He focused instead on his new impressions of the Soviet astronomical community. One of the major differences between the IAU meetings in Moscow in 1958 and the Leningrad conference, he noted, was the freedom he had to travel about the city, to visit the homes and offices of Soviet colleagues such as Ogorodnikov and Mikhailov for long evening conversations, and to assess firsthand the quality of their laboratories and instruments. He visited Sharonov's laboratory at the Leningrad University ("located in a former girls' school... terribly run down, on the lower floor... but the laboratory, consisting of five rooms on the second floor, was all right... The equipment was simple, home-made, but adequate"), had dinner with Mikhailov, and celebrated his fifty-fifth birthday at a small party hosted by Ogorodnikov. Frequently he conversed in French, needing no translator[62]. The details Kuiper sought to convey to American officials

[61] Kuiper to Subramanyan Chandrasekhar, 26 Dec. 1960, 7 Jan. 1961, Box 10, Kuiper papers.

[62] Kuiper, "Report on Symposium" (cit. n. 48), p. 2.

on his return on the whole resembled those in the major assessment he and Randic had made for the CIA earlier that year; the evaluation of Kozyrev was the greatest change. Yet his impressions of the organization and practice of Soviet science had shifted. He found fierce competition among scientists at the "second echelon" as well as those at the top. "Behind the apparent solid front of Russian scientists there is a surprising amount of in-fighting and professional rivalry, of an intensity that seems to exceed that occasionally found in the U.S. It seems as if the methods by which Russian politicians come to the top are used also among the scientists. Such infighting, he discovered, existed between Kozyrev and Massevitch, and similar conflicts had apparently led Krassovsky to attack Kozyrev's credibility and results. "Krasovsky had lied to me at a reception in my home in Wisconsin on Thanksgiving 1959," Kuiper declared, "as I have now discovered and verified"[63] It was clearly difficult for him to accept that scientific objectivity had been so easily compromised by such personal squabbles, or that his own institutional ambitions had been jeopardized in the process.

After returning to the United States, Kuiper took steps to encourage the study of lunar transient phenomena. Although Kuiper did not personally launch studies of possible lunar outgassing since he remained baffled, despite his acceptance of Kozyrev's evidence, he did permit others at his Lunar and Planetary Laboratory in Arizona to do so. (The acronym LTP, for "lunar transitory phenomena," was later coined by a member of Kuiper's staff). He also used his influence to encourage other government-funded lunar mapping programs, including one launched at the Lowell Observatory by the Air Force's Aeronautical Chart and Information Center, to monitor the Moon for signs of possible change. Attempts to locate additional examples of such phenomena generally did not succeed, despite the increasing sophistication of lunar spacecraft as well as ground-based studies, and many lunar scientists found the matter of Kozyrev's plates unsatisfactorily resolved[64].

[63] Kuiper to Berkner 27 Dec. 1960 (cit. n 60), p. 3; and Kuiper, "Report on Symposium", p. 3.

[64] Kuiper to Homer Newell, 7 Nov. 1963, Kuiper papers; J. M. Burley and Barbara M. Middlehurst, "Apparent Lunar Activity: Historical Review", *Proceedings of the National Academy of Science,* 966. 55:1007–1011; Winifred Sawtell

VI. CONCLUSION

Kuiper's programs of lunar research benefited enormously from federal and military patronage after the launch of Sputnik. The flood of government support for lunar research after 1958 helped strengthen the existing institutional foundations for solar system astronomy, permitted the construction of new, dedicated telescopes, and encouraged the development of new interdisciplinary research programs within traditional academic departments of astronomy. Solar system astronomy at both Chicago and Arizona grew rapidly as a direct result of the commitment of national resources to the space race. Still, new telescope construction did not keep pace with this unprecedented expansion, causing competition to increase sharply between solar system astronomers and members of other specialties for telescope time.

As director of the Yerkes-McDonald observatories and subsequently of the Lunar and Planetary Laboratory, Kuiper recognized that successful administration required him to fulfill a variety of roles. He was most comfortable providing his patrons the kind of knowledge that astronomers engaged in solar system research after World War II were well positioned to offer: expertise in preparing lunar maps, in evaluating the kinds of research that could be undertaken by *in situ* spacecraft studies, and in assessing the limits of knowledge in the various cross-disciplinary fields that contributed to lunar studies. Here the role of administrator fit comfortably with his duties as a scientist and leader of a professional field. Yet to maintain lunar research as a competitive subfield of astronomy, Kuiper found it necessary to promote the value and relevance of the field actively to new federal and military patrons, and to keep the field before the public eye. Seeking a secure niche and aware of the high scientific and political value attached to Soviet lunar results, Kuiper successfully promoted himself to patrons as an expert interpreter of Soviet science.

