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The new prophet: Harold C. Urey, scientist, atheist, and defender of religion

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The New Prophet:
Harold C. Urey, Scientist, Atheist, and Defender of Religion

A dissertation submitted in partial satisfaction of the requirements for the degree Doctor of Philosophy

in

History (Science Studies)

by

Matthew Benjamin Shindell

Committee in charge:

Professor Naomi Oreskes, Chair
Professor Robert Edelman
Professor Martha Lampland
Professor Charles Thorpe
Professor Robert Westman

2011
The Dissertation of Matthew Benjamin Shindell is approved, and it is acceptable in quality and form for publication on microfilm and electronically:

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Chair

University of California, San Diego

2011
# TABLE OF CONTENTS

Signature Page ........................................................................................................ iii

Table of Contents ................................................................................................... iv

Acknowledgements ................................................................................................. v

Vita ........................................................................................................................ xxi

Abstract ................................................................................................................. xii

Introduction ............................................................................................................ 1

Chapter 1, The Making and Remaking of an American Chemist: From a Country Boyhood to World War I ......................................................... 25

Chapter 2, Farm Life and Scientific Stardom: From the Barrett Chemical Company to World War II ................................................................. 94

Chapter 3, Atomic Trauma and New Territory: The Rise of Nuclear Geochemistry in Chicago ................................................................. 162

Chapter 4, Calling for the New Prophet: A Skeptic Argues for the Importance of Religion .................................................................................. 213

Chapter 5, To Hell with the Moon: The Cosmochemist’s Failed Quest for a Rosetta Stone ................................................................. 262

Conclusion ............................................................................................................. 312

Works Cited .......................................................................................................... 316
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ABSTRACT OF THE DISSERTATION

The New Prophet:
Harold C. Urey, Scientist, Atheist, and Defender of Religion

by

Matthew Benjamin Shindell

Doctor of Philosophy in History (Science Studies)

University of California, San Diego, 2011

Professor Naomi Oreskes, Chair

During the 1950s, American chemist and Nobel laureate Harold C. Urey began advocating the necessity of a new “prophet” to bring together the inspiring scientific view of the universe with the moral teachings of the traditional western religions. This was necessary, he claimed, because neither science nor society could survive without these moral teachings. Likewise, Urey believed that these religions could not survive if they were not brought up-to-date with scientific progress. This dissertation is a social and
cultural biography that examines this turn to religion in light of Urey’s religious upbringing in the Church of the Brethren at the turn of the century, his scientific training and rise to fame during the 1910s and 20s, his turn to earth and planetary science after World War II, his attempts at political activism during the Cold War, and his participation in NASA’s lunar exploration program during the 1950s and 60s.

Urey’s turn to religion was not based on a faith in god. He was a self-avowed atheist. This turn was instead a product of two Cold War crises – the postwar trauma of the Manhattan Project and the Cold War trauma of McCarthyism and the loyalty-security system. The first of these crises pushed Urey’s postwar research program away from the isotope separation work that had made him famous and into the earth and planetary sciences. The second crisis pushed Urey’s public rhetoric away from an optimistic scientific utopianism and hope of a world united under one government. He instead turned toward advocating a new, meaningful engagement between science and spirit. This engagement was difficult to foster, even with the largest and potentially most inspiring scientific projects, as Urey discovered in his work with NASA.

Urey’s intervention in the “Big Science” of NASA was no more successful than his intervention in Cold War politics. The bureaucratization of science during the Cold War made it difficult for Urey to champion his view of the moon as a cosmogonic Rosetta Stone. Although scientists found themselves better funded than before WWII, their own agency within the new bureaucratic structure of science was limited.
Introduction

“Harold C. Urey is a phenomenon far too complex to be accounted for by a formula with only a few parameters.”

Joel H. Hildebrand

“The Air that Harold C. Urey Breathed in Berkeley” (1963)

An American Chemist

On Thanksgiving Day 1931, Harold C. Urey and his collaborators Ferdinand G. Brickwedde and George M. Murphy experimentally proved the existence of an isotope of hydrogen with mass two. It was an isotope that until then had been considered either unlikely to exist or too rare to detect. Returning home late to a holiday dinner with friends that had already begun, he told his wife, “Well, Frieda, we have made it.” This discovery, which would win Urey the Nobel Prize in 1934, solidified the scientific reputation of a poor farm boy from Indiana. It also solidified his position as a representative of American science.

Even before the Nobel Prize, Urey – a young Columbia faculty member with a Ph.D. in physical chemistry from Gilbert N. Lewis’s Berkeley laboratory and a postgraduate year in Niels Bohr’s Copenhagen Institute for Theoretical Physics but with

3 “Harold Urey - Biographical Memoirs (Period 1923-1939),” typescript, n.d., 10, Box 191, Folder 9, SLM. This story changed over the years. In 1969 Urey told Forbes that he discovered his success when, on the way to the Macy’s Thanksgiving Day Parade with his wife and children, he stopped at his laboratory to check his experiment: Mary Harrington Hall and Harold C. Urey, “As I See It,” *Forbes*, July 15, 1969, 48.
few accomplishments in the field – had already developed a reputation as an expert at the interface of nuclear physics and physical chemistry. Urey was neither the first American scientist, nor even the first American chemist, to win the Nobel Prize. Two American chemists, Theodore Richards and Irving Langmuir, had already beaten Urey to the prize, as had the American physicists Robert A. Millikan and Arthur H. Compton and the biologist Thomas H. Morgan. Still, observers saw Urey’s achievement as particularly symbolic – he was one of the first chemists of international renown who had been trained entirely in the United States.\(^4\) Coming from a poor background, he had proceeded with no access to the elite educational institutions of the East Coast, and had instead been educated in the Indiana public schools and state universities in the far-flung western states of Montana and California. As his students would later write of him, he represented a new breed of homegrown scientist: “the native American scientist inspired by the problems of pure science, working not toward practical applications, but attempting to formulate the natural laws of the universe.”\(^5\)

When, prior to the announcement that Urey would receive the Nobel, the British physicist Ernest Rutherford congratulated Urey and his American colleagues on opening up research so quickly on this new form of hydrogen, the New York Times took this as acknowledgement from abroad that Americans were beginning to make a larger imprint upon physical research:

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\(^4\) Although Richards received his B.S. and Ph.D. from American universities (Haverford and Harvard), he had been raised in England and France, and, like most chemists of his generation, had also studied in Germany prior to taking up a university appointment in the United States. Langmuir studied in Gottingen under the direction of Walther Nernst. By contrast, Urey had studied at the University of Montana and the University of California.

\(^5\) Harmon Craig, Stanley L. Miller, and Gerald J. Wasserburg, eds., Isotopic and Cosmic Chemistry (Amsterdam: North Holland Publishing Co., 1964), iii.
Fifty years ago we could match the more brilliant European scientific luminaries only with Willard Gibbs, a solitary creative genius, who, despite his position at Yale, made no impression at home with a strange doctrine of ‘statistical mechanics,’ which is now the guiding principle in physics. Today we have at least a hundred mathematical physicists, chemists, astronomers and biologists of the first rank, who draw crowds of students when they lecture at Berlin, Paris, Cambridge or London. In a sense this is a return of bread cast upon the academic waters; for these men finished their education under such great masters as Wien, Nernst, Planck, Thomson and Rutherford himself.6

The Times also took the opportunity to celebrate the fact that this new generation of scientists was working against the perception that the American intellect was solely concerned with practical matters of production and profit: “In an era when the United States is looked upon abroad as the land of materialism, the place where only the profit-making motive counts, it is good to read Lord Rutherford’s words and to realize that not only the spirit of scientific research, but the ability to carry on the work of the great, lies within our laboratories.”7 As Urey’s case illustrated, in some areas of science that had traditionally been dominated by European schools – especially after the devastation of World War I – the United States was actually becoming the preferred place to train.8

By the New York Times’ assessment, Urey and his less famous contemporaries signified nothing less than the rebirth of science in the New World; the young members of the American scientific elect, who made their homes in newly-founded institutions of science in New York, Chicago, Berkeley, and Pasadena, were “pioneers who have given

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7 Ibid.
8 On the effects of WWI on physics and chemistry, see Mary Jo Nye, Before Big Science: The Pursuit of Modern Chemistry and Physics, 1800-1940 (Cambridge MA: Harvard University Press, 1999). The increase in the numbers of American scientists and institutions of science after WWI (and the connection of this new scientific community to Bohr’s Institute) is presented in Daniel Kevles, The Physicists: The History of a Scientific Community in Modern America (Cambridge MA: Harvard University Press, 2001), chap. 14 “A New Center of Physics.”
an impetus to physical science greater even than that which it felt in the romantic days of Faraday, Maxwell, Kelvin, Liebig and von Helmholtz.” The American physicist Karl T. Compton, in an assessment of American science that used Urey as its primary example, drew upon this same pioneer metaphor when he claimed that “While geographical frontiers have shrunk, the boundaries of science are wider than ever before, with more areas for exploration.” The scientists were the inheritors of a great European tradition, but also the American pioneer spirit, as Vannevar Bush would reaffirm a decade later.

A Religious Past

After winning the Nobel, it was difficult for Urey to escape the public eye. Not only were excerpts from his public addresses reprinted in the *New York Times* and other papers of record around the country, but the papers also reported on personal events such as the births of his children. Especially within New York City, Urey was a scientific celebrity; he was certainly not as famous as Albert Einstein (who had emigrated to the United States in the same year that Urey won the Nobel Prize), but he did seem to possess a less enigmatic persona. Quickly a public image emerged of Urey as an all-American. Along the lines of a scientific Horatio Alger story, this image was based upon the narrative of Urey’s journey from a small-town, country boy to a scientific star. Urey’s reputation was that of a smart man, not a genius or even an intellectual. His colleagues credited him with a tenacious character and an uncanny ability to concentrate and apply

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9 “Our Place in Science.”
his work ethic to the most complicated scientific or political problems. Throughout his career, this image of a man who had pulled himself up by his bootstraps within the scientific community would allow Urey to speak uncondescendingly to a wide variety of audiences.

It was a narrative with which Urey felt quite comfortable; it allowed him to paint himself as having emerged from “among the ordinary, common people of the United States,” and to connect his story to claims about the importance of giving young people from all walks of life the educational opportunities that they needed to succeed. This rags-to-riches narrative did emphasize certain factual elements of Urey’s past. He was born in a small town in Northeastern Indiana, and his father did die young and leave the family to struggle on a series of unproductive farms – in short, Urey’s star did indeed rise from very humble beginnings. But this narrative also marginalized those elements of his life that might jeopardize his all-American status. One troublesome element that was regularly excluded was the specific nature of Urey’s religious upbringing.

Throughout much of his career, Urey distanced his public persona from the fact that he was born into the German Baptist Brethren Church (known after 1908 as the Church of the Brethren), and was raised in a family within which the men traditionally adopted the pious lives of lay ministers in the church. While Urey did identify his father in his 1934 Nobel Laureate profile as the “Rev. Samuel Clayton Urey,” he did not specify the family’s religion. While he emphasized the claim that he was descended from the pioneers who settled Indiana, he did not mention that those pioneers were themselves the

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12 Harold C. Urey, “Unpublished Autobiography”, n.d., 1, Box 1, Folder 5, HCU.
descendents of Pennsylvania Dutch colonists.\textsuperscript{13} Even in situations in which he made a point of drawing attention to the role that religion had played in his youth, Urey was vague about his upbringing. The young, scientifically active Urey tended to present himself as having had a generic Christian upbringing, and connected himself always with a fictitious universal Christianity; he avoided drawing attention to the highly specific – or “peculiar” (as the Brethren often described themselves) – aspects of his former life in the Brethren. Urey’s colleagues, in the biographical memoirs they produced after his death in 1981, likewise glossed over his childhood and his life before his graduate student days at Berkeley.\textsuperscript{14} On the rare occasions when Urey’s religion was invoked, it was only to illustrate his life-long commitment to pacifism and the root of his abhorrence of war.\textsuperscript{15}

\textbf{A Cold War Crusade}

And yet, during the Cold War, Urey became a champion for religious conviction. While he never claimed to worship anything other than the universe itself, he nonetheless argued amongst his colleagues that it was only those with true “religious courage” who

\textsuperscript{13} The Nobel biography of Urey was originally written for the award presentation in 1934, and was later published in Nobel Foundation, \textit{Nobel Lectures, Chemistry 1922-1941} (Amsterdam: Elsevier, 1966). A draft version of this brief biography also exists: “Biography Prepared for Nobel Prize Committee,” typescript, n.d., Box 1, Folder 9, HCU.


\textsuperscript{15} Urey’s own discussion of his Brethren roots in his autobiography is included primarily to illustrate the roots of his pacifism. In a 1970 biographical introduction of Urey prepared by his colleague Joseph Mayer, Mayer reproduces this point, Joseph Mayer, “[Biography of Harold C. Urey],” typescript, 1970, Box 1, Folder 11, HCU.
were willing to stand up against McCarthyism. Viewing the corruption and chaos in the world around him, he wondered whether it was not daily family worship – which had played such a strong role in his own upbringing – that was lacking. In his public speeches, he insisted that it was the traditional moral teachings of the western religions that would save the world from nuclear devastation. “It would be tragic,” he said in 1956, “if science gave man the greatest view of the universe that he has ever had and destroyed the effectiveness of the teachings of our great religions.”

As Urey saw it, society needed something to worship and some notion of good and evil. The dawn of the Atomic Age made the absence of a universal moral code more dangerous than ever, and the need to “rise above” all the more urgent. Science itself seemed to hang in the balance, as Urey became convinced that the practice of science – containing no ethical norms of its own – produced valid knowledge only because science’s practitioners had received ethical instruction through society’s dominant religions. Both society and science would suffer if a solution was not found.

Urey found his answer in the hope that the language of the divine might be replaced by that of the magnificent. A “new prophet who [could] accept the facts of science and at the same time … give inspiration to fill this great void” might be able to “make use of the magnificent view of the universe supplied by science and the materialistic necessities and luxury supplied by its applications to give us a sound moral

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16 Harold C. Urey to Louis Finkelstein, August 11, 1949, Box 44, Folder 11, HCU.
17 Harold C. Urey, “The Intellectual Revolution [revision],” typescript, 1956, 18, Box 141, Folder 12, HCU.
18 Urey first introduced the “new prophet” idea in 1959: Harold C. Urey, “Science and Society - Cooper Union Conference,” typescript, November 2, 1959, 4, Box 141, Folder 23, HCU.
life and noble aspirations.”19 It is thus interesting (and uncommented upon in the history of space science) that Urey’s most influential book, *The Planets*, was first delivered in Yale University’s annual Silliman Lecture Series, a memorial series endowed in 1883 by the family of Hepsa Ely Silliman and established with the express purpose of “illustrat[ing] the presence and providence, the wisdom and goodness of God … [through an] orderly presentation of the facts of nature or history,” rather than through scripture.20 *The Planets* included Urey’s original view of the moon as a possibly captured object, which eventually evolved into his conviction that it was a geochemical “fossil” from the solar system’s earliest days (and thus a key with which to unlock the ultimate mystery of the solar system’s formation). The book also anticipated Urey’s involvement in the National Aeronautics and Space Administration’s (NASA) Apollo lunar exploration program, where his theory of the moon’s origin was used by NASA – specifically Robert Jastrow and Homer Newell – along with his reputation as a Nobel laureate to legitimize scientifically an otherwise political program.21

**Science and Religion**

Urey advocated a new relationship between science and religion – one in which the scientist’s task was to provide the new prophet with an inspiring view of the universe

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19 Harold C. Urey, “Religion Faces the Atomic Age,” typescript, February 3, 1958, 4, Box 141, Folder 15, HCU.
and humankind’s place within it. What was the connection between Urey’s pioneering use of radioactive isotopes in earth and planetary science and his concurrent turn toward the promotion of the universe as an object of religious or spiritual inspiration? This dissertation suggests that it was more than coincidental. Two crises in fact pushed Urey toward this view of science and religion. The first crisis occurred at the end of WWII. Urey was demoralized by his experience as the Director of the Substitute Alloy Metals (SAM) lab, Columbia University’s contribution to the Manhattan Project, and was frightened by the introduction of atomic weapons into the world. Urey abandoned the isotope separation work that had made him famous and instead turned toward new applications of the thermodynamic properties of isotopes to the earth sciences. At the University of Chicago, Urey launched a second career as an isotope geochemist and developed his oxygen thermometer for the measurement of paleoclimate.

At the same time, Urey became an outspoken critic of atomic weapons and an advocate of world government. This put him on the defensive against McCarthyism and the House Committee on Un-American Activities (HUAC), and brought him under the scrutiny of the Federal Bureau of Investigation (FBI). This eventually led to Urey’s second crisis. By the end of the 1940s, Urey’s hopes for world government were already all but dead. He became a reluctant supporter of weapons stockpiling and the development of the hydrogen bomb as a preventative measure against nuclear war. In 1953, Urey’s failed intervention in the trial of Julius and Ethel Rosenberg sent him into what his colleague Hans Suess described as a “severe mental trauma.”

Suess in fact recognized both of the crises that I have identified. He also recognized that Urey’s prewar progressivism and his embrace of Danish-style socialism was “not a recipe in the postwar U.S.A. for a quiet life…” Cohen et al., “Harold Clayton Urey. 29 April 1893-5 January 1981,” 643.
followed within a matter of months by the death of Urey’s mother, his primary role model and moral compass.

These two crises taken together – both of which were deeply personal but also intertwined in Cold War science and politics – came to define Urey’s postwar work and the meaning that he found in it. In his post-WWII career, Urey set out to accomplish the type of scientific work that would expand intellectual horizons. Before public audiences, he implored religious thinkers to expand their understanding of Genesis with the grand view of the universe science provided. Meanwhile his contributions to NASA, including his criticisms of their aims in lunar research, reflected his desire to reveal to the public their place in the awe-inspiring universe. It was Urey’s commitment to this work that underlay his constant head-butting with NASA administrators and fellow scientists – particularly the astronomer Gerard Kuiper and the astrogeologists from the United States Geological Survey (USGS) – who shared neither his understanding of the moon’s significance, nor his willingness to demand that science speak to the transcendent.23

Urey’s Cold War narrative of science as a force for inspiration in the necessary task of maintaining religion stands in contrast to what historians of 20th century science have come to understand as the dominant view – that of science as a secularizing force with its own set of values. Very astute historical work has recently demonstrated how this dominant view was constructed and promoted by such figures as Robert Merton, James Bryant Conant, and Vannevar Bush. This work has shown how these and other advocates of science used this view both to pave the way for a new multiculturalism and (as the

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relationship between science and the state grew closer) to constitute scientific research as “pure,” shoring up science’s Cold War moral economy.\textsuperscript{24} The history of the construction of this dominant view has, along with other recent historical work on the 20\textsuperscript{th} century, given us a multidimensional view of Cold War science. We have learned much about the emergence of Cold War institutions, the transition to Cold War liberalism among scientists, the paradoxical nature of science’s ability to speak truth to power, and the fate of some of the Cold War’s most heroic, tragic, and in some cases enigmatic figures.\textsuperscript{25}

There is another dimension to the story of Cold War secularism which most historians have not yet examined: the story of those scientists – many of whom worked within the selfsame institutions where the secularist view was forged – who rejected this view. Working in the shadow of the bomb he helped to create, Harold C. Urey was one of


these scientists. His story not only helps us to trace the complex historical relationship between science and religion into the 20th century, it also illustrates how these complexities spilled over into the early days of space science. It therefore illuminates what was at stake, at least for some, in the Big Science of the last century.

Urey’s alternative view invites us to work through the “more sensitive historiography” that is subservient neither “to the triumphalist rhetoric of scientific rationalism nor to religious apologetics” for which science studies scholars have recently been pushing. Urey’s view also seems to confirm for the 20th century what John Hedley Brooke and other historians concerned with “science in theistic contexts” in earlier centuries have argued convincingly: that claims of secularity must not be taken at face value, since these secular visions have often contained elements of the sacred in one form or another. Recent events such as debates over global warming, the age of the Earth, and Plate Tectonics suggest not only that we should no longer treat the 20th century as an exception in this regard, but that we should be invested in a more nuanced understanding of the recent history of science and religion.

The biographical approach has much to contribute to the historical study of science and religion. Recent biographical work in the history of science and religion has suggested that when historians examine “the shape science and religion take when meeting in the biographies of scientists,” such studies reveal “something about the formative influences of religious background and belief on the ambitions, loyalties, and moral choices that mould scientists’ lives, and even on their predilections for certain subjects and theories.”

Along these lines, Nicholas Rupke suggests that the scientist’s worldview is not synthesized out of thin air, but is formed within the context of their life. He suggests that we think of biography in terms of “life geography,” borrowing the phrase from David Livingstone, whose conceptualization of this geography insists that we “[take] seriously the spaces in which people enact and narrate their own lives.” We might interpret these spaces broadly, to include not only the physical spaces of institutions, cities, and nations, but also the less physical spaces of social and political context. Biography allows us to situate science and religion within individuals, and then to analyze how dependent this relationship is upon the networks, communities, and social movements within which the individual moves.

This view of biography assumes that as historians we can distinguish what might be termed “mere” biography from biography written with historical constraints, and with the complementary goals of “deploy[ing] the individual in the study of the world outside

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29 Livingston quoted in Ibid., 27.
that individual and … explor[ing] how the private informs the public and vice versa.”

Rather than providing an uncritical celebration of a distinguished life, this approach moves us “beyond easy platitudes to engage in what Clifford Geertz famously called ‘thick description.’” This turn toward thick biographical description has already moved into science studies, where the profession has recently seen, among other things, the publication of Charles Thorpe’s social biography of J. Robert Oppenheimer, Michael Gordin’s treatment of Dmitrii Mendeleev’s science and politics, and Matthew Stanley’s synthetic approach to Arthur Eddington’s life as both a scientist and a Quaker.

These recent biographies demonstrate that the genre can give historians of science a way to tie together the various and sundry levels of analysis developed within history and science studies, along with a way to navigate essential social and historical contexts. According to Gordin, this becomes possible when we avoid focusing exclusively on our biographical subjects, allow them to emerge within their contexts as the heterogeneous selves that they are, and do not simply portray them as the center of cultural and social currents. Instead, “one should also emphasize the currents … [think] of [the subject] as a packet of tracer dye in a turbulent stream, and then concentrate on what the consequent patterns can tell us about the stream rather than the dye.”

Biography has particular relevance for the study of science in the Cold War. Thomas Söderqvist, has suggested that scientific biography is well suited to discussions

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of the Atomic Age, since its “great men/big theories/dire consequences-kind of biographical poetics matches the unique chain of events [that] could have resulted in the eradication of all of humankind in one singular political act.”

One might extend this logic to the entirety of the Cold War. Biographies, be they social ones influenced by sociology and science studies or more traditional ones concerned with great individuals, have already played a major role in Cold War historiography of science.

Scholars have already focused on the scientists who bore responsibility for the introduction of atomic weapons. Silvan Schweber’s *In the Shadow of the Bomb: Oppenheimer, Bethe and the Moral Responsibility of the Scientists* explored the moral issues raised by the decision to develop and use the atomic bomb, as well as the very different ways in which Oppenheimer and Bethe dealt with the guilt and self-doubt they experienced. Oppenheimer has been a popular target for biographical treatment. David Cassidy examined the fate of Oppenheimer’s brand of humanism in the Cold War security state that characterized the so-called “American Century.” Charles Thorpe’s social biography made Oppenheimer’s life less about the man himself and more about the broader cultural and institutional contexts in which he moved. This revealed how the Cold War “routinization” of nuclear weaponry, power, and atomic scientists themselves – shrouded as it was in waves of paranoia and secrecy – made the humanistic authority

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Oppenheimer wielded politically and socially problematic, inside and outside of bureaucratised scientific networks.  

It has not escaped my notice that the culmination of Urey’s crises occurred at roughly the same time as Oppenheimer’s. Just as Oppenheimer lost his optimism for a better world in 1947, so too did Urey. In the 1950s, the attacks on Urey by HUAC tapped into the same anti-Communist discourse, rooted in nativism and anti-intellectualism, that Thorpe’s biography describes. What is missing, of course, is the “encoded anti-Semitism.” While Urey’s critics could and did attack him for his naïveté, his established public biography made him clearly an American and a Christian (even if lapsed). And just as Thorpe has also argued that Oppenheimer reconstructed his public identity after his security hearing along the contours of the archetypal intellectual, so too did Urey’s crises afford him an opportunity to reimagine his own public role. Urey chose to intervene in what he believed to be a historic conflict of science and religion in order to prevent what he feared would be a decline of civilization. This intervention was perhaps just as unrealistic as Oppenheimer’s choice of public intellectual.

This dissertation is strongly biographical in nature, but it also incorporates elements of the social and cultural history of science. Urey’s life and career – particularly his dual role as a working scientist and public figure – have been chosen as the points of entry into the story of science and religion in the Cold War precisely because his life and his views problematize the Cold War secularization paradigm. That he did work on issues

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37 Thorpe, Oppenheimer.
38 Ibid., 251.
that are seen to have religious import – earth and solar system history and the origin of life – and that he was often questioned publicly about the religious implications of this work, also makes him a significant figure for such a study.

The purpose of this study is not to attempt to celebrate, justify, or endorse Urey’s view of the science-society dynamic. Rather, by following Urey’s attempts to negotiate his scientific and public work, this dissertation provides a better understanding and elaboration of the institutions and communities through which he moved. Following Urey in this way opens a window into the cultural, political, and institutional contexts within which 20th century debates over science and religion should be understood. If Conant and Bush represent the victorious Cold War position on science and society, then Urey presents an interesting road not taken, the resistance to which illuminates the ways in which the dominant view gained stability in the Cold War context. By affording symmetry to Urey’s attempts to construct Cold War science very differently from Bush and Conant’s model, and by shadowing his career inside and outside of the laboratory from the 1910s to the Apollo project, we gain entry into many of the sites where the relationship between science and religion (and more generally science and society) was discussed, debated, and put into practice in the 20th century.

Chapter Structure

Chapter one takes on Urey’s biography from his birth in 1893 to his work in the Barrett Chemical Company during World War I. Throughout most of his career, Urey presented his childhood story only as an illustration of how a poor American country boy
could grow up to be one of the world’s most famous chemists. Certain developments within the American land grant universities did facilitate this upward trajectory, and these are discussed in this chapter and in chapter two. This chapter also attempts to put Urey’s authorized biography within the context of the history of the Church of the Brethren. Urey’s papers contain only the occasional brief description of his life in the Brethren, and his own unpublished autobiography is virtually silent on the matter. However, published Brethren sources do make it possible to recover many aspects of Brethren life that likely affected Urey’s childhood and upbringing within a pious Brethren home. As some of the Brethren sources also chronicle the movements of the Urey family during the end of the 19th century, they can also be used in conjunction with genealogical sources to add some detail to the selective account that Urey left.

Putting Urey’s early life in a Brethren context allows for an examination of the social, cultural, and intellectual resources available to Urey as a child and young adult. Here we find, for example, that both of Urey’s parents followed what was by their generation becoming a well-established educational path within the Brethren – attending rural public schools before attending normal college and returning to the schoolhouses as teachers. We also find that Urey’s own generation tended to seek even higher education, largely thanks to a college movement within the Brethren in the second half of the 19th century. Even while the Brethren practiced a lifestyle very similar to that of the Mennonites and gravitated toward Biblical literalism, they were strangers neither to science nor to intellectual developments such as higher criticism. By the end of the century they were engaging with those parts of higher criticism which they felt were constructive, and paid close attention to archeological explorations of the Holy Land. By
the 1870s they had also established modern scientific curricula within their colleges. One testament to the advances the Brethren had made up to this point is the fact that several of the published sources cited in this chapter are dissertations that were produced by Brethren of Urey’s generation who entered the social sciences and produced social and cultural studies of their own communities in the early 20th century.

Chapter one ends with a discussion of Urey’s decision not to claim conscientious objector status during WWI, and to instead have his draft status changed by going to work in the wartime chemical industry. This decision was presented in Urey’s autobiography as a decision that put him in conflict with the Brethren prohibition on fighting or resistance of any kind. However, Brethren sources indicate that the church was not very efficacious in the years between the Civil War and WWI in teaching its peace message to young members. Also, during the years leading up to WWI the church – which in its past had taught its members to remain aloof from the government and the outside world – was in fact developing a more “constructive” view of citizenship within which members could now make good on their obligations to the state. This constructive redefinition of citizenship within the Brethren allowed Brethren men to tender whatever nonviolent service to their government their conscience permitted – making Urey’s decision to go to work in an essential war industry a valid (although not obvious) Brethren choice. An alternative explanation for Urey’s decision not to claim his Brethren conscientious objector status is presented, suggesting that prejudice against the Brethren along with wartime anti-German hysteria played a role in Urey’s decision. This is also presented as an explanation for Urey’s efforts to distance himself from his Brethren past during his rise to scientific celebrity.
Chapter two begins after the Armistice, as Urey left the Barrett Chemical Company first to take up a teaching position at his alma mater, the University of Montana, and then moved on to graduate school at the University of California. It then follows Urey’s biography up to his role in the Manhattan Project. Following Urey from Montana to Berkeley, Copenhagen, Johns Hopkins, and finally Columbia University, we find that Urey’s early success came primarily from being a tenacious worker who found himself in remarkable institutions and connected to eminent scientists. In Berkeley, Urey benefited from Gilbert N. Lewis’s physical chemistry program and the industrial structure – Gilman Hall – that Lewis had constructed on the Berkeley campus. Lewis’s program and facilities allowed the faculty and students of Berkeley to do chemical experiments in thermodynamics that rival what was being done in Europe. Much of Lewis’s success in Berkeley was itself attributable to the increasing prestige of American chemistry after WWI, and the place of chemistry in American culture after the war. Chemists returned to their academic and industrial posts after the war as heroes, having beaten the superior German chemists at their own game. Lewis’s program also built bridges between the chemists and physicists of Berkeley, and Urey benefited from instruction in atomic and molecular physics to which few chemists of that time had access.

Urey next found himself in Copenhagen, at Niels Bohr’s new Institute at the University of Copenhagen. Here Urey discovered that although he was one of the most mathematically inclined chemists to pass through Berkeley in the 1920s, he did not have the mathematical talent to participate in the theoretical work of quantum mechanics. Like many Americans who visited Copenhagen in those early years of Bohr’s Institute, Urey discovered that his education was not up to European standards. Still, Urey benefited
from his time in Copenhagen and connected himself to the network of European scientists that he met there. When he returned to the United States and took up a position at Johns Hopkins University, Urey now had a reputation that exceeded his proven abilities. He made himself a champion for teaching more theoretical and mathematical mechanics to chemists and made a reputation for himself for his ability to explain the new science coming out of Copenhagen to uninitiated American audiences. Although he did not make much progress establishing his own research program at Hopkins, Urey nonetheless was accepted within the highest ranks of American physical scientists.

After the move to Columbia University, Urey’s experimental program began in earnest. Although research budgets at Columbia were small, Urey was able to build a grating spectrometer and begin work in molecular spectroscopy. When the discovery of a heavy isotope of oxygen hinted at the existence of a heavy hydrogen isotope, Urey drew upon his network of connections within the American scientific community to make up for his own lack of low-temperature facilities. The discovery of heavy hydrogen in 1932 won Urey the Nobel Prize and secured his reputation as America’s leading isotope chemist. When hostilities began again in Europe and an American crash effort to produce an atomic bomb began, it was Urey who was trusted to oversee the necessary separation of uranium isotopes.

Throughout these years between WWI and WWII, Urey does not seem to have questioned the importance of the work he was doing. He instead seems to have sought out (or perhaps stumbled into) the strongest institutions and chased what he felt were important scientific questions. His experience during WWII as the Director of Columbia’s Substitute Alloy Materials (SAM) laboratory, however, soured him on the
isotope separation work that had made him famous and suddenly forced him to reevaluate his career as a scientist.

Chapter three picks up Urey’s career at this point of crisis. After the war, Urey moved to the University of Chicago, where a group of nuclear scientists had come together to help Chicago’s President Robert M. Hutchins found a new Institute for Nuclear Studies (known today as the Enrico Fermi Institute). The Institute represented a new arrangement for American research scientists. The scale of work in Chicago was much larger than that done at any university before the war. The Institute was funded by a new cooperative relationship between university support, industrial partnership, and government/military contract. This relationship began during the war, but was developed and solidified during the Cold War at institutions like the University of Chicago. This chapter discusses the nature of this new relationship and the work it made possible. As Urey moved away from isotope separation and toward a new research program that used stable isotopes to tackle questions in the geosciences and cosmogony, this new research program was shaped along the contours of the new Cold War system.

Having discussed the shift in Urey’s research agenda after the war, chapter four is an examination of the change in Urey’s rhetoric during the Cold War. Urey’s public speaking career began in the 1930s when he won the Nobel Prize. During these Depression-era years, Urey presented an optimistic view of science and its effects upon society. Science had swept away old superstitions and had made possible, at least in principle, a world of shared abundance. Urey during this time was an idealist and believed that the United States was taking steps toward the utopia he envisioned. Along
these lines, Urey welcomed President Franklin Roosevelt’s New Deal as evidence that scientific progress and social improvement went hand-in-hand.

During WWII and the years immediately after the war, Urey maintained some of this optimism. Although he now told his audiences that he feared the weapons he and his colleagues had created, he hoped that the reality of these weapons would convince political leaders around the world that a world government with real sovereignty was necessary. These hopes faded, however, by the early 1950s, as Urey’s experiences with McCarthyism and the Cold War took their toll on his optimism. In 1953, the same year that Urey’s mother died, he took a stand for the suspected atomic spies, Ethel and Julius Rosenberg, only to see the couple executed and his own character assassinated by the press and the House Committee on Un-American Activities. At this point, Urey’s hope that he could affect political change faded almost entirely. His hopes now turned to religion. Although he possessed no real faith of his own, he argued publicly that religion was needed if humankind were to survive the atomic age.

The final chapter of this dissertation is an examination of the lunar work that Urey did with NASA beginning in the late-1950s. After NASA used Urey’s version of the moon in order to sell Congress on the scientific importance of lunar missions, Urey became a supporter of the space program as a vehicle through which he would be able to perform publicly and in full view of the world exactly the type of work he had described in his public speeches. It would be a stage upon which he could produce an inspiring view of the origins of the solar system. While NASA did use Urey’s moon publicly to sell the lunar program, they also drew upon the history of American exploration and presented the moon as the next great frontier. Within NASA, the idea of the moon as an
extension of the earth prevailed for scientific and technical reasons as well. Urey found himself at odds with his bitter rival, the astronomer Gerard Kuiper, and what seemed to Urey to be poorly trained hordes of planetary geologists who could not see the moon for what he believed it really was. Thus Urey’s plans came to naught and his public rhetoric was never fully intertwined with his scientific practice.
Chapter 1

The Making and Remaking of an American Chemist: From a Country Boyhood to World War I

“It was hard to learn much about his early life by talking to him. Like another famous and durable American, he did not look back.”

James R. Arnold

Harold C. Urey’s arguments for retaining the traditional moral teachings of Judaism and Christianity during the Cold War brought to the surface elements of his past that he had worked to obscure during his rise to scientific fame. In the 1930s and 40s, Urey had constructed a public persona that painted him as having emerged from “among the ordinary, common people of the United States.”

When a woman whose bags he had carried from a train wrote to thank the Nobel laureate for making so humble a gesture, Urey responded that, “I always remember that I am a son of middle class people from Eastern Indiana and that I have been exceedingly fortunate in life, for otherwise I would have been a second rate farmer in Eastern Indiana. There are lots of people like this in the world who never have an opportunity. Why should those of us who are fortunate be conceited?”

In his unpublished autobiography, written when Urey was well into his seventies, his depiction of his childhood went beyond his earlier humility; setting aside any illusion that the family had been middle class, Urey emphasized the rural, tragic, and economically harsh conditions his family experienced in Indiana at the turn of the

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40 James R. Arnold, “Harold C. Urey,” typescript, June 1, 1981, 1, Box 191, Folder 9, SLM.
42 Harold C. Urey to Mrs. Charles R. Locke, December 8, 1952, Box 54, Folder 9, HCU.
century. Urey used this version of his life to highlight the unlikely nature, and perhaps distinctly American character, of his scientific fame.

The Horatio Alger-like story that Urey told of his rise from very poor beginnings to scientific stardom was true. However, it obscured one very important aspect of his origins: that he was born into the Church of the Brethren and was raised by a pious mother in a family of Brethren ministers. This distancing was not intended to obscure a religious past per se. In many situations, Urey made a point of drawing attention to the role religion had played in his youth. However, in these instances he was vague about his upbringing. In 1941, for example, while a participant in the Jewish Theological Seminary of America’s Conference on Science, Philosophy, and Religion, he made confusing references to the “Hebraic-Christian religion, the religious atmosphere in which all of us have been raised,” and the “Christian way of life [that] is the mother of our democratic way of life.”

This chapter attempts to recover the religious aspects of Urey’s upbringing that were omitted from his official biographical and autobiographical writings. This is done not in an attempt to recover Urey’s theism or to claim that Urey was a particularly Brethren scientist, but rather in order to explore what resources – educational, cultural, and otherwise – were available to him during his youth, as well as what cultural resources from his past Urey drew upon when he found himself in crisis during the Cold War. His religion may not have been a personal comfort to him, but his early experiences within this very traditional religion certainly helped to shape his understanding of Christianity and its role in social life. Finally, this exploration of Urey’s religious past – including his

43 “Monday AM + AFT Sessions”, September 8, 1941, 37, 113, Box 36, Folder “2nd conference,” CSPR.
movement away from the religion – helps to explain the historical reasons why Urey felt that this part of his life had to be hidden.