The Kozyrev controversy holds much significance for the sociology of science. Kuiper's role as a scientist-entrepreneur was strained by Kozyrev's reported discovery of lunar volcanism, which threatened his theories of lunar evolution and challenged his credentials in advising

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Cameron, "Lunar Transitory Phenomena," *Sky Telesc, 1990,*9:265–268; and personal discussion with E. M. Shoemaker, Flagstaff, Arizona, 4 Dec. 1990.

government agencies about the lunar environment Many of the steps that Kuiper took to interpret Kozyrev's character and credentials at a distance were motivated by his desire to defend his own theoretical concepts, to which he was deeply attached. There is no question that his abrupt reversal of opinion about Kozyrev's evidence in Leningrad was eased by new interpretations of the spectrum by Soviet astronomers that left his theoretical framework undisturbed. Further, Kuiper was no less attentive to social context following the Leningrad encounter than before: he lost no time attempting to discover why his Soviet sources had led him astray, or in communicating these new insights to American colleagues and patrons. It should not be concluded, however, that scientific evidence played anything less than a primary role in shaping Kuiper's view, for only after holding Kozyrev's evidence in his hand did he be come convinced of its authenticity and significance. These social dynamics support arguments by Peter Galison, Yves Gingras, and Silvan Schweber that while personal and social programs must be taken into account in explaining the course of scientific controversies: phenomenological constraints cannot be overlooked in determining the outcome. The events also show that public demonstrations of scientific results continue to play important roles, as Steven Shapin and Simon Schaffer have recently argued for the seventeenth century[65].

Kuiper's attempts to comprehend Kozyrev's work also illustrate how limited communications were between American and Soviet scientists during the Cold War. If the Soviet Union was difficult to understand politically or culturally, the workings of its scientific community sometimes seemed equally opaque. The willingness of American scientists to accept the idea that Soviet scientific results were tainted by political interference reflected anxieties felt by Americans during this

[65] Peter Galison, *How Experiments End* (Chicago: Univ. Chicago Press, 1987); Yves Gingras and S»van S. Schweber, "Constraints on Construction", *Social Studies of Science. 1986,* 16:372-383; and Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump*: *Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton Univ. Press, 1985). Relevant literature is voluminous; for recent reviews see Jan Golinsky, "The Theory of Practice and the Practice of Theory: Sociological Approaches in the History of Science", *Isis*, 1990, 81:492-505; and Timothy Lenoir, "Practice, Reason, and Context: The Dialogue Between Theory and Experiment," *Science in Context*, 1988, 23–22.

time as much as actual conditions abroad. That Kozyrev's plate was a singularity and thus unusually difficult to verify does not invalidate its ability to symbolize the broad problem of evaluating evidence from distant scientific communities[66].

Kuiper can be regarded as representative of many American scientists during the Cold War. His deeply felt commitment to solar system astronomy made him anxious to understand new developments in the field, and he was genuinely concerned to understand Soviet progress in his subject. He used his network of contacts to evaluate the reliability of new and controversial results, especially those produced by relatively unknown workers. But his willingness to serve the national and political aims of his patrons meant that he needed his contacts as much for intelligence-gathering operations as to understand the science at hand. Kuiper's view of science was thus strongly influenced by political considerations and the economic and political significance attributed to science after 1945. His arguments about Soviet results owed much to his need to be seen as an objective judge of Kozyrev's work as well as correct in his views on the lunar interior. Science divorced from national priorities or international competitiveness had no place in Kuiper's outlook.

Kuiper's experience as an entrepreneurial manager of science in the 1950s was ultimately shaped by conflicting obligations, and it is difficult to say whether his perceptions of Soviet science would have differed had he not faced the competitive pressures of maintaining large-scale research programs. He veered from an eager belief that the Soviets were fallible and their deceptions wide-scale to forceful assertions that he had been misled over Kozyrev. In a stilted yet sincere gesture at the conclusion of the Leningrad conference he toasted Kozyrev's discovery, declaring that Americans "will take home [a] very deep impression: the one-ness of our civilization and the efforts we must all make to preserve it".[67] Certainly the contradictions were in part inherent in Kuiper's character, but they also reflected the character of American science in the Cold War.

New York City, NY, USA 1992.

[66] On the question of singularities in a disciplinary context see Galison, *How Experiments End* (cit. n. 65).

[67] Kuiper, "Motion of Thanks", Box 11, Kuiper papers.

Postscript [2007].

The possibility that transient lunar phenomena were more than exceedingly rare events continued to intrigue astronomers and planetary scientists in the 1960s, as the race to the moon intensified. The U.S. unmanned lunar vehicle Ranger 9, launched in March 1965, was aimed at Alphonsus, photographing the crater's pitted floor with increasing resolution until transmitting its final image three seconds before impact[68]. At Lowell Observatory, researchers attached to the Army Chart and Information Center's lunar mapping program began observing the Moon's unlit portions. In October 1963 they announced their visual discovery of a ruby red glow near the crater Aristarchus that persisted for twenty minutes[69]. Amateur astronomers, aware of Kozyrev's observations and other momentary visual sightings of glows or obscurations, maintained systematic vigils through the 1960s, amassing hundreds of additional observations[70]. Like the Moonwatch teams that hunted Sputnik and its immediate successors in the late 1950s, amateurs played a significant role in planetary sciences research at the start of the space age. Their contributions were perhaps greater than those amateurs made to natural history, the other famous intersection between amateur and professional scientific communities[71].

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[68] Harold C. Urey particularly influenced NASA's decision to target Ranger 9 to Alphonsus; see Urey to J. Green, Aug. 7, 1961, Box 82, Urey papers, and R. Cargill Hall, *Lunar Impact:* (cit. n 46): 299–300.

[69] John Hall, "Date Concerning the Lunar 'Eruptions' reported at Lowell Observatory," Box 7, and Kuiper to Homer Newell, Nov. 7, 1963, Box 8, Kuiper papers; Thomas B. McCord, "The Search for Lunar Luminescence [submitted to *Astrophys. J.*],"in "Geological Sciences at the California Institute of Technology: Report for the Years 1963 to 1966 on the research activities of the Division of Geological Sciences," draft report, p. 211, Division of Geological Sciences files, California Institute of Technology archives.

[70] Burley and Middlehurst (cit. n. 64) and Cameron (cit. n. 64).

[71] Patrick McCray, "Amateur Scientists, the International Geophysical Year, and the Ambitions of Fred Whipple." *Isis* 97, 4 (2006): 634–658, and McCray, *Keep Watching the Skies! The Story of Operation Moonwatch and the Dawn of the Space Age* (Princeton: Princeton University Press, 2008); Sharon E. Kingsland, *Modeling Nature: Episodes in the History of Population Ecology* (Chicago: University of Chicago Press, 1995).

When U.S. Apollo spacecraft began departing Earth for lunar orbit in 1968, astronauts were briefed to look for transitory phenomena. Several sightings were made, although none seemed definitive. Advised by Mission Control in Houston that amateur astronomers had reported a transitory glow in Aristarchus, then still in lunar night, Neil Armstrong peered out his window at the darkened, faintly earth-lit surface drifting by below. It was July 19, 1969, just as Apollo 11 was entering lunar orbit, and two days before the first lunar landing. Armstrong radioed that he saw an "area that is considerably more illuminated than the surrounding area. It just has — seems to have a slight amount of fluorescence to it".[72] Armstrong thought that the region he was looking at was Aristarchus, but he could not be certain. While tantalizing, Armstrong's observation shed little light on this phenomenon. When Kuiper died in 1973, and Kozyrev a decade later, whether the Moon was geologically dead or not remained unresolved[73].

Lunar activity became a lively topic again at the turn of the twenty-first century. Hints that the Moon was not entirely quiescent (from Apollo analyses as well as transient phenomena sightings) convinced NASA managers of the importance of continued efforts to detect sources for the Moon's tenuous atmosphere, and in 1998 the US-launched Lunar Prospector's Alpha Particle Spectrometer identified radon gas, interpreted as evidence for recent lunar out-gassing[74]. The most in-

[72] Quoted in Cameron, "Lunar Transitory Phenomena" (cit. n. 64) The most comprehensive biographical treatment of Armstrong is James R. Hansen, *First Man: The Life of Neil A. Armstrong* (New York: Simon & Schuster, 2005).