This chapter first presents a biography of Harold C. Urey written along the lines that Urey himself would have recommended – excluding most mentions of his religious past and focusing instead upon economic hardship and family tragedy. This biography draws primarily upon a short unpublished autobiography found in Urey’s papers, and also upon biographical writings by Urey’s peers. After this biography is established, this chapter then contextualizes Urey’s life within the history of the Church of the Brethren. This history draws upon published Brethren sources, some of which chronicle the movements of the Urey family, upon cultural and historical studies of Brethren life and society, and also upon genealogical resources that have previously been kept within the Urey family. Finally, this chapter uses Urey’s decision to go to work in a chemical company during World War I rather than claim conscientious objector status – the moment that he claimed forced him to make the decision to go against his upbringing and also turned him into a chemist – as a point of inquiry within which Urey’s reasons for hiding his Brethren past can be scrutinized.
The Synthetic Biography of Harold C. Urey

“As a boy growing up in rural Indiana and Montana, he was isolated from the technological changes going on in the cities. He once recounted his impressions on seeing his first automobile, at the age of seventeen.”  
James R. Arnold  

“I would like to bring out another aspect of Urey’s background – his growing up on a farm in Indiana, the heartland of grass roots America. Pitching hay on his father’s farm would carry weight with readers who are sympathetic to the rugged individualism of Americans.”  
Stanley L. Miller  
Untitled note (nd.)

At an unknown date sometime in the late-1960s or early-1970s, when Urey was already in his late-seventies, he sat down to write an autobiographical memoir. It was not the first time Urey had reflected on his life; Urey had plenty of opportunities in the 1960s alone to practice framing the life history of his public persona. In 1963 the Princeton University graduate student Daniel Kevles traveled to La Jolla, California to interview the famous chemist about his discovery of deuterium, his 1934 Nobel Prize in chemistry, and his later role in the Manhattan Project. In that same year, Harriet

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45 Untitled and undated note. Box 18, Folder 10, SLM
46 Urey, “Unpublished Autobiography.” It is not clear whether Urey intended for this memoir to grow as time went on, or if it was meant only as a draft later to be adapted into a short biographical children’s book about his life and career: Alvin Silverstein, Harold Urey: The Man Who Explored from Earth to Moon (New York: J. Day Co., 1970). However, it is possible that the children’s book itself was in fact the impetus for more mature reflection on his life and career. Based on the events described (the assassination of Martin Luther King, Jr. and Urey’s 77th birthday), we can safely say that Urey began writing his memoir no earlier than 1968 and finished it no later than 1970. If we assume that Urey might have begun writing this memoir in 1968, he would have been 75 years old.
Zuckerman, a Columbia University graduate student and protégé of the sociologist Robert K. Merton, visited Urey and collected his “reminiscences” for her study of American Nobel laureates.\textsuperscript{48} Only one year after being visited by Kevles and Zuckerman, the historian of physics John L. Heilbron again interviewed Urey, primarily about his early career.\textsuperscript{49} Before the 1960s were over one final researcher, Ian I. Mitroff, came to interview him about his views on lunar exploration.\textsuperscript{50}

These interviewers were not the first to chronicle Urey’s story. By this point in his life, as both a Nobelist and a Manhattan Project alumnus, he had already been interviewed by a slew of journalists and others who were interested in the scientific enterprise in America. He as much as recommended to Zuckerman in her 1964 interview that she consult the occupational psychologist Anne Roe’s monograph from a decade earlier, \textit{The Making of a Scientist} – since he felt Roe had described well the elements of his early life that had contributed to his eminence.\textsuperscript{51} Urey seemed particularly attracted to Roe’s emphasis on the role that tragedy and hardship could play in the development of an eminent scientist. He told Zuckerman that in addition to having been one of Roe’s


\textsuperscript{49} Harold C. Urey, “Interview with Dr. Harold Urey, Session 1,” interview by John L. Heilbron, March 24, 1964, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA.


\textsuperscript{51} Urey had been a participant in Roe’s study of scientists, although her subjects were treated with anonymity in her book: Anne Roe, \textit{The Making of a Scientist} (New York: Dodd Mead, 1953).
participants, he also felt he fit the profile she had described perfectly: “[She] concluded that well-known scientists are the eldest, there has been tragedy in their lives… I was the eldest. My father died when I was six, and left the family in great poverty. Mother married a second time, and so forth. We’re likely to be the sons of schoolteachers or preachers or something like this. My father was a schoolteacher and a lay preacher, and so forth. Right on the line.”52 Urey must have made a similar recommendation to Kevles, who reproduced one part of this interpretation in The Physicists when he wrote that “Urey lost his father, a farmer and minister of the Brethren Church in Walkerton, Indiana, at age six, and his faith not many years later.”53 Following Urey’s lead, Kevles implied that Urey’s loss of faith and his subsequent development of a “secular brand of faith” in science as an intellectual and psychological move motivated primarily by the very tragic loss of his father at an early age.

Urey’s early life was filled with tragedy and economic hardship. Born in Walkerton, Indiana on April 29, 1893, the rural and agricultural character of the northeastern part of Indiana where he was born and raised would define much of the economic context of Urey’s early life. It is not too much of a stretch to describe life in this part of Indiana at the turn of the century as pre-industrial. In pre-WWI Indiana, rural and urban communities remained separate and distinct, and culturally very little flowed between the two. Manufacturing, which was already transforming social and cultural life in other parts of the country, was experiencing dramatic growth in the region during this time. An expanding railroad system had swelled the population of northern Indiana.

53 Kevles, The Physicists, 225.
during the second half of the nineteenth century, transporting an immigrant labor force to work in the north’s new factories. Between the end of the Civil War and the turn of the century, the number of factories in Indiana tripled. By 1900 the state’s 18,000 plants employed 156,000 workers. But industrial work would not take the place of agriculture in Indiana as the leading occupation until after the war.

Farming communities were still the lifeblood of the state’s economy, and the increase in the number of farms and the cultivation of new acreage kept pace with this growth in the industrial workforce. Even in the north, where most of the new manufacturing industries were located, farming continued to be a prosperous way of life. Well into the first two decades of the twentieth century, what manufacturing did take place in Indiana mostly consisted either of processing agricultural materials, or of making items directly from crops, animal parts, or timber; the main manufacturing industries were milling, meat packing, lumber, and liquor. The subordination of manufacturing to agriculture would be reversed in the 1920s, when northern Indiana became a hotbed for the manufacture of sewing machines, automobiles, and railroad cars. During Harold’s childhood and adolescence in Indiana, agriculture was still king, and the way of life on many farms – although not untouched by the introduction of new farm equipment and a greater contact with family members residing in urban or urbanizing areas – retained the

traditional practices of previous generations.\textsuperscript{56} The fact that he told colleagues he didn’t see a car until he was seventeen-years-old can be explained by the fact that automobiles were not in wide use in Indiana until 1910, the very year that he turned seventeen.\textsuperscript{57}

Both of Harold’s parents, Samuel Clayton Urey and Cora Rebecca (Reinoehl) Urey were born in the mid-nineteenth century and raised on eighty-acre homestead farms owned by their respective families in Fairfield Township, DeKalb County, Indiana. Samuel, whose father John Thomas Urey died in 1879, had along with his older brothers George and John and his sisters Nancy E., Rebecca Ellen, and Etta May helped their mother Elizabeth (Hostetler) Urey with the everyday running of the family farm. On their plot in Corunna, Indiana, the family lived together in a modest log cabin. Eventually, Samuel left the farm to attend Tri-State Normal College in Angola, Indiana, and received his BA from that institution by 1890. For some years before leaving for college, beginning as early as 1866, Samuel taught in the various country schools in and around the Fairfield and Richland Township areas. After receiving his degree and marrying, he returned to teaching and in 1892 took on the post of school superintendent in Walkerton.\textsuperscript{58}

There is some reason to believe that Cora’s family was wealthier than Samuel’s, as her mother Martha (Eckhart) Reinoehl’s family owned several other real estate and business holdings in the area in addition to their farm and Cora’s father Solomon

\textsuperscript{56} Carmony, \textit{A Brief History of Indiana}, 40.
\textsuperscript{58} Harvey Hostetler and William Franklin Hochstetler, \textit{Descendants of Jacob Hochstetler, the Immigrant of 1736} (Elgin: Brethren Publishing House, 1912), 117; Paul E. Reinoehl, \textit{A History of the Fairfield Cemetery: The Most Famous Man Buried Therein, Dr. Harold Clayton Urey, the Fairfield Township Schools, the Hamlet of Fairfield Center} (Ashley: Reinoehl, 1998), 162-165. In Urey’s unpublished autobiography he states that his father attended Angola College. The sources cited here agree that he attended Tri-State Normal College in Angola, Indiana.
Reinoehl was at one time township assessor and school trustee. Still, Cora’s life before marriage was fairly similar to Samuel’s. She had also left the farm to attend Tri-State Normal College, and prior to marriage she taught in the public schools of the Fairfield and Richland areas. Although in his unpublished autobiography Harold presented his father’s trajectory from log cabin to college degree in this time and place as a rare and unlikely event, it seems to have been a fairly common occurrence within Cora’s family. She was the only one of the four Reinoehl sisters to attend college, but all four of her brothers received college degrees. Three of her brothers, Job, Isaiah, and William attended Tri-State College; a fourth brother Charles attended Indiana State Normal College, Indiana University, and Columbia Teachers College before receiving a Ph.D. from the University of Chicago in 1921 and marrying a cousin of Samuel Clemens. The educational path was clearly not unknown to the Reinoehl family.

Tragedy struck the Urey family not long after Harold was born and would continue to be a major part of the family’s life for the remainder of his childhood. Two of Samuel’s sisters contracted tuberculosis and would die of the disease in 1894 and 1898. Samuel also found that his job in Walkerton was not worth keeping. According to one account, the position turned out to be in a troubled school where unruly students had usurped control from the teachers. Like many of those who took the position before him, Samuel found the attempt to impose discipline at the school exhausting and stayed in the

59 Charles M. Reinoehl and George B. Eckhart, *History of an Eckhar(d)t Family Whose Three Sons (John, Henry, George) Came to America Before 1850* (Bryan TX: The Scribe Shop, 1952), 44.  
60 Ibid., 44-55.  
61 Hostetler and Hochstetler, *Descendants of Jacob Hochstetler*, 117. Urey wrote in a letter to a Kendalville, Indiana newspaper reporter that Samuel’s third sister also succumbed to tuberculosis and died: “The family was unfortunate because three girls and my father died of tuberculosis.” Harold C. Urey to Kendalville News-Sun, April 5, 1963, Box 51, Folder 14, HCU.
job for only one year. With the young Harold still “only a babe in arms,” Samuel moved
the family back to the Fairfield Township area, where they likely lived on Samuel’s
widowed mother’s farm. Samuel again took up teaching in the country schools of the
Fairfield area, and Cora gave birth to a second son, Clarence Monroe Urey, in 1895.
There would be little time to celebrate the growth of the small family, however; in the
same year that Clarence was born Samuel was diagnosed with tuberculosis.

On the advice of a doctor who suggested that California’s dry climate might
improve his health, Samuel moved the family by railroad in 1897 to Glendora, California.
Here he left teaching behind and instead found work in a citrus packinghouse, hammering
together wooden shipping crates. Samuel was also a lay minister in the Church of the
Brethren and was able to find a post ministering to the local congregations in Covina and
Colton. At this time, however, the Brethren were still wary of a professional ministry
and for this reason did not provide their ministers with any kind of salary. The family was
destitute. Even as she was pregnant with their third child, Cora took in washings to
supplement the small pittance that Samuel earned from the packinghouse. Meanwhile, the
manual labor of building crates prevented any improvement to Samuel’s health. The
family’s luck seemed destined to continue its downward trajectory.

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62 Reinoehl, *A History of the Fairfield Cemetery*, 163. According to Reinoehl’s account, Samuel Urey was
forced to take on the role of the strict disciplinarian, and was remembered amongst the alumni of the school
in Walkerton for “the strap he carried in his pocket” (162).
63 Upon being told in the 1940s by her then adult son that there were some “old-timers” in Walkerton who
could remember him as a small boy, Cora is reported to have “smiled and said, ‘They have good memories
in Walkerton. You were only a babe in arms when we left there.’” This exchange was reported in “Dr.
Harold C. Urey Stops in Walkerton (photocopy),” *Walkerton Independent* (Walkerton, Indiana, May 23,
1946), WHS.
65 M.M. Eshelman et al., *A History of the Church of the Brethren of Southern California and Arizona* (Los
Angeles: District Meeting of Southern California and Arizona, 1917), 30.
Realizing that he was dying and that he could not provide for his family, Samuel suffered a complete mental breakdown on the Fourth of July 1897. The breakdown occurred after he had finished preaching an evening sermon in the Colton church. Finding that nothing could console him, Samuel was taken to the Southern California State Asylum for the Insane in San Bernardino. He remained institutionalized within the Asylum for eight months. Shortly after his release in March of 1898, Samuel traveled alone to his mother’s farm to visit his sister’s deathbed. After returning to Cora and his three young children in California, Samuel told his wife “This California climate has done nothing for my Tuberculosis, we may as well move back to Indiana.” In June of 1899, Samuel moved the family back to his mother’s farm.\textsuperscript{67}

On October 30, 1899, Samuel died, leaving Cora and their three children in the care of his mother. They remained on the Corunna farm and in 1903 Cora married her mother-in-law’s hired man, Martin Alva Long. The extent of Harold’s memories of his father seems to have been small, and limited primarily to his father’s encouragement of his education: “He was able to teach me to write a bit, and he always insisted that I should bring books home at night and read a small amount to him.”\textsuperscript{68} He remembered attending a one-room country school in DeKalb County during these days, where instruction was limited to reading, writing, arithmetic, and a small amount of history, while “Certain subjects of the modern curriculum were not pursued.”\textsuperscript{69} For the most part, Harold did not excel in primary school. By his own recollection, he barely passed the graduation exam required of Indiana grade-schoolers. He did learn to read closely and

\textsuperscript{67} Ibid., 164-165.
critically: “[There] is one unforgettable lesson I learned from the lady who taught all grades in this one room – she taught me how to study. She showed me how to read and understand every phrase, clause, sentence, and paragraph, and hence to read an assignment only once. That lesson helped me through school and remains with me to this day.”\footnote{Ibid.}

When Harold was eleven, his grandmother died and the family moved off of the Corunna farm and further into the country, where they farmed onions for very small returns.\footnote{Urey, “Unpublished Autobiography,” 1.} Despite the family’s continued economic hardship, and the fact that Harold was now limited mostly to socializing with his siblings, in later life he described this period as an ideal country boyhood, remembering that he slept in the attic of a log house, fished and swam in a local lake, and weeded onions with his brother and stepfather during the summers: “It was a very pleasant life on the whole – terribly hot in the summertime, however, in the onion field.”\footnote{Urey to Kendallville News-Sun, April 5, 1963; Thomas, “Harold C. Urey,” 219.}

Harold was able to leave the farm when it was time for him to enroll in high school in Kendallville, Indiana. Before dying, Samuel had insisted that his life insurance money be used for the education of his three children: “So when I finished grade school, this money was waiting, and I was able to go off to high school in Kendallville, then later Waterloo. If it hadn’t been for that, I’d still be in Indiana, working as an unsuccessful farmer – I just can’t see me being a successful farmer.”\footnote{Thomas, “Harold C. Urey,” 219.} This money went toward room and board with relatives on the outskirts of Kendallville. Here he lived with his maternal grandparents and other relatives from his mother’s side of the family throughout his high

\begin{footnotes}
\item{70} Ibid.
\item{71} Urey, “Unpublished Autobiography,” 1.
\item{72} Urey to Kendallville News-Sun, April 5, 1963; Thomas, “Harold C. Urey,” 219.
\item{73} Thomas, “Harold C. Urey,” 219.
\end{footnotes}
school years. When harsh winter weather did not prohibit it, he rode his bicycle the
twelve miles between the Kendallville area and his stepfather’s farm to visit his mother
and his siblings on the weekends.

By Urey’s own account, he became aware of his country mannerisms during his
high school years in Kendallville: “What a raw youngster I was in high school –
exceedingly timid, immature and unaccustomed to a town of 5,000 people. My associates
in high school were so much more sophisticated than I.”74 As he would later write to a
childhood friend, “What a crude country boy I was at that time, and you treated me so
well.”75 Another of Urey’s high school classmates remembered that Urey attended an
Amish grade school in the country before moving to Kendallville for high school and that
when he first arrived he was very shy and quiet around his fellow classmates.76 If Urey
entered high school a “raw youngster,” he did his best to shed those raw characteristics
during his time in Kendallville: “Here I studied four years of Latin, four years of English,
two or three years of Mathematics through Algebra and Geometry, a year of Biology, a
year of Physics, no Chemistry, and some Political Science and History (Ancient and
American). I was somewhat of an orator and won an oratorical contest. … I was also on
the debating team.”77

75 Harold C. Urey to Eloise Redmond, December 1, 1976, Box 77, Folder 30, HCU.
76 Marguerite Cramer knew Urey well and, along with Redmond, was one of the few childhood friends with
whom Urey kept in touch in his later years. Her memory of his having attended an Amish school may
represent conflation on the part of the interviewee between the Brethren and the Amish (an easy mistake to
make if Urey dressed in traditional Brethren clothing during these years), or it may be that the grade school
that Urey attended was in fact Amish in orientation. Cramer’s memories of Urey were included in Terry
Houholder, “Kendallville Graduate Worked on Manhattan Project in World War II,” Kendallville News-
Sun (Kendallville, Indiana, November 4, 1999), Special Edition: Reflections of the Century, 1900-1999,
Harold’s mother remained the strong female influence in his life: “She was stern, but extremely just and loving. We were always very close.” Upon winning the Willard Gibbs Medal in 1934, it was his mother, after a long list of his former professors, whom Urey thanked most profusely: “But I must also recall a most valuable thing which I learned as a small boy from my mother. It was she who taught me that ‘man does not live by bread alone but by every word that proceedeth out of the mouth of God.’ Of all of the lessons that I have learned in my life, this one has been most valuable.”

Harold’s stepfather is hardly mentioned in his autobiography, although in a separate account he remembers that Alva was “most kind to us. For instance, it was a common thing in those days to keep children home from school to help with the crops. I don’t remember this ever happening to me more than 1 or 2 days in my entire youth.” Harold seems to have looked more to his schoolteachers as male role models. One such role model was his high school Latin teacher, Mr. E. E. King, who inspired Harold to consider taking up a career as a Latin teacher. Urey’s newfound studiousness earned him his high school nickname, “Professor.” “It is interesting that a country boy who barely passed the examinations out of the grade school of Indiana led his high school class immediately, and continued to lead his classes in college from then on,” remembered Urey. King was just the first in a line of instructors who would come to influence Urey’s image of himself as a scholar and help him gradually to reshape himself from a

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country boy into a cosmopolitan man of science. “I had good teachers, and I loved my work.”

Once he had finished high school, Urey followed briefly in his parents’ footsteps, going into the small country schoolhouses of Indiana to teach as a schoolmaster. After a short time teaching in Indiana, he moved to Montana in 1912, where he joined his mother and family on another unsuccessful farming effort near Big Timber. Urey again took up teaching, this time in one-room schoolhouses in mostly undeveloped country. He first taught in a small wooden building that housed fifteen to twenty students south of Livingston, Montana in Gallatin County, at the foot of Absarokee Range. After teaching in this school for one year, he moved across the valley and into the Gallatin Mountains, where he taught for one additional year in a school in a mining camp.

Teaching did not earn Urey much money, and so he often lived with the families of his students. While teaching in the mining camp, he lived with the Wilson family. The son of the family, Brian O. Wilson, decided to go to college at Bozeman, Montana. According to Urey, this was when he first considered acquiring a college education: “This gave me the idea that if perhaps I were going to get ahead in the world, I should go to college also.” In order to decide where to attend college, Urey consulted his uncle, the superintendent of schools in Helena, Montana. This uncle must have suggested that Harold not follow his former student to Bozeman. In the fall of 1914, just as World War I

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83 Life in Montana during this time was dominated by the Anaconda Copper Mining Company, and so it is highly likely what this was an Anaconda mine.
was beginning in Europe, Harold went to Missoula, Montana and enrolled in the
University of Montana.\textsuperscript{85}

Harold had not been able to put aside much money while teaching, and so it was
no easy matter for him to afford tuition at the University of Montana. According to one of
his later collaborators, he slept and studied in a tent during his first academic year and
during the summer “worked on a road gang laying railroad track in the Northwest.”\textsuperscript{86} In
Urey’s own account of his years in Montana, he made no mention of living in a tent. He
did, however, remember waiting tables in the girls’ dormitory and, in his second year,
taking a job as an assistant in the biology department.\textsuperscript{87} According to the University of
Montana’s \textit{Annual Register} from 1916, he was also awarded an endowed prize after
being named “the best student in the Department of Biology.”\textsuperscript{88} He also remembered
spending his summers performing hard labor: “One summer I spent working on the
railroad which was being electrified running through Missoula. I worked partly on this
railroad and partly on an irrigation project up the Missoula River region. This was hard
work, and I much preferred school teaching to this.”\textsuperscript{89}

Harold had originally intended to study psychology at the University of Montana.
The Psychology Department’s policy of not admitting freshmen students discouraged him
from this path, and instead he registered in chemistry and biology courses. This turned

\textsuperscript{85} Ibid. Many sources (including the finding aid to the Urey papers, Kevles, and Urey himself at times)
claim that Urey attended Montana State University. However, Urey’s papers show that he attended the
University of Montana. The confusion may arise from the fact that at this time, the university was known as
the State University of Montana.
\textsuperscript{86} Ferdinand G. Brickwedde, “Harold Urey and the Discovery of Deuterium,” \textit{Physics Today}, September
1982, p. 34.
\textsuperscript{88} State University of Montana (Missoula), \textit{Twenty-First Annual Register: 1916-1917} (Missoula: Bureau of
out to be a happy coincidence. The botanist and naturalist Morton J. Elrod had come to the University of Montana in 1894 as head of the university’s science department. Elrod was one of the university’s most active faculty members, and managed to build up the biology department of the university, founded the university’s Biological Station on Flathead Lake in 1899, and served as President of the Montana Academy of Sciences, Arts and Letters.  

Elrod’s activities made his department one of the university’s most dynamic at this early stage of its development.

In the Biology Department Urey met his next and perhaps most influential scientific role model, Archibald Wilmot Leslie Bray. Bray, born in Sheffield, England in 1883, was only ten years Urey’s senior. Before arriving in Montana, he had received his natural science degree from Cambridge University. In Urey’s own apocryphal account of Bray, the Englishman is presented as something of a scientific hobo. Urey claims that Bray was eager to see the United States and had spent all of his money on boat passage across the Atlantic. With no funds, Bray “started his sightseeing by train – freight train.”  

Urey’s account goes on to claim that Bray was kicked off of the train in Missoula, Montana, where he next pursued a job at the university. As he had no credentials with him, he took a job as a janitor in the university. At some point, Urey’s story goes, the university realized that they had a Cambridge-educated biologist in their midst and Bray was promoted to the position of Assistant Professor in zoology.  

By Bray’s account, however, he traveled through most of the United States and some of South America at the turn of the century, before returning to England in 1905 to

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91 Thomas, “Harold C. Urey,” 221.
92 Ibid., 222.
attend Cambridge. In between graduating from Cambridge and taking up his position in Montana, Bray traveled through Newfoundland for two years, spending considerable time as a teacher in Labrador, and then spent two years after this traveling through the rest of Canada. After his time in Canada, Bray received a graduate degree in philosophy from the University of Oregon and did some graduate coursework at the University of Montana just on the eve of Urey’s arrival.\(^93\) Starting in 1913, Bray spent one year in Montana as an instructor and three years as Assistant Professor. As for Urey’s account of Bray as a janitor, according to his obituary Bray held several jobs before “settling down as an educator,” including cowhand, muleteer, cabin boy, hotel porter, ditch digger, and draughtsman.\(^94\)

According to Urey, Bray was a born educator. Indeed, with teaching fellowships at Harvard and as a founding member of the Rensselaer Polytechnic Institute’s Department of Biology, it was as an educator that Bray would distinguish himself throughout the rest of his career.\(^95\) It was Bray perhaps above any other influence that helped Urey to shed the skin of the “conscientious, blue-eyed Indiana farm boy.” Bray enjoyed working with students outside of class and organized a group of young students including Urey into a philosophical club that he called The Authentic Society. In 1915 this organization became the Alpha Delta Alpha fraternity.\(^96\) A history of the fraternity written sometime just after the end of WWI stated that the Society and the fraternity were

\(^{93}\) Archibald W. Bray, “Who’s Who in Rensselaer Polytechnic Institute”, 1936, 1, RPI.
\(^{94}\) “Beloved Teacher,” Rensselaer Polytechnic Institute Alumni News, February 1943, 19, RPI.
\(^{96}\) Alpha Delta Alpha eventually became the Delta Omicron Chapter of the Kappa Sigma Fraternity: “Guide to the Kappa Sigma Fraternity. Delta Omicron Chapter (State University of Montana) Records 1916-1978”, n.d., ADA.
modeled after the Cambridge Apostles, a 19th century “free discussion society” within Cambridge which consisted of the young Alfred Lord Tennyson and John Stuart Mill, among other “men of world prominence.” During Bray’s time in Cambridge he was a member of the “Authentic Club,” a reorganized version of the Apostles, and served as Captain of their Association Football team. Bray brought to Montana “the spirit of this Authentic Club”:

Professor Bray was a First Class King’s Scholar, Triple distinction. At the University of Montana he found five men who decided to organize a society similar to that found at Cambridge; and they called it the Authentic Society of the University of Montana. … It continually grew in importance, and became one of the strong organizations of the University, intellectually and politically. Weekly meetings of the society were held; and the men were so continually together that a strong fraternal spirit developed.

The fraternity seemed to attract the best and brightest of students, and in its first seven years consistently reported the highest grade point average and graduation rate of any fraternity on campus. The membership took on extracurricular discussions and debates on various topics. Bray acted as an advisor to this group of young men; when the group went about organizing a reunion in the 1960s, they invited Bray’s widow to join the reunion and indicated in their correspondence that a photo of Bray hung in a place of distinction next to a portrait of Sir Galahad in the fraternity’s living room.

Urey’s participation in the Authentic Society and Alpha Delta Alpha changed the nature of his Montana education. In retrospect, Urey concluded that under Bray he had been given a traditional tutorial education: “I soon realized that under him I was getting a

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97 Alpha Delta Alpha, “The State University of Montana”, 1918, 2, Box 1, Folder 1, ADA.
100 Alpha Delta Alpha, “The Fraternity Situation”, 1923, 9, Box 1, Folder 1, ADA.
101 Everett G. Poindexter to Harold C. Urey, November 8, 1961, Box 100, Folder 10, HCU.
Cambridge education and was gradually changing from a little country boy to more nearly a man of the world. I changed my major to biology and chemistry, and from then on never had any trouble making straight A’s. Professor Bray was just a splendid, model teacher who opened up the whole fascinating world of science to me. “102 Under Bray’s direction, biology became Harold’s principal love during his college years. He was particularly interested in protozoology: “I was immensely fascinated in the first days of my course in biology with these small microscopic animals, and thought that in some way they represented the simplest organisms, and that perhaps the origin of life on the Earth was bound up in the study of these organisms.”103 Under Bray he conducted his first research project, a study of the protozoa of a backwater slough of the Missoula River. Urey graduated from the University of Montana in only three years, and in the summer of 1917, he again worked under Bray gathering ticks in eastern Montana in an effort to solve the area’s spotted fever problem.104

Urey had entered the University in 1914, just as the hostilities in Europe were beginning, and by the time he had graduated and began working as a field biologist under Bray, American involvement in the war was beginning. The Selective Service Act became law on May 18, 1917 and registration for the draft began on June 5, 1917. According to the records of Alpha Delta Alpha, thirty-seven of its forty-one members enlisted in the military once America joined the war, and the fraternity formed a special

103 Ibid.
104 Ibid.
cooperative relationship with the Missoula’s Student Army Training Corps, holding one
tent as an unofficial fraternity headquarters within the camp.  

If Bray was a strong influence on these young men, then it is likely that he supported their enlistment. He himself enlisted as a biologist in the service of the Chemical Warfare Service (CWS) in Washington, D.C., where he worked under the Harvard chemist and Chief of the CWS Defense Section, Arthur Lamb. There Bray investigated biological methods of detecting gas weapons. He advised Urey to join the war effort with his chemical training, telling the budding scientist that “A trained chemist should serve on the chemical side.”

Urey felt some pressure to join his fraternity brothers in military service and take part in the excitement of the war effort, but also felt torn between his impulse to prevent “Kaiser Bill and the whole German theory of government” from “spreading to the rest of the world,” and the “early training of the pacifist character that I had received as a child.” Nonetheless, Urey did go to work in the chemists’ wartime project of explosives production. If this decision was a compromise between Urey’s religious and patriotic obligations, the ground of defining chemical work as patriotic war service was already well worn.

American chemists as a professional group had spent a great deal of effort in print and lobbying the Wilson administration and Army to treat chemical work as an alternative form of war service. In addition to establishing the CWS within the Army,

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105 Neil D. McKain, “Alpha Delta Alpha and the S.A.T.C.,” 1919, 1, Box 1, Folder 1, ADA.
chemists had sought exception from conscription (or reassignment once in the camps) by promoting work within the chemical industries as a form of enlistment. This position is illustrated in a 1917 front-page editorial from The Chemical Engineer, titled “The American Chemist Must Enlist.” Admitting that among Americans “The desire to do one’s ‘bit’ is universal,” the journal insisted that “the development of our national stamina and power is not a question of the existence of [America’s resources], but rather one of their intelligent application.”¹⁰⁹ The chemical community was such a resource, and the chemist “must be ready to give [his whole-hearted efforts] without hesitation.”¹¹⁰ But the chemist should not by any “mistaken sense of duty” present himself at a nearby recruiting station; he should instead present himself to the national census of chemists, administered by the US Bureau of Mines and the American Chemical Society (ACS):

“[By] all means let him send his name at once to be enrolled among those upon whom the government can call, and then let him be ready to submit his talents and training to the direction of our leaders at a moment’s notice. It is in this way, and in this way alone, that he can be sure he is serving his flag to the best of his ability.”¹¹¹

The chemical census had been proposed at the April 1917 meeting of the ACS in Kansas City, Kansas. It was passed at this meeting as part of a set of resolutions that read as follows:

> Resolved, That we reaffirm the tender to the President of the United States of the services of the members of our Society in all the fields in which we are qualified to act.

¹¹⁰ Ibid.
¹¹¹ Ibid.; The census of chemists is described in “Chemical Warfare Service,” The Journal of Industrial and Engineering Chemistry 10, no. 9 (September 1918): 683.
That the security and welfare of the country demand the organization of all the men and facilities of the United States, so as to insure the greatest possible service and value for each.

The progress of the war thus far principally teaches us that modern warfare makes extraordinary demands upon science, food supply, and finance.

For the protection and success of our men under arms we recommend the use, in their respective fields, of all trained chemists, physicists, and medical men, including advanced students of these subjects.

To this end, in collaboration with the United States Bureau of Mines, we are preparing a census of chemists. With no desire to avoid field service for men of training in the professions named, we urge that those of special ability be held to the work they can best perform. Thus we may avoid unnecessary loss from lack of control of the tools and requirements of war.\(^{112}\)

Once the census was in effect, Major General William L. Sibert, Director of the CWS, sent an open letter to US Chemists. In no uncertain terms, Sibert told the American chemist that “This is a chemical war: therefore the War Department must have immediately available all possible information regarding chemical materials and chemical man power.”\(^{113}\) The letter encouraged every American chemist to fill out his census form: “American chemists are presented at this moment with one of the greatest opportunities to serve their country by the simple process of answering this questionnaire with all possible speed.”\(^{114}\)

It was not difficult for Urey to find work in the chemical industries during the war. The chemical companies were actively recruiting American chemists to work in their wartime facilities; according to one wartime chemist who worked alongside Urey at the Barrett Chemical Company, Barrett’s recruiters scoured the country looking for


\(^{113}\) “Chemical Warfare Service,” 684.

\(^{114}\) Ibid.
“anyone who was warm and breathing.”\textsuperscript{115} Barrett had recruited Urey’s freshman chemistry professor, Fred H. “Dusty” Rhodes (who had by this point moved on to Cornell) to head its research division, and Rhodes recruited Urey to follow him.\textsuperscript{116} Alpha Delta Alpha reported, when it listed the departures and military placements of its members in 1917-1918: “…Harold Urey received a good offer from the Barrett Coal Tar Products Company of Philadelphia, which he decided to accept. A.D.A. very much regretted to loose [sic] his company but was very glad to hear of his good fortune.”\textsuperscript{117} With the encouragement of his two professors, Urey went to work in Barrett’s Frankford Plant in Philadelphia, Pennsylvania, where he helped to prepare chemical materials for munitions production, including the preparation of toluene for tri-nitro toluene.\textsuperscript{118} In Urey’s account, this was the moment at which he ceased to be a biologist and became instead a chemist.

\textsuperscript{115} John C. Warner, interview by John A. Heitmann, February 8, 1984, 16, CHF.
\textsuperscript{117} “Alpha Delta Alpha in 1917-1918”, 1918, 1, Box 1, Folder 1, ADA.
\textsuperscript{118} Urey, “Unpublished Autobiography,” 4-5. According to Warner, Urey was responsible for some of the more mundane aspects of work at Barrett; this is not surprising given Urey’s very junior status at the time: “I thought he was doing pretty uninteresting work compared to what I had to do. I think he was working on getting resins polymerized and out of solvent naphtha. I guess that was the first step before we started separating the xylenes,” Warner, interview, 18.
Growing Up in the Brethren Renaissance

“No biography of Harold would be complete without reference to his political opinions. Raised in a pious family, members of the pacifistic ‘Church of the Brethren,’ he has had an abhorrence of war, exceeded by his greater abhorrence of totalitarianism and oppression, and his passion for justice.”

Joseph Mayer
“Biography Written by Joseph Mayer” (1970)

“Perhaps I should say something about my general philosophy of life. I was raised in a very conservative religion, the Brethren Church, and began to drift away from this point of view while in high school. I read Bob Ingersol [sic] as a child, and began to doubt the precepts of the religion in which I was raised. As I have gone on to college and to higher education, this point of view has continued.”

Harold C. Urey
Unpublished Autobiography

Family tragedy and economic hardship were the terms within which Harold Urey chose to frame his early life, and there is no denying the role that misfortune and poverty played in his life up until he found work as a chemist. However, illness and economic forces alone did not determine Urey’s life and career. Urey’s longtime colleague Joseph Mayer knew from his own discussions with Urey that his “pious” upbringing within the Church of the Brethren had helped to shape the chemist’s approach to political and ethical dilemmas. In his autobiography, Urey gave his Brethren upbringing credit for being the source of his pacifism, but also minimized the role that it played in other

respects, claiming to have begun moving away from the church in his teenage years after reading the works of such agnostic Freethinkers as Robert G. Ingersoll.  

Urey does not specify which of Ingersoll’s works he read, but many of the works of Ingersoll presented a historical interpretation of the Bible that questioned its divine inspiration and thus argued against any form of literalism in its interpretation. Ingersoll argued that the Bible – like works of history, law, government, and science – was not “above and beyond the ideas, the beliefs, the customs and prejudices of its authors and the people among whom they lived”; the evidence he presented that the Bible’s authors were culturally situated spoke against “one ray of light from any supernatural source.”

Not only was the Bible mistaken with regard to the place of the earth in the heavens, the motions of the sun and planets, and most matters of “creation, astronomy, geology; about the causes of phenomena, the origin of evil and the cause of death,” Ingersoll also argued that it was not “any nearer right in its ideas of justice, of mercy, of morality or of religion than in its conception of the sciences”.

If Jehovah had been civilized, how much grander the Ten Commandments would have been … All that we call progress – the enfranchisement of man, of labor, the substitution of imprisonment for death, of fine for imprisonment, the destruction of polygamy, the establishing of free speech, of the rights of conscience; in short, all that has tended to the development and civilization of man; all the results of investigation, observation, experience and free thought; all that man has accomplished for the benefit of man since the close of the Dark Ages – has been done in spite of the Old Testament.

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122 A prime example of this position would be Robert G. Ingersoll, About the Holy Bible: A Lecture (New York: C. P. Farrell, 1894). This was only one of many slim twenty-five-cent volumes produced by Ingersoll during his career as a public speaker.
123 Ibid., 12.
124 Ibid., 12, 14.
125 Ibid., 18.
During the Cold War, Urey would present claims about the importance of the Judeo-Christian worldview – and in particular Biblical teachings such as the Ten Commandments – in civilizing the west and allowing for the rise of science that conflicted with Ingersoll. Still, it is not difficult to imagine the great impact that the Freethinkers might have had on the young Urey, living within a pious household.

There is no reason not to take Urey at his word in this regard, and this section is not intended as an attempt to recover Urey’s theism or even to claim that he was a particularly Brethren scientist. Rather, retrospectively reading Urey’s life up until WWI within a Brethren context is intended to provide a more complete picture of the social and familial world within which Urey grew up. Within this context we can ask what resources – cultural, educational, or otherwise – were available to him because of his religious affiliation, and how this helped to shape the realm of possibilities open to him as a young man. We can also then ask why Urey chose to obscure this background from his public persona for most of his career. A better understanding the Brethren context of Urey’s younger life will also allow for a more complete appreciation of the resources he would draw upon later in life when formulating his argument for the “New Prophet” and the mutual reliance of science and religion.

Harold Urey was born into the German Baptist Brethren Church (known after 1908 as the Church of the Brethren). Five men and three women founded the Brethren movement in 1708 in the German principality of Sayn-Wittgenstein-Hohenstein. All eight of these original members were of Reformed or Lutheran Church backgrounds and had been inspired by the teachings of the German Radical Pietists to believe that the
church would not be fully reformed until the church of Christ’s apostles was recreated in its original form. In Wittgenstein the Brethren’s original eight members, under the direction of their leader Alexander Mack, took a solemn covenant of “good conscience with God, to take up all the commandments of Jesus Christ as an easy yoke, and thus to follow the Lord Jesus . . . even unto a blessed end.” Expelled from their home states for their radical views, they had come to reside in Wittgenstein, where the principality’s sovereign, Count Henrich Albrecht, was willing to give refuge to religious dissenters. In addition to influential Pietist preachers such as E. C. Hochmann von Hochenu, their time in Wittgenstein put the Brethren in contact with the Mennonites who also enjoyed the protection of Count Albrecht. From the Mennonites they adopted an Anabaptist interpretation of Christianity, believing that the only true baptism was adult baptism.


From its original eight members the Brethren movement grew both geographically and numerically within Europe thanks primarily to missionary activities in neighboring parts of Germany, Switzerland, and the Netherlands. Their new converts, who often found themselves no longer welcome in their home states, congregated in Krefeld on the lower Rhine River where they again lived alongside a local Mennonite population that had found refuge in the region due to their contributions to the local textile market. Here, because of their pious ways and their modest dress, the Brethren were considered by local authorities to be simply a variant of Mennonites. However, because the Brethren were more aggressive than their Mennonite counterparts in their missionary activities, they soon experienced repression at the hands of local Krefeld authorities for recruiting and converting from the congregations of local established churches. Facing repression in Krefeld, a group of twenty families left for Pennsylvania in 1719. They settled in Germantown, Pennsylvania, which had been founded by Mennonites and Quakers from Krefeld in 1683. After a second Brethren migration in 1729, the Brethren movement was completely transplanted in the New World, and Germantown became the church’s mother congregation.128

These several Brethren families, along with the Lutherans, Mennonites, and Amish, were among the 80,000 German immigrants who joined Penn’s Quaker colony before 1770. This number included roughly 4,000 Mennonites, 500 Amish, and 1,635 Brethren – the groups that would collectively come to be known as the Pennsylvania

As in Krefeld, the Brethren’s appearance and practices often confused their neighbors into thinking that they were a variant of Mennonite. One distinctive practice that did set the Brethren apart from these other sects was their commitment to “trine immersion” – a form of baptism in which the adult church member was taken into a flowing river or stream and dunked forwards three times. This unique practice, perhaps combined with an English misunderstanding of the German word “Taufer” (simply meaning Baptist) earned the Brethren the popular nickname “Dunkers” (also sometimes written as Dunkards, Tunkers, Tunkards, Tumblers, and other similar variations).

Aside from their outward appearance and their unique baptismal practice, their inward or spiritual life remained something of a mystery to outside observers in the Colonies, and even to fellow Baptists. Writing just prior to the American Revolution, the Philadelphian Baptist minister Morgan Edwards said of the Brethren that “It is very hard to give a true account of the principles of these Tunkers as they have not published any system or creed, except what two individuals have put forth; which have not been publicly avowed.” In part this was because the Brethren defined themselves primarily by their objections to the creeds and practices of other established churches, and also because they considered themselves to be non-creedal; believing that creeds represented over-interpretations of, additions to, or subtractions from the Gospels, they instead sought to make the entire “primitive text” of the New Testament their guide to faith and life. As followers of the primitive text, Edwards did note that “Their acquaintance with the Bible

129 Mennonite and Amish numbers from 1770 are taken from James O. Lehman and Steven M. Nolt, Mennonites, Amish, and the American Civil War (Baltimore: Johns Hopkins University Press, 2007), 9; Morgan Edwards, Materials Towards a History of the American Baptists (Philadelphia: Printed by Joseph Crukshank and Isaac Collins, 1770); Bowman and Bowman, Brethren Society, 16.
is admirable.”¹³¹ After a description of his own personal observations of Brethren life Edwards concluded that “they are meek and pious Christians; and have justly acquired the character of the Harmless Tunkers.”¹³²

Edwards’ description of Brethren beliefs and practices noted their preference for “plainness of language and dress,” which he compared to a similar preference among their Quaker neighbors.¹³³ Writing more than a century later, the Brethren leader Martin Grove Brumbaugh described the so-called “plain dress” of the Brethren as the outward expression of obedience to God and of nonconformity to the outside world. According to Brumbaugh, the distinctive dress likely was adopted in Pennsylvania, and its similarity to the Quaker style of dress was likely an indication of Brethren political sympathy with this group and its commitment to peace.¹³⁴ Edwards also noted the Brethren’s dissenting attitude toward military and legal authority: “[they] will neither swear nor fight. They will not go to law.”¹³⁵ He noted that rather than taking communion on a weekly basis, they instead held an annual “Lord’s Supper,” which included a “love-feast,” the communal washing of feet, and several other “ancient attendants.”¹³⁶ Finally, Edwards described the Brethren’s style of communal worship and method for selecting ministers from within their ranks: “…every brother is allowed to stand up in the congregation to speak in a way of exhortation and expounding; and when by these means they find a man eminent for knowledge and aptness to teach, they choose him to be a minister, and ordain him with imposition of hands, attended with fasting and prayer and giving the right hand

¹³¹ Ibid., 67.
¹³² Ibid.
¹³³ Ibid., 66.
¹³⁴ Brumbaugh, A History of the German Baptist Brethren in Europe and America, 547.
¹³⁶ Ibid.
of fellowship.”

This method of selection, which continued well into the 19th century, allowed the Brethren to choose from amongst themselves those men whom they felt possessed an exceptional connection with the Gospels, and also to perpetuate an informal infrastructure of unsalaried lay ministers with no special education or training.

Although many of the beliefs and practices that Edwards described in 1770 persisted in the following centuries, life within the Brethren was not entirely static. In Pennsylvania prior to the American Revolution, many of the Brethren lived agricultural lives on the tracts of land sold to them by Penn’s company. However, having the center of their religious community in Germantown also allowed them to engage in other activities such as textile manufacturing, printing, and, to a lesser extent, education. According to some Brethren scholars, the Brethren were positioned to become intellectual leaders among the Pennsylvania Dutch. This came to an abrupt end, however, when their refusal to take up arms against the English crown brought on persecution by the Revolutionaries.

This refusal was in some cases related to a genuine sympathy for the government that had granted them religious freedom in the New World, and in other cases was simply a result of the Brethren’s proscriptions against swearing oaths and fighting. Persecution took the form of imprisonment and in some instances the seizure of Brethren property (including their main printing presses in Germantown). Feeling they were no longer welcome in Philadelphia, many of the Brethren migrated further into the countryside where they took almost exclusively to an agricultural way of life. Cut off from the influence of the burgeoning American cities of the East Coast, as well as from their own

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137 Ibid., 67.
printing activities, the Brethren entered into what some Brethren scholars have referred to as the “dark ages” or “wilderness period” of their movement. Characterized by withdrawal, stagnation, an unwillingness to abandon the German language or ethnic German ways, the cessation of all evangelistic and missionary activities, and an attitude of suspicion and opposition to most outside cultural or educational endeavors, this period lasted from the American Revolution until the eve of the Civil War.  

By the time Harold Urey was born, the Brethren were in the midst of a period of great cultural change that some Brethren scholars have termed (in direct contrast to the “dark ages” of the movement) the “Brethren Renaissance” or the “Brethren Awakening.” In many ways Samuel and Cora were in the vanguard of these changes, and represent a growing segment of Brethren society that was feeling the effects of increased interaction with the outside world. The prelude to these changes began in the 1830s, as the Brethren were encountering the public school system and receiving mandatory instruction in America’s “common schools.” The first generation to be educated in the public schools took over the leadership of the church in the 1860s. Far more Anglicized and educated than their parents, these young Brethren no longer spoke

139 Several Brethren commentaries on this period describe it in this way. Gillin, for example, wrote that “In matters of education and science, they are content with theories that have been outgrown for almost a century. Within the last ten years, however, there has begun a veritable renaissance among them”: Gillin, “The Dunkers,” 203-204. Hanle also describes a Brethren Awakening that came to fruition during the latter half of the 19th century within Brethren colleges, coming into full force in the 1890s: Robert Vail Hanle, “A History of Higher Education Among the German Baptist Brethren: 1708-1908” (Ph.D., Philadelphia: University of Pennsylvania, 1974), 231. This “awakening” of the Brethren coincided roughly with America’s Third Great Awakening, and shared many of the same characteristics, although it was not as dominated by revivalism. Rather, it was dominated by missionary activities and gradual social and cultural reform within its own ranks.
or understood German, and they were increasingly adamant in their push for English-language services and bilingual hymnbooks.\textsuperscript{140}

One subset within this generation was pushing even further toward ending the isolation of the Brethren. These were the Brethren who, after completing their own primary school education, returned to the schoolhouses as schoolmasters, or even went on to higher education. Many among this group went on to become writers and publishers, reviving a pre-Revolutionary tradition in Brethren print culture that flourished between the 1850s and 1880s.\textsuperscript{141} These Brethren, unlike the previous leadership of the church, were primarily concerned with publishing, educating, and evangelizing. They founded Brethren schools and colleges and promoted progressive ideas within the church.\textsuperscript{142} By the turn of the century the Brethren college movement had produced seven Brethren colleges. Hanle argues that, along with the forces of socialization, the college movement pushed the Brethren – who had entered the 19\textsuperscript{th} century as a sectarian “voluntary society of non-conforming pietists” – further toward the mainstream of American Protestant denominationalism.\textsuperscript{143}

Samuel Urey was born in 1866, just as the educated Brethren described above were gaining a leadership role within the church. On his mother’s side of the family, Samuel was descended from several generations of Pennsylvania Dutch who traced their

\textsuperscript{140} Bowman and Bowman, \textit{Brethren Society}, 97.
\textsuperscript{142} The earliest account of the Brethren higher education movement is Solomon Zook Sharp, \textit{The Educational History of the Church of the Brethren} (Elgin: Brethren Publishing House, 1923); The most comprehensive source is Hanle, “A History of Higher Education.”
\textsuperscript{143} Hanle, “A History of Higher Education,” 229; Although it does not address the Brethren outside of Pennsylvania, a similar story is told in Donald R. Fitzkee, \textit{Moving Toward the Mainstream: 20th Century Change Among the Brethren of Eastern Pennsylvania} (Intercourse: Good Books, 1995).
origins back to Jacob Hochstetler, one of the early Amish settlers of Pennsylvania.\textsuperscript{144} The family’s Brethren roots trace back to Jacob’s son Christian, who joined the German Baptist Brethren sometime in the 1760s.\textsuperscript{145} Christian became the first in a long line of Brethren ministers in the Hostetler family, and helped to found the Brethren church in Somerset County.\textsuperscript{146} Thus the tradition of Brethren ministry was well established within the Urey family long before Samuel was born. Brethren church records show that Samuel carried on this tradition, ministering to Church of the Brethren congregations near his hometown of Corunna, in the Walkerton area, and in California.

That Samuel received a higher education and went into education indicates that his family likely took a relatively progressive view toward the modern world. Moreover, before his illness struck Samuel seems to have been moving toward affiliation with more urbanized Brethren communities. Samuel did come from a rural background to be sure, and the move to Walkerton, located in St. Joseph County, was a move to a more urbanized part of Indiana than his hometown of Corunna. St. Joseph County’s population in 1900, 58,881, was more than twice that of DeKalb County, in which Corunna is located. Whereas only 28% of the population in DeKalb County lived in urban areas, in St. Joseph County the population was 70% urban. Furthermore, while the population of St. Joseph County would rise in the next decade to 84,312, DeKalb County’s population would actually decrease by more than 600.\textsuperscript{147}

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\item[\textsuperscript{144}] Hostetler and Hochstetler, \textit{Descendants of Jacob Hochstetler}, 117.
\item[\textsuperscript{145}] Ibid., 37.
\item[\textsuperscript{146}] Ibid., 59; Jerome E. Blough, \textit{History of the Church of the Brethren of the Western District of Pennsylvania} (Elgin: Brethren Publishing House, 1916), 410.
\item[\textsuperscript{147}] Population data retrieved from the Minnesota Population Center, \textit{National Historical Geographic Information System: Pre-release Version 0.1} (Minneapolis: University of Minnesota, 2004), http://www.nhgis.org/.
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These statistics taken on their own would seem to indicate that the move to Walkerton represented a move away from traditional Brethren society and toward a more secular, urbanized community. While this is somewhat true, census numbers also indicate that St. Joseph County boasted a Brethren population that was nearly five times the size of DeKalb County’s, giving it a much higher concentration of Brethren. But moving from Corunna to Walkerton was more than simply a move from one traditional Brethren community to another, larger traditional Brethren community; the Brethren of Walkerton were more urbanized than those of Corunna. Exactly what this means is difficult to determine. In the 1890s, the church was still reeling from its “Great Divide” – a schism in the church that broke it into three branches: from most conservative to most progressive, these were the Old Order Brethren, The Church of the Brethren (although not as traditional, this middling group aligned itself with the Old Order Brethren), and the Brethren Church. Many of the events that led up to the split occurred in Indiana, which along with Ohio had become a pivotal state in the expansion of the Brethren out of Pennsylvania. The split took place at the Brethren’s 1882 Annual Meeting near Milford, Indiana. The Divide split families and friends, not to mention congregations, along progressive and conservative lines. In the 1900 US Census, when given the chance to identify as either a “Dunker (conservative)” or a “Dunker (progressive),” all of the respondents in St. Joseph and DeKalb Counties chose to identify with the conservative branch, meaning that the more progressive Brethren Church did not have a strong

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presence here. However, it is impossible using these statistics to determine just how traditional they were.

According to Gillin, all branches of the Brethren began to liberalize after the divide. In towns like Walkerton, which were not exclusively Brethren communities, the Brethren’s increased urbanization required new knowledge and an altered perspective on old phenomena. While this did not necessarily mean a rejection of tradition, it did often require its reevaluation. On top of this, those who found themselves working in the schools, as Samuel did, had to refine their speech (softening their Pennsylvania Dutch accent and mannerisms), make themselves presentable to non-Brethren audiences, and learn to accommodate other religious viewpoints.

Samuel Urey’s position as the superintendent of schools in Walkerton demonstrates that he was the product of the education movement within the church. His next move to California, although it may have been motivated primarily by health concerns, shows that he also worked with those who were at the forefront of the Brethren higher education movement. The Santa Fe Railroad opened up cheap passage from the Midwest to California, and the Ureys were among those Brethren who decided to move to the West Coast. They were among the more than 600 Brethren that the Brethren minister M. M. Eshelman brought to settle in Southern California by 1905. Eshelman was himself a college-educated minister, as well as a promoter of the Brethren college movement. He had previously helped to establish McPherson College in McPherson, Kansas, and in 1890 worked to open a Brethren college in Lordsburg, California. There is

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150 Ibid., 186-199; Bowman and Bowman, Brethren Society, 146.
no evidence that Samuel Urey participated in the activities of what became Lordsburg College (known today as the University of La Verne). However, Samuel ministered alongside Eshelman and other pro-education ministers in Covina, California, the “mother congregation” of the Southern California Brethren, from 1894 to 1897. He was then sent to Colton, California, where he was to help to establish a mission church. Unfortunately, Samuel’s health failed him after only a few months in Colton, after delivering twenty-eight sermons, teaching fourteen Sunday school sessions, and converting one soul. As described above, Samuel, Cora, and the young Harold returned to the care of Samuel’s widowed mother in Corunna.

Although much had changed within the church thanks to the efforts of the educational reformers, much of Brethren life in the late-19th century would still have been recognizable to a church member from earlier in the century. The wilderness period had left its mark on the Brethren, in the form of an “agrarian myth” that held that rural life represented all that was good, noble and Godly while cities were viewed as “modern Sodoms” where life was base, wicked and depraved. Life amongst the Brethren, especially those who remained tied to the agricultural way of life, still revolved around the task of returning the church to the exact conditions in which Jesus and his Apostles had left it – in this way restoring and preserving the original Apostolic church. Along these lines, the sect still aspired to practice “Primitive Christianity,” preserving the original faith in the ways most similar to the life described in the Gospels. And they still rejected all creeds, placing “unique emphasis upon the outward form of the Christian

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152 Ibid., 76.
ordinances: baptism, communion, feetwashing, anointing, and the holy kiss should be practiced exactly as originally instituted by Christ and his apostles.\textsuperscript{154}

Their emphasis on recreating the primitive church in its outward form made all Brethren of the early 19\textsuperscript{th} century (as well as many at the end of the century) incredibly concerned with behavior, and to a lesser extent matters of theology. Since the time of Edwards’ observations, the Brethren had become more willing to articulate some of their shared theological commitments (although most of these were still tied to elements of behavior and outward appearance). From the New Testament they had determined four primary requirements of Christ’s followers: that they display a childlike faith that was spontaneous, unquestioning, and heartfelt; that they be self-denying and unified in spirit and mind, renouncing all self-will and self-interest; that they display outward expressions of obedience and self-denial; and that they separate themselves in all ways from the heathen world around them.

Faith was displayed in tangible acts, meaning that Brethren life in the 19\textsuperscript{th} century was filled with taboos. If anything, the enforcement of these outward forms of Christian life had become stricter as reforms of other aspects of the religion were tolerated. As Bowman and Bowman stress, the strict prescriptivism of Brethren leadership from the mid- to late-19\textsuperscript{th} century in enforcing the traditional Brethren taboos should not be read as an indication that Brethren life was unchanging. Much the opposite, the heightened sense of a need for formalized rules of behavior is an indication that this was “a church straining to adapt traditions of faithfulness, unity, and separation to new conditions of

\textsuperscript{154} Bowman and Bowman, \textit{Brethren Society}, 26.
progress, growth, dispersion, mobility, schooling, Anglicization, and proliferation of printed material.”

Brethren dress, already noted as plain in 1770 by Edwards, had remained fairly uniform from the period between the American Revolutionary War and the Civil War, but the code of dress was neither formally described nor enforced until the 1860s. In 1866 the church’s Annual Meeting (the mechanism by which the group reached consensus on whatever issues were troubling it) prescribed a particular haircut for brothers – combed either down the middle or straight back, but never in the fashion of the time. In 1877 the rules were extended to prescribe a specific cut of coat with a raised collar and no lapels. In the year that Harold Urey was born, the Annual Meeting affirmed the importance of plain dress, especially among its ministers and officers. Any violation of this code by “[following] the fashions of the world” would “violate the order of the Gospel,” and would bring the offending member “under the counsel of the church.” Annual Meeting affirmed this decision again in 1897, where plain dress was now referred to as “Gospel plainness.” Such restrictions on dress and adornment remained in place until after WWI.

Hogan describes the “Plain Clothing” worn by Brethren congregants and its significance in religious and social life during the period of Harold Urey’s childhood thus:

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155 Ibid., 119.
156 Ibid., 114.
157 Annual Meeting text is quoted in Sappington, The Brethren in Industrial America, 100.
158 Bowman and Bowman, Brethren Society, 200.
The men wore full beards, plain hats with broad brims, coats and vests buttoned all the way to the neck, coat collars were straight with no lapels, shirt collars close to what we now call clerical, and neckties were worldly luxuries never to be worn. Clothes were always drab in color, frequently black. Women wore plain bonnets. Under these it was customary to wear a small white “cap” of very thin materials which served as a prayer-covering when the bonnet was removed during worship. Dresses must be of the plainest kind with no ruffles, colorful touches, lace collars or even white trimmings. Jewelry was never to be worn and “gold was a signal to Satan.”

Brethren periodicals carried advertisements from tailors who would take measurements by mail, produce and ship plain garments to those Brethren who no longer made their own clothing. The prohibition of superfluities extended beyond dress, forbidding the church’s brothers and sisters from attending shows, feasts, celebrations, dances, theaters, fairs, and festivals, and also prohibiting such vanities as birthday celebrations, or such commemorations of violence as Independence Day. For the most part, they preferred a simple and ostensibly pre-modern lifestyle.

Outwardly, Samuel and Cora (and later Alva and Cora) must have resembled a traditional Brethren man and wife (otherwise neither Samuel nor Alva would have been permitted to minister within the church). Many of their views were likely still traditional. Likely they still held the view that the Bible was an infallible record of human history, and that all great advancements in society were due to the Bible. And even as they did participate in two of the most reformed aspects of Brethren life – education and missionary activity – this would only have brought them under increased scrutiny regarding taboos on both behavior and dress. While spreading primitive Christianity at

159 Hogan, “The Intellectual Impact of the Twentieth Century,” 16.
160 One example of a Phillipson Clothing Company advertisement from the Brethren’s Family Almanac, 1897, is reproduced in Sappington, The Brethren in Industrial America, 69.
the end of the 19th century became a “veritable passion” of the Brethren, it was nonetheless highly prescribed. That Samuel was called upon to take up missionary activities in California reveals a few aspects of his character. It first of all reveals that, despite his illness, he met the unwritten requirements of missionary ministers: that he displayed energy, adaptability, and education. The formal expectations of such ministers, as spelled out in the 1890 Annual Meeting, would also have had to be met: They were to be sound in the faith and “dressed in the order”; they were to submit wholly to the control of the church’s Mission Board; they were to defend and teach the principles of the Gospels and the “doctrine and peculiarities of the church, as defined and applied by Annual Meeting,” and to exemplify the same in their daily lives; and their wives were expected to be “a true help-meet” who would live by the same Gospel principles as their husbands.

While the Brethren with whom the Ureys interacted in college and in Southern California can be characterized as reformers, the more rural Brethren that Harold grew up around in the Corunna area after his father’s death likely did not share these views. The majority of Brethren held that the Bible was the only guide to living as a Christian, and that what the Bible advocated was a life of discipleship based on a strict commitment to the Ten Commandments, as interpreted by Christ in the Sermon on the Mount. The most conservative of the Brethren at the turn of the century also continued to reject any form of conformity to the customs of the outside world. Brethren life in agricultural communities

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162 Bowman and Bowman, *Brethren Society*, 129.
163 Ibid., 141.
that were unaffected by urbanization was thus heavily prescribed; nonconformity to the outside world meant strict conformity to the rules of Brethren society.

According to Dove’s 1932 sociological study of the Brethren, Brethren families in agricultural communities at the turn of the century typically remained isolated: “The Brethren family of the early days was a unique institution. Located generally in the open country, Brethren homes were more or less isolated from the larger culture centers and from each other. Each family constituted its own culture center for the most part, and developed an independence and unity all its own.”164 Urey’s family, as his autobiography emphasized, was pioneer stock, having settled the wilderness of Indiana. It is likely that this frontier lifestyle persisted to some degree within the Urey household, since they did not have much money after the death of Samuel. For the frontier Brethren, Dove tells us, life mostly revolved around the production from raw materials of all of the necessities of life – including clothing and food. As such, each member of the family was an essential part of “an industrial, social, moral and religious organization.” Dove also tells us that this lifestyle was dominated by a religious faith that included a “strong insistence upon the simple life,” and that often led the family to impose “a stern and puritanic [sic] discipline upon its members.”165 The home was the center of religious life for a Brethren family, and home life included a daily routine of scripture reading and worship. Dove comments, “The home without daily family worship was considered to be without true Christianity.”166

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164 Dove, “Cultural Changes in the Church of the Brethren,” 209.
165 Ibid.
166 Ibid., 213.
Each family member in a traditional Brethren household was thus a worker, a producer, and a contributor to the religious life of the family. This description of the family as industrial and social unit rings true in Urey’s case. Although he was never held home from school, he did remember doing hard labor on his grandmother’s and stepfather’s farms and claims to only have been exposed to a larger social unit when he left the farm to attend high school. In addition to Urey’s memory of morning prayer and family worship, a letter that Urey wrote to the minister of his boyhood church, the Cedar Lake Church of the Brethren, reveals some of the specific aspects of his family’s religious life. Here he describes riding in a horse and buggy to church on Sundays, observing a strict dress code, and a traditional separation by gender practiced within the church:

I attended this grand, old, modest, white, country church from about the age of 6 to 18, from the years of 1899 to 1917. We drove “old Bob” in a “surrey with a fringe on top”, five miles from my grandmother’s farm to church. In this church, and its Sunday School, I learned my knowledge of the Bible mostly. … I also attended church in Kendallville, Indiana while I was going to high school in the wintertime, and attended other churches after that when we moved to Montana. … In the old days, the women and men sat in different sections of the church. The women wore long skirts and long sleeves with high-necked collars, a marked contrast to the clothes of today. The men wore coats with no place for neckties. The sermons were about ½ hour long, and my father and stepfather both preached in this church. The communions and the full-scale meal in honor of the Lord’s Supper, where we washed each other’s feet, took place once a year. The winter meetings, when it was necessary to travel long distances in the snow, were often attended by only a few people, and this building was very cold. These are a few memories of these ancient days of my life. I remember particularly the Lord’s Prayer, the Sermon on the Mount, and, of course, the Ten Commandments. These are my favorite quotations from the Bible.

167 Harold C. Urey, “Evolution vs. Miraculous Creation”, n.d., 11, Box 144, Folder 25, HCU.
168 Harold C. Urey to Reverend Arthur Morris, August 7, 1974, Box 59, Folder 12, HCU. These passages that Urey claimed to remember from his boyhood are the same passages he tended to reference in his 1950s
Urey’s memories fit with Brethren accounts of what traditions were maintained in the church in the early decades of the 20th century.

Science and the Brethren

What was the Brethren’s relationship to science? Brethren tradition held that the Bible was an unquestionable record of human history, and that it continued to be a force in the shaping of society. In the words of Brethren ministers Owen Opperman and Charles M. Yearout – words that seem as though they could have inspired Urey’s later position – the Brethren agreed that all of the “comforts and learning and great improvements” of America were produced by the Bible’s impact upon society: it had elevated man from “a state of barbarism and superstition to a high plane of morality and enlightenment.”169 While one might imagine that this understanding of the Bible – which did often lead to literalism – would lead to conflict with science, it also surprisingly led the Brethren to be exceptionally interested in some aspects of science. For instance, they had a strong interest in anthropology and archeology that grew out of their interest in reconstructing the Apostolic Church of Primitive Christianity. Along these lines, they followed the work of anthropologists in the Holy Land. According to Hogan, the Brethren periodicals reported on excavations around the world, but the ones in the Tigris-Euphrates area were most completely described.170 In their publications, Brethren authors

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169 Opperman and Yearout quoted in Hogan, “The Intellectual Impact of the Twentieth Century,” 85, 86.  
170 Ibid., 82.
also reported on the deciphering of the Rosetta Stone, the Behistian Inscriptions, and the Hammurabic Code.¹⁷¹

One Brethren scholar describes the Brethren of the latter half of the nineteenth century as “a curious combination of superstition and scientific openmindedness.”¹⁷² In addition to their interest in ancient cultures, they also pursued phrenology and “manufactured, sold, and consumed great quantities of patent medicines.”¹⁷³ (Samuel Urey had reportedly studied phrenology, and concluded after studying the shape of the newborn Harold’s skull that he would be a great man.¹⁷⁴) Two of the Brethren’s greatest printers and reformers of the period, Henry Kurtz and James Quinter, urged their fellow Brethren to read *Scientific American*, from which they would be kept up to date on the latest and most scientific methods of farming and ranching.¹⁷⁵ Even if a Brethren household did not subscribe to *Scientific American* at the turn of the century, progressive publications such as *The Gospel Messenger* frequently cited information from secular publications including *Science* and *Scientific American*.¹⁷⁶

There were of course occasions when science seemed to contradict existing Brethren interpretations of the Bible. However, in these instances, the non-creedal nature of Brethren life tended in many cases to allow for the accommodation of new knowledge. In a 1901 *Brethren Lesson Commentary*, for example, the Brethren reevaluated the age of the earth. Here they decided not to hold Archbishop Usher’s date of 4004 B.C. for the

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¹⁷¹ Ibid., 81.
¹⁷³ Ibid.
¹⁷⁴ Reinoehl, *A History of the Fairfield Cemetery*.
creation of man. They noted that “The Bible was not written to teach chronology, and most dates, especially those prior to the Exodus, are matters of speculation and not revelation.”177 And in 1900, the first President of the Brethren-owned Manchester College – which would soon be contributing to the production of scientists and scientific publications – articulated the position as follows:

Many things that the scientist seeks to understand are omitted from the Genesis record, but the author of Genesis omits nothing that is necessary for a clear conception of the origin of God's chosen people through whom God revealed himself. Had the church kept in view the purpose of the writer of Genesis she might have saved herself from many unhappy conflicts with science. He aims to reveal God's majesty and power as Creator and Controller of the universe and leaves the question of physical science for others to settle.178

In general, the Brethren saw neither science nor higher criticism as damaging to their view of faith. The authorship of the Bible – whether it was written by Moses or was a composite work composed by several authors at different times – did not matter. It was no less plausible to believe that several authors had been divinely inspired than that Moses alone had received instruction. With their investment in reconstructing the historical church, Brethren intellectuals even welcomed those parts of higher criticism that they felt were constructive, while maintaining their belief in the Bible’s “supernatural” element.179

It would not have been necessary for Urey to leave the Church of the Brethren in order to pursue science. The first science curricula appeared in Brethren colleges as early

178 E.S. Young quoted in Ibid.
179 These were the views expressed at the 1910 Annual Meeting, reproduced in Hogan, “The Intellectual Impact of the Twentieth Century,” 127-128.
Brethren colleges where science was taught at the turn of the century promoted the harmony of science and religion. Here the Brethren essentially adopted a view of nature somewhere between the “two books” view of Sir Francis Bacon, and the separate spheres argument popular today. In the words of F.B. Myers, God had infused all mind and matter with truth, and this truth was classified under two “great heads,” moral and scientific. These two heads would not yield the same truths, but they would be supplementary. Again in words that sound similar to Urey’s later position, the Brethren minister W.I.T. Hoover argued that science was no substitute for religion, as it gives no answer to what “must be, but only what is; not what ought to be or we would like to have to be, but merely the already existing.” Thus nature and the Bible were both seen as divine creations infused with truths, but these truths were supplemental, not overlapping.

Had Urey wanted to study science in a Brethren college, he might have chosen to attend Manchester College in Manchester, Indiana. Here the Brethren marine biologist Albert B. Ulrey had in 1891 become the college’s first biology instructor. Ulrey was born in 1860, to a father who was both a farmer and a teacher near Liberty Mills, Indiana. Ulrey had followed a familiar Brethren educational trajectory: studying at Indiana State Normal School in Terre Haute. After this, he entered Indiana University, where he studied under David Starr Jordan. Ulrey then went on to study at the Marine Biological Laboratory at Woods Hole, Massachusetts, the Rush Medical College, the Northern Illinois College of Ophthalmology, the Pasteur Institute in Paris, the Zoological Station in Naples, and the German universities. By 1891, Ulrey had brought what he had learned of

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181 Meyers and Hoover quoted in Hogan, “The Intellectual Impact of the Twentieth Century,” 140.
European laboratory research to Manchester College and was publishing scientific papers in the Proceedings of the Indiana Academy of Science. After ten years at the college Ulrey went on to take a position as Chair of the Biology Department of the University of Southern California. But during his time in Manchester, Ulrey constructed a small but modern biological teaching laboratory, taxonomy collection and herbarium. He also instituted a science curriculum at Manchester that emphasized, among other things, laboratory work as a method of instruction, direct mentoring of the students by instructors, outdoor field work and collecting, attention to current scientific literature, and independent student research. From a religious standpoint, Ulrey also emphasized the value of scientific study to religion.\textsuperscript{182} By 1896, Manchester College had instituted courses in the sciences of geography, geology, physics, chemistry, physiology, zoology, and botany.\textsuperscript{183}

**Leaving the Faith**

“[He] feels it is alright to be unconventional inside, as long as you don’t advertise it on the outside.”\textsuperscript{184} H.C. Urey’s daughter-in-law quoted in “Harold Urey, Adventurer” (1965)

According to Urey’s account, the influence of Brethren life waned during his high school years in Kendallville, and it continued to wane as he pursued his education further.

\textsuperscript{182} An account of Ulrey’s career and contributions to the science curriculum of Manchester College is found in William R. Eberly, *The Story of the Natural Sciences at Manchester College* (Manchester College, 2005), chap. 2 “The First Ten Years.”
\textsuperscript{183} Ibid., 14.
\textsuperscript{184} Shelton, “Harold Urey, Adventurer,” 359.
The moment when Urey had to choose between his childhood religion’s absolute prohibition on war and his desire to participate in the struggle against German imperialism is presented in Urey’s autobiography not primarily to claim this religious past, but to illustrate how this religious commitment had been diminished to an intellectual value for Urey. But having now examined Urey’s life within the context of Brethren life at the turn of the century, it is possible to reevaluate this decision to go against his Brethren upbringing from different angles. The purpose of this is not to deny Urey’s intellectual reasons for choosing the struggle against imperialism (and later fascism and tyranny) over his religious upbringing, but rather to open up additional explanations for this decision. Exploring this decision will also open up avenues for exploring the reasons why Urey chose not to make his Brethren past a part of his official autobiography. The reasons for Urey’s omission were in fact overdetermined during the period of WWI, and included peer pressure within Urey’s social group, national and local prejudices against ethnic Germans and conscientious objectors, the development of a chemical service ideal, changes within the Brethren church and its perceived relationship to the outside world, and understandings of race and whiteness during this period.

The first explanation to consider is one that would largely be in keeping with Urey’s official biography. Urey became self-conscious of his country ways (which might be read as his Brethren plainness of dress and speech) during his high school years. Urey states that he, a wild country boy, was confronted with far more sophisticated peers from the city. Urey did his best to fit in during these years. This effort to fit in continued into his years as a schoolteacher and a college student. Self-conscious about his rural Indiana accent, he purchased a *Webster’s Unabridged Dictionary* and spent his spare time
practicing his pronunciation. At college he likely also changed his style of dress to match that of his peers. He would later in life become known for never being seen out of his three-piece suits and ties, earning him a reputation as the “best-dressed man in his laboratory.”

The culture shock Urey experienced fits well with that described by other Brethren youth of the early- to mid-20th century upon entering into more diverse and heterosocial educational environments. These young Brethren reported feeling ashamed of their characteristic Brethren appearance and speech patterns. This experience was a common phenomenon among Brethren young adults when they were put into high schools or colleges that segregated them not by religion but by generation. The experience could be a liberating one. According to Bowman and Bowman, this movement from the trans-generational family to the mono-generational peer group within non-Brethren high schools and colleges provided Brethren youth from traditional households with a “zone of invisibility.” Within this zone they were permitted to “experiment with questionable customs and practices without the knowledge or censure of their home community.” For Urey this zone would have been defined by the absence of his primary living role model, his mother Cora.

During his high school and college years, and within this zone of invisibility, Urey was also becoming increasingly exposed to non-Brethren male role models. The most influential of these was Bray. According to Urey, it was Bray who helped him to

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185 Ibid., 351.
187 Bowman and Bowman, Brethren Society, 202.
shed the skin of the “conscientious, blue-eyed Indiana farm boy.”188 With the interest that Bray took in the budding young scientist, and with the absence of a strong male role model at home, it is possible that Bray became something of a surrogate father to Urey during this time of transition. Bray’s advice that Urey should be working “for the chemical side,” combined with the pressure to enlist along with the great majority of his fraternity brothers, may have been a strong enough impetus to push Urey to join Rhodes at the Barrett Company in Philadelphia. The peer pressure to enlist certainly went beyond the fraternity; Montana surpassed all other states in enlistment rates and draft quotas. With 12,500 volunteers and another 28,000 draftees, almost 10% of the entire population of Montana went to war.189

If Urey had been willing to go against peer pressure and claim conscientious objector status based on membership in the Brethren, there would have been real consequences: those related to claiming German identity during the war years, and those related to being a religious objector. Urey began classes at the University of Montana just as the war in Europe was beginning and he graduated in the same year that the US entered the war. Looming in the background of his college years was a tense period of neutrality within which an Anglo-American population (which was already negatively predisposed toward ethnic Germans) grew suspicious of the imagined German agents and agitators that wartime propaganda told them might be in their midst. By the time draft registration began in June of 1917, the stage was set for anti-German hysteria and a hostile climate of intolerance within which would occur hundreds of instances of German

American abuse. Ethnic German Americans, accused of offenses ranging from poisoning American enthusiasm for the war to outright treason, were pressured to buy Liberty bonds, saw their presses put under arbitrary scrutiny and their organizations accused of being funded by the German government, and were subjected to humiliating, mob-induced rituals including tarring and feathering, forced marching, and flag-kissing ceremonies.  

Once the war began, Americanization became a national and public crusade. Two federal organizations, the Council of National Defense and the Committee on Public Information, were formed with one of their purposes being to oversee the Americanization of the foreign born and the augmentation of Americans’ feelings of unity and nationhood through involvement in an intensified campaign for Americanism. German Americans who preserved any ethnic or cultural ties to their German heritage – even if they themselves drew no connection between their cultural affinity and any political allegiance to the German Imperial government – faced what Frederick Luebke described as a “crisis of loyalty.” While they had before the war defined their German heritage as part of their Americanness, and had often credited their cultural background for the measure of their success in the new world, they now found these parts of their identity treated as marks of disloyalty. Especially troublesome to “true” Americans was the perceived clannishness of some groups of ethnic Germans – particularly those who had immigrated recently or who had for generations refused

190 Frederick C. Luebke, *Bonds of Loyalty: German-Americans and World War I* (DeKalb: Northern Illinois University Press, 1974), 244.

assimilation. Needless to say, they lumped most “sectarian” religious Germans into this latter group.