[73] In the early 1960s Kuiper backpedaled from his unqualified acceptance of Kozyrev's finding, after mulling over the puzzling fact that emission lines on Kozyrev's spectrum did not extend into shadowed regions, which caused him to consider fluorescence as an alternative explanation. Though he continued to accept that Kozyrev's spectrum was genuine, he remained baffled, writing, "I cannot bring myself to believe what I saw" in the spectrum he inspected at St. Petersburg. On these issues see Kuiper to Gilbert Fielder, April 5, 1963, Box 18, Kuiper papers, and Ernest Opik, "Evolution of the Moon's Surface", *Irish Astron*. J. 8 (1967): 38–52. A popular account of the Kozyrev-Kuiper controversy is Ronald E. Doel, "The Lunar Volcanism Controversy", *Sky and Telescope* (Oct. 1996):26–30.

[74] Stefanie L. Lawson et. al, "Recent Outgassing from the Lunar Surface: The Lunar Prospector Alpha Particle Spectrometer", *J. Geophys. Res*. 110 (2005): 1029.

triguing new finding came in November 2006. After carefully analyzing an unusual 8 square kilometer region of the Moon called the Ina structure — photographed in exquisite detail by the Apollo 15 orbiter in 1971 — a research team led by Brown University planetary scientist Peter H. Schultz concluded that Ina was younger than ten million years. Indeed, since Ina lacked new impact craters, had razor-sharp edges lacking signs of erosion, and had a spectral signature indicating extreme youth, Schultz and his colleagues argued that Ina might be younger still, and proposed that these features result from recent, episodic out-gassing from deep within the Moon[75]. Schultz's declaration that the Moon may not be so dead after all sparked a burst of media stories reminiscent of headlines announcing Kozyrev's announcement of lunar volcanism forty-eight years before. His analysis–relying on in-situ satellite measurements and data sets shared by researchers worldwide — encountered far less skepticism than Kozyrev's announcement of a single spectrum at the height of the Cold War[76].

On the 15th of September 2007, the Japanese SELENE spacecraft (nicknamed Kaguya) blasted off from the Yoshinobu launch complex on the island of Tanegashima, bound for the Moon. It was soon joined by the Chinese lunar probe Chang'e 1, which set off from the Xichang launch facility in southwest China the following month. India's first mission to the Moon (Chandrayaan 1) is now scheduled for launch in October 2008. In contrast to the Apollo era, where the race to the Moon was primarily stimulated by Cold War political rivalries, the present era of lunar exploration is driven by long-term interest in exploiting

[75] Peter H. Schultz, Matthew I. Staid, and Carl M. Pieters, "Lunar activity from recent gas release", *Nature* 444 (9 November 2006): 184–186.

[76] For a sampling, see Nikhil Swaminathan, "Surface Gassing May Be Evidence of Volcanically Active Moon", *Scientific American* (Nov. 8, 2006), at http://www.sciam.com/article.cfm.chanID=sa003&articleID= C93F91E5-E7F2-99DF-3A07B4F1CA67A272 [accessed December 23, 2007]; Henry Fountain, "Observatory: The Moon Sighs", *New York Times* (November 14, 2006), at http://www.nytimes.com/2006/11/14/science/14observ.html [accessed December 23, 2007], and Richard Harris, "Moon's Surface Shows Sign of a Gas Burp", National Public Radio Morning Edition, November 9, 2006.

lunar minerals for envisioned manned operations[77]. That a half century elapsed between this new round of missions and the dawn of the space age perhaps ought not surprise us: over five decades also separated pioneering expeditions to Earth's poles from the more sustained research operations (and eco-tourism) of recent years[78].

If researchers involved in the Kaguya, Chang'e, and Chandrayaan missions ultimately disagree over their findings, they should not be surprised if these future controversies involve nationalistic suspicions, intelligence-gathering, and efforts of intermediaries to obtain first-hand insights into instrument reliability and international scientific politics. To not anticipate this is to ignore a clear lesson that history offers.

[77] Stephen Clark, "Moon orbiter successfully launched from Earth", *Spaceflight Now* (September 14, 2007), at http://www.spaceflightnow.com/h2a/selene/070914launch.html [accessed December 23, 2007], and Stephen Clark, "Chinese launch spacecraft to explore the moon", *Spaceflight Now* (October 24, 2007), at http://spaceflightnow.com/news/n0710/24chinamoon/ [accessed December 23, 2007].

[78] Stephen J. Pyne, *The Ice: A Journey to Antarctica* (Iowa City: University of Iowa Press, 1986); see also Steven J. Dick, *Consequences of Exploration: Learning from History* (part 2) in http://www.nasa.gov/missions/solarsystem/Why_We_03_pt2.html [accessed December 22, 2007].