Ethnic German groups before the war – especially those with roots in the US that stretched back to the colonial period – viewed America to be a nation without any official ethnic identity. For the most part they brushed off the insistence of their Anglo-American counterparts that the American nation was one of distinctly Anglo-Saxon character and regarded the maintenance of their cultural identity as an expression of American freedom.\footnote{Luebke, Bonds of Loyalty, 50. Although the term Anglo-Saxon did imply a shared German and English heritage, many of the ethnic Germans being considered here – particularly the Pennsylvania Dutch Germans – were considered poor Palatine refugees, peasants, or “swarthy” Germans. An account of the Palatine Germans and their struggles for acceptance in Britain and America is found in Philip Otterness, Becoming German: The 1709 Palatine Migration to New York (Ithaca: Cornell University Press, 2004).} This was certainly the case for “Church Germans” such as the German Lutherans and the Pennsylvania Dutch Mennonites, Amish, and Brethren, who viewed it as their constitutional right to continue using the German language in their daily lives and religious practices. They had emigrated from German states with restrictive religious policies to find religious freedom in their adopted homeland and so saw no connection between the expression of their ethnocultural heritage and any political sympathy with the German imperial government, let alone an antagonism toward the US government. Those churches that chose to defend their German heritage became easy targets for anti-German abuse.\footnote{Luebke, Bonds of Loyalty, 232.}

As the political and social climate grew more intolerant, however, ethnic Germans of all stripes suddenly faced intense pressure to assimilate to what Anglo-Americans presented as the true American culture. Efforts to defend the validity of their culture’s claim as being constitutive of American culture at large were interpreted as proof of
disloyalty. “For the German-American who valued his cultural heritage this meant, at best, that he was a second-class citizen, inferior to Americans of English antecedents; at worst it meant that he was perceived as the agent of a foreign despot.”

Although it did not free them from all suspicion, some German Americans responded to the campaign for uniformity by embracing the tenets of the emerging “superpatriotism” that defined loyalty as a subordination of everything – including ethnicity, conscience, and individuality – to the successful prosecution of the war. Having disassociated himself from the church already while in his “zone of invisibility,” and having already grown accustomed to fitting in with his non-Brethren peers, it could be that the still young and impressionable Urey (whose genealogy was largely German but whose last name was recognizably English) was also drawn toward superpatriotism – or at least away from his German roots and identity.

Within this context of anti-German sentiment, it is also important to consider the local situation within which Urey found himself. Western states including Colorado, Montana, Nebraska, and North and South Dakota all took superpatriotism to even further extremes than did their eastern counterparts. Partly this was due to a desire in the western states to impress upon their eastern counterparts that they were no longer wild and unsettled territories, but it was also due to the fact that local industries found superpatriotism to be a very effective weapon against unions. Whipped into a superpatriotic frenzy, these western states passed laws restricting or banning completely the teaching of German in public schools.

194 Ibid., 78.
195 Ibid., 226.
In Montana, the State Council of Defense extended the ban on German-language instruction to private and parochial schools and also abolished German in the pulpits of the state and specified that all German-language textbooks be removed from schools and libraries. At Lewistown, there was a public rally that included the burning of German high school textbooks and books by German authors. Meanwhile, those who were perceived as having any loyalty or sympathy for Germany faced physical harm, tar and feathering, or worse. The war only amplified the already prominent Anglo-American nativism of the western states; according to Van Nuys, “Regardless of birthplace or background, [immigrants and ethnics in the West] were told that one was either unequivocally American or not American at all and thus deserving of the severe condemnation reserved for the disloyal.”

Montana was one of the worst places to be German or a conscientious objector, let alone both. The Council of Defense was noticeably overzealous in its spreading of war propaganda, and got carried away in its search for wartime traitors and slackers. According to K. Ross Toole, the state was “in the vanguard of America’s loss of sense and judgment.” Toole notes that People of German extraction were hounded and often found it completely impossible to demonstrate their loyalty. It was not sufficient to “kiss the flag” in public and to present receipts for contributions to the Red Cross, nor even to show a sheaf of Liberty Bonds. … Letters from neighbors testifying to the ‘suspicious’ activities of other neighbors flooded into the state and county Councils of Defense. The state Council of Defense, in

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197 Luebke, Bonds of Loyalty, 252.
198 Malone, Roeder, and Lang, Montana, 278.
199 Van Nuys, Americanizing the West, 51.
200 Ibid., 14.
201 Malone, Roeder, and Lang, Montana, 277-78.
particular, urged upon all Montanans that they watch their neighbors and report at once any ‘suspicious activity’ or lack of enthusiasm for the war effort.\(^{203}\)

According to Gutfeld, “patriotic support of the war effort [in Montana] reached the level of hysteria,” which played itself out on a day-to-day basis through the persecution of ethnic German-Americans and other immigrant groups.\(^{204}\) It reached its height with the passage of Montana’s Sedition Act of 1918 (the inspiration for the Federal Sedition Act), which effectively made it a punishable offense for any person to write or speak against the war or conscription. The law “made no distinction between those who opposed the war on principle and those who actually threatened national security” and its violation was punishable by twenty-years imprisonment or a fine of $20,000.\(^{205}\)

Claiming the status of conscientious objector based on his affiliation with a German-American peace church would have been problematic to say the least. Church Germans in the west faced trials. German Lutherans came under fire from one federal district judge for “instead of trying to remove the foreign life out of their souls, and to build up an American life in them, they have striven studiously from year to year, to stifle American life, and to make foreignness perpetual. That is disloyalty.”\(^{206}\) In Glendive, Montana, an unruly mob hanged a Mennonite nearly to death for pacifist views.\(^{207}\)

Had he been granted conscientious objector status, Urey would have faced further persecution within the Army. There were provisions in the Selective Service Act for noncombatant service, but Woodrow Wilson did not define what alternative service

\(^{203}\) Ibid., 140.
\(^{204}\) Gutfeld, Montana’s Agony, 13.
\(^{205}\) Toole, Twentieth-Century Montana, 155.
\(^{206}\) Van Nuys, Americanizing the West, 51.
\(^{207}\) Malone, Roeder, and Lang, Montana, 270.
would be until March 20, 1918 (well after the chemists had already secured such service for themselves) and Secretary of War Newton D. Baker did not issue supporting orders until June of that year.\textsuperscript{208} While alternative service remained undefined, objectors in Army camps faced severe pressure to take up arms. Their combatant peers – many of whom were also Christians but came from denominations that had found justification for the war – reviled conscientious objectors as cowards, yellow, Huns, and pro-Germans.\textsuperscript{209}

A confidential memorandum from Baker dated October 19, 1917 made such coercion official policy. If coercion failed and the objectors still refused to drill, Baker ordered that they be segregated to prevent their unpatriotic ideas from spreading. While in isolation, conscientious objectors were subjected to physical and mental abuse.\textsuperscript{210} They were often starved on bread and water, hanged by their wrists, and drenched with icy water.\textsuperscript{211} The numbers testify to just how powerful coercion and separation were as tactics for converting the objector into a combatant soldier – of the 20,873 recognized conscientious objectors who were called to service, only 3,989 men pursued their objector status after spending some time in an Army camp.\textsuperscript{212}

This policy of coercion and segregation was especially hard on church Germans who pursued conscientious objector status. For refusing to drill, 130 Mennonites were convicted and sentenced to prison terms of 10-30 years.\textsuperscript{213} Two Mennonite conscientious

\textsuperscript{209} Ibid., 18.
\textsuperscript{210} Luebke, \textit{Bonds of Loyalty}, 258.
\textsuperscript{211} Gerald E. Shenk, \textit{“Work or Fight!”: Race, Gender, and the Draft in World War One} (New York: Palgrave Macmillan, 2005), 61.
\textsuperscript{212} Capozzola, \textit{Uncle Sam Wants You}, 56.
\textsuperscript{213} Luebke, \textit{Bonds of Loyalty}, 259.
objectors died from the harsh treatment they received while in military custody. Four Hutterites from South Dakota were imprisoned at Alcatraz; beaten, starved, mistreated in just about every way possible, two of the four died of pneumonia after transfer to Leavenworth. The situation was no easier for Brethren youth who claimed conscientious objector status. Upon visiting Virginia’s Camp Lee, one Brethren minister reported that he had seen firsthand the harsh treatment of Brethren youth who refused army training: “Almost every indignity conceivable was heaped upon them. They were cursed, called the vilest of names, threatened, and stood up as gazing stock for the soldiers, but they stood true as steel, and won a great victory. However, they were afterward placed in a servitude almost equal to imprisonment.” Within this context of western anti-German and anti-conscientious objector sentiment, and with such grave consequences, it is not difficult to imagine why Urey might have shied away from claiming his identity as a member of the Church of the Brethren.

Brethren scholars who have examined the WWI period have concluded that the church had not done an adequate job of articulating its anti-war position to its younger members. According to Rufus Bowman, the church had been so focused on its educational and missionary activities since the Civil War that it had come to take its peace position for granted. Furthermore, these new activities had made the church as a whole less legalistic in its positions and more tolerant of the individual consciences of its members. As for young, draft-age Brethren like Urey who were attending non-Brethren colleges, Bowman notes that “Brethren young people were going to public schools and

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214 Shenk, *Work or Fight!,* 61.
colleges. They were becoming vital factors in community life. It was harder to break with their community associates on the peace question than in the days when the church maintained an exclusive church fellowship. ... The Church of the Brethren faced World War I with her young people unprepared for the struggle.\textsuperscript{217} Although Urey and other Brethren of his generation would have certainly grown up aware of their church’s position on peace, based in the teachings of Christ, he would not have had any specific instruction on what this meant in the face of modern warfare.\textsuperscript{218} Brethren responses to the draft were inconsistent. Some Brethren chose to train for war (a few even became officers), some took noncombatant service as conscientious objectors, and some refused service altogether. Thus it is not surprising that Urey went to work in a war-related industry, and that his brother Clarence served overseas in the Army.

Perhaps more important than the Church of the Brethren’s lack of instruction on the question of peace and modern warfare was its changing interpretation of citizenship. While the church had traditionally held a dualistic view of the church and the government, and had actively avoided any form of civic obligation such as voting or serving on juries, in the late 19\textsuperscript{th} and early 20\textsuperscript{th} centuries the church was beginning to hold a more “constructive” view of citizenship. This new view was evidenced by the fact that many Brethren were now voting in state and federal elections (an act that could have resulted in expulsion from the church in the mid-19\textsuperscript{th} century), and a small number of Brethren even held political positions. This new view was articulated in the Annual Meeting’s response to the Selective Service Act’s provisions for conscientious objectors,

\textsuperscript{217} Ibid., 164.
\textsuperscript{218} Ibid., 163.
which cautiously separated the Brethren’s hatred of war from any hint of animosity toward the US Government:

> Averring our loyalty to the civil authorities, and desiring to serve our country in the peaceable arts and productive industries, we commit ourselves to a constructive patriotism and loyal citizenship of real service. Therefore, we resolve, with one heart and soul, to be patriotic and loyal in the highest sense to our beloved country to which our forefathers fled for religious liberty. We resolve to invest our lives and all our energies for the conservation and promotion of all that is true, good, and noble in our institutions of home and school, church and state, and in the high ideals of liberty and humanity which we covet for the whole world.…

> We believe in constructive patriotism, therefore, we dedicate ourselves anew, and more earnestly than before, to the promotion of the great and fundamental interests of the church and state, namely, the cause of the Sunday School, missions, and Christian education. We would lay upon the conscience of every member of the church the solemn obligation of making sacrifices commensurate with the sacrifices made by those who are not exempted from military service.219

Exactly how one might interpret the “sacrifices commensurate” with military service was a matter of one’s personal conscience. Chemical work, although it dealt with the production of explosives, might well have seemed like a reasonable option.

This advocacy of “constructive patriotism” and the acknowledgement of the personal consciences of its members was a part of the Church of the Brethren’s ongoing process of Americanization. The church was taking steps to bring itself in line with mainstream protestant denominations, although it still maintained its Anabaptist and Pietist interpretation of Christianity. This fits with the phenomenon described by Luebke, that those ethnic German churches that had already started down the path toward assimilation used the war and the emergent superpatriotism as an excuse to ignore lingering conservative objections to abandoning ethnic ways perceived as backward by

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219 Ibid., 165-166.
mainstream Christians. According to Luebke, progressive ministers and deacons of these assimilationist churches took positive action, seeking appointments to local councils of defense or to Red Cross and Liberty bond drive committees, and actively worked to alter the relationship between their church and their government.

Gerald Shenk, a non-Brethren historian of the WWI draft, presents the Brethren’s downplaying of its position on pacifism in just this light. Whereas Bowman argues that the Brethren had simply neglected their peace position in the face of other activities, Shenk argues that it was the result of “[discomfort] about being perceived as being different, and perhaps not ‘100% American.’”\(^{220}\) The Selective Service System itself became a location of negotiation for the relationship of the state and the church as it shed its isolationist ways. Ministers such as G.O. Stutzman became officials within local draft boards and acted as Selective Service advisers to their young congregants. These ministers helped the local boards to distinguish valid claims of Brethren membership (as Anabaptists, draft-age members were not always baptized and initiated into full church membership, and so this was not a straight-forward task), and they helped the Brethren youth they served to claim conscientious objector status.\(^{221}\) Meanwhile, the church formed a special Central Service Committee to represent the Brethren before the government in draft-related matters, as well as a War Relief and Reconstruction Committee.\(^{222}\)

Shenk also suggests that the Brethren’s anxiety about their peculiarity and their resulting efforts to assimilate may have had something to do with race. Shenk claims that

\(^{220}\) Shenk, *Work or Fight!*, 60.
\(^{221}\) Ibid.
\(^{222}\) Bowman, *Church of the Brethren and War*, 182.
the group was racialized as inferior whites, describing them as “white but outside of the mainstream of American culture.”\textsuperscript{223} Certainly the period between 1840 and WWI saw increasing scrutiny of what Jacobson refers to as “the internal divisions of whiteness.”\textsuperscript{224} It was “a shift from one brand of bedrock racism to another – from the unquestioned hegemony of a unified race of ‘white persons’ to a contest over political ‘fitness’ among a now fragmented, hierarchically arranged series of distinct ‘white races.’”\textsuperscript{225} By and large this fracture of whiteness was the result of the wave of new immigrants entering the United States from Southern and Eastern Europe. Scholarly work has already described in great detail the hesitance of Anglo-Saxon white Americans to bestow the full privileges of whiteness upon the Irish and Eastern European Jews.\textsuperscript{226} However, while the racial prejudices of this era did focus on these immigrant groups – Celts, Slavs, Hebrews, Iberians, and Mediterraneans – one of the primary categories by which these groups were ostensibly judged (and what the pseudoscience of the day was designed to test) was their fitness for self-government. It was by this category that the true Anglo-Saxon was distinguished from the inferior European, and even groups with colonial antecedents could come under scrutiny.\textsuperscript{227}

The Brethren, Amish, and Mennonites had a history of being classified as “other” by their Anglo counterparts in the New World. Writing in 1751, Benjamin Franklin distinguished the “swarthy” Pennsylvania Dutch from the beautiful Saxon Germans, and

\textsuperscript{223} Shenk, \textit{Work or Fight!}, 59.
\textsuperscript{225} Ibid., 42.
\textsuperscript{227} Jacobson, \textit{Whiteness of a Different Color}, 42.
wondered why these “Palatine boors” should be “suffered to swarm into our settlements and by herding together establish their language and manners to the exclusion of ours”? In a letter to Peter Collinson, Franklin also criticized the intellectual qualities of the Germantown inhabitants, calling into question their ability to participate in Anglo-style government:

> The Germans who come hither are generally the most stupid of their own nation, and as ignorance is often attended by credulity when knavery would mislead it ... it is almost impossible to remove any prejudices they may entertain. ... Not being used to liberty they know not how to make a modest use of it. Unless the stream of importation could be turned from this to other colonies, they will soon outnumber us, that all the advantages we have will not be able to preserve our language and even our government will become precarious.229

In the early years of the new nation, after their persecution at the hands of the Revolutionaries and throughout their wilderness period, the Brethren did little to prove their capacity for democratic participation. Choosing to live in isolated communities and to engage the English-speaking outside world in only the most limited ways, the Brethren only seemed to confirm their backwardness.

Despite their steps toward modernization toward the end of the 19th century, at the turn of the century the Brethren were still seen as a problematic group within modern America. In his 1911 monograph, the eugenicist Charles Davenport singled out Amish and Dunker communities as groups that had in effect committed race suicide by erecting “the barrier of religious sect ... again and again to insure the intermarriage of the faithful

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228 This line from Franklin’s “Observations on the Increase of Mankind” is reproduced in Roy Lawrence Garis, Immigration Restriction: A Study of the Opposition to and Regulation of Immigration into the United States (New York: The Macmillan Company, 1927), 10.

229 Ibid.
only.”230 In his role as head of the Eugenics Record Office, Davenport sent one of his field workers, Amey Eaton, to Lancaster County, Pennsylvania to study the Amish. Her study led him to the conclusion, based on Eaton’s reported rate of epilepsy and other physical defects among this one sect, that the equivalent of a genetic time bomb was waiting to wipe out the entire community – “a defect is in the blood of some of the strain that in time will affect the entire sect who remain in that part of the country.”231 Although he briefly considered the claims of other social scientists (including Gillin) that recent liberalization within the Dunkers was leading to a gradual lifting of the prohibition against marrying outside of the church, he nonetheless concluded that, after intermarrying within the congregation for several generations, “we can but regard such small sects as eugenically unfortunate.”232 “It is difficult to see how any religious sect would have a tenet so opposed to the laws of Nature and God,” Davenport wrote, “as practically to compel consanguineous marriage.”233

In the first decades of the 20th century, consanguineous marriage was certainly on the mind of Urey’s relative, Harvey Hostetler, who in 1912 wrote a genealogy of the descendents of Jacob Hochstetler. In the book’s preface Hostetler wrote, “There is … one practical service that this work may render, and that is in pointing out relationships that should be avoided in intermarriage. This work presents over seven hundred families where descendants of our ancestor have married each other.”234 Although Hostetler went on to warn his relatives of the negative effects of inbreeding, he concluded positively that

231 Ibid.
232 Ibid., 202-203.
233 Ibid., 202.
234 Hostetler and Hochstetler, *Descendants of Jacob Hochstetler*, 12.
“The record in these pages is the record of large families, and there is no evidence that our family is in immediate danger of extinction.” He left it to those relatives most acquainted with the instances of inbreeding within the family to determine “whether the intermarriages … have resulted in harm to the offspring.”

Edwin Black suggests that Davenport’s condemnation of the Amish and their kin was directed not only toward the rate of crippling birth defects within their closed populations, but also toward their social attitudes. According to Black’s interpretation, Davenport regarded the Pennsylvania Dutch as defective due to their rejection of modern technology and their pacifism. While Davenport does not admit this bias explicitly in the work cited above, it is certainly true that the eugenics movement was to a great extent the scientific expression of the fracturing of American whiteness described by Jacobson. The Brethren tended to fall on the wrong side of those categories drawn up to distinguish the varying degrees of whiteness during this period. That the Amish and the Dunkers seemed to reject modernity, and that they had a long history of refusing to participate in government, civil society, and the military, certainly would have marked them as something less than fully white.

That their urban white counterparts viewed the Brethren skeptically is evident in an 1884 leaflet circulated in reaction to the announcement that the progressive Brethren

236 Edwin Black, War Against the Weak: Eugenics and America’s Campaign to Create a Master Race (New York: Four Walls Eight Windows, 2003), 53.
238 According to Garner, “A person racialized as white can be ideologically exiled from this privilege, or may pursue values seen as antagonistic, or adhere to a minority religion, or be from another country…” Steve Garner, Whiteness: An Introduction (New York: Routledge, 2007), 11.
minister Martin Grove Brumbaugh was running for county superintendent of schools in Huntingdon, Pennsylvania:

[I]t will be well for the directors to consider the consequences if the Dunkard is elected. His headquarters are at the Dunkard Normal School and will likely remain so, even if he had declared to the contrary in the Local. If he is elected Huntingdon County will be supplied by Dunkard teachers, even if they are brought from Va., Mo., Ohio, etc. The main idea is to make Dunkardism popular. They want to make this county the Dunkard centre of a large radius. They are the most sectarian church in the U.S., except the Catholic. Why, in their estimation to be a Dunkard is greater than a king! Therefore, vote for any one in preference to the Dunkard, even if he does come around with his deceitful smiles.239

The text of the leaflet makes it clear that the Brethren, like the Catholic, was suspected of being incapable of understanding and respecting democracy.

Urey may have taken these prejudices into account when deciding to leave the Brethren, and later when choosing to omit his Brethren upbringing from his public biographical statements. It is likely that Urey began actively concealing his Brethren heritage while in Montana. Even prior to the outbreak of war in Europe and the national campaign for Americanization, Montana, like other western states, had its own version of the “genuine American” – one that amplified the prejudices described by Jacobson. Montanans celebrated the Frontier American, which they defined as a modern nation-builder undiluted by alien blood or influence. The Frontier American defined himself against non-white peoples and their perceived inability to benefit from Anglo-American forms of government, their failure to be modern, and their cultural stagnation.240 Thus it is not surprising that it was in Montana where Urey spent his spare time practicing the

240 Van Nuys, Americanizing the West, 12.
pronunciation of words from *Webster’s Unabridged Dictionary*, attempting to shed his country accent.\(^{241}\)

Some clues to Urey’s attitudes toward whiteness and race, and toward the desirability of concealing that which would call whiteness into question, can be found in his later career. After winning the Nobel Prize, Urey became a champion of equality within his own lab at Columbia. He was well known among Jewish students as being a professor who would not only take them on as graduate students, but who would fight for them within the department. One female Jewish student whom Urey took under his wing was Mildred Cohn. Two statements made in the 1930s to Cohn confirm that Urey knew intimately the phenomenon of “passing” as white in 20\(^{th}\) century America, and that he regarded the shedding of ethnic identity as the logical response to racial prejudice. In the first instance, Urey told Cohn of a discussion he had had with his Jewish colleague, the Columbia physicist I. I. Rabi: “Urey told me that he had suggested to Rabi, a friend in the physics department, that he move to Leonia, New Jersey, where Urey and many other Columbia faculty members lived. Rabi’s response was that he preferred to remain in Manhattan among his own people; he meant his fellow Jews. Urey added, ‘I can understand that. If fascism comes to this country, I can conform but he cannot.’”\(^{242}\)

In the second instance, Urey described to Cohn what she as a Jewish woman might do in order to pass in white society:

> Urey discovered anti-Semitism [at Columbia]. He didn’t know about it up to this point. He really didn’t. And at one point he said to me, “You know,” he said, “Why don’t you go out to the Midwest where there’s no prejudice and marry a non-Jew and forget that you’re a Jew? Then you

\(^{241}\) Urey’s time practicing with the dictionary is also described in Shelton, “Harold Urey, Adventurer,” 351.  
won’t have these problems.” So I said, “Has it ever occurred to you that maybe I don’t want to forget that I’m a Jew?” He was genuinely surprised that one wouldn’t want to get rid of such a handicap.243

These episodes suggest that Urey knew that his whiteness was, in Garner’s terms, salvageable.244 His own experience had taught him that one could “forget” those things about oneself that attracted the prejudice of others.

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243 Mildred Cohn, interview by Leon B Gortler, January 15, 1987, 33, CHF.
244 Garner suggests that “inbetween people” during this period, who were phenotypically white but were racialized for reasons having nothing to do with skin color, always had the option of “salvaging” their full whiteness by adopting the proper markings of culture and class: Garner, Whiteness, 65.
In a little more than a decade after the end of WWI, Harold C. Urey rose to scientific stardom. That such accolades as the internationally prestigious Nobel Prize, or even the more provincial Willard Gibbs Award of the American Chemical Society would one day be bestowed upon this farm boy from Indiana was not at all obvious to Urey himself during the 1920s. Although he found himself in many of the most distinguished schools and institutes after the war, he nonetheless struggled to find his place in science. In the early years of his career at Johns Hopkins University and Columbia University, this relatively unaccomplished scientist managed to climb the ranks of academe largely because he was connected to great people and institutes, and because he had an eye for the “big problems” of science.

Being in the right place at the right time, and studying with the right people, is no simple feat. Urey’s career during this period was shaped by several economic and social factors. First is the 20th century westward migration of scientific talent from the private colleges and research universities of the East Coast, the recognized center of US scientific activity. Moving into the periphery, these physical and chemical talents found jobs in the science programs of the relatively young but growing land-grant and state universities. Two chemists from Harvard who made their way west became important forces in Urey’s graduate education. The first of these was Richard Henry Jesse, Jr., who

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245 An account of the growth of these universities up to 1920 and the place of science departments in this growth can be found in Roger L. Geiger, To Advance Knowledge: The Growth of American Research Universities, 1900-1940 (New York: Oxford University Press, 2004), chap. 1 “The Shaping of the American Research University, 1865-1920.”
took up the position of Professor of Chemistry at the University of Montana in 1912. After the war, Jesse sent Urey to study with his Harvard colleague, Gilbert N. Lewis. Lewis, with a Ph.D. from Harvard and postgraduate time spent in Germany, was already developing a reputation as one of the world’s top physical chemists while in Cambridge. Lewis moved to the University of California, Berkeley in 1912 to establish an outpost of physical chemistry that would come to rival those in Germany, let alone Harvard.

In addition to migration, the postwar rise in the prestige of chemical research in the US allowed for the growth of Lewis’s Berkeley College of Chemistry, and also stimulated cross-fertilization between the chemists and the physicists of Berkeley. Thus Urey was able to benefit not only from Lewis’s world-renowned program in physical chemistry and thermodynamics, but also from newly hired physicists such as Raymond T. Birge who were interested in atomic structure. It was this type of cross fertilization, along with the incorporation of physical instruments in the chemical laboratories of American universities that allowed American chemists and physicists to claim a leadership role in the emerging disciplines of quantum chemistry and physics. It was in this field that Urey would develop his own reputation – spending time after Berkeley in Copenhagen and returning as “the man who was just back from studying in [Niels] Bohr’s institute.”

But even with such gains, American physical scientists still felt a great sense of inferiority to their European colleagues. This is clear in Urey’s own account of his time in Copenhagen at Bohr’s Institute. Urey was among the first Americans to visit Copenhagen, and he did so in the ad-hoc days of quantum mechanics, before it had been fully articulated by Erwin Schrödinger and Werner Heisenberg. Still, Bohr’s Institute

showed Urey and his American peers that science in America was looked down upon by their European colleagues, and that their training – particularly in mathematics – was inferior to that of the Europeans. This generation of American physicists set their sights on bringing homegrown American science up to par. The physicist I.I. Rabi, who traveled to Copenhagen and much of the rest of Europe only a few years after Urey, admitted that the European experience gave his generation a target: “It’s true that America was backward in physics – really underdeveloped – but [Edward U.] Condon and I and some others promised ourselves that we would end this. And we did.”247

Urey, Rabi, and their contemporaries took this challenge seriously. Their efforts to improve graduate training and research in chemistry and physics culminated not only in Nobel Prizes for Urey, Rabi, and several of their students and colleagues, but also the successful wartime mobilization of the chemistry and physics communities and the completion of the Manhattan Project.248 For many, including Rabi, the success of the Manhattan Project and other US wartime scientific projects and laboratories was proof of the superiority of the American system. After the war Rabi argued against the commonly held fallacy that the sudden ability of American science to rise to the atomic challenge was due to the arrival of émigré physicists such as Enrico Fermi; he held instead that America’s success was due to the large size, many-tiered structure, and democratic nature of America’s scientific community. The small elite group that dominated German science before and during the war was doomed to failure by its arrogance.

248 The authoritative source on the rise of the American physics community up to WWII is of course Kevles, *The Physicists*. 
This chapter describes Urey’s upward trajectory from the end of WWI to his role in the Manhattan Project, where he was wartime Director of Columbia University’s Substitute Alloy Metals (SAM) laboratory. In tracing his movements in Berkeley, Copenhagen, Johns Hopkins, and Columbia, emphasis is placed upon the local culture of physical chemistry and research and the cultural, intellectual and experimental resources available to the developing scientist. All of these resources would be important to Urey as he cultivated the persona of a true cosmopolitan man of science.

From Wartime Chemistry to California

“The war had turned off a biologist and produced a chemist. I returned to Montana and taught there as an instructor for two years, and then decided that I would go on to get a doctor’s degree. I was now 28 years old, and this meant that if I was going to get a doctor’s degree, it was time that I started. Accordingly, in the fall of 1921, I went to the University of California, arriving there at Berkeley in August of that year.”

Harold C. Urey
Unpublished Autobiography (n.d.)

After the armistice, Urey stayed on as a research chemist in the Barrett Company’s Frankford plant for one year before returning to the University of Montana in 1919 as an instructor. The “decreased chemical activity” of the company after the war necessitated the change. But there were also forces in Montana attempting to pull him back. The departure of Urey’s mentor A.W.L. Bray had opened up a position within the biology department. Morton J. Elrod, the head of the department and also Urey’s former instructor, was well acquainted with the young scientist’s qualities as a teacher and a

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250 Ibid.
researcher.\textsuperscript{251} In a letter to the university’s president, Elrod suggested that Urey should be employed at half time, at a salary of five or six hundred dollars, to run the laboratory for the department’s elementary classes. His recommendation noted that he was “exceptionally good in the laboratory with elementary students.”\textsuperscript{252} Urey’s presence would not fill Bray’s position entirely, but it would at least ease some of the burden of the biologist’s absence. Elrod’s original request was not met, and Urey instead returned to Montana as a chemistry instructor.\textsuperscript{253}

Elrod’s description of Urey at this time provides a glimpse of what his immediate postwar plans were. While it did seem that Urey was interested in “[devoting] his time as a student to Chemistry and Physics,” he nonetheless did not seem to have decided that he would have a career as a physical chemist; rather, he had given Elrod the impression that “He expects to make Biological Chemistry his life work.”\textsuperscript{254}

None of the available source material can shed much light on the question of exactly when and why Urey decided to pursue physical chemistry. Elrod’s description of Urey’s plans seems to indicate that Urey was already thinking about physical chemistry – or at least thinking of the connections between disciplines – during the war. He certainly was already aware of the exciting developments occurring in physical chemistry during his undergraduate training – even if he had no opportunity to pursue an interest in the field while in Montana. By his own account, Urey had limited exposure to physical chemistry in these early years, and primarily through his chemistry professor, Richard

\textsuperscript{251} The finding aid to Elrod’s papers indicates that he possessed what may be the only surviving copy of Urey’s 1915 paper, “Biology of a Slough Near Fort Missoula.” Box 12, Folder 28, MJE.
\textsuperscript{252} Morton John Elrod to F. C. Scheuch, n.d., Box 100, Folder 10, HCU.
\textsuperscript{253} Urey, “Unpublished Autobiography,” 5; this change in employment was also reported in “Personal Notes,” \textit{Journal of Industrial & Engineering Chemistry} 12, no. 1 (1920): 93.
\textsuperscript{254} Elrod to Scheuch, n.d.
Henry Jesse, Jr. (who was also the university’s Dean of Men). By Urey’s description, Jesse was an analytical chemist and the closest thing to a physical chemist that Montana had to offer. Although he practiced “the regular, old-fashioned descriptive chemistry with not too much attention to physical chemistry,” Urey nonetheless credited Jesse with calling his attention to the exciting developments in the field, such as Bohr’s atomic model. However, Jesse was not able to do much more than point the way; Urey “didn’t understand it at all, not a bit of understanding whatever at that stage.”

It is possible that Urey was more influenced by Jesse than he remembered, and it is very likely that Jesse was more aware of physical chemistry than Urey realized. Urey in fact remembered incorrectly in his interview with John Heilbron that Jesse had done his doctoral work at the University of Illinois; his time in Urbana-Champaign between 1909 and 1912 was spent as a member of that university’s faculty, not as a student. Prior to this he had been one of Harvard University’s rising stars. After graduating from the University of Missouri in 1902 (where his father was the University’s president), Jesse went on to receive both his Masters and Doctorate from Harvard in 1907 and 1909. It was in Harvard’s chemical laboratories, under the direction of Gregory P. Baxter and a grant from the Carnegie Institution, that Jesse had determined a new atomic weight for chromium. It was only after completing his chromium work, and after spending a brief time as one of Harvard’s teaching fellows, that Jesse left the East Coast.

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255 Urey, “Interview with Dr. Harold Urey, 1,” 2-3.
256 The Carnegie Institution grant was Baxter’s and was in the amount of $1000 for the purpose of “Research upon atomic weights.” In addition to Jesse’s work on chromium, Baxter’s team also determined new weights for cadmium, manganese, bromine, lead, arsenic, iodine, silver, and phosphorus; Gregory Paul Baxter and Richard Henry Jesse, Jr., “A Revision of the Atomic Weight of Chromium: The Analysis of Silver Dichromate,” in *Researches upon the Atomic Weights of Cadmium, Manganese, Bromine, Lead, Arsenic, Iodine, Silver, Chromium, and Phosphorus* (Washington DC: Carnegie Institution of Washington, 1912).
Jesse’s chromium work certainly was analytical, as Urey remembered it, but he was located only a stone’s throw away from one of America’s new outposts of physical chemistry – Arthur A. Noyes’s Research Laboratory for Physical Chemistry at MIT. Not only was Noyes’s lab in close proximity to Jesse, but Gilbert N. Lewis, who would become Urey’s graduate advisor at Berkeley, was employed as a researcher in Noyes’s laboratory throughout Jesse’s graduate student career, and was acting head of the laboratory during 1907 and 1908. In 1910, Jesse published a paper with Lewis’s Harvard graduate advisor, Theodore W. Richards. While Jesse was not a member of Noyes’s research team, he nonetheless would have been aware of the excitement surrounding the lab.

Two chance events had pushed Urey into chemistry – first the refusal of Montana’s psychology program to accept freshmen students led him to the biology department and to A.W.L. Bray, and then WWI shifted his primary interests from biology to chemistry. Precisely what made him decide after spending two years teaching in Montana to leave for Berkeley in 1921 is unclear. In his interview with Zuckerman, he went so far as to suggest that the move to California was not entirely premeditated, but rather “was sort of a blundering decision, I would say, not well thought out.” That Jesse might have nudged Urey in the direction of Lewis’s California laboratory is one possible explanation. In one version of Urey’s biography, it was Jesse who wrote to

257 Patrick Coffey, Cathedrals of Science: The Personalities and Rivalries that Made Modern Chemistry (New York: Oxford University Press, 2008), 54.
Lewis recommending Urey as a graduate student, helping him to secure a fellowship that would allow him to attend Berkeley.\textsuperscript{260}

A family connection such as the one that brought him to Montana in the first place may also have played a role in Urey’s choice of location. Urey’s family was beginning to move from Montana to the West Coast. In 1921 Samuel and Cora’s youngest child, Harold’s sister Martha, had finished an undergraduate degree in Education from McPherson College, a Brethren college in McPherson, Kansas. Martha next moved to Seattle, Washington, where her husband pursued a graduate degree at the University of Washington, and in 1925 she became the Dean of Women at LaVerne College, a Brethren college in LaVerne, California. Likewise, Harold’s younger brother Clarence had by the mid-1920s also moved west, settling in Eugene, Oregon. Urey’s mother and stepfather seem to have followed a similar path, moving from Montana to Seattle, Washington by way of Weiser, Idaho, and finally to southern California.\textsuperscript{261}

\textsuperscript{260} Thomas, “Harold C. Urey,” 224.
\textsuperscript{261} Although the sources do not give the dates for Urey’s mother’s move, it seems likely that she and her husband followed Martha; Reinoehl and Eckhart, \textit{History of an Eckhar(d)t Family}, 49-50.
Lewis’s Lab

“In many ways, the privately endowed universities of the United States have been the leaders in all the universities in the United States, and for the most part completely outstripped the state universities in excellence. However, at this time and in the years following there was one outstanding exception. That was the University of California at Berkeley, and particularly its department of Chemistry.”

Harold C. Urey
Unpublished Autobiography (n.d.)

“On both the theoretical and experimental side, thermodynamics first came west of Dodge City when G. N. Lewis went to Berkeley in 1912. Before 1912 the men west of Dodge City were busy defending their cows, their riparian rights, and their women; and they slept with their six guns.”

Don M. Yost
“Elements of Thermodynamics” (1967)

Regardless of what brought Urey to Berkeley, in 1921 he found himself a junior participant in what was becoming known as one of the best physical chemistry programs in the world. While Don Yost’s 1967 assessment of the state of science in Berkeley prior to G.N. Lewis’s arrival is a bit too dramatic to be believed, the fact is that great transformations were occurring in the region in the 1910s and 20s. Just as WWI had transformed Urey from a biologist with chemical interests into a working chemist, it had also helped to accelerate a trend that at the turn of the century began transforming the University of California from an obscure intellectual backwater into a burgeoning contender among America’s top research universities – especially in the area of physical chemistry.

During the war, university chemistry programs and the chemists who served within them had provided their services to the state. Public universities, already accustomed to serving local commercial interests, were particularly apt at mobilizing scientists for military service. What set California apart from other public universities, however, was its ability to maintain the heightened level of funding and activity after the war had ended. According to John Heilbron and Robert Seidel, nowhere in the country “were the consequences of the mix [of the interests of science, industry, and government during the war] more enduring and efficacious than in California.”

The scientific and technical nature of WWI prompted the California astronomer George Ellery Hale to initiate the National Research Council (NRC) within the National Academy of Sciences (NAS) with the aims of bringing American scientists to the service of their country and of demonstrating to the government the usefulness of scientific research. Under the direction of the NRC and California’s State Council of Defense – the scientific contributions of which the NRC singled out as being especially helpful to the national effort – both Caltech and the University of California threw themselves wholeheartedly into the war effort.

Of particular use to the war effort were the universities’ chemists. Henry P. Talbot, Professor of Chemistry and Chemical Engineering at MIT asserted in *The Atlantic Monthly* in 1918 that the war was “preeminently ‘a chemists’ war,’” with

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chemists contributing not only to the research and development of gases and explosives, but also to food conservation and dye production at home.\textsuperscript{267} Raymond T. Birge, who arrived in Berkeley’s physics department in 1918, noted the conspicuous absence of his colleagues in the chemistry department: “But this was a ‘chemist’s war’ (just as World War II was a ‘physicist’s war’), and it was the chemistry department that was hit hardest. … Most of the chemistry department was on war leave (five in all) and those who remained on the campus were working on war problems.”\textsuperscript{268}

One such missing chemist was Gilbert N. Lewis. Upon enlisting in 1917, Lewis was commissioned as a Major in the Army’s Chemical Warfare Service and in January of 1918 was sent to France. Here he first took up the position of Director of the CWS Laboratory in Paris. After a visit to the front during the German offensive of 1918, Lewis made a detailed and compelling report to CWS Chief, General Amos A. Fries, and was appointed Chief of the Service’s Defense Division. His major task in this position was establishing the American Expeditionary Force Gas Defense School – a training program for gas officers who would leave qualified to give the “all clear” signal when it was safe for their unit to remove their protective equipment. Once up and running, Lewis’s training program turned out as many as two hundred officers per week. Gas casualties, which had accounted for the greatest number of losses before Lewis’s program was initiated, soon contributed a relatively small percentage. This drop in American gas casualties, along with work on increasing the efficacy of American mustard gas attacks,

\textsuperscript{267} Henry P. Talbot, “Chemistry at the Front,” \textit{The Atlantic Monthly}, August 1918, 265.
\textsuperscript{268} Raymond T. Birge, \textit{History of the Physics Department, University of California, Berkeley: Volume I. The First Half Century, 1868-1918} (Berkeley: University of California, 1968), VI (15).
earned Lewis a promotion to Lieutenant-Colonel and made him Chief of the Training Division of the CWS.\textsuperscript{269}

Talbot concluded that the war was teaching the nation that American science and its methods were deserving of greater respect at home, and that the chemist could act as the ambassador for science once the war was concluded: “the achievements of the chemist in the war should entitle him to increasing respect and to a highly responsible share in national life and in the councils of those who will direct our national policies.”\textsuperscript{270}

As the tide of war turned in June 1918, the \textit{New York Times} ran the headline, “War of Chemicals Reaches a Climax: Advance of Germans, with Deadly Gases, Calls Attention to Progress of Allies Outstripping Enemy Scientists.”\textsuperscript{271} The article sang the praises of the German-educated American chemists who had “caught the spirit of investigation from Germany and put it in practice on this side of the ocean.”\textsuperscript{272} It was these chemists who had brought chemistry to the American university and, from the 1880s to the outset of the war, had “all unconsciously been preparing for just such a situation.”\textsuperscript{273} In September, reporting on the fourth annual National Exposition of Chemical Industries, the \textit{Times} reported that American chemists were not only winning the war, they were also surpassing Germany in the production of quality dyes and other domestic products.\textsuperscript{274}

The chemical companies profited greatly from the chemists’ war. The net earnings for the Barrett Company, where Urey had found work, went from $1,280,476 in

\textsuperscript{270} Talbot, “Chemistry at the Front,” 274.
\textsuperscript{272} Ibid.
\textsuperscript{273} Ibid.
1914 to $4,247,858 in 1916. This paled in comparison to the Atlas Powder Company and the Hercules Powder Company, where profits soared from $294,150 and $1,247,255 to $2,639,790 and $16,688,873, respectively, not to mention DuPont, where profits went from $4,831,793 to $82,013,020 during this same period. In addition to these monetary gains, the chemical industries and the American Chemical Society made sure to capitalize on their increase in popularity. Reporting on the third National Exposition of Chemical Industries, Thomas H. Norton wrote in The Chemical Engineer that “The intelligent and patriotic citizen could not leave the halls of the exposition without the profound conviction that our chemists form a first line of national defense, that without their wholehearted, far-reaching co-operation the power of the legions we send across the Atlantic would be sadly crippled.” The Chairman of the Exposition Advisory Committee, Charles H. Herty, addressed the Exposition’s opening day crowd with language that translated the chemists’ goals – in the war and at home – into a distinctly military vocabulary:

The battle for national self-containedness in that portion of the line held by the American chemist is progressing favorably. … On the left flank a steadily increasing force of the ablest American chemists is being gathered to capture the hill of “obsoletism of army equipment” and is providing the great armies we are now raising with the most efficient forms of modern chemical means for both offensive and defensive warfare. … On the right center the terrain of “Congressional apathy” has been partly won, as typified by favorable protective legislation for our dyestuff industry… On the left center the quagmire plains of “public indifference” have been largely dried and made passable through the clearing skies of a sympathetic daily press, which has constantly emphasized the value to the independence of the nation of a full-rounded chemical industry.

275 “War Profits of Chemical Corporations,” The Chemical Engineer 25, no. 8 (October 1917): 281.
277 Herty’s speech was printed in Ibid., 291.
This sympathetic press, the chemists reminded themselves, was due in no small part to the activities of the Press and Publicity Committee of the ACS, and in general to the activities of the Society on behalf of the chemical community. The ACS had been a mouthpiece through which the American chemists, as a whole, could “spread the gospel of chemistry” throughout the war. 278 As Herty emphasized at the same event one year later, if permanent chemical independence was going to be achieved, it would be due to “close cooperation between the chemist and the American people, which can only be brought about when the chemist takes the people into his full confidence regarding the problems whose successful solution is a matter of joint responsibility.”279 The war had gone a long way in helping the chemists to drum up “sound and loyal public opinion,” and American success in the war would secure a national “atmosphere of good will” toward chemistry.280

Herty was right. While in Germany the loss of the war would lead to a lessening of the prestige of physical science and a breakdown of the internationalism that had previously characterized European physical and chemical research, in the Allied countries the chemists were treated as heroes.281 Lewis and his Berkeley colleagues, who had succeeded in proving that they could “outgas the Germans,” and who had also developed tools and techniques that allowed American troops to survive gas attacks, “enjoyed something of the reputation at the Armistice that atomic physicists did on V-J

279 Herty’s opening address was printed in “Fourth National Exposition of Chemical Engineers,” The Journal of Industrial and Engineering Chemistry 10, no. 10 (October 1918): 826.
280 Ibid., 827.
Lewis returned from the war with a distinguished service record; for his war work his own government awarded him the Distinguished Service Medal and France awarded him the Cross of the Legion of Honor. These distinctions, along with the postwar prestige of the Berkeley chemists, positioned Lewis after the war to become “the dominant force for building up [Berkeley’s] research capacity in physical science.” It also further captured the attention of the NRC, which made its mission in the interwar years the improvement of American universities and international competitiveness in scientific research. But Lewis had already brought dramatic changes to Berkeley’s chemistry department even before the European conflict began.

Lewis had come to Berkeley from MIT in 1912 at the invitation of the university’s then president, Benjamin Ide Wheeler. Wheeler’s vision when he accepted the presidency in 1899 was to transform the relatively poorly funded university into a premier research university that could rival those in which he had trained on the East Coast and in Germany. Before accepting the job, he secured for himself the sole authority to set salaries and appoint new faculty. Once in the presidency, he made research and public service criteria for promotion, in addition to (and in some appointments in place of) the university’s existing teaching requirements. In order to expand the campus, he supplemented the University’s sparse budget with a successful fundraising campaign that targeted California’s many millionaire progressives, chief among them Phoebe Apperson Hearst. By 1914 Wheeler had convinced Hearst to spend

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282 Heilbron and Seidel, Lawrence and His Laboratory, 10.
284 Heilbron and Seidel, Lawrence and His Laboratory, 10.
twice the amount of money on the expansion of Berkeley’s campus as had the state of California since the university’s founding. Money from Hearst paid not only for new academic buildings, but also for scholarships, scientific research equipment, publication subsidies, and new faculty positions, as well as activities and events for faculty and students.\textsuperscript{286}

As part of his effort to fill his growing university with research-minded faculty, Wheeler approached Lewis in 1911. Wheeler noticed that the chemistry department already had formed ties to California’s economy by providing chemical assistance to local industries. However, the College of Chemistry was still one of the university’s lesser departments, attracted few graduate students, and its faculty published little. He marked the department early in his presidency as a possible development project. When a position opened up in the College with the departure of Fredrick G. Cottrell, Berkeley’s only physical chemist, Wheeler decided to appoint a new physical chemist at a senior level.\textsuperscript{287}

By this point, in Noyes’s laboratory, Lewis already had experience administering a large research program and had developed a distinctive managerial style. But it was not easy to tempt Lewis, who had recently been promoted to full professor at MIT, away from a laboratory where he and his colleagues enjoyed world-class research facilities and complete freedom from teaching responsibilities. As Lewis wrote to Wheeler, “I have had the opportunity … of talking with several men who have known the University of California and its chemical department. They all seem to agree that this is at present one

\textsuperscript{286} Ibid., 105-106.
of the weakest of your departments, and that it will need considerable reorganization before a satisfactory graduate department can be developed."³⁸⁸ To win Lewis over, Wheeler offered him complete control of the College of Chemistry. For his own part, Lewis presented Wheeler and the UC regents with a hefty list of demands that included a new laboratory, equipment funds, a salary of five thousand dollars (the highest salary within the physics department was only four thousand dollars), a fifty-percent budget increase to cover hiring new instructors, and a staff consisting of a mechanic, glassblower, bookkeeper, and administrative assistant. Wheeler and the regents capitulated to all of Lewis’s demands.³⁸⁹

Some of Lewis’s colleagues predicted that his move to Berkeley would cut him off from America’s centers of scientific activity, and would thus become a form of intellectual exile. Other scientists who had moved to California before Lewis had found little in the surrounding community to stimulate their work. The biologist Jacques Loeb, whom Wheeler had brought to Berkeley in 1902, had by 1910 become “disillusioned with the dream of building a ‘Woods Hole of the West’” and had left for the Rockefeller Institute.³⁹⁰ But Lewis had a tendency to be reserved and distant to begin with. He could be difficult to get along with and often made little effort to make friends. A certain amount of estrangement from his East Coast colleagues may have appealed to him. As Coffey describes it, “Lewis was uncomfortable in the larger scientific world and built his

³⁸⁸ Quoted in Ibid., 244.
³⁸⁹ Lewis’s letter to Wheeler in which he presents his demands is reproduced in William L. Jolly, From Retorts to Lasers: The Story of Chemistry at Berkeley (Berkeley: College of Chemistry, 1987), chap. 10 “Lewis’s First Years at Berkeley”; The highest salary in the physics department belonged to F. Slate, as noted in Birge, History of the Physics Department, Vol. 1, VI (6).
³⁹⁰ Servos, Physical Chemistry from Ostwald to Pauling, 245.
chemistry department at Berkeley as a support system for himself, staffing it almost exclusively with scientists who had trained there.”

What a support system it was. When Wheeler approached Lewis in 1911, the total payroll for the College was $23,068 – almost four thousand dollars less than that of the physics department. Immediately after his arrival in 1912, the College’s payroll grew by seventy-eight percent to $41,081 while that of the physics department remained unaffected. These new salaries went to the physical chemists whom Lewis had brought with him from the east – William C. Bray, Merle Randall, Richard C. Tolman, and in 1913 Joel C. Hildebrand and G. Ernest Gibson. Lewis saw to it that the faculty he brought with him enjoyed many of the same benefits as he. As Hildebrand would later describe the move,

In 1913 I had left a department in the east that had been managed as a benevolent despotism, and joined the group of young iconoclasts in Berkeley that Lewis had organized just one year earlier. I left a teaching load of eighteen hours a week to assume one never more than eight; I left a department whose “head” supervised all thesis research, and I joined one whose leader sent new graduate students around among the staff free to select the one to supervise his research.

The increase in funds and intellectual manpower, not to mention the freedom from a heavy teaching load, quickly affected the activities of the department. In the 1912-13 academic year, four of the twelve members of the chemistry faculty produced eighteen papers, a significant increase from the five papers published by the faculty during the

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291 Coffey, Cathedrals of Science, xv.
Seven of these new papers were Lewis’s. The department also became more attractive to graduate students during this time, and Lewis would use this to his advantage in sculpting his empire from within while severely limiting outside influence. By the time Urey arrived in Berkeley Lewis had hired seven additional junior faculty members, all of them graduates of his own program who had been advised by himself, Gibson, Tolman, or Hildebrand.

Breathing In the Berkeley Air

“Some properties of the atmosphere of a chemical laboratory are obvious, even obtrusive, others are not. Some are detected by the organs of sense, others only by the aid of good sense. … By 1921, in Berkeley, the pervading odor was generated by Gilbert Lewis, who smoked daily some eighteen five-cent, Philippine cigars. The tinfoil in which they were wrapped was merely a disguise of their real quality. They served to differentiate the participants in the departmental colloquium into two groups, the aerobic and the anaerobic. Lewis claimed that he generated the smoke at great expense, and that it should therefore not be allowed to escape.”

Joel Hildebrand

“The Air that Harold C. Urey Breathed in Berkeley” (1963)

When Joel Hildebrand wrote in 1963 of “The Air Harold C. Urey Breathed in Berkeley,” he was speaking both literally and metaphorically. Just as Lewis’s cigar smoke permeated the College of Chemistry’s laboratories and seminar rooms, so too did Lewis’s philosophy toward chemical research and education. This philosophy was forged through his own experiences as a graduate student at Harvard and in Germany, and as a

295 Birge, History of the Physics Department, Vol. I, VI (5).
296 Calvin, Gilbert Newton Lewis, 3. The one exception to this trend was C. Walter Porter, who was a Berkeley graduate but had been advised by Henry C. Biddle, a faculty member hired before Lewis’s arrival.
member and temporary director of Noyes’s lab at MIT. These experiences led him to take a relatively free approach toward the direction of activities in his College. Rather than attempting to control the institute by fiat, he instead concentrated on building up talent and fostering an environment in which both students and faculty would feel encouraged to pursue new and interesting chemical questions.

Lewis had been a student of Theodore W. Richards at Harvard and had not enjoyed Richards’s close supervision. Richards was of the opinion that “assistants who are not carefully superintended may be worse than none, for one has to discover in their work not only the laws of nature, but also the assistant’s insidious if well meant mistakes.” He was no more likely to give free reign to even his most brilliant students: “The less brilliant ones often fail to understand the force of one’s suggestions, and the more brilliant ones often strike out on blind paths of their own if not carefully watched.”298 By contrast, Lewis avoided directly advising students and took a hands-off approach when directing their research.

Lewis also was hesitant to require much coursework of his graduate students. Rather than forcing students to sit through lecture courses, students were encouraged to find the information they needed in books. Seminars were offered on special topics, and these almost entirely focused on “hot” topics on which an instructor was preparing to publish.299 Rather than making the students recipients of information, Lewis instead made them participants in the cooperative work of the laboratory. Urey remembered, “It was immediately obvious that grades per se were not important things in this department. … I

have never known what my grades were in any course that I took because it was unimportant.”

Graduate students in the department were in fact taught not to think of themselves as students, as the program Lewis had devised recognized few distinctions between his graduate students and his faculty. Rather, as Urey recalled, the size and structure of the program encouraged them to interact as colleagues: “There were only some 30 or 35 graduate students in the Department of Chemistry at Berkeley at that time. We were sufficiently few in number, and the professors were sufficiently few in number that to a large extent all of us knew everybody else – professors and students alike. I argued principles of physical chemistry with my fellow students and with my professors almost daily.”

Hildebrand similarly remembered that in these early years, as the faculty was still happily adjusting to their reduced teaching schedules and increased research budgets, the amount of casual discussion between professors and students was remarkable: “The members of the department became like the Athenians who, according to the Apostle Paul, ‘spent their time in nothing else, but either to tell or to hear some new thing.’ Anyone who thought he had a bright idea rushed to try it out on a colleague. Groups of two or more could be seen every day in offices, before blackboards or even in the corridors, arguing vehemently about these ‘brain storms.’”

Nowhere was Lewis’s egalitarian approach to management more evident than in the College’s Friday evening seminar. As Urey described it, “all of the people in the

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301 Ibid. Birge states that the graduate enrollment in chemistry during this time was in fact sixty-one students: Raymond T. Birge, History of the Physics Department, University of California, Berkeley: Volume II. The Decade 1918-1928 (Berkeley: University of California, 1968), VII (34).
department attended – organic, physical and all – and none of us would have missed this seminar for anything else. Professors sat around a table in the middle of the room, and the students on a raised platform around the outside. Professor Lewis smoked that heavy cigar throughout the whole discussion.” Hildebrand’s account reinforces the importance of these seminars:

The feature of the new department that undoubtedly stands out most prominently in the memories of all graduate students and staff members was the “department meeting.” This was really a colloquium, separate from the meeting for considering departmental affairs. … Lewis would say, “Mr. Blank, will you tell us about your work?” Mr. Blank might be a member of the faculty or a graduate student; his research might be concerned with physical, inorganic, organic, analytical, or applied chemistry. No one could claim, as was often the case in Germany at the turn of the century, “Das ist aber mein Gebiet.” Any hot problem was fair game.

As William Jolly described them, the seminars typically consisted of two talks: the first was a review by a graduate student of a published paper, and the second was a presentation of original research being prepared for publication by a faculty member, advanced graduate student, or a postdoctoral student. These weekly seminars, which constituted the extent of Lewis’s teaching, helped him to keep all of his chemists discussing and arguing the finer points of the latest chemical work amongst themselves, and also allowed him subtly to hone the research ideas of the faculty and students and “thrash out” any inconsistencies.

The freedom and resources Lewis bestowed upon graduate research were incredible. Not only were the graduate students able to choose their research advisors on

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304 Hildebrand, “The Air,” ix. The German translates to, “But this is my territory.”
305 Jolly, From Retorts to Lasers, 62.
306 Calvin, Gilbert Newton Lewis, 7.
their own, and to change them whenever they chose, they were also given “the run of the store rooms and laboratory facilities.”307 This was no small thing. In 1912 a temporary, three-story, wooden building (the Chemistry Annex) was built to house graduate research in physical chemistry. By 1918, money from a state bond issue obtained by Wheeler had built a brand new, state-of-the-art research facility that chemists from all corners of the world would envy – Gilman Hall.308

Upon Gilman Hall’s completion, the Journal of Industrial and Engineering Chemistry ran a seven-page spread, complete with floor plans and photographs, of what was surely one of the world’s best-equipped university chemistry facilities.309 The building was industrial in character, consisting of two main floors and a furnished attic, along with a basement, sub-basement, and sub-sub-basement. The walls, floors, and roof of the building were all built of reinforced concrete. The rooms – labs, offices, and classrooms – were each assigned a specific purpose in the building’s floor plan, but were also built in such a way that significant changes could be made at any time. All of the building’s piping and electrical conduits, for example, were exposed on the ceilings to facilitate any changes that a space might require, and removable wooden panels had been placed in the walls separating the rooms to facilitate further re-appropriations of space.

Aside from being accessible, the piping and wiring systems were also versatile. Each lab was supplied with gas, low-pressure air, suction, oxygen, and distilled water. In addition to this, a researcher could easily outfit his lab with high-pressure air, high vacuum, steam at 30 or 250 lbs, liquid ammonia, crude fuel oil, and electricity of varying

307 Gerald E.K. Branch quoted in the Appendix of Ibid., 23.
voltages in AC or DC current (the generators for which could be controlled from any room). In addition to providing ideal laboratory space, the building also boasted large and well-outfitted shops, including a carpenter, sheet metal, and plumbing shop, an instrument shop, a glass blower’s shop, and a student shop within which students could machine their own tools and instruments.\footnote{Ibid., 634-637.}

Faculty offices were attached to their labs, and as Glenn Seaborg wrote, “To a large extent [Lewis] ran the College from his laboratory.”\footnote{Seaborg quoted in Jolly, From Retorts to Lasers, 60.} Lewis passed most of the administrative duties of running the college to his junior colleagues. Even within his own laboratory, Lewis mostly acted as a research director. William Jolly described the structure of work in Lewis’s lab as a precursor to the postdoc system that largely emerged after WWII: “Lewis was an unusual professor. He never taught any undergraduate courses and did relatively little laboratory work himself. But he had a succession of top-notch assistants (we would now call them post-docs) who helped him carry out the experimental work that he dreamt up. … He also frequently assigned junior faculty members (and who were they to argue?) … to help him with his experimentation.”\footnote{Ibid., 61.}

Lewis’s hands-off approach with graduate students “[allowed] the graduate student the greatest possible latitude … [and helped them acquire] initiative, morale, and a fine spirit of cooperation among themselves and the faculty.”\footnote{Melvin Calvin, “Gilbert Newton Lewis: His Influence on Physical-Organic Chemists at Berkeley,” Journal of Chemical Education 61, no. 1 (January 1984): 18.} However, Lewis’s approach may have worked for some students better than others. Urey, for example, although he thrived in the Berkeley atmosphere, felt as though his dissertation project
would have been more successful had he been more closely advised: “I never quite knew whom I was working with; whether I was working with Lewis or whether I was working with Olsen, and I don’t think Olsen ever knew exactly. … Lewis didn’t watch very much what I was doing, and nor did anyone else.” At Lewis’s suggestion, Urey had taken on a dissertation project that was a thermodynamic study of the conductivity of molecular cesium vapor. Urey’s work was difficult and butted up against the limits of quantum mathematical tools that were still being developed. As his colleagues pointed out in his NAS memoir, “Although the correct quantum mechanical formulation of the rotational partition functions had not yet been established, Urey was able to calculate entropies for diatomic gases … by evaluating an approximate rotational specific heat function and using moments of inertia which he calculated from spectroscopic data.” But while he ultimately was disappointed by his doctoral dissertation, he did feel that the project and his two years as a graduate student in the Berkeley environment gave him his “real start as a research scientist.”

Lewis’s model of undirected research was based upon Noyes’s MIT laboratory. Noyes had made a good sum of money from the licensing of a chemical process he had developed. In 1903 he used these funds in partnership with MIT and the Carnegie Institution to build a laboratory within which he and the faculty of his choosing could have complete freedom to do research in physical chemistry. This essentially became a

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318 Coffey, Cathedrals of Science, 40.
“research colony” within which he, Lewis, and a select few could be shielded from the otherwise utilitarian bent of MIT at the turn of the century. Prior to Berkeley, it was in Noyes’s laboratory that Lewis had felt most comfortable, his reputation as a world leader in physical chemistry growing thanks to his talents in the laboratory and as a writer of elegant prose, while he remained protected from having to interact personally with students or the larger scientific community.

Quantum Chemistry from California to Copenhagen

Urey might not have been proud of his dissertation, but it did provide him with two publications – one on the heat capacity of gases and the other on their ionization. Furthermore, Urey’s interest in these two subjects was piqued and he had become acquainted with the theoretical properties of atoms and molecules. He had also, thanks to his laboratory experience, become quite adept at the technique of molecular spectroscopy. But most important, Urey’s work at Berkeley had exposed him to the newly emerging field of quantum chemistry – a field that American chemists would come to dominate in the 1920s and 1930s. Riding on the heels of chemists like Richards, Noyes, and Lewis, the generation of American chemists who had studied physical chemistry under Ostwald and Nernst in Germany and imported the field to the United

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319 Servos, Physical Chemistry from Ostwald to Pauling, 120.
States, a new generation of chemists was rising that would usurp some of Germany’s authority in pure chemical research.

From Bohr’s 1913 paper on the hydrogen atom into the 1920s, the quantum physicists of Europe seemed intent upon laying out the theoretical principles within which the facts and laws of chemistry could be fit. Their goal was to use their physical theories and mathematical formulae to “create a mathematical and theoretical chemistry.” However, as Mary Jo Nye points out, very few of these physicists had any meaningful understanding of chemistry. German chemists contributed very little to the emergence of the new quantum chemistry. Instead, a roster of young American chemists and physicists made the most significant contributions to this new field – a list that included Robert S. Mulliken, John C. Slater, John Hasbrouck Van Vleck, Linus Pauling, Edward U. Condon, J. Robert Oppenheimer, Ralph Kronig, I. I. Rabi, Clarence Zener, David Dennison, Philip M. Morse, Henry Eyring, John G. Kirkwood, George E. Kimball, E. Bright Wilson, Hubert M. James, Francis O. Rice, and Urey.

Part of the reason why the Americans were successful in this new field where the Germans were not was that chemistry and physics departments in American universities during the 1920s and 30s were developing and strengthening ties to each other. In universities like Berkeley where these relationships were forged, the difference between a young chemist and a young physicist was nominal. Raymond T. Birge, who as a young

323 Ibid., 253.
faculty member in Berkeley’s physics department helped to bridge the gap between his department and Lewis’s chemists, put it succinctly: “…the difference between physics and chemistry [at Berkeley] could be illustrated as follows: When [William F.] Giauque and [Helen L.] Johnston discovered the isotopes of oxygen by means of an analysis of band spectra, that was chemistry because the work was done in Gilman Hall. When [A.S.] King and I discovered the isotope, carbon 13, by means of an analysis of band spectra, that was physics because it was done in LeConte Hall.”

Lewis agreed with this sentiment, writing in 1926 that the recent cooperation between the two departments meant that “Many of the investigations prosecuted in one laboratory might equally appropriately be carried on in the other.” But Lewis was quick to point out that chemistry had adopted physical instrumentation and methods such as spectroscopy not because the “problems of chemistry have become less interesting than they formerly were,” but because “modern science has developed a great number of weapons for attacking them.”

In this respect, Urey was fortunate to have studied at Berkeley when he did. Had he arrived even only a few years earlier, he would have found the departments of physics and chemistry hardly in communication with each other. When Birge arrived in 1918, he found that “relations between the two departments were hardly cordial, and (this is literally true) G.N. Lewis … and E.P. Lewis, new chairman of the physics department, took over the Chair of the Physics Department in 1918 and committed himself to building the department up to the level of the College of Chemistry. After failing to recruit a senior physicist, Lewis decided to focus instead on hiring promising young physicists such as Birge: Seidel, “Physics research in California,” 50; Geiger, To Advance Knowledge, 212.

Quoted in Birge, History of the Physics Department, Vol. II, VII (12).

Quoted in Ibid.
were not even on speaking terms.”

Birge explained the mutual indifference of the two departments with two factors. First was the problem of jealousy: “In view of the fact that for the previous six years the chemistry department had been treated (by the Administration) like a prince and the physics department (relatively speaking) like a pauper, could not help but produce considerable ill-feeling on our part, at least.”

Second was Lewis’s “low opinion” of the physicists, which had kept him from setting foot in the physics department during his first half-decade in Berkeley.

After Birge’s arrival, however, relations between the two departments began to change. Birge, himself a spectroscopist, had an interest in the new atomic theory being developed in Europe and had come to Berkeley “a ‘missionary of the Bohr atom.’” Finding the Berkeley physics department relatively uninterested in atomic structure, Birge turned instead to the physical chemists. G.N. Lewis had proposed his own model of the “static” atom that had been further elaborated by his colleague Irving Langmuir (what became known as the “Lewis-Langmuir” atom). While the chemists proved to be a hostile audience to Bohr’s model, they nonetheless accepted Birge into their weekly department seminars: “I started to attend these weekly meetings, the only physicist, I believe, to do so. There was always free and vigorous discussion and practically every remark I made was promptly contradicted! In spite of such discouragements I stuck it out.”

Birge’s interest in the discussions going on in Gilman Hall earned him a reprimand from his own department chair for paying too much attention to the chemists.

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329 Ibid.
331 Ibid.
332 Ibid.
But despite the reprimand, over the course of Birge’s first two years in Berkeley, he and Lewis forged new lines of communication between their respective departments. In the Spring 1920 semester, when Birge and two of his fellow physicists offered a course on “Radiation and Atomic Structure,” several members of the chemistry department attended. In the following year, which would have been Urey’s first year in Berkeley, Birge’s course became a full-year course with the hefty title, “Radiation and Atomic Structure: A discussion of recent work in the fields of electric discharge through gases, spectroscopy, X-rays, and magneto-optics, bearing upon the general problem of atomic structure.” This course attracted the attention of chemistry staff and graduate students alike, and “Presently every graduate student majoring in physical chemistry was sent to take the course for credit.”\(^3\) The warfare between the Berkeley chemists and physicists had ended.

Birge, whom Urey found to be “a very inspiring professor,” became one of the three greatest influences on his scientific development at Berkeley – the other two being Lewis and Hildebrand. As he told Heilbron, Birge was “the one who interested me a great deal in physics by (running) seven hours of courses and talking to me about the Bohr atom at the time.”\(^4\) Birge’s own correspondence with his mother during this period (which he shared with Urey later in life) shows that the two came to know each other well; their discussions of physics were not limited to the classroom, but also took place on idyllic walks along the canyon trails in Muir Woods.\(^5\)

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\(^3\) Ibid., VII (12).
\(^4\) Urey, “Interview with Dr. Harold Urey, 1,” 1.
\(^5\) The passage from Birge’s letter to his mother is dated June 26, 1922 and is included in a letter from Raymond T. Birge to Harold C. Urey, February 16, 1965, Box 14, Folder 1, HCU.
Birge wrote for Urey in 1924 gives some idea of Birge’s impressions of Urey’s qualities and credentials:

…I was closely affiliated with Dr. Urey’s work while he was here at Berkeley. … Dr. Urey is a very unusual man. He started as a chemist, but has since shown remarkable interest and ability in mathematical physics. I should call him now a mathematical physical chemist. From my own knowledge – not as extensive as it might be – Prof. R.C. Tolman is perhaps the only other man in this country who should be similarly classified. I mention this mainly because Prof. Tolman is perhaps the only one in this country who is really qualified to judge Dr. Urey’s previous research work, and he has in print referred to it in very complimentary terms…

After providing brief descriptions of Urey’s two published papers, Birge went on to describe Urey’s initiative in bringing this work to completion without the benefit of external direction. He also noted that by doing so, Urey had even gone beyond the understanding of his faculty advisors:

To me the most significant point in this connection is the fact that Dr. Urey worked up this material with practically no assistance. I helped him as best I could as to the general facts regarding atomic structure, but I freely confess that a year ago my knowledge of the statistical side of these problems was lamentably poor. … I am just beginning to appreciate the importance of Dr. Urey’s work. I have never had any question as to the brilliance of his intellect, but when he talked over his problems with me, I frankly was not familiar enough with the matter to give him any real help. I trust I will not be misunderstood when I add that there was no one else here at Berkeley who was familiar enough with this field to help him.

Birge finished his assessment of Urey by likening him to two already well-known young pioneers of the quantum movement, John C. Slater and John Hasbrouck Van Vleck, both of whom received their Ph.D.s at Harvard at the same time that Urey was in Berkeley.

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336 Birge enclosed this letter of recommendation in Ibid.
337 Ibid.
Birge predicted that these three young men would help to “bring America’s ranking in mathematical physics up to the European level.”

**Der Kopenhagener Geist**

“The period of my work with Bohr’s Institute was just a year, from August 1923 to August 1924, but it had a strong influence on my life. I started out as a little country boy from Indiana – which is a place with a very colloquial American point of view. Then, suddenly, I went to another part of the world, to a country I had hardly even noticed on the map. And what I found amazed me. I was impressed by the very bright and charming Danish people.”

Harold C. Urey
Quoted in “Harold C. Urey” (1963)

The young men who traveled to Copenhagen in the 1920s would turn the tables on their European colleagues. Whereas the European quantum physicists would apply quantum mechanics to chemistry, while the German chemists remained unimpressed, the Americans who spent time in Copenhagen as students would return to the United States and develop a true chemical quantum mechanics – a quantum chemistry. They would help to move American physical science from the periphery to the center of the world scientific community. They would be of the last generation who felt obliged to travel to Europe for study. When émigré scientists began leaving Europe during the 1930s, they would play host to them in the departments they had built, proud to at last be able to introduce the international component to the salon model they had brought back with them from Copenhagen, Berlin and Göttingen. But when these young Americans first

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338 Ibid.
340 Nye, *From Chemical Philosophy to Theoretical Chemistry*, 228.
arrived in Copenhagen, they had little to recommend them as revolutionary scientific minds.

Most of the early group that traveled to Copenhagen in the 1920s found that they were ill prepared for the intellectual work of Bohr’s Institute. Urey, for example, discovered that Birge had overestimated his grasp of mathematical physics. He wrote to Birge in the 1960s that “one of the things I learned in Copenhagen as a result of my association with Kramers and Slater and Pauling and Heisenberg was that I could not do theoretical work and keep up with these men.”

He told Heilbron, “I did not have the mathematical equipment and the mathematical ability to be an effective theoretical person.” And in his unpublished autobiography Urey wrote that “I tried to do some theoretical physics at Bohr’s Institute… Perhaps the most important thing that I learned was that these friends of mine at the Institute … would be able to do a much more thorough job in theoretical physics than I could possibly do, and that my strength in science would lie elsewhere.” Indeed, his contributions to quantum chemistry would be in the experimental realm.

Although Urey was butting up against his own mathematical and theoretical limitations, he nonetheless enjoyed his time in Copenhagen and felt personally transformed by his experience there. Urey basked in “Der Kopenhagener Geist,” the “spirit” embodied in Bohr’s Institute and its coterie. Bohr designed the Institute to be “a place free from nationalistic emotion, which would revive and reaffirm scientific internationalism,” an “international gathering place for physicists in the postwar

341 Harold C. Urey to Raymond T. Birge, February 25, 1965, Box 14, Folder 1, HCU.
342 Urey, “Interview with Dr. Harold Urey, 1,” 7.
period,“ and “an environment of vigorous intellectual engagement and affectionate esprit de corps.” Within its walls he had gathered a “modernist enclave.”

In addition to their own poor mathematical training, another problem that faced the young Americans in Copenhagen was the relatively low status of American science in Europe. Rabi recalled that American physics was held in contempt during the late-1920s when he was in Europe. In Hamburg in 1927, Rabi sought the latest issue of the American journal *The Physical Review* in the library only to find that it was not held in high esteem. The journal was only ordered and shelved once all of the year’s issues had been published. Rabi concluded that Europeans did not feel it was important to have constant contact with the latest American discoveries in physics, since most of the major discoveries were still being made in Europe. Having been trained amidst the triumphalist science boosting of the post-WWI period in America, this must have come as a shock to the young Americans.

Like the other Americans who came to the Institute during these early years, Urey was only a marginal participant in the international collaboration that went on there. But he participated as best he could. Unlike some of his fellow Americans from East Coast universities, he likely felt comfortable right away with the informality of the Institute, its lack of classrooms, papers, textbooks and laboratory exercises, and its

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345 Davies, “American Physicists Abroad,” 16.

346 Davies characterizes Bohr’s institute as a “wholly ‘modernist’ enclave”; Ibid., 14.


348 A description of these early years of the Institute and the burgeoning international community that collected there is found in Peter Robertson, *The Early Years: The Niels Bohr Institute, 1921-1930* (Copenhagen: Akademisk Forlag, 1979), chap. 2 “A Fruitful Mysticism (1921-22).”
insistence that physics was a “series of conversations.” He later jokingly told colleagues that he learned physics in the cafés of Copenhagen while dining with Bohr’s assistant, the Dutch theorist Hendrik A. Kramers. Kramers was his primary contact at the Institute. Kramers – described by Shannon Davies as “the prototype of the cultured pipe-puffing European physicist” – took an interest in Urey, despite his mathematical shortcomings, and the two met for lunch on an almost daily basis. Under Kramers’s instruction, Urey acquired some advanced mathematics and attempted to do theoretical work on an orbital problem. He was even able to write and publish one theoretical article with Kramers’s editorial help.

In addition to Kramers, Urey also met several of Bohr’s closest collaborators. He forged a life-long friendship with the Hungarian physical chemist, George de Hevesy, who was in Copenhagen working on the properties of hafnium. He met the German theoretical physicists, Werner Heisenberg and Wolfgang Pauli, when they visited the Institute. And after his time at the Institute was up, he traveled around Europe introducing himself along the way to the German physicists Albert Einstein and James Franck, and the Swedish chemists Svante Arrhenius and Theodor Svedberg.

Outside of the Institute, Urey also became incredibly fond of the city of Copenhagen, which seemed to represent something other than the “old Europe” where Lewis and Langmuir had studied, and yet also different from the capitalist American

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351 Davies, “American Physicists Abroad,” 32.
cities he had come to know: “Copenhagen, during the 20s, was a city of bicycles, and I bought myself a bicycle which I used to travel … [to] the Institute daily, but in addition to that used it on the weekends to look over the whole city of Copenhagen. I marveled at the wonders of this city. There were poor sections of the city, but there were no ghettos.” His hosts during his stay were the Danish historian Aage Friis and his family. Friis was at that time engaged in trying to rebuild the international character of the European historical community, which in the early 20s was still suffering from the effects of WWI. The Friis family accepted Urey “as though I were one of the family,” and through them he grew quite fond of Danish culture and European socialism.

Urey, who practically adopted a Danish view of the world, had a very different experience from Rabi, who would later report that the result of his encounter with Europe and its anti-American chauvinism was to “feel the greatness of America”: “The Germans’ misunderstanding of the United States was so great that I was known as a chauvinist because I would argue with them all the time. There were plenty of things in the United States I didn’t like – plenty of things. But not what they talked about. They didn’t like the things about us that were good. After all, we had an honest-to-goodness democratic system – you could live in it.” Urey’s interactions with Friis, Bohr, Kramers and Hevesy would help him to redefine his own views of life and “civilization.” As far as he could determine, aside from the absence of slums, “the primary difference in the

354 Ibid., 8.
355 For an account of Friis international activities during the early 20s see Karl Dietrich Erdmann, Toward a Global Community of Historians: The International Historical Congresses and the International Committee of Historical Sciences, 1898-2000 (New York: Berghahn Books, 2005), chap. 7 “Overcoming Nationalism in the Study of History: Brussels, 1923.”
economic standard between Danish families and American families was in our ownership of automobiles.”

He wrote to Kramers from Baltimore that he missed the lifestyle he had known in Copenhagen, and shared his disappointment that he could no longer travel by bicycle but would have “to follow the crowd and buy an automobile.”

He lamented that “People enjoy themselves here in such different ways.” Later in life Urey complained that Americans “leave our towns so ugly and dirty. I feel terribly apologetic about it. For instance, why don’t we put all power lines underground? We have not improved those dirty old poles in my lifetime. I feel very angry about the appearance of them. We’d have a far more attractive country if we used some of the ingenuity that the Danes Employ. Even with quite limited resources, they have made their country beautiful and stimulating.”

Irwin Jacobs, founder of the telecommunications firm Qualcomm, recalled that during a visit to the Urey home in idyllic La Jolla, California during the mid-1960s the chemist cornered him, having heard that he was from the electrical engineering department, and pulled him over to a window overlooking La Jolla Cove: “[he] showed me all the electrical wires criss-crossing the view. ‘How can I get rid of all these wires?’ he asked me.”

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359 Quoted in Davies, “American Physicists Abroad,” 59.
360 Quoted in Ibid.
From European novice to American Expert – Johns Hopkins

“I had quite a bit of trouble getting started. You see, I thought I was going to be a theoretical chemist when I went to Copenhagen, and I just concluded I couldn’t do this thing. So I had to get back and start a completely new line of work. And it took quite a while before I got started at it.”

Harold C. Urey
In an interview with John Heilbron (1964)

Urey later claimed that he had given up on theoretical work while at Copenhagen.

In fact, he continued attempting to turn himself into a theoretical chemist even after he had returned to the United States and taken up a position at Johns Hopkins University.

Before leaving Copenhagen, Urey wrote to Edwin C. Kemble (Slater, Mulliken and Van Vleck’s adviser at Harvard) to tell him that he was applying for a fellowship from the NRC and wanted to spend a year at Harvard working under Kemble’s direction on the theoretical problem he had begun with Kramers. Although Urey was offered the NRC fellowship and did show up in Cambridge at the end of his European tour, he spent only two weeks there before leaving to accept a position at Johns Hopkins University.

By mail, up through February 1926, Urey continued to pursue Kemble’s advice on theoretical matters and sent him copies of papers he wanted to revise for publication. At the same time, he also sent these papers to Slater, Mulliken and Bohr. The process must have been demoralizing, as Kemble and Mulliken’s responses primarily contained corrections of errors in Urey’s math and Bohr seems to have put off responding for quite some time. On his end, Urey struggled to keep up with Kemble’s theorizing, and also

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364 Harold C. Urey to Edwin C. Kemble, June 2, 1924, Box 49, Folder 4, HCU.
deferred quite often to Kemble’s authority. Urey struggled to answer the questions Kemble asked him, only to be told in a following letter that it had been a “simple” matter. In one letter, nearly every sentence Urey writes seems apologetic. He seems to have felt as though his slow learning curve was trying Kemble’s patience and that every correction the physicist made only opened the door for more difficulty: “I believe that I see your line of argument at last and alas think that there is an ambiguity in the whole thing yet. I am rather anxious about this for if you are right I am in for a hullova [sic] time. … Do I have this right now? Then it seems to me that everything you say follows.” Finally, in February 1926 Urey sent one last letter to Kemble at the end of which he wrote: “Have been working on some experimental work for the last few weeks and find it very interesting after working on theoretical things for so long.”

Urey had accepted a position as a Research Associate in the chemistry department of Johns Hopkins University with an annual salary of $2400. Here he was dismayed to find that the chemistry graduate students were largely ignorant of atomic physics and of mechanics in general. In a 1929 roundtable discussion on “The Teaching of Atomic Structure to Physical Chemists,” Urey reported that there was “no doubt that chemists entering our graduate schools do not know any mechanics worth mentioning and it is completely useless to talk about teaching wave or matrix mechanics to people who do not know any classical mechanics, no matter how desirable that would be.” Likewise, their math skills were well below par: “It is certainly true that graduate students of chemistry have forgotten most of the mathematics that they ever knew and, what is more

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365 Harold C. Urey to Edwin C. Kemble, June 18, 1925, Box 49, Folder 4, HCU.
366 Ibid.
discouraging, they have in many cases acquired a fear of the subject.”

This bothered Urey, who wanted “to see physical chemists have some of the fun in the revolutionary [sic] developments” in quantum mechanics.

As Urey saw it, there were only two possible roads for chemistry as a profession: either the chemists’ training should be revamped to include greater amounts of theory and mathematics – the road of the “pure scientist” – or chemists should “admit that we and our science will take the same position relative to physicists and physics that engineers and their subjects now hold relative to the latter.” At Hopkins, Urey chose to create pure scientists. Here he instituted a required course on atomic physics within the chemistry department. This course had “indifferent success” in its first incarnation, but Urey was able to increase its efficacy by inserting six weeks of mechanics into the beginning of the course before ever even addressing atoms or molecules.

Borrowing from his experiences with Lewis and Kramers, Urey also advocated the seminar approach to graduate instruction. As he explained in the roundtable: “I have found in giving a lecture course that the lecturer works very hard, but the students do nothing at all. The lazy method of sitting back and letting the students give reports on papers and subjects is the quickest way of getting a group of students working on the subject and they learn far more.” Just as at Berkeley, the seminars were focused on recent research and hot topics. During the academic year 1927-28, Urey’s seminar addressed the recent work of

370 Ibid.
371 Ibid.
372 Ibid.
the German physicist James Franck on the effect of light in dissociating molecules. In the following year, the topic was the study of band spectra.\(^{373}\)

Urey also attempted to build inroads with the Hopkins physics department. Although intended primarily for the benefit of his chemistry students, and as such emphasizing the “experimental side” of research on atomic structure, Urey’s courses nonetheless attracted a number of physics graduate students. In addition to the physics students, Urey also made overtures to the physics faculty. He attended the physics seminar on a regular basis and there befriended a few of Hopkins’s physicists – including Robert W. Wood, Karl Herzfeld, and Frank Price. On the whole, however, Urey found the physics department to be “pretty old-fashioned” and under the “imperial” grip of Joseph Ames, who was at that time also a university provost and the head of the Physical Laboratory. Urey’s impression was that Ames and his physicists were not interested in the new quantum mechanics, nor did they seek to understand it: “No one could say anything that Ames didn’t like, and Ames was very much of a classical physicist. And Wood, of course, knew nothing about modern Physics. And A.H. Pfund was the other man. Again, he didn’t know anything about modern physics. It was pretty much of an old fuddy-duddy department in a certain way.”\(^{374}\) Elaborating on the deficiencies of Wood, whom he considered to be one of the department’s most talented physicists, Urey told Heilbron that Wood simply “never understood quantum physics at all”: “His

\(^{373}\) Ibid., 284-285.  
\(^{374}\) Urey, “Interview with Dr. Harold Urey, 1,” 18. Ironically, Ames would later go on to play a role in the founding of NASA.
experimental ability and a correct instinct for what was interesting [were his great strengths] . . . But Wood never understood it in modern terms.\textsuperscript{375}

Urey may not have been impressed with these older members of the physics faculty, but the young German-born theoretical physicist Maria Goeppert-Mayer and her husband, the physical chemist Joseph Edward Mayer, who arrived at Hopkins at the end of Urey’s time there, greatly impressed him. The Mayers would eventually join Urey at Columbia and follow him to the University of Chicago after WWII. All three scientists would end their careers at the University of California, San Diego. In addition to these life-long colleagues, Urey’s fellow research associates at Hopkins were Francis O. Rice and F. Russel Bichowsky.

Urey and Bichowsky traveled to Washington DC once a week to attend a seminar at the Bureau of Standards. This seminar became more important to Urey than the “fuddy-duddy” physics seminars in Baltimore. Here he met the geophysicist Merle Tuve, the x-ray crystallographer Ralph W.G. Wyckoff, and the physicists Arthur E. Ruark, Ferdinand Brickwedde, Otto Laporte, William Meggers, Samuel Allison, Paul Foote and Fred Mohler. Many of these men – the physicists especially – would become part of Urey’s core group of colleagues and collaborators over the next few decades. At these seminars, the participants chose their own presentation topics. One of the hot topics that Urey and Laporte gravitated to was the work coming out of Copenhagen. Here Urey found that one of his great talents was in explaining the new publications in quantum mechanics. While the mathematics of Schrödinger and Heisenberg’s new wave

\textsuperscript{375} Harold C. Urey, “Interview of Dr. Harold Urey by John L. Heilbron”, March 24, 1964, 15-16, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA.
mechanics admittedly went beyond his own abilities, he nonetheless gained a reputation within this circle as someone who could decipher what these developments meant to practicing physicists and chemists.

Urey’s explanations of quantum mechanics would not remain limited to the classroom or the intimate seminar format. To reach a wider audience, Urey teamed up Ruark, who was at that time a member of the Atomic Structure Section of the Bureau. The product of Ruark and Urey’s collaboration from 1926 to 1929 was the book, *Atoms, Molecules and Quanta*.\(^{376}\) While this book contained no original work, it was one of the first significant attempts to explain the advances in quantum mechanics to English-speaking scientists who were not specialists in the field. The work of the book was divided between the two men according to their respective talents: “The work was divided between us, with Ruark taking somewhat the more mathematical side of the problem and I the more descriptive sides.”\(^{377}\) As in the courses he designed at Hopkins, Urey’s book began with the experiments in classical mechanics that led to quantum mechanics; only the last two hundred pages (the book was nearly 800 pages long) addressed the most recent contributions of Bohr, Schrödinger, and Heisenberg.

Reviewers of the book noted that it went “a long way toward filling this very real need [for a summary of the new atomic physics],” that it was written in a style that was “comprehensible to the reader who has not been trained in advanced physics,” and that it presented both the experimental and theoretical aspects of the subject in such a way that

both chemists and physicists would find the book useful.\textsuperscript{378} In the same year that the Ruark and Urey volume was published, Urey was made the founding editor of the \textit{Journal of Physical Chemistry}.\textsuperscript{379}

Accounting for Urey’s quick professional rise within American academia, Kevles claims that Urey’s time in Copenhagen had “completed his transformation from an uncertain neophyte into a bantam cock of a physical chemist.”\textsuperscript{380} Urey’s experience, however, more attests to the importance of the reputation he acquired simply from his association with Bohr’s Institute and its circle of European physicists. And in this he was not alone. Rabi experienced something similar upon his return from Europe. When he started as a lecturer at Columbia in 1929, Rabi had accomplished little but “was the life of the place”: “Students were flocking around, and I was in correspondence with and close to other physicists who were well known, and so on. I was in the mainstream.”\textsuperscript{381} Rabi credited his “mainstream” status for the fact that “After the first year, even though I didn’t publish anything, I was given a promotion to assistant professor… After the second year, I was given a raise. The third year, I still hadn’t published much of anything, and they wanted to make me an associate professor.”\textsuperscript{382} Urey felt similarly when, after accomplishing very little at Hopkins, he was offered a job at Columbia University.

Although he had yet to make a lasting original contribution to quantum chemistry by the time he left Hopkins, Urey had nonetheless become one of the young leaders in the field. His math skills might have kept him from joining the ranks of Heisenberg or

\begin{itemize}
\item[378] Hugh M. Smallwood, “Atoms, Molecules and Quanta [Book Review],” \textit{Journal of the American Chemical Society} 52, no. 6 (June 1930): 2588.
\item[379] Nye, \textit{From Chemical Philosophy to Theoretical Chemistry}, 252.
\item[380] Kevles, \textit{The Physicists}, 225.
\item[381] Bernstein, “Profiles (I.I. Rabi - I),” 94.
\item[382] Ibid.
\end{itemize}
Schrödinger, but his few publications in the statistical thermodynamics of gases had marked him as one of the promising young American chemists who could apply the theoretical work of these theorists to traditionally chemical topics. Moreover, his ability to explain quantum mechanics to the uninitiated had moved Urey from the periphery of the Copenhagen circle to the center of the American physical science community (even if he did still defer to some of his more mathematically-minded colleagues at home). However, while his status may have risen high enough to be entrusted with the editorship of the new journal, he was not yet the scientist who would be regarded as the logical choice to head up the wartime effort to separate uranium’s fissionable isotope.

Urey’s own assessment of his time at Hopkins was dim. Much of the work Urey did while in Baltimore he would later refuse to cite, telling Ruark, for example, that he would not reference papers that were incorrect, even if they were his own. However, Urey did manage to publish a handful of research papers during his Hopkins years dealing with the applications of molecular spectroscopy to chemistry, including one on the structure of the hydrogen molecule ion. He also took the opportunity to collaborate with his two fellow research associates, Bichowsky and Rice. With Bichowsky Urey continued working on the Zeeman effect (the splitting of a spectral line in the presence of a magnetic field), a topic brought to his attention by Kramers in Copenhagen, and published one paper on the possible magnetic qualities of a spinning electron. However, Urey came to regret publishing the results of this collaboration.

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385 Urey’s original work on the Zeeman effect was published as Urey, “On the Effect of Perturbing Electric Fields.” His work with Bichowsky resulted in Harold C. Urey and F. Russel Bichowsky, “A Possible
Urey and Bichowsky’s paper followed on the heels of an earlier contribution by two graduate students from the University of Leiden, George E. Uhlenbeck and Samuel A. Goudsmit, in which they first proposed the concept of electron spin. Urey and Bichowsky claimed in their own contribution that the idea had occurred to them “quite independently and for largely the same reasons,” but that they had “carried the idea somewhat further” than Uhlenbeck and Goudsmit. Urey later came to question whether or not he and Bichowsky really had gone beyond their peers in Leiden, and felt sorry that their competing claim on the concept might have prevented Uhlenbeck and Goudsmit from winning the Nobel Prize. In his interview with Heilbron, Urey admitted that his decision to publish had been based on his own ambitions:

I’ve always been a little bit sorry we published it, because I think it prevented a Nobel Prize for the spinning electron to Goudsmit and Uhlenbeck. I always feel sorry about it, and I wrote to the Nobel Committee telling them so. Because I don’t think we added much beyond what Goudsmit and Uhlenbeck did, maybe we only added confusion. It was a completely original idea with us, this I always insist on, but I’m sorry that we just didn’t shut up. It was a matter of young people, you know, as I often say, trying to get ahead, and so forth. If I had been a little older and a little more mature I wouldn’t have done it.

With Rice, Urey began producing his first graduate students. Together they co-advised two doctoral dissertations on the mechanism of homogeneous gas reactions, one concerning blackbody radiation and its effects on a molecular beam of nitrogen pentoxide
and the other the absorption spectrum of this same gas. On his own, Urey advised two doctoral dissertations that dealt with the properties of atomic and molecular hydrogen.

Urey’s fear that Hopkins would continue to be an inhospitable place to do interesting research seemed to be confirmed when Ames assumed the presidency in 1929. By Urey’s account, this began a thirty-year decline in the university’s excellence, as he and several of his colleagues left to find universities where their talents might get them promoted. Urey had no qualms about telling Heilbron that “This man destroyed the university.” When Columbia University offered him the opportunity to leave Hopkins to take up the post of Associate Professor of chemistry, Urey leapt at the chance.

**Columbia and the Discovery of Heavy Hydrogen**

Despite the improvements Urey made in the teaching of theoretical and mathematical chemistry, Hopkins was not Berkeley. Urey found himself missing the robust experimental facilities of Gilman Hall, and later admitted that he felt “foolish” not to have gone back to California where he could have picked up his dissertation research once again and brought it in-line with the new quantum mechanics. While much of Urey’s frustration at Hopkins had to do with being in a department that was not interested in thermodynamics and heat capacities, it also had to do with the university’s lack of

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facilities within which he could perform the type of low-temperature experiments that this work would require. This was at least part of the reason that he continued working on theoretical problems for so long after he had already recognized his limitations.\textsuperscript{392}

Once Urey had moved to New York, the resources available to him as an experimental physical chemist were greatly increased:

In a way, the transfer to Columbia University seemed to me a more likely place for extensive development than Johns Hopkins. It was fortunate in a way, because at Columbia there was a spectra apparatus which was not being used by anybody, and which I could use for studying the spectrum of hydrogen and discovering heavy hydrogen. If I had remained at Hopkins, no such apparatus would have been available to me, since Professor Wood [in the physics department] was using this apparatus continuously.\textsuperscript{393}

At Columbia Urey also now had his own research assistant, George Murphy. In the basement of Columbia’s Pupin Hall, Urey and Murphy built the “spectra apparatus” Urey mentioned in the above quotation into a twenty-one foot grating spectrograph of their own design.\textsuperscript{394} While they would eventually use this apparatus to discover heavy hydrogen, they initially used it to study the relative abundances of other recently discovered isotopes, such as those of nitrogen and oxygen. Their driving question at this time was whether or not these abundance ratios differed in samples of different chemical origin.\textsuperscript{395}

Urey’s scientific output increased greatly while at Columbia. Whereas he was publishing only one or two papers per year while at Hopkins, Urey published seven

\textsuperscript{392} Ibid., 14.
\textsuperscript{394} “Harold Urey - Biographical Memoirs (Period 1923-1939),” 5.
papers in 1931 alone. In part this increase in productivity was related to the number of collaborative projects and dissertations in which Urey was involved. In addition to his work with Murphy, Urey had many other irons in the fire. He was studying the absorption spectra of chlorine dioxide with a doctoral student, Helen Johnston, and finding that isotope effects were helpful in the analysis of the observed spectral bands. With Ray Crist he was studying the chlorine isotopes and attempting to separate them using a photochemical method. With Charles Bradley he was studying the recently discovered Raman effect, and using this phenomenon to determine the normal vibrational frequencies of the polyatomic molecule silico-chloroform. As many of these publications were released in or around 1931 – the same year that the Urey, Murphy and Brickwedde announced their discovery of deuterium – we might consider this Urey’s annus mirabilis. The work done during this period, along with his earlier work on gases at Berkeley, certainly did position Urey as one of the word’s leading experts in statistical and spectroscopic chemistry.

But his work was also moving him in the direction of isotopes, and it was in this subfield of physical chemistry that Urey would achieve his lasting fame. He was nudged in this direction by one of his former colleagues at Berkeley, William Giaque. Giaque had earned his Ph.D. under Gibson in 1922, and Lewis had quickly offered him a faculty position within his department. Like Urey, Giaque had also developed a relationship with Birge and abandoned any strict separation of chemistry and physics. When Urey left Berkeley in 1923, Giaque took over his work on the entropy of gases and, using

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Berkeley’s low-temperature facilities, did the type of work on heat capacities that Urey complained he could not do at Hopkins. In 1929, just as Urey made his move to Columbia, Giaque and Johnston published a series of papers based on their work on the heat capacity of liquid oxygen from 12°K (-261°C) to its boiling point. Two of these papers reported isotopes of oxygen of masses 17 and 18. These discoveries held implications for possible isotopes of hydrogen.

Work on isotopes was relatively new. In 1913, the same year that Bohr introduced the world to his atomic model, the radiochemist Frederick Soddy proposed and J.J. Thompson experimentally confirmed the existence of different types or species of atom occupying the same place on the periodic table and differing only in mass. By 1919 the British physicist Francis W. Aston had constructed a mass spectrograph at his Cavendish laboratory that used magnetic and electric fields in order to separate and measure isotopes by their atomic weights. Aston had previously measured the atomic weight of hydrogen with his instrument and found a value that strongly agreed with the value determined by chemical means. Aston’s method, however, had assumed a standard atomic weight for oxygen of 16. With Giauque and Johnston’s announcement of the two new isotopes of oxygen, “it was necessary that there be a heavy isotope of hydrogen.”

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As isotopes were understood at the time, not many physicists or chemists believed that a heavy isotope of hydrogen could be found. Physicists considered the heavy isotope of hydrogen unlikely to exist. The concept of the neutron had yet to be introduced, and so the nucleus of the atom was at this time considered to be composed of protons and nuclear electrons. Isotopes were understood to differ in mass because they possessed differing numbers of these two nuclear components. An additional electron in the nucleus of hydrogen – understood in normal hydrogen to consist of one single proton – would have effectively negated the hydrogen nucleus’s charge. Even after Giauque and Johnston’s work led to the reconsideration of Aston’s measurements, the heavy isotope’s discovery seemed like an incredible challenge. Calculations by Birge and the astronomer Donald Menzel at Berkeley indicated that if the heavier isotope did exist it was exceedingly rare – composing only about one part in 4500 in naturally occurring hydrogen. But Urey had already suspected the existence of deuterium, based on a chart he had constructed in his office depicting the possible arrangements of protons and electrons in nuclear structure. The chart had as its abscissa the number of electrons and as ordinate the number of protons. Looking at his chart Urey noticed that the known nuclei of light elements conformed to straight line segments within which would fit a heavy isotope of hydrogen.

Urey and Murphy were confident that if the isotope existed, and if they could enrich a sample of hydrogen with its heavy isotope, then they could use their spectrograph to detect it. Urey devised a method of enrichment that exploited the thermodynamic qualities of hydrogen and the theoretical differences in vapor pressure of
its isotopes at their triple point. Urey hypothesized that distilling five-liter quantities of liquid hydrogen down to a residue of only two cubic centimeters of liquid at the correct temperature and pressure would produce a several-fold increase in the concentration of the heavy isotope. In the early 1930s, however, there were only two laboratories in the United States that could reliably achieve the 20.28 K (-252.87°C) temperature required to produce liquid hydrogen in large quantities, let alone distill the liquid at its triple point of 13.84 K (-259.16°C). One such lab was at Berkeley, where liquid hydrogen and air plants had been installed in the Gilman Hall basement and subbasement. These were the facilities used by Giauque. The other was at the National Bureau of Standards. Urey called upon Brickwedde, his old seminar colleague and Chief of the Bureau’s Low Temperature Laboratory. Brickwedde had a longstanding interest in atomic structure and had even written his Masters thesis at Hopkins on Bohr’s atomic model. When Urey approached him and outlined his experimental design, Brickwedde agreed enthusiastically to the collaboration.

Brickwedde’s equipment was in need of repair and reassembly, and so it would be several months before the enriched hydrogen samples could be produced and analyzed. In the meantime, Urey and Murphy went ahead with their experiment using a commercially prepared tank of hydrogen gas. They were surprised to find that they could detect the predicted spectral line of the heavy isotope even from this un-enriched sample. While the spectral line of normal hydrogen appeared on the plate after an exposure time of only one second, a fainter line in the predicted location of H-2 appeared after an exposure time of

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402 Urey’s method required that the hydrogen be distilled at its triple point – the temperature and pressure at which the solid, liquid and gas phases of the hydrogen can coexist in thermodynamic equilibrium.

403 Ferdinand G. Brickwedde, “Atomic Models as Proposed by Bohr” (MA, Baltimore: Johns Hopkins University, 1924).
one hour. Urey was excited by this positive result, but he decided not to report his findings immediately. He and Murphy bided their time, eliminating possible sources of error in their apparatus and methods. When they finally did receive the enriched samples from Brickwedde, they found the H-2 line with an exposure time of only ten minutes. They therefore concluded that the H-2 lines they had seen in their earlier runs truly were H-2 lines.

The discovery of heavy hydrogen – which Urey named deuterium – brought on a flurry of research around the country. Urey’s colleagues estimated that between 1931 and 1934 more than two hundred papers concerning deuterium appeared in print. Lewis, who managed to be the first to isolate a highly concentrated sample of heavy water, was one of the most significant contributors to this literature, writing more than twenty-five papers on deuterium and heavy water during these years. According to Lewis’s student and research assistant, Jacob Bigeleisen, “Lewis jumped on the bandwagon.” He was able to work so rapidly on the problem of heavy water because he already had a large store of enriched water from the electrolytic cells Giauque had used when generating hydrogen gas for liquefaction in his earlier experiments. Urey began to resent Lewis’s sudden activity in deuterium studies, and felt that his former teacher was unfairly attempting to outpace him using the incredible advantages of his superior chemical facilities and reputation:

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404 Urey later learned that this commercially prepared hydrogen had in fact been enriched. The hydrogen had been prepared through the electrolysis of water, which favors molecular H2. In 1933 Lewis prepared the first highly concentrated samples of heavy water using electrolysis: Gilbert N. Lewis and Ronald T. MacDonald, “Concentration of H2 Isotope,” *Journal of Chemical Physics* 1 (June 1933): 341-344.


406 Bigeleisen quoted in Coffey, *Cathedrals of Science*, 220.
Edward W. Washburn was, I thought, somewhat slow in developing the electrolytic separation of the hydrogen isotopes, and it seems that Professor Lewis … also felt that Washburn and I were rather slow about this, and so he undertook to prepare heavy water pure, and of course was the first one to prepare pure heavy water. Washburn and I were working on it with considerably less facilities at our disposal than the chairman of an enormously important chemical department in the United States.  

As Urey wrote to Birge in the 1960s, “It would have seemed to me that in a similar situation one might have thought [Lewis] would have invited me to come to California and would have helped me to develop the work and taken pride in a former student instead of somehow trying to take credit for himself.”

Lewis’s interest in heavy hydrogen and heavy water came to an abrupt end in 1934 when Urey alone won the Nobel Prize for his discovery of deuterium. Lewis, it seems, had worked under the impression that he and Urey might share the prize (in fact rumors that the prize would be shared were prevalent in the days leading up to the announcement). In addition to stopping all work on deuterium, Lewis published nothing at all for the next eighteen months and resigned from the National Academy of Sciences. Lewis soon estranged himself from Urey, who struggled for years after winning the prize to re-ingratiate himself to his former professor.

That Urey expected to be welcomed back into the Berkeley fold after his discovery of heavy hydrogen is also suggested by his correspondence with Kenneth Pitzer, one of Lewis’s recruits and his successor as Chair of the College of Chemistry. Within one year of writing the abovementioned letter to Birge, Urey wrote to Pitzer complaining that his own contributions to the thermodynamic properties of deuterium

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408 Harold C. Urey to Raymond T. Birge, June 21, 1968, Box 14, Folder 1, HCU.
409 Coffey, Cathedrals of Science, 221.
and methods for its separation were never included in the revisions to Lewis’s textbook on thermodynamics. Urey noticed that both he and Langmuir (whose contributions to refining the Lewis-Langmuir atomic model Lewis had never appreciated) were largely absent from the text and wondered why they should be treated as “outcasts.”\footnote{410}{Harold C. Urey to Kenneth S. Pitzer, April 3, 1967, Box 74, Folder 29, HCU.} In another letter to Pitzer he wrote, “It is curious to me that I have never been accepted as part of the honored graduates of Berkeley. It has gone on for close to 40 years. Probably my fault. But why?”\footnote{411}{Harold C. Urey to Kenneth S. Pitzer, March 27, 1967, Box 74, Folder 29, HCU.} An excerpt from Urey’s interview with Heilbron sheds some light on the possible reasons for wanting to leave Columbia and return to Berkeley:

…Columbia has been kind of a dead dull place, in chemistry particularly; and it has been that way ever since the turn of the century. Somehow a university gets a certain tradition, and you just can’t change that … [there was] a great deal of personal jealousy between people in the department instead of friendly boosting of each other. At Columbia I had very few friends. They weren’t really friendly to each other. It wasn’t just a matter of the outsider coming in from Hopkins, being the ugly duckling that everyone picked on. It wasn’t that. They weren’t friendly to each other, those that were there. Or the people who have been there since.\footnote{412}{Harold C. Urey, “Interview with Dr. Harold Urey, Session 2,” interview by John L. Heilbron, March 24, 1964, 3, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA.} This lack of collegiality in the department would become even more apparent to Urey as department Chair between 1939 and 1942, a time he looked back upon with no fondness.

In addition to the faculty’s unfriendliness to each other, Urey also felt politically at odds with the rest of his department, which was noticeably Republican. This became clear during the 1936 presidential election between Franklin D. Roosevelt and Alfred...
Landon. Urey believed in the New Deal and took on the role of political activist during this period, sponsoring such organizations as the American Association of Scientific Workers and speaking publicly on the importance of sharing America’s wealth and its burdens. In Peter Kuznick’s account of this period, he identified Urey as “clearly [representing] the left wing of the American scientific community” and as a leader in the movement for social responsibility. But in Columbia’s Department of Chemistry, Urey’s colleagues did not share his political views. As Cohn remembered, “in 1936 when Roosevelt was running against Landon, Urey sported a Roosevelt button. But he was the only member of the chemistry department who did. The others all had Landon buttons.”

Urey also felt that the department was intolerant of anyone who didn’t fit the norm of white protestant male. This was made especially apparent to him by the experiences of his Jewish graduate students. Urey discovered more than deuterium during the 1930s; as Cohn put it, “Urey discovered anti-Semitism” during this same period. While Cohn insisted that Urey had never known anti-Semitism before moving to New York, he became a quick study. Having grown up and attended college as something of an outsider to Anglo-American culture, he no doubt saw something of his own struggle in his Jewish students’ experiences at Columbia. Many of Urey’s graduate students during the 1930s were Jews who had grown up in New York City or elsewhere on the East Coast and attended Columbia because they did not want to leave their families for the West.

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414 Cohn, interview, 35.
415 Ibid., 33.
Coast. To their dismay, these students found that anti-Semitism was alive and well in this prestigious New York university.

Rabi, who in 1929 was the first Jewish physicist hired at Columbia, recalled that it was very difficult for Jews to get university jobs in the 1920s and 30s. While many advisors were willing to take on Jewish students and assistants in their laboratories, they did not do much to help to place them. The situation was no different at Columbia, where Rabi felt he was only hired because Heisenberg had recommended him for the job (a job for which Rabi had not applied because he felt there was no hope) while on an American lecture tour. Cohn remembered similarly. Discussing the case of one potential Jewish faculty hire, Cohn said, “It never came to pass, and it was obvious to everyone concerned that the reason was because he was a Jew.”

Cohn also remembered that a similar situation faced women in the sciences: “I have talked to other women of my generation, and they tell me that even though their professors took them on as graduate students, they never really expected them to have careers, particularly if they were getting married.” But Urey was different: “Urey never took that view. He assumed that I would have a career whether I was married or not. … [From] that point of view [I was lucky], because I know women who told me that their professors didn’t bother trying to get them jobs.”

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416 Jerrold R. Zacharias remembered that “They [Columbia University administrators] were willing to have [Jews as] graduate students, but not to have [Jews as] faculty. And this was true of Bell Labs and all other universities,” quoted in John S. Rigden, Rabi: Scientist and Citizen (Cambridge MA: Harvard University Press, 2000), 104.
418 Cohn, interview, 35.
419 Ibid., 38.
Urey soon gained a reputation among Jewish graduate students as someone who would fight for them within the department, help them to find support during their graduate research, and help them to find jobs once they had graduated. Mildred Cohn, one of the few Jewish women to attend Columbia’s chemistry department in the late-1930s, remembered Urey as “the only professor in the chemistry department at Columbia who was concerned with the welfare of the graduate students in those depression years. Were they paid enough as teaching assistants? Were the long hours they worked interfering with their research?” In order to protect Cohn from long hours of work outside of the lab, Urey offered her a loan, telling her, “Ever since I got the Nobel Prize, I’ve wanted to use some of that prize money to help my students. So, why don’t you let me lend you some money, and some day when you have a job, you can pay me back.”

Still, Urey didn’t believe that his support alone would make up for the department’s antagonism toward Jewish students. Nor did he believe that they could transform themselves in quite the way that he had. Rather, he seems to have believed that his identity was malleable in a way that some others were not. As he told Cohn of Rabi, “I can conform but he cannot.”421 Behind closed doors he even advised David Altman, a Jewish student from Cornell who had been accepted to both Columbia and Berkeley, that he would do better to avoid the intolerance of Columbia.422 As noted in the previous chapter, he also advised Cohn at one point that she might escape prejudice by moving somewhere like the Midwest where he believed anti-Semitism was not such a prominent part of academic or social life.

420 Ibid., 29.
422 Coffey, Cathedrals of Science, 219.
Urey’s generosity was not limited to his students. Although he alone won the Nobel Prize for the discovery of deuterium, he chose to split the prize money equally with his two collaborators. He also shared money with unsupported colleagues whose work he admired. Shortly after winning the Nobel Prize, Urey was awarded a $7,600 research grant from the Carnegie Institution with no strings attached. Half of this he gave to Rabi, who would go on to win the Nobel Prize in 1944. As Rabi remembered, “I had had nothing to do with his discovery [of deuterium]. What a greatness in Harold Urey – what a tremendous magnanimity to do something like that! He had a deep faith in me. When he came back from receiving his Nobel Prize, he told somebody, referring to me, ‘that man is going to win the Nobel Prize.’ … This money set me free. It made me independent of the Physics Department.” It is possible that, in addition to believing in Rabi’s work, Urey also saw something of himself in Rabi. Like Urey, Rabi came from a very pious family and occupied two worlds because of it. While in the outside world Rabi was a secular scientist who put little stock in God as anything more than a useful “heuristic principle” in understanding the mysteries of the physical universe, with his Orthodox family at home, Rabi “was a good son,” conforming to his family’s views and showing a genuine respect for the traditions of his ancestors.

424 Ibid., 50.
Isotope Separation and WWII

Whether or not Urey had the support or friendship of his Columbia colleagues, the now forty-one-year-old chemist was a Nobel Prize winner and thus now had the ability and opportunity to pursue whatever lines of research he chose. With increased resources from external grants, Urey was now able to support a small team of graduate students and postdoctoral researchers, and work proceeded at an increased pace. Much of his work for the remainder of the 1930s followed the plan he laid out in his Nobel address. With his graduate students he explored the chemical differences between hydrogen, deuterium, and their respective compounds. Using spectroscopic data his lab was able to calculate the changes in nuclear mass, spin, entropy and free energy that resulted from the substitution of hydrogen and deuterium. Urey also worked with his graduate students and research assistants to calculate the exchange reactions involving the isotopes of the other light elements. Once they had calculated the equilibrium constants for the exchange reactions, they were then able to develop new chemical methods of isotope enrichment using distillation columns. Urey and his team – which from 1934 to 1939 included his research assistant Harry Thode, John Huffman, Clyde Hutchinson, and David Stewart – began distilling and fractionating high concentrations of the rare isotopes of carbon, nitrogen, oxygen, and sulfur.425 With a separate group of colleagues and students – Irving Roberts, Mildred Cohn, and Isidor Kirshenbaum – Urey began putting these isotopes to work in research projects including the study of chemical reaction mechanisms and the differences in vapor pressure of isotopic compounds. But in addition to the work being

done in his own lab, Urey was also providing enriched isotopes to biochemists at Columbia and at universities around the country for work on metabolism and body chemistry. Isotopes had begun as the purview of the physicists, Urey’s work in the new field of isotope chemistry was laying a strong claim for the physical chemist.

In early 1939, as tensions were rising in Europe and Germany was preparing its invasion of Poland, the Jewish physicists Lise Meitner and Otto Frisch – in exile in Stockholm and Copenhagen – reported their discovery of a new type of nuclear reaction which they termed nuclear fission. Experimentally they had bombarded the heavy element uranium with neutrons and split the uranium nucleus. Their note in Nature observed that “These two nuclei will repel each other and should gain a total kinetic energy of c. 200 Mev.” The resulting emission of neutrons from the fission of uranium suggested to the physicists the potential for a powerfully explosive chain reaction. As conditions in Europe moved closer to military conflict, this announcement led to no small amount of concern on the part of physicists in England and America. Particularly excited were the émigré physicists such as Enrico Fermi, Leo Szilard and Albert Einstein who had fled the Nazis in Europe. They worried that this information and its implications were also available and obvious to the Germans. A movement grew among the physicists at Columbia, Princeton, and the University of Chicago to impress upon American political leadership the destructive potential of nuclear fission and the importance of

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developing atomic capabilities before the Germans.\textsuperscript{427} Still believing in the superiority of German science, the urgency of this message is understandable.

In March 1940, Columbia physicists John Dunning, Eugene Booth and Aristid Grosse confirmed that U-235 was the fissionable uranium isotope using a small sample concentrated in Alfred O. Nier’s mass spectrometer. By this time, Urey had already begun considering methods for separating the isotopes of heavier elements – proposing in print a method that would utilize a countercurrent flow centrifuge. He was also by now the recognized world leader in isotope separation. In April 1940 Urey joined a group of concerned Columbia faculty members. This group, headed by Columbia Professor of Physics and Dean of Graduate Studies, George Pegram, began proposing to the Naval Research Laboratory and President Roosevelt’s Committee for Uranium that research on the separation of uranium isotopes should begin at once at Columbia. The Navy in return asked Urey to organize an advisory committee of experts to counsel the Committee for Uranium. This group of experts, which included Urey himself, reviewed the uranium problem at the Bureau of Standards in June, recommending immediate investigations of both isotope separation and the chain reaction.\textsuperscript{428} In the Fall of 1940, Urey began work on isotope separation by centrifuge under a Navy contract while Fermi accelerated his own work on the chain reaction problem.\textsuperscript{429}

\textsuperscript{427} An account of the physicists’ increasing anxiety and their calls for action is found in Richard Rhodes, \textit{The Making of the Atomic Bomb} (New York: Simon & Schuster, 1986), chap. 10 “Neutrons.” Rhodes’s book is in general one of the most complete histories of the American atomic bomb program.


Urey was eager to get American scientists involved in the war effort even before the perceived atomic threat. After the outbreak of the war, he distributed lapel buttons to his students and assistants that read “Defend America by Aiding the Allies.” But Urey’s preoccupation with the deteriorating international situation in Europe had begun much earlier. Kept up to date on events in Europe through the reports of his European friends and colleagues, he feared for their safety and commiserated with them over the rise of fascism. When the situation in Europe threatened his colleagues, Urey did what he could to secure positions for them in the United States, as well as to help them get out of Europe. Most famously, Urey helped Enrico and Laura Fermi to make a home in Leonia, New Jersey.

But Urey’s concern went beyond the Fermis. In his letters to Hevesy during this period, Urey reported his desire to help the Jewish physicist in whatever way he could, and also related his frustrations at the “very strong feeling in this country for keeping out of the war.” Urey helped Hevesy to open a joint bank account in New York City and deposited $1,000 of Hevesy’s money so that he could bring his family to the United States if he had the chance. After the invasion of Denmark, Urey worked with Warren Weaver of the Rockefeller Foundation to attempt to locate Hevesy and get him out of Denmark.

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431 Although no correspondence between Urey and Aage Friis survives from this period, it is worth noting that Friis was involved in these same activities in Denmark: Niels Bohr and Finn Aaserud, Popularization and People (1911-1962) (Amsterdam: Elsevier, 2007), 435.


433 Harold C. Urey to George de Hevesy, March 21, 1940, Box 42, Folder 27, HCU.
Europe on a three-month invitation from the Foundation. A letter to Linus Pauling, in which Urey explained that Otto Redlich had been fired from an Austrian university for being Jewish and asked whether or not Caltech could find a position for him, shows that Urey had become involved in helping Jewish scientists to flee Europe as early as 1938.

It is not surprising, then, that Urey took Fermi and Szilard’s warnings seriously and was one of the earliest supporters of work on nuclear fission at Columbia. He accepted without reluctance the position of chairman of the Advisory Committee on Nuclear Research that would give technical advice to the President’s Committee on Uranium. Once work had begun, Urey coordinated investigations into possible means of uranium isotope separation at the University of Virginia, Harvard, and Columbia, where teams experimented with centrifuges, gaseous diffusion, and chemical separation.

But the program soon grew too large and cumbersome for Urey to handle. From mid-1940 to mid-1941, Urey was working with a Columbia staff of five faculty members, three other personnel, and a research budget of $29,700. At the end of 1941, the program moved from the research stage to the engineering and construction phases. By the end of 1942, Urey had a staff of 180 technicians, and by the end of 1943 Urey had more than 700 people working on gaseous diffusion alone, with hundreds more at universities and laboratories throughout the eastern United States.

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434 Harold C. Urey to Warren Weaver, May 30, 1940, Box 42, Folder 27, HCU; Warren Weaver to Harold C. Urey, June 7, 1940, Box 42, Folder 27, HCU.
435 Harold C. Urey to Linus Pauling, June 16, 1938, Box 419, Folder 1, LP. In this letter to Pauling, Urey also indicated that he was working with the Institute of International Education to help secure grants for the fleeing scientists, but that the official grant proposals had to come from sponsoring institutions.
437 Ibid., 5.
Urey later described his position during the war as “a sort of glorified personnel officer” and complained, “I didn’t do any scientific work myself.” As his research assistant from this period, Karl Cohen remembered, “He had little taste for administration, and the burden weighed heavily on him.” Urey’s unhappiness was also felt at labs outside of Columbia. As James Arnold, who worked on isotope separation “about three tiers below” Urey at Princeton remembered, “The research project at Columbia became very large, and deeply involved with engineering. My professors at Princeton thought this aspect was uncongenial for Urey. This view is consistent with the character of the man I knew later.” And Urey himself would later tell Heilbron that the war, in addition to heightening his own dislike for the atmosphere of Columbia, also added new dimensions of unpleasantness to his life in New York that nearly led him to a nervous breakdown:

I was most unhappy during the war. I had bosses in Washington who didn’t like me, and I had people working for me who didn’t like me. Imagine a more miserable situation – where you can’t resign, but nobody wants you around! About the worst situation you can get in. When the war was over I got out. I was very close to a nervous breakdown during the war. Old General [Leslie] Groves would send his physician around to look me over. … After the war he saw how perked up I was and so forth, he wondered what had happened to me. “Well,” I said, “I have good bosses, that’s all.”

441 Urey, “Interview with Dr. Harold Urey, 2,” 3.
In a separate interview, Urey admitted that he felt General Groves was one such boss who didn’t like him, and claimed that Groves had been “very suspicious” of him during the war.\(^\text{442}\)

Part of Urey’s frustration came from the several reorganizations of the program during the war. These ultimately replaced “The Greek democracy of volunteer scientists” with “central direction and mission-oriented laboratories.”\(^\text{443}\) The first such shift reorganized the Scientific Advisory Committee to the Committee on Uranium and placed it under the National Defense Research Committee (NDRC), headed by Vannevar Bush.\(^\text{444}\) As a member of a new Committee on Uranium (which now omitted foreign-born scientists), Urey was given broad responsibilities for formulating the entire research program in isotope separation, and given a budget of $100,000 for the task.\(^\text{445}\) But this arrangement too would soon be reorganized. The creation of the Office of Scientific Research and Development (OSRD) under the Executive Office of the President saw Vannevar Bush replaced by James B. Conant as the head of the NDRC as Bush became the head of the OSRD. The Committee on Uranium became the S-1 Committee of the OSRD and its membership was expanded.\(^\text{446}\)

Urey remained a member of the Committee, although Bush and Conant now assumed much of his authority for the over-all program in isotope separation.\(^\text{447}\) Urey did not respond well to this new arrangement, and was particularly resistant to Conant as a

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\(^{444}\) Ibid., 3.

\(^{445}\) Ibid., 4.


supervisor. Urey would later deride Conant as a poor manager. During the war, Conant suspected Urey of complaining to other project scientists about his dissatisfaction with his management – an offense that Conant regarded as disloyalty. According to Conant’s son, his father had “very little use” for Urey, whom he believed “shot from the hip and acted emotionally.”\(^448\)

But Urey’s frustrations also had to do with the pressures of bringing a facility online for the separation of uranium isotopes. By the end of 1942, the S-1 Committee had settled on a gaseous-diffusion plant, and had been granted a budget of $100 million for the plant’s construction in Oak Ridge, Tennessee. The plant would be designed and built in collaboration with the Kellex Corporation, whose engineers promised that a 600-stage pilot plant could be built within ten months. Urey, however, was not convinced that all of the pumps, seals, instruments, controls, valves, pipe assemblies and barriers could be designed and built within this time period – and it was he who bore the responsibility of overseeing these developments.\(^449\)

The 600 barriers through which the uranium gas would be diffused were one of Urey’s biggest headaches, and were also the source of tension between Urey and Groves. Two barrier designs – one that did not work well but that Urey thought could be improved and another that was untested and would require a hold-up in plant construction – brought Urey and Groves into conflict. As Cohen remembered, “With ten thousand workers building a huge diffusion plant at Oak Ridge, Groves had to have a successful barrier, even if somewhat late. Urey was not prepared to redefine his objectives to call a

\(^{448}\) Quoted in Hershberg, James B. Conant, 489.
late plant a success.”

Urey was worried that a delay in the construction of the pilot plant would make it unlikely that the plant would play any relevant role in the war effort, but Groves made the decision in early 1944 to go with the new barriers. After this point, “Urey remained the nominal head of the SAM Laboratories till 1945, but his heart was not in it.”

Thus while other scientists involved in the Manhattan Project would later admit to losing enthusiasm for the project after V-E Day, Urey’s disenchantment began at least a year earlier. Also, as opposed to many of his colleagues, Urey not only came to regret unleashing the atomic bomb upon the world, but also developed an aversion to the isotope separation work that had built him into an eminent man of science. Urey later told Zuckerman, “[At] the beginning of the war I had been working on separating isotopes and I was tired of the job and at the end of the war I was still more tired.”

More significantly, Urey seems to have feared that continuing to work in isotopes might bring him increased support: “[If] you tried to separate isotopes and it proved to be important to the Atomic Energy Commission to put the amount of manpower on the job, I couldn’t possibly repeat that, you see.” Indeed, in the years that followed the war Urey, now at the University of Chicago, would develop relationships with the emergent government and military funding agencies, while at the same time working to maintain a level of support that allowed him to direct a successful research program without becoming a manager.

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450 Ibid., 12.
451 Ibid.
453 Ibid.
Chapter 3

Atomic Trauma and New Territory: The Rise of Nuclear Geochemistry in Chicago

After WWII, Harold Urey’s enthusiasm for the isotope separation work that had made him famous was gone. Already soured on Columbia’s Department of Chemistry before the war, Urey took the opportunity to move along with several of his Manhattan Project peers to the University of Chicago. Here they established three new science institutes, one of which was devoted entirely to advancements in nuclear science. Within Chicago’s Institute for Nuclear Science, after an initial period of aimlessness, Urey reoriented his research program toward the use of isotope chemistry in the geosciences.

In his Chicago lab, Urey transformed the mass spectrometers that the Manhattan Project had helped to refine into instruments for the detection of light isotope ratios. With his new instruments, he and his students developed new methods of using the natural abundances of stable isotopes to unlock geological information such as the record of ancient ocean temperatures and climate. He also applied similar methods to the meteorites, in attempts to determine the cosmic abundances of the elements. With his graduate student Stanley Miller, Urey even attempted to answer the question of life’s origin in the early solar system. In addition to making Urey one of the founders of isotope geochemistry, cosmochemistry, and origin of life research, this work allowed him to contribute to the cosmic narrative he advocated in his later speeches.

This chapter focuses on Harold Urey’s entry into the field of isotope geochemistry and examines four dimensions of this move that illustrate the causal links between the Cold War and Urey’s new paleotemperature research program at Chicago. While Urey’s
new research program was not classified or in any way weapons-related, it was
nonetheless constructed along the contours of the Cold War. First, this chapter examines
Urey’s move to the University of Chicago after the war and considers the University and
its newly formed Institute for Nuclear Studies as a Cold War institution. Second, it
examines the negative effect that Urey’s wartime work had on his prewar research
program and the way in which technology developed during the war made a new isotope-
driven research program possible. Third, it considers the various channels through which
Urey funded his research program. While some of the contributing funding agencies such
as the Geological Society of America (GSA) and the American Petroleum Institute (API)
existed prior to the war, others such as the Office of Naval Research (ONR), the Atomic
Energy Commission (AEC), and the National Science Foundation (NSF) were
newcomers to the field. In dealing with all of these funders, Urey wielded his clout as an
eminent nuclear chemist, while at the same time playing to what he perceived to be the
wants and desires of the funding agencies. As an atomic insider he also used his clout to
direct AEC funds toward projects that he felt were worth funding, including the further
development of the mass spectrometers he needed for his new research program. Finally,
this chapter briefly examines the way in which the structure of the University of
Chicago’s Institute for Nuclear Studies and the postwar contract system of research
allowed Urey to position himself as an adviser to a group of younger scientists who he
cultivated to carry on his research independently, and thus facilitated the westward
movement of nuclear geology.
The Institutional Context: Shaping the University of Chicago Institute for Nuclear Studies as a Cold War Institution

“…the unprecedented galaxy of scientists assembled there [at Los Alamos] began to disperse. They were anxious to return to their customary academic habitat, but with a new attitude: the wartime effort had ushered in ‘Big Physics,’ the use of large-scale equipment and the availability of massive financial support. Fermi, together with a group of other brilliant senior scientists, (e.g., Willard Libby, Cyril Smith, Leo Szilard, Edward Teller, Harold Urey) and their junior wartime associates (e.g., the physicists Herbert Anderson, Bob Christy, John Marshall, Leona Marshall, Darragh Nagle, and the chemists Nathan Sugarman and Anthony Turkevitch) accepted offers from the University of Chicago. Some kind of ‘package deal’ was involved (it is rumored that the same “package” had proposed themselves earlier to the University of Washington, but that the deal fell through).”

Valentine L. Telegdi

There is substance to the rumor hinted at by V.L. Telegdi in the above passage. Prior to accepting any new positions after the war (and, for that matter, prior to the use of the atomic bomb), the atomic scientists did in fact go shopping for an institution that they felt they could shape into a center for postwar nuclear science, and where they could continue to work at the pace and scale to which they had become accustomed during the war. Urey discussed the prospect of moving as a group with his Columbia colleagues Enrico Fermi and Joe and Maria Mayer, as well as the physicist Edward Teller. The group seems to have initially set their sights on the Pacific Northwest, where living conditions would be “more delightful” than what they were used to on the East Coast and in the Midwest. (Fermi was especially interested in enjoying “a more congenial place

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455 Teller was then faculty at George Washington University but had moved to Urey’s Columbia University SAM lab in 1941 before becoming attached to Compton’s University of Chicago Metallurgical Laboratory the following year.
than the crowded cities of the east. On the group’s behalf, Urey took a trip to the West Coast in the summer of 1945, before the University of Chicago had made any official offers, and attempted to broker a deal.

In July, Urey reported to Teller that he had been to the University of Washington and had met there with the president of the University and the members of the chemistry department, exploring the possibility of carrying out their plans there. While Urey reported favorably about the encouragement he received from the President and the faculty, he ultimately decided that the prospects were not as good as they might appear. There were definite risks associated with getting involved with a state university that had not yet been initiated into the wartime world of contract research. Urey explained to Teller:

It would be necessary not only to secure the support of the people on the campus, but also the support of the Board of Regents and the Legislature of the State. I think it would involve a considerable educational program, and it would be very difficult to feel any certainty about their steady support, for Boards of Regents and Legislatures change their membership and may reverse any previous policy that has been adopted. If we undertook to develop such a group there we must look forward to a number of years of struggle without any certainty that we shall succeed.

Urey wrote to Fermi at around the same time that no one at Washington seemed to have any idea of how to organize or fund an institute like the one Chicago was proposing. Furthermore, Urey was only able to convince Washington to make definite offers to Fermi and himself. He was not sure he could get offers for Teller or the Mayers, and he was determined to keep the group together.

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456 Enrico Fermi to Harold C. Urey, July 11, 1945, Box 34, Folder 4, HCU.
457 Harold C. Urey to Edward Teller, July 7, 1945, Box 90, Folder 2, HCU.
458 Harold C. Urey to Enrico Fermi, July 5, 1945, Box 34, Folder 4, HCU.
The prospects at Chicago, on the other hand, looked much brighter. So bright in fact that “the Washington plans faded rapidly in the background.”459 Prior to his trip west, Urey felt he had been receiving cautious hints about a postwar position at the University of Chicago. He felt convinced that the university’s President Robert M. Hutchins knew that he could only raise the necessary money for a new institute if he had a stellar group of scientists to help found it.460 Upon his return to New York through Chicago, he received a firm offer. In addition to offering Urey a position at the new Institute and in the Chemistry Department, Chicago told Urey that they had offered a position in the Physics Department to Teller, and that they were seriously considering Joe Mayer. Chicago also informed Urey that Cyril Smith had already accepted a position and that they were continuing to try and persuade Fermi to join as well. “It therefore seems to me that the group that we were thinking of will all appear together at Chicago,” Urey wrote to Teller.461 Equally important was the guarantee on Chicago’s part that “plans were under way for adequate funding from private sources to finance a big development.”462 This showed that Chicago “understood the trend of the times and that it would not confine the activities of basic research to the meager laboratories and still more inadequate funds available before the war.”463 Fermi became convinced that Chicago was the place to be after the war once he received personal assurance from President Hutchins.

459 Urey to Teller, July 7, 1945.
460 In a volume dedicated to Urey on his 75th birthday, Hutchins remembered that “It seems to me, as I look back on it, that I spent most of my time at the University of Chicago trying to persuade Harold Urey to join the faculty.” Robert M. Hutchins, “The Man We Love,” in Isotopic and Cosmic Chemistry, ed. Harmon Craig, Stanley L. Miller, and G.J. Wasserburg (Amsterdam: North-Holland Publishing Company, 1964), v.
461 Urey to Teller, July 7, 1945.
462 Ibid.
463 Samuel K. Allison, “Thoughts on 10th Anniversary of First Chain Reaction,” typescript, November 7, 1952, 8, Box 25, Folder 7, SKA.
that the Institute would be fully supported and that it would embody an extensive program in the expansion of nuclear physics.

Urey deferred making any definite commitment to Chicago until he had conferred with all of the members of the Columbia “package.” Meanwhile, as the summer went on, negotiations continued between Urey and Chicago, now represented by Robert S. Mulliken (a fellow isotope chemist who had served as the director of the Information Office of Chicago’s Plutonium Project). In August of 1945, Mulliken and Urey sat down to discuss future research programs at Chicago. After this, Urey summoned the Mayers to Chicago for a three-day conference concerning the Institute and asked that the three of them discuss their collective position in person beforehand. Meanwhile, Urey also drew up a plan for an isotope separation program at Chicago that required roughly $100,000 for salaries and equally as much for instruments, and sent this plan to Chicago’s Dean of Physical Sciences, Walter Bartky.

To Urey, who felt equally at home working with his physicist colleagues as he did his fellow chemists, it was important that the Institute continue the wartime trend of not recognizing a boundary between the two disciplines. By December Samuel K. Allison, who would be the Institute’s first Director, was looking outside of Chicago’s wartime Metallurgical Laboratory alumni for recruits. As Allison described the plans for the Institute in December of 1945 in a letter to inorganic chemist Don Yost, “…it is the avowed purpose of the Institute to have physics and chemistry under the same roof.” All of the senior members of the Institute were to have a joint appointment in the Institute

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464 Robert S. Mulliken to Harold C. Urey, August 3, 1945, Box 56, Folder 9, HCU.
465 Harold C. Urey to Joseph E. Mayer and Maria Goeppert-Mayer, August 18, 1945, Box 56, Folder 9, HCU.
466 Harold C. Urey to Walter Bartky, August 6, 1945, Box 12, Folder 29, HCU.
and either the Physics or Chemistry Department, and would instruct courses and advise
graduate students within those departments; the Institute itself would not be a degree
granting entity. Thorfin Hogness described this joint appointment – and the new
institute model Chicago was developing – as a compromise between the strengths of the
American and German university models:

During the last war we realized that practically all the basic ideas in
science had come from Europe. Then, with Germany out of the picture and
Russia moving up, we felt this country had to develop its own basic
science to survive. We felt, too, that Germany succeeded so well in
fundamental research because it had the institute type of research
organization. There was very little teaching but mostly all research in such
programs as the thirty Kaiser Wilhelm Institutes in existence before World
War II. This was the reverse of the United States practice, where there was
relatively little basic research and great demands for teaching by research
men in universities. Industrial research was almost entirely of the applied
or practical variety. But we at Chicago decided to modify the German
system by setting up basic research institutes where the staff need not
teach but could if they wished.

Hogness overstated the claim that basic research had hardly existed in America prior to
WWII, but in this way he echoed similar claims being made at the time by proponents of
federally funded American research such as James Conant and Vannevar Bush.

As for the wartime atmosphere that had up until then prevailed at Chicago,
Allison assured Yost that “there is no intention of keeping [this atmosphere] alive with
emphasis on technical details, haste, and military applications. We hope to study nuclear
physics and chemistry in the most fundamental manner.” In a 1947 article in Scientific
Monthly, Allison went further in his description of the Institute’s marriage of physics and
chemistry, this time in more nuclear terms: “In it an attempt is being made to attain the

467 Samuel K. Allison to Don M. Yost, December 4, 1945, Box 3, Folder 3, HCU.
469 Allison to Yost, December 4, 1945.
complete fusion of the sciences of physics and chemistry, at least in their advanced aspects. Permanent facilities for the Institute were by then under construction, but the work of the Institute was already underway. The main three tasks of the Institute Allison defined as researches in nuclear physics, radiochemistry, and the chemistry of the separation of isotopes. But despite these foci, the lab was not organized around specific research programs. Instead, “Each member [was] free to follow what seem to him interesting and promising investigations.”

While the members of the Institute were eager to shake off the atmosphere of wartime work and get back to “fundamental” research, one wartime trend that continued at the Institute was the maintenance of an in-house staff of engineers and technologists. Unlike other departments within Chicago’s Division of Physical Sciences, the Institute had the authority to make both academic and technological appointments. As such, Allison explained that the Institute was an “outgrowth of the experiences during the war, in which physical scientists from many fields cooperated with engineers and technologists in the successful effort to liberate nuclear energy in macroscopic amounts.” Indeed, in 1953 the Institute’s “approximately fifty electrical and mechanical engineers, draftsmen, and research assistants” more than matched its thirty-eight faculty members and research associates.

The University of Chicago of course had a financial interest in seeing the Institute flourish, and had some idea of how the Institute would bring funds into the university.

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471 Ibid., 484.
473 Samuel K. Allison, “Description of INS Activities,” typescript, December 9, 1953, Box 24, Folder 8, SKA.
During the war, as a result of research and training contracts from the government’s Office of Scientific Research and Development (OSRD) and the military, the University’s annual budget swelled to $32,290,945 at its height in the year 1944-45, approximately three times its prewar level.\textsuperscript{474} Much of this funding came from government-financed training programs for pilots, as well as the establishment of a military institute on campus even prior to America’s entrance into the war.\textsuperscript{475} At the University, soldiers were trained in meteorology and weather forecasting, civil affairs, and area and language training “for future administrators of conquered and liberated territories.”\textsuperscript{476}

By the end of 1942, the University of Chicago had garnered 103 separate war-related contracts with the federal government, prompting Hutchins to declare that “The university is rapidly becoming a technological institute.”\textsuperscript{477} But a good deal of the funding came from contracts related to the University’s involvement in developing the atomic bomb. During the war, Chicago ranked seventh in the nation among the OSRD’s non-industrial contractors in terms of the amount of money received ($6,742,070.64), and fifth in the nation in terms of the number of contracts awarded (53).\textsuperscript{478} Approximately $3 million of this funding was overhead for their role in the Manhattan Project, for which the

\textsuperscript{474} Robert M. Hutchins, “The State of the University: A Report by Robert M. Hutchins”, September 25, 1945, 4, Box 26, Folder 6, RMH.
\textsuperscript{476} Ibid., 103.
\textsuperscript{477} Ibid.
\textsuperscript{478} These figures are taking from James Phinney Baxter, \textit{Scientists Against Time} (Boston: Little Brown and Co., 1946), 456.
university not only coordinated the initial uranium studies, but also oversaw the industrial plants at Oak Ridge and Hanford.\textsuperscript{479}

The OSRD and military contracts essentially changed the way that science was organized within the university. The uranium-related contracts were awarded chiefly thanks to the efforts of Chicago’s Arthur H. Compton, who drew upon the reputations of fellow University of Chicago physicists such as Arthur Dempster, Samuel Allison, and William Zachariasen to attract contract support during the early period of development of the uranium project. Compton became the architect of the wartime model of research at Chicago. He styled himself as a “bridge-builder between three diverse and separate sorts of people, each inclined to be rather suspicious of the others: to wit, government and the military, business and engineering, and a hastily assembled array of academic physicists and chemists.”\textsuperscript{480} And it had been his decision, a little more than a month after being put in charge of the uranium project, to move many of the scientists who had been working on achieving a chain reaction at Princeton and Columbia to the University of Chicago campus under the umbrella of what he code-named the Metallurgical Project.\textsuperscript{481} The Project grew quickly and by May of 1942 the Metallurgical Laboratory payroll included 153 persons, about 75 of whom were research associates or assistants.\textsuperscript{482} According to Jack Holl,

Compton adapted the practices of both the OSRD and the academic world. Employing a structure similar to that of the OSRD, he organized scientists and engineers in groups by discipline, roughly parallel to those of academic departments. Using his university experience as a guide,

\textsuperscript{479} McNeill, \textit{Hutchins’ University}, 99.
\textsuperscript{480} Ibid., 105.
\textsuperscript{481} Jack M. Holl, \textit{Argonne National Laboratory, 1946-1996} (Urbana-Champaign: University of Illinois press, 1997), 8.
\textsuperscript{482} Ibid., 10.
Compton established advisory committees of scientists and appointed group leaders with responsibilities similar to those of an academic department chair or laboratory director.\footnote{483}{\textit{Ibid.}, 8.}

As early as 1943 Compton imagined that the Laboratory might have a postwar life as a leading institution in the maintenance of scientific and technical leadership in nuclear research. Before the end of the war, Compton met with President Hutchins and persuaded him to pull the university out of its military researches as soon as possible, but to retain management of Argonne Laboratory and build a complimentary academic program. Under Compton’s guidance, the Metallurgical Laboratory prepared for its postwar life.\footnote{484}{\textit{Ibid.}, 33.}

Many of these trends begun under Compton continued in the Institute after the war. Compton’s success at winning government contracts, and Hutchins’ encouragement of this relationship (hesitant as it often was), began transforming Chicago before the war had even ended in ways that would become characteristic of Cold War institutions. However, Compton’s role as mediator between the government, industry, and the scientists during the war left many of his scientific colleagues in Chicago less than enamored with his management style.\footnote{485}{John A. Simpson, “Arthur Holly Compton, 1892-1962,” in \textit{Remembering the University of Chicago: Teachers, Scientists, and Scholars}, ed. Edward Shils (Chicago: University of Chicago Press, 1991), 78.} The Metallurgical Laboratory’s wartime director left to become president of the University of Washington, St. Louis as peacetime research began at the Institute. Nonetheless, Compton had inaugurated at Chicago a \textit{modus operandi} very similar to what Rebecca Lowen has described as the “Cold War University” – administrators, scientists, and an array of patrons that included the
government and the private sector together defined research goals, and the university benefited from the overhead that the new contract system generated.  

After the war, the Institute’s twelve-million-dollar program relied on funding from unclassified government contracts, industrial sponsorship, and university funds. This mixture of funding sources allowed the Institute to perpetuate the scale of research established during the war, as well as the overhead that contract research generated. The earliest major funders of the Institute included, of course, the Office of Naval Research (ONR). As Geiger has described, the Navy was particularly eager to catch up in developments in nuclear science, and also was inspired by the idealism toward research that prevailed at institutions like Chicago after the war. ONR was therefore one of the most aggressive patrons of university science after 1945. ONR contracts were tailored to meet the expectations of the scientists returning to their university labs; they respected academic styles of research and allowed investigators to initiate their own proposals. At Chicago, as in other leading research universities, “ONR emerged as the ideal patron of science.”

Outside of the ONR, and later the AEC, major funders also included industry. As the New York Times reported in 1951, the university had offered companies the opportunity to buy “memberships” in the Institute at rates ranging from $20,000 to $50,000 a year. By the time of the Times article, twenty-eight firms had “chipped in.”

Lowen, Creating the Cold War University, chap. 6 “Building Steeples of Excellence.”


The companies did not enter into these memberships without expectations. The Times reported: “For their money they share in facilities they couldn’t buy for the same sums individually. They have a share in what is described as the world’s largest nuclear studies program of a privately supported university.” The contributing industrial firms received research reports ahead of publication and had first claim on the applications of any science developed at the Institutes. Technical representatives from the firms also attended regular meetings and visits at the Institute, where they were allowed to tour the labs and observe the scientists’ research firsthand. The initial subscribers included several oil companies (Standard, Shell, and Sun Oil), metal companies (the Aluminum Company of America, US Steel, Bethlehem Steel, Inland Steel), and various other concerns including DuPont, Westinghouse, and American Tobacco. Several of these companies, including DuPont, Westinghouse, and Standard Oil had also been among the principal industrial contractors with the OSRD during the war. By the second anniversary of the Institute these firms had benefited from at least three major developments: radioisotope methods for determining the presence of oil in an exploratory field, radioisotope studies of the action of fertilizers and the uptake of chemicals in plants, and the development of a “noise” thermometer for high temperatures.

Beyond government and industrial funds, Hutchins was also adamant that the University of Chicago must make a substantial contribution to the funding of the Institute, so that its members would not be forced to accept funding that would require the Institute to pursue “research with the University should not undertake.” He was

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491 Du Pont and Westinghouse were awarded 59 and 54 contracts, respectively, amounting in both cases to nearly six million dollars: Baxter, Scientists Against Time, 456.
equally adamant in his position that the Institute would not accept classified research contracts: “…the University cannot decline to cooperate with the government in the national interest. It should make clear, however, that in its view the national interest is best advanced though the freest exchange of information among all workers in this field.” This last position resonated well with that held by the Emergency Committee of Atomic Scientists, of which Urey was a leading member.

**Moving Away from Isotope Separation**

When Urey first entered negotiations with Chicago for his postwar position in the Institute, no atomic weapons had yet been used in the war. The isotope separation program that Urey outlined to Bartky was postmarked on the very day that the first atomic bomb was dropped on Hiroshima. By the time the war had ended, Urey had been so “traumatized” by his wartime experience as Director of Columbia University’s SAM lab that he could no longer muster any enthusiasm for the prospect of continuing isotope separation work. Hutchins claims to have witnessed Urey becoming more and more “disturbed” as the project “drew closer and closer to what I now regard as its catastrophic end.” According to Joe Mayer, the trauma of war work stuck with Urey for some time even after taking up residence at the Institute, causing him “to drift, looking for new

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493 Hutchins, “The State of the University: A Report by Robert M. Hutchins,” 6. That classified contracts were not sought within the INS does not mean that the Institute’s scientists and engineers were not involved in classified projects; many of them maintained their security clearance and were involved in the research activities of Argonne National Laboratory or advised the AEC.

494 The word ‘traumatized’ is no overstatement. It is precisely the word Urey’s colleagues chose to use in describing his post-war state of mind: Cohen et al., “Harold Clayton Urey. 29 April 1893-5 January 1981,” 643.

495 Hutchins, “The Man We Love,” v.
fields to conquer.” And Hans Suess remembered that while most scientists were able and eager to return to their prewar research programs, Urey was “anxious to get away as far as possible, in time as well as in space, from everything connected with weaponry and means of destruction,” including his prewar work on isotope separation.

Aimlessness and angst were not characteristic for Urey, who before the war approached his scientific projects with great enthusiasm and what his colleagues described as a childlike curiosity. In his Nobel address more than a decade earlier, Urey had excitedly reported the thermodynamic properties of isotopes to the world and had spent a considerable part of the address speculating upon the possible methods of separating the isotopes based on these differences – work that, once put into practice at Columbia with a string of graduate students, lab assistants, and grants from private foundations such as the Carnegie Institution, came to define the research program in Urey’s lab up through WWII. In these earlier years, this work was mainly of interest to a relatively small cohort of physicists and chemists throughout the world who were interested in the structure and behavior of the elements and their isotopes, and to an even smaller group of biologists interested in using these isotopes as experimental tracers. During the war, however, these efforts took on a faster pace, and Urey’s workforce grew exponentially as he took on the directorship of the SAM lab.

If Urey’s war trauma was the primary reason he was aimless in the immediate postwar years, his activities on behalf of the control of atomic weapons was a close second. As is described in the next chapter, Urey’s activities with the various

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497 Hans E. Suess, “Harold C. Urey,” copy, December 1981, 1, 10.12, SE.
498 James R. Arnold, “Harold C. Urey Chair in Chemistry, Inaugural Address,” typescript, April 29, 1983, Box 139, Folder 9, SLM.
organizations of the scientists’ movement after the war consumed him for the first few years after the war. He told The New Yorker, “I’ve dropped everything to try to carry the message of the bomb’s power to the people … because if we can’t control this thing, there won’t be any science worthy of the name in the future.” 499 This was a great commitment. Even though Urey had been a popular public speaker before the war, his postwar pace, combined with the urgency of the atomic problem, was difficult for him to handle. “Publicity ruins a scientist,” Urey told reporters. “The phone rings all the time and you can’t settle down. I’m thinking of ripping out the phone and changing my name. I have a stack of mail I haven’t even read, let alone answered. Hell, I’m no public figure! Who am I not to be reading my mail?” 500

But even if his enthusiasm for isotope separation had waned, in the immediate postwar years Urey was still optimistic about the future of nuclear science. Urey took on a leadership role as one of the Institute’s most senior and eminent members. Along with Willard Libby and Joe Mayer, Urey took a good deal of responsibility for the working atmosphere of the Institute. In 1946 these three Berkeley alums initiated a Thursday afternoon seminar at the Institute that closely imitated the colloquia they had been required to attend as graduate students under Gilbert N. Lewis. Here Joe Mayer, who took the lead in the Institute’s seminar, played the role of Lewis and picked from the day’s attendees who should present on their research. According to Hutchison, “No speaker or program was announced beforehand. The discussion was entirely spontaneous and informal. The blackboard, not slides and transparencies, was used. Sometimes there

500 Ibid., 24.
were two or three at the blackboard commenting on each other’s equations or graphs.”

Any kind of science that seemed interesting was discussed at these seminars, and the discussion was never discipline-specific. The researchers were pushed to apply their atomic expertise to the widest range of problems they could imagine. While Mayer would run the seminars, often it was Urey who would end up dominating the day’s discussion – either presenting his ideas about whatever articles he had read most recently, or critiquing whatever ideas others had brought up on that day. In this way, Urey’s postwar aimlessness was allowed to express itself in a productive setting.

At the end of 1946, while still in search of a new line of active scientific work, Urey was forced to put his aimlessness aside briefly in order to prepare and deliver that year’s Liversidge lecture before the Chemical Society of the Royal Institution, London. The Liversidge lecture was one of Urey’s last outstanding prewar commitments. In this lecture Urey chose to update the earlier isotope exchange equilibriums that he and L. J. Greiff had calculated and published in the 1930s, this time using a more sophisticated method developed for the SAM lab by Jacob Bigeleisen and Maria Goeppert-Mayer. In their work in the 1930s, Urey and Greiff had shown that relatively large differences in the physical and chemical properties of isotopic compounds could be detected – differences that were then exploited in the various separation techniques developed in the intervening years. Revisiting the thermodynamic properties of isotopes now, with his postwar aversion to separation, Urey’s mind instead latched onto another way that these chemical differences could be exploited.

In one section of his paper, Urey discussed the geological abundances of the isotopes of carbon and oxygen. Here, Urey noted that certain processes in nature tended to result in isotope enrichment. Aquatic carbonate-precipitating organisms, which use oxygen in their metabolic processes, tend to concentrate oxygen-18 (the more common of oxygen’s two heavy isotopes) preferentially. The shells of these organisms often contained up to four percent more of the isotope than their surrounding waters. This enrichment was temperature sensitive, Urey’s tables suggested, with a change in 25° C resulting in a change in the O-16/18 ratio of 1.004 relative to the water. “These calculations suggest investigations of particular interest to geology,” Urey commented. He further speculated that, with the mass spectrometers Al Nier had recently developed at the University of Minnesota, a researcher could determine the O-18 ratio of carbonate rock samples to within an error of ± 0.001, and possibly discover the temperature at which the rock was deposited with a certainty of within 6° C or less. Although Urey admitted that there was still a great deal of experimental investigation left to perform before the method could be put to use, he felt confident that oxygen isotopic abundances were well-suited for the determination of historic temperature changes. He concluded his paper by stating that the same small differences in the thermodynamic properties of isotopes and their compounds that “make possible the concentration and separation of the isotopes of some of the elements [in the laboratory] … may have important applications as a means of determining the temperatures at which geological formations were laid down.”

503 Ibid., 581.
Although Stephen G. Brush’s account of the postwar rise of geochemistry and cosmochemistry speaks to Urey’s ability through his prestige to attract researchers to the new fields that he pioneered, in the beginning of his work on paleotemperature Urey seems to have had difficulty finding younger scientists to work in his new research program. The first postdoctoral fellow Urey did attract was Samuel Epstein, a young Canadian with mass spectrometer experience who was recommended to Urey by his former lab assistant, Harry G. Thode. Thode had assisted Urey in the mid-1930s with the design and operation of separation systems for isotopes of nitrogen, carbon, and sulfur. After working in Urey’s lab, Thode returned to his native Canada, where he established himself as a scientist in his own right and became a professor at McMaster University (later becoming president of that university).

As Epstein later remembered it, even though Urey had already publicized his speculation about the possibility of using isotopes in carbonate rocks to determine paleotemperature, there was no one lining up to work with him on the problem. When Epstein arrived, he became part of a small research team of four, consisting of himself, Urey, an engineer named Charles McKinney, and a graduate student named John McCrea. Once research got underway, Epstein found a different Urey than that described above by Hutchins, Mayer and Suess: “It was a joy to see Harold make a comeback in the scientific academic world. He never walked up a set of stairs one step at a time, always two steps at a time. His enthusiasm for his research was contagious. I clearly remember him coming into the laboratory dressed meticulously in a white shirt and coming home

with a shirt stained with oil because he couldn’t resist the temptation of changing a dirty oil pump or some other work that was usually left to the younger set." Now feeling at home in the Institute and excited again by what promised to be a fruitful research program, Urey was able to leave behind the traumas of war work.

Moving now into more biological and geological territory, Urey was not entirely a fish out of water. As an undergraduate, Urey had done some natural history work in the field before World War I had put him to work in the chemical industry and set him more firmly in chemical work. Nonetheless, moving into this new field meant that Urey had to develop a new network of scientific contacts and collaborators. In addition to Epstein, McKinney, and McCrea, Urey also drew upon colleagues in Chicago’s Department of Geology. In the postwar years, the Department of Geology at Chicago experienced an unprecedented increase in funding – its typical equipment budget of $1,500 per year suddenly jumped to $45,000 for the first three postwar years. This influx of money allowed the Department to invest in new equipment and allowed for the conversion of some existing facilities into state-of-the-art analytical chemistry laboratories. To accompany these changes, the department also hired new faculty, including geochemists such as Julian R. Goldsmith, Hans Ramberg, and Kalervo Rankama. These men, particularly Goldsmith, would work with Urey, Libby, and Brown to bridge the gap between the Departments of Chemistry and Geology, and all would assist in proposing a joint curriculum in geochemistry for students in either department who wished to become

geochemists. Starting in 1946, the department, now feeling itself to be in competition with their counterparts in physics and chemistry, adopted the attitude that “Anyone on the staff who was not opening up brand-new fields was a piece of dead wood.”

In 1947, Urey secured the cooperation of the German-born paleoecologist Heinz Lowenstam, who had left Germany before the war and was working for the Illinois State Geological Survey. The Department of Geology hired Lowenstam in 1948 specifically to work with Urey on his paleotemperature studies, and Lowenstam’s salary was paid through Urey’s research contracts.

Beginning in 1948 Urey reached out to scientists at the Scripps Oceanographic Institute in La Jolla, California, as well as other marine laboratories, in search of shells from animals of widely different biological classification, living in the same water and at a constant known temperature so that he could determine whether or not any differences existed in the way different organisms affect the ratio of oxygen isotopes laid down in their shells. He also wanted to grow animals of his own in temperature-controlled aquaria so that he could begin establishing his temperature scale.

Once these shells were acquired, Epstein and Lowenstam worked to develop methods for preparing uncontaminated samples of carbon dioxide gas from the calcium carbonate shells and to establish the oxygen isotope temperature scale. The first published results of this work appeared in 1951, shortly before Epstein and Lowenstam

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507 Harrison Brown, “Formal Establishment of Training in Geochemistry”, April 4, 1950, Box 15, Folder 37, HCU.
508 Fisher, The Seventy Years of the Department of Geology, University of Chicago, 1892-1961., 58.
509 Ibid., 61.
510 A notebook from Urey’s lab shows that his team was assembling its own aquaria. By April of 1947 they had prepared three large (36” x 14” x 14”) and four smaller (16” x 8” x 10 ¾”) aquaria that combined parts from a local Hyde Park pet store with scientific instruments either machined or modified in the Institute: “Hard Notebook: L.S. Meyers, Jr., Nuclear Studies”, n.d., Box 1, HCU INS.

In 1950, Urey’s lab further benefited from the growth and expansion of the Department of Geology. It was in that year that Cesare Emiliani received his Ph.D. from the department and went to work for Urey’s paleotemperature group for the next six years. While with the group, Emiliani studied the shells of foraminifera exhumed in long deep-sea cores. Using these, the group was able to study temperature variations in the Pleistocene and determine the length and severity of the ice ages. The acquisition of these deep-sea cores was also evidence of Urey’s diverse and expanding scientific network.

Beginning in 1950, Urey’s lab began collaborating with the Lamont Geological Observatory, where Maurice Ewing had developed a method of piston coring seafloor sediment. Throughout the 1950s Ewing and his colleague David Ericson sent core samples to Urey’s lab where Emiliani and the lab’s technician, Toshiko Mayeda, prepared and analyzed the samples in the mass spectrometer.

But this work would also require Urey to have a working and reliable mass spectrometer, as well as an active program in spectrometry within which to do this research. After years of struggling to do so, he was able to bring one online in 1948. The story of how this came to be illustrates well Urey’s clout within the AEC, and the adeptness with which he was able to wield this clout in the creation of his new research program.
Building Mass Spectrometers

In Ronald Doel’s history of the American planetary astronomy community, Doel emphasizes the significance of mass spectrometers in the birth of isotope geochemistry as a source of the prestige and authority that Urey and his Institute colleagues brought with them into the geosciences. Mass spectrometers, which vaporized, ionized, and propelled chemical samples through an electromagnetic field, separating and then detecting the ions based on their mass, were the most sophisticated and precise instruments available for the measurement of isotope ratios. Not only did these instruments become more widely available after the war and accessible to scientists who had never used them before – biologists and geologists among them – but the type of work that Urey and Brown initiated at the University of Chicago would not have been possible without these instruments. However, while it is true that the Chicago isotope geochemists were among the instrument’s first users after the war, it would be a mistake to assume that they had automatic access to these instruments because of their wartime work.

Nothing was automatic about the transfer of this technology from its wartime service to its postwar diffusion into academic laboratories – even at the Institute. Although Urey and his collaborators did believe before the war’s end that the transfer of this technology would occur smoothly and without any great lapse in research, they in fact faced a rough road ahead trying to recreate the progress they had experienced during

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the war. Bringing the new generation of mass spectrometers on-line in American laboratories after the war took at least two years of work on the part of Urey, his research team, and his Minnesota colleague Alfred O. Nier. Making these instruments a postwar reality required Urey to wield much of the clout he had built up as a Nobel laureate and a Manhattan Project alumnus, and to sell the instrument’s usefulness to funding agencies that had not traditionally funded research in atomic science.

Urey’s claim in his Liversidge lecture that oxygen isotope ratios could help to unlock geologic temperature records was predicated on the availability of isotope ratio mass spectrometers sensitive enough to detect the small differences in oxygen-18 concentration that would result from temperature changes. Urey was confident that the mass spectrometers designed by Nier during the Manhattan Project had the necessary precision. Nier had done much of his wartime mass spectrometer work within the isotope separation program Urey directed, and so Urey had an intimate knowledge of just how far the instruments had come during the war. Even before his research priorities had shifted from separation to the development of new geochemical methods, Urey was already imagining that he would have access to Nier’s machines at the Institute. When he sent his estimate of the costs involved in creating an isotope separation program in the Institute to Bartky, Urey had expressed his believe that some of the set-up money could be saved by getting Nier’s instruments directly from the Army. This belief was reiterated in a letter to Leo Samuels, Head of the Department of Biochemistry at the University of Utah School of Medicine, in which Urey speculated that the mass spectrometers used during the war might be given second lives in university labs, where among other things they

513 Urey to Bartky, August 6, 1945.
could assist analytically in research involving isotope tracers. But despite Urey’s optimism, the mass spectrometers were not quite so easily or cheaply obtained. Even as he was beginning to put his research team together and to approach funding agencies for the money he needed in order to operate his new program, he was also worrying that this lack of equipment might keep him from getting his program off the ground.

Urey knew firsthand just how far the mass spectrometer’s development had come during the war. Before the war, mass spectrometers were a rarity in university labs. Even Urey, who as a Nobelist was able to raise a larger-than-average research budget from private funds, had only achieved mixed success using mass spectrometers in his Columbia laboratory. As the instruments were not yet commercially available and were expensive to build and maintain, very few researchers had the instruments and those who did had either built them themselves or had turned to the small handful of instrumentalists who were competent to do so. The two most widely recognized prewar builders of mass spectrometers were the experimental physicists Nier in Minnesota and Princeton’s Walker Bleakney. Mildred Cohn, Urey’s graduate student during the 1930s, recalls that Urey first hired William Wallace Lozier, a student of Nier’s who had spent some postdoctoral time in Bleakney’s lab, to build a Bleakney-type mass spectrometer in his

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514 Harold C. Urey to Leo Samuels, November 24, 1945, Box 69, Folder 21, HCU.
lab. Unfortunately, a working instrument was never produced under this arrangement and Cohn was sent directly to Bleakney’s Princeton lab to have her experimental products analyzed in Bleakney’s instrument.\(^{516}\) Urey then hired a second physicist, this time a student of I. I. Rabi’s, to rebuild the spectrometer and get it into working order. When this student left Columbia for another position, he left Urey’s lab with a lack of expertise in the use of the instrument, as it was only he and Urey who knew how to operate the homemade instrument.\(^{517}\)

During the war, however, mass spectrometers had improved significantly, to the point that they could potentially be mass-produced and sold commercially. Some of these improvements were made under Urey’s direction of the gaseous diffusion program. The gaseous diffusion method pushed uranium hexafluoride gas through a series of porous barriers and separated the lighter isotope from the heavier one based on the relative ease with which the lighter isotope traversed the barriers. The concept was realized in the Oak Ridge gas diffusion facility, designated K-25, which went into operation in the spring of 1943. Because of the corrosive properties of uranium hexafluoride, leaks were a problem in the K-25 facility, as even a small leak would gum up the barriers and force the entire system to shut down. Urey enlisted Nier to design an adapted version of the 60° sector mass spectrometer he had designed before the war to be used as a leak detector. Dozens of the devices were mass-produced by General Electric for the Oak Ridge plant.\(^{518}\)

Leak detection was not the only area within the Manhattan Project where mass spectrometers made a contribution. In 1942 Nier built seven spectrometers for uranium


\(^{517}\) Cohn, interview, 24.

isotope analysis, one of which was sent to General Electric to serve as a prototype for the several dozen instruments they produced for monitoring the process streams of the magnetic and gaseous diffusion plants at Oak Ridge. For this purpose, there were 100 GE-manufactured Nier-type mass spectrometers at use in K-25. The placement of these machines within the Manhattan Project helped to refine the mass spectrometer in both design and use. Not only was the device now being produced by an industrial manufacturer like GE, but its tenure at Oak Ridge meant that a larger cohort of physicists and chemists than ever before was being trained to use it and coming to think of it as a useful and reliable machine.

Urey’s expectation that the Army would be willing to sell its mass spectrometers, or that they would at least be willing to hand one over to a private firm so that the instruments could be reproduced for sale to researchers turned out to be unrealistic. Nonetheless, he and Nier were determined to make the instruments available to researchers in one form or another. In September of 1946, after the wartime design of the spectrometers had been declassified, Urey wrote to A.V. Peterson, District Engineer for the Manhattan District, about the prospect of having the Army send a mass spectrometer on temporary loan to a private firm. Although Urey was not primarily concerned with tracer research, he pointed out to Peterson that these instruments would be useful in such peacetime research, and that they would soon be sought after for this purpose: “I myself would like to get an instrument of this kind, and I have no doubt many others would like them also.” Nier wrote to Peterson to reinforce Urey’s request, and also promised “that

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520 Grayson, Measuring Mass, 16-17.
521 Harold C. Urey to A.V. Peterson, September 5, 1946, Box 74, Folder 8, HCU.
in the event that the Manhattan District could temporarily release one of the instruments, I would be very glad to cooperate with anyone who could be found to manufacture other instruments of the same kind."522 After the Manhattan District Research Division and the Patent Office of the OSRD studied the proposal, the two scientists were told that the Army had no intention of letting go of their instruments, and knew of no existing procedure that would allow them to do so.523

This negative response had been anticipated. In an earlier letter to Nier, Urey expressed his doubt that going through the Army would yield any result, and encouraged Nier to think about other routes that might bring the instrument to market. He lamented that the lack of reliable analytical instruments was “the greatest impediment to the use of stable isotopes,” the very field that he was eager to promote.524 Urey and Nier did find a way to use their prominent positions in the world of isotope separation in order to encourage the instrument’s postwar development. They aligned themselves with an NRC committee on cancer research that wanted to see stable isotope tracers used in medicine – the Committee on Growth. In July of 1946 Nier applied to the Committee for an annual grant in the amount of $17,000 to be used in the construction and operation of a thermal diffusion plant for the production of Carbon-13 at Minnesota. The grant would be used to supplement already existing and planned facilities at the university and would, Nier promised, allow him to provide the Committee with a portion of the Carbon-13 (in the form of enriched methane gas) produced in the plant that was proportional to the

522 Alfred O.C. Nier to A.V. Peterson, October 1, 1946, Box 69, Folder 21, HCU.
523 A.V. Peterson to Harold C. Urey, October 23, 1946, Box 74, Folder 8, HCU.
524 Harold C. Urey to Alfred O.C. Nier, October 7, 1946, Box 69, Folder 21, HCU.
Committee’s support. As the head of the Committee on Growth’s Committee to Negotiate Purchase of Stable Isotopes, Urey approved the grant and encouraged the Committee to make the grant as quickly as possible so that Nier could begin work immediately.

In a December 1946 report to the Committee, Nier reported that the funds received from the grant were being used for two primary purposes – the separation of carbon isotopes by thermal diffusion (as stated in the original application), and the development of a new mass spectrometer to be used in tracer research. Nier reported that the instrument was already in operation and that it was able to perform isotopic analysis of carbon, nitrogen, oxygen, and hydrogen, and that with some modification it could also be used for other elements or for general gas analysis. He also reported promising commercial prospects: “The work has progressed so far that I have attempted to find a company which will make a complete instrument so that it may be generally available.” Nier had in fact already identified a company, the Consolidated Engineering Corporation (CEC) of Pasadena, California, and had already begun collaborating with CEC’s Harold Washburn. In January of 1947 two men from CEC visited Nier’s lab for several weeks and by February had indicated to Nier that they would begin production of the instruments as soon as possible.

525 Alfred O.C. Nier to Philip Owen, July 18, 1946, Box 69, Folder 21, HCU.
526 Joseph Ney to Harold C. Urey, July 9, 1946, Box 69, Folder 21, HCU; Harold C. Urey to Joseph Ney, July 12, 1946, Box 69, Folder 21, HCU.
527 Alfred O.C. Nier to Committee on Growth, December 18, 1946, Box 69, Folder 21, HCU.
528 Alfred O.C. Nier to Harold C. Urey, February 11, 1947, Box 69, Folder 21, HCU.
Nier also wanted the instrument to be available to labs that could not afford a commercially produced instrument, and for this purpose was preparing a paper for the *Review of Scientific Instruments*:

In keeping with the spirit of the terms of our grant, I am making available to researchers who request the information fairly complete detail drawings, parts lists and circuit diagrams so that they may construct their own instruments if they desire. Also Mr. R.B. Thorness of our shop has agreed to construct a limited number of mass spectrometer tubes for those who may want them. … Our intentions for the future include working out simplified electrical circuits for those who may wish to construct their own instruments but may not have the necessary electronic skill at their disposal. … [It] is my feeling that many of the laboratories who could use an instrument of this sort will not be in a position to purchase a perfected commercial instrument and if given sufficient data could with some help put together an instrument which might not be quite as reliable or accurate but which would be adequate for many purposes.  

All of this was with the intention of stimulating the growth of isotope research. Reiterating his request for a continuation of funding, Nier told the Committee, “If this grant were received I would continue general development work along the same general line attempting to find ways to simplify the instrument in order that it could be built more cheaply and used more widely.”

As it turned out, Urey was one researcher who ended up building his own spectrometers rather than buy the CEC’s manufactured spectrometers. Throughout the process of promoting the instrument’s development, Urey had grown impatient. “I should like to get some mass spectrometers going here as soon as possible,” he wrote to Nier. “I feel rather desperate after the war because I find that my colleagues and apparatus both have all been completely disorganized by the war activities. Some times I have thought

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529 Nier to Committee on Growth, December 18, 1946.  
530 Ibid.
that I never again will get started doing scientific work. I shall be very unhappy if that is the case.”

When a mass spectrometer was not immediately forthcoming, Urey decided to import mass spectrometry know-how into his laboratory, as he had in the 1930s. After having no luck drumming up interest among possible young collaborators in the States, he contacted Thode and learned of the then 27-year-old Epstein. Epstein sent along his credentials to Urey in the summer of 1947 and reported that in Thode’s lab he was working on perfecting a mass spectrometer tube in which solids could be analyzed without having to be converted into gaseous chemical compounds. That Epstein was a competent physical scientist with a focus on the technical aspects of mass spectrometer research fit perfectly with Urey’s new research goals. Less than one week later Urey responded to Epstein’s inquiry with an offer of $4,400 per year in salary, along with an apartment above the garage of his Hyde Park home for $60 a month. Urey also informed him that he would be working with McKinney, a young physicist from the Oak Ridge facility, who would be tied primarily to the “mass spectrometer problem,” and that there would also be two or three technicians to prepare the samples and perform the routine analyses in the spectrometer. By February of 1949 Urey’s lab had constructed two mass spectrometers for the oxygen work. The designs were based on the specifications provided by Nier, as well as Thode’s published descriptions of a sector mass spectrometer for isotope abundance measurements.

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531 Harold C. Urey to Alfred O.C. Nier, February 28, 1947, Box 69, Folder 21, HCU.
532 Samuel Epstein to Harold C. Urey, July 17, 1947, Box 31, Folder 2, HCU.
533 Harold C. Urey to Samuel Epstein, July 23, 1947, Box 31, Folder 2, HCU.
534 Harold C. Urey to Samuel Epstein, March 4, 1949, Box 31, Folder 2, HCU.
As it turned out, however, Nier’s design did not produce measurements as precise as Urey needed for his paleotemperature work. McKinney and Epstein had to make modifications to the mass spectrometer’s electronic equipment, including the addition of a more stable power supply, a more reliable amplification system, and an improved emission regulator. In addition to these modifications of the electrical components, the team also had to develop a method that would allow them to change samples rapidly, and to attach a Brown potentiometer so that the state of balance of the collected ion beams could be continuously detected and recorded.535

When Urey began to foresee success with his own instrument, he wrote to CEC and invited Harold Washburn, Nier’s collaborator within the company, to come to Chicago to see firsthand the modifications that McKinney and Epstein had made to the Nier design. CEC had not yet moved into production of their Consolidated-Nier Isotope Ratio Mass Spectrometers, designated Model 21-201. Urey suggested that CEC would be interested in possibly adapting their instrument for the purposes to which Urey and his group were putting it. Optimistically, Urey added, “I am hoping very much that this temperature idea does prove valuable, and that in that case geologists will be interested in this wrinkle and that you will be able to supply instruments for them if they want them.”536 In September of that year H. Wiley, one of CEC’s field men, attended Urey’s lecture on “Oxygen Isotopes in Nature and in the Laboratory” at the American Association for the Advancement of Science meeting in Washington, DC, in which Urey

536 Harold C. Urey to Harold Washburn, March 1, 1948, Box 21, Folder 14, HCU.
described some of the modifications he had made to Nier’s design. In Wiley’s field report he told CEC management that Urey talked a great deal about the finer points of mass spectrometers and that the chemist was “all steamed up on finding out how warm the seas were 60 million years ago. If that works, he’ll tell you how warm they were 1 billion years ago”:

This involves measuring $\text{O}^{18}/\text{O}^{16}$ which, of course, means money in our pockets if we ever get around to delivering a few 21-201’s. If production realized that until they get out those 21-201’s we won’t know how warm the seas were 60 million years ago, they might put on the pressure! … [The] moral of the story is that one must have a mass spectrometer capable of very high accuracy. At this point Urey said that “mass spectrometers are so complex and troublesome that they work only 20% of the time.” He stressed this point as I sank lower and lower in my chair. … He suggested a number of other applications of the stable isotope measurements in the geological and chemical fields and did not touch biology or biochemistry. It was a first-class talk, but he sure blackened the name of mass spectrometers!

Later that year, Washburn did send a man to Urey’s lab to examine the DC amplifier designed by McKinney, and also asked Urey to help estimate the commercial market for the instrument. Urey reported back that he would try to feel out interest at the Geological Society of America meeting in November, but that his own sense was “that mass spectrometers of the kind we are using should be exceedingly valuable in geology, not only for the type of studies we are making but for many others.”

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537 Urey’s talk was published as Urey, “Oxygen Isotopes in Nature and in the Laboratory.”
539 Harold Washburn to Harold C. Urey, October 19, 1948, Box 21, Folder 14, HCU.
540 Harold C. Urey to Harold Washburn, October 25, 1948, Box 21, Folder 14, HCU.
Funding the New Program

Urey was already in contact with the American Petroleum Institute and the Geological Society of America, and had used funding from the two organizations to pay his instrument team’s salaries and buy components for the mass spectrometers. Both the Shell and Standard Oil Companies were already sponsors of the Institute, and on a tour of the facility in 1947 a representative of Shell met with Urey and heard about his research plans. The Shell representative came away impressed. As he wrote to the chairman of the API’s Advisory Committee on Fundamental Research on Occurrence and Recovery of Petroleum, Urey’s research was “of considerable interest, since, if successful, it will help measure one more of the many unknown variables of importance to the origin of oil.”\footnote{H. Gershinowitz to E.G. Gaylord, April 8, 1947, Box 6, Folder 1, HCU.}

Furthermore, he noted that Urey’s program could possibly fit into the API’s ongoing Project 43, a broad investigation of the transformation of organic matter into petroleum that the API had been funding since 1942, and which included a research team at MIT investigating the effects of radioactivity on the transformation of marine organic materials into petroleum hydrocarbons.\footnote{The history of Project 43 research at MIT is described in Robert Rakes Shrock, \textit{Geology at MIT 1865-1965: A History of the First Hundred Years of Geology at Massachusetts Institute of Technology}, vol. 2 (MIT Press, 1982), chap. 20 “Research on the Origin of Petroleum at M.I.T. (1942-1954).”}

Urey’s research program did not in fact become a part of Project 43, but the API nonetheless regarded the work as having the potential to contribute to their understanding of the processes that produce oil. That they would have this impression was partly Urey’s own doing. In his initial courting of API funding, Urey had speculated that “It may be that oil deposits occur in places where the temperature at which they were deposited was...
unique in some way, and if this should be the case then it might furnish one additional tool for geological exploration for oil." But while Urey requested $12,000 for the construction and maintenance of his instruments, the API was only willing to grant him $5,000 for 1948-49. This was nowhere near the approximately $9,000 Urey estimated it would cost just to build his mass spectrometers.

In the summer of 1947 Urey also contacted the GSA and, at the recommendation of W. H. Newhouse from Chicago’s Department of Geology, requested funds from the GSA’s Penrose Bequest. Here he played up the potential that his work would replace existing qualitative methods of determining paleotemperature – namely paleoecological studies of the fossil organisms found within geological samples – with more quantitative methods. The GSA granted Urey $17,900 for salaries to support one chemist, one physicist, and three technicians. This amount, even when combined with the API funding Urey was also receiving, still did not approach the $50-100,000 Urey estimated he would need in order to build his instruments and establish the new methodology.

In the second year of his research program, 1949, the amount of funding Urey had at his disposal increased dramatically, as he began a new contract with the Office of Naval Research. This contract of $30,000 was extended at roughly the same level for three additional years. The ONR was an ideal funding agency for these early years of Urey’s research program, as the Navy preferentially funded research into the development of new methods and techniques. Once Urey’s methods had been

543 Harold C. Urey to T.V. Moore, June 25, 1947, Box 86, Folder 36, HCU.
544 C.A. Young to Harold C. Urey, March 9, 1948, Box 6, Folder 1, HCU.
545 Harold C. Urey, “Penrose Application,” July 17, 1947, Box 37, Folder 10, HCU.
546 Henry Aldrich to Harold C. Urey, September 22, 1948, Box 37, Folder 10, HCU.
established, however, the ONR informed him that they would no longer be able to fund his research.\textsuperscript{548} This of course put pressure on Urey to find a funding agency that could take the ONR’s place. Fortunately, he was able to find two funding agencies that were together able to raise his funding level to new heights. In 1953-54, Urey received $55,956 from the AEC (with matching funds from Chicago) and $21,400 from the NSF.\textsuperscript{549} With more than $75,000 in contract funding, this was a banner year for Urey’s research program. For the remaining years at Chicago, up until he left for the University of California, San Diego in 1958, Urey would successfully keep his external funding at or slightly above this new level.

Approaching these new funding agencies again required Urey to wield his clout as an atomic insider. Although Urey had decided to leave isotope separation work behind, much of his clout within the AEC was attached to his expertise in the field of isotope separation and his past position as the head of Columbia’s SAM lab. For this reason, it was not only difficult, but also impolitic, for Urey to completely close the door on isotope separation. Remaining connected to the AEC’s concerns over isotope separation and flexing his expertise in this area at the AEC’s behest helped Urey to maintain the prestige that he had earned from his wartime service. It also allowed him to keep abreast of precisely what the AEC’s concerns were, allowed him at times to define these concerns, and made it easier for him to frame his new projects in language that would garner AEC approval.

\textsuperscript{548} ONR to Harold C. Urey, August 22, 1952, Box 95, Folder 1, HCU.
\textsuperscript{549} Harold C. Urey, “Proposal for Extension of Research on the Natural Abundance of Deuterium and Other Isotopes in Nature”, February 1953, 6, Box 92, Folder 9, HCU. NSF to Harold C. Urey, July 20, 1953, Box 68, Folder 1, HCU.
One such example of this is Urey’s reluctant agreement to chair the Committee on Isotope Separation for the AEC’s Division of Research in early 1951.\textsuperscript{550} As he described in a letter to Ken S. Pitzer, the director of the Division, “Long ago I developed a subconscious reaction to all separation jobs. It is, first, that any separation project is an enormous amount of hard and uninteresting work, and second, that it is very likely that all new schemes for separating isotopes will not work.”\textsuperscript{551} Nonetheless, accepting this position allowed Urey to exert some influence over the direction of isotope work in the United States and put him in constant contact with Pitzer. The Committee itself represented a high priority within the AEC’s Division of Research; it was responsible for reviewing the existing literature and techniques in isotope separation, the immediate and long-range needs within the atomic energy program for the separation of isotopes, and the work in progress on isotope separation within the AEC, and finally recommending to the Division of Research what steps should be taken for the investigation and development of specific isotope separation techniques.\textsuperscript{552}

In addition to keeping Urey and his fellow Committee members connected to the Division of Research, the work also kept them connected to classified places of atomic research and classified materials. Members of the Committee also had facilities installed in their offices for the storage of classified documents (if they did not already have such facilities), which involved clearance procedures for secretaries and technical assistants. The members also received a classified bibliography of sources held in classified libraries at the National Labs and their associated universities. The first meeting of the Committee

\textsuperscript{550} Kenneth S. Pitzer to Harold C. Urey, February 9, 1951, Box 92, Folder 8, HCU.
\textsuperscript{551} Harold C. Urey to Kenneth S. Pitzer, Jan 1, 1951, Box 92, Folder 8, HCU.
\textsuperscript{552} Kenneth S. Pitzer to Harold C. Urey, February 9, 1951, Box 92, Folder 8, HCU.
took place at Oak Ridge in February of 1951, and during their stay at Oak Ridge the Committee members were given a full tour of the facilities. Later meetings took place in the New York Operations Office of the AEC, and at the University of Chicago, with its associated Argonne National Laboratory. Under the auspices of the AEC’s Division of Research, members of the Committee also toured the DuPont laboratories in Wilmington, Delaware, where they discussed heavy water problems. They visited the Washington DC headquarters of the AEC to learn about the raw materials situation. They met with scientists and technicians at General Electric, Yale, and Brookhaven National Laboratory.\footnote{Joseph Platt to Harold C. Urey, February 23, 1951, Box 92, Folder 8, HCU.}

With his knowledge of the inner workings of the AEC, Urey was able to finesse his proposals for isotope geochemical and cosmochemical work into proposals that seemed, at least superficially, to be directly related to the AEC’s concerns and enlisted Urey’s prestige as the discoverer of heavy hydrogen and heavy water. While Brown in 1949 floated a “Proposed Program for the Accumulation of Quantitative Data Concerning: the Chemical Composition of Meteorites and the Earth’s Crust; the Relative Abundances of Elements in the Solar System; the Ages of the Elements and Planets,” and hoped that the AEC would at least fund those parts of the program that were performed at its Argonne facility, Urey’s later proposal was far more politically savvy in its name and form. Urey’s proposal for “Research on the Natural Abundance of Deuterium and Other Isotopes in Nature” outlined an intentionally broad research program that included work
to be done on meteorites, igneous rocks and fossils, with the stated aim of discovering how the abundance of hydrogen isotopes had changed over time.\textsuperscript{554}

In addition to addressing the AEC’s concerns over deuterium and heavy water and their abundances in nature, Urey’s proposal also stressed the scientific attention that his work on paleotemperature was receiving, thus tapping into the AEC’s desire for visible scientific rewards from unclassified and non-military projects. Brown must have learned his lesson. When in 1952 Brown left Chicago for Caltech, he submitted a research proposal to the AEC for a “Study of the Fundamental Geochemistry of Critical Materials and the Development of Economic Processes for Their Isolation,” which would fund Patterson’s work on the age of the earth and meteorites, as well as geochemical studies of limestones, shales, dolomites, and phosphatic rocks.\textsuperscript{555}

\textbf{Training Geochemists in Chicago, Moving Geochemistry to the West}

The young scientists who worked under Urey and other senior members of the Institute received the support they needed in order to make themselves known in the broader scientific world, but there was little room within the Institute for these younger scientists to move up. The senior scientists who had helped to found the Institute dominated it. Afterward their training, these young scientists moved on to geology and

\footnotesize\textsuperscript{554} Harrison Brown, “A Proposed Program for the Accumulation of Quantitative Data Concerning: the Chemical Composition of Meteorites and the Earth’s Crust; the Relative Abundances of Elements in the Solar System; the Ages of the Elements and Planets,” 1949, Box 15, Folder 37, HCU; Harold Urey, “Atomic Energy Commission Contract No. AT(11-1)-101 To Investigate the Natural Abundance of Deuterium and Other Isotopes in Nature,” February 1953, Box 92, Folder 9, HCU.

\footnotesize\textsuperscript{555} Harrison Brown, “Proposal for Research Project, Study of the Fundamental Geochemistry of Critical Materials and the Development of Economic Processes for Their Isolation,” 1952, Box 14, CGD.
chemistry departments further west, including Caltech and the University of California, where the administrations were interested in the AEC money that geochemical work attracted. These scientists were no doubt eager to define their own research programs and, to some extent, replicate the structure of research in Chicago. In this way, Chicago was a victim of its own eminence and success. When, at the end of the 1950s, Urey realized that he had trained and populated some of the top programs in the West, and as he had continuing trouble keeping his own research program going, he also joined these scientists in the movement of geochemistry to the West, becoming a founding faculty member of the University of California, San Diego.

Urey’s AEC contracts guaranteed him the ability to hire and keep young scientists and engineers as they made a name for themselves publishing the results obtained in his laboratory. His contract from 1953 shows that his lab received roughly $55,500 from the AEC (a budget that did not increase substantially during his time in Chicago), $38,600 of which was allocated to the payment of salaries to the three scientists and six technicians and mass spectrometer operators working on the project. While the university paid the bulk of Urey’s salary, and provided $6,000 worth of additional miscellaneous services from the machinists, electronics engineers, and glassblowers already employed at the university, the AEC money allowed Urey to pay his full-time scientific and technical workers without being beholden to the university or the department. Urey also used the AEC contract process in order to secure raises for his scientific personnel. When one of his scientists had begun to be recognized for the work they were doing in his lab, Urey

556 The University of Chicago’s annual financial contribution to Urey’s research program was roughly equivalent to that provided by the AEC.
told the AEC, as in the case of Cesare Emiliani, that he “should either try to give him a reasonable job with me or else secure a very good job for him elsewhere.” Once the AEC had approved Urey’s budget, Emiliani indeed received a considerable raise.

But Urey did not want to retain any of his scientists indefinitely. Urey was proud of the fact, and reported such to the AEC in his 1955 renewal proposal, that many of the smaller projects funded under the umbrella of the previous years’ contracts had been “taken over almost completely by the young men working on the problems.” He went on to elaborate:

Dr. [Harmon] Craig has become a fully mature scientist with an international reputation. He is invited to a Gordon Research Conference this summer as an important contributor, and to England for another conference. Dr. Emiliani has become well known for his work on paleotemperatures. Dr. [Gerald] Wasserburg has done outstanding work on the K40-A40 dating. Wasserburg goes next fall to California Institute of Technology as an Assistant Professor; Craig and Emiliani are not definitely placed yet but I believe that they will be shortly. One of the greatest feelings of accomplishment that I have had since the war comes from the growth of some of my young men into these positions.

Urey suggested that the AEC might “get some small glow of satisfaction” knowing that its support had allowed these young scientific careers to blossom.

Urey was also invested in keeping his research group relatively small. Not only did he not request major increases in the funding he received from the AEC, he specifically asked them to allow him to keep his program at its existing level. In 1954, for example, Urey wrote under the “Scope and Purpose” section of his renewal proposal that “I do not wish to have these contracts become much larger, for were this to happen I

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557 Harold C. Urey, “Proposal for Extension of Research on the Natural Abundance of Deuterium and Other Isotopes in Nature,” 5, February 1953, Box 92, Folder 9, HCU.
could not follow my own work but would be forced to become an administrator.°° It was not the first time he had made this request. In 1953 he had outlined to the AEC, again in the renewal application, the reasons for wanting to keep his research program at roughly the size it had been previously: “If I have fewer technicians on the job it will be impossible to keep the mass spectrometers running. If I have fewer men of the Ph.D. level working on this problem the mass spectrometers will not do effective work; and if the project becomes larger than it is I would become principally a manager of a project instead of a scientist working and thinking for myself.”°°° As Urey preferred it, his role was primarily as an adviser to the scientists working under him. He was even hesitant to include his name on the publications that came out of his lab, unless he had done something more than advise the researchers working on that particular part of the project.

The Origin of Life in the Cold War

The case of Stanley Miller provides a good illustration of how the structure of Urey’s research program produced young scientists, as well as how the concerns of the Cold War often intruded upon research in Urey’s lab. By the early 1950s, Urey’s interest in geochemistry and new research in meteorites had led him to consider the chemistry of the solar system and the origin of the earth. From this, Urey’s interests wandered into the question of the origin of life. As Urey told one historian who wrote to him in the 1970s, an article in Science written by a well-known colleague from Berkeley’s Radiation

°°° Harold Urey, “Proposal for Extension of Research on the Natural Abundance of Deuterium and Other Isotopes in Nature,” 4, February 1953, Box 92, Folder 9 HCU.
Laboratory had sparked this interest. This article had proposed that the chemistry of life had been formed in an oxidizing atmosphere by high-energy discharges, and the researchers had used Berkeley’s 60-inch cyclotron to subject carbon dioxide gas to 40 mev helium particles.

Urey felt that his Berkeley colleague’s approach was “quite irrelevant to the problem of the origin of life.” Near the end of 1951, Urey began preparing a paper of his own on the subject, and presented his preliminary argument before one of the Institute’s weekly seminars. Here he presented his point of view – that the early earth had a highly reducing atmosphere consisting primarily of hydrogen, ammonia, methane and water. Electrical discharges within this atmosphere, such as lightning, formed the first carbon compounds, establishing the prebiotic conditions within which life could emerge.

Sometime between Urey’s initial presentation and the publication of his paper, Alexander I. Oparin’s hypothesis that life emerged under reducing conditions also came to Urey’s attention. It is likely, however, that Urey was already set on a reducing atmosphere by this time. As he reported to the AEC, he had been led to this conclusion by his own research team’s finding that the igneous rocks of the earth had a far lower abundance of deuterium than the average of the earth, and that meteorites had less deuterium than the mean of the oceans. “We interpret this latter result to mean that 10 or 15% of the present amounts of water on the earth has been dissociated and the hydrogen has escaped,” Urey wrote to his patron, “leaving the deuterium mostly behind. This leads

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561 Harold C. Urey to David Feinman, August 5, 1977, Box 35, Folder 24, HCU.
to the conclusion that the original atmosphere of the earth was highly reducing and that an oxidized atmosphere has been produced only by the escape of hydrogen.”

Nonetheless, Urey incorporated Oparin’s hypothesis as support for his own claims. In Urey’s paper, he speculated that an experiment on the production of organic compounds from water and methane in the presence of electrical discharges might help to shed light on the problem. He must have suggested a similar experiment in his seminar presentation: “I went back to my office, after the lecture, and Stanley Miller appeared. He said he wanted to do his doctoral dissertation on the subject.” Urey was at first hesitant to allow Miller to take on the project: “I was afraid it might not turn out well for a dissertation, but Stanley could not be discouraged.” With Urey’s guidance, Miller designed an experiment that would simulate the conditions Urey proposed. According to Urey, the experiment paid off quite quickly: “…in three months he had made the experiments and had secured evidence of compounds characteristic of living things.”

In early 1953, Urey and Miller were ready to publish Miller’s results. Urey felt he faced a dilemma. Only one journal in the US could bring such a groundbreaking and interdisciplinary experiment to the attention of all of the relevant disciplines, and this was Science. But sending the paper to Science, Urey complained to Edward Condon, would mean having to wait for months for the paper to be published. If he sent it to a more specialized journal, it would be published faster, but it would not have the impact that Urey desired: “We wish to send off a brief note for quick publication about this. It is of interest to zoologists, chemists, biochemists, geologists, and astronomers. Where should

564 Harold C. Urey, “Report on Contract AT(11-1)-101”, February 15, 1953, 1, Box 92, Folder 9, HCU.
566 Urey to Feinman, August 5, 1977.
we publish this in the United States? If I send it to Science it will be six months before it will get in print. If I send it to the [Journal of the American Chemical Society] it will be out in a few weeks, but will miss a large fraction of the audience.”

Urey did send Miller’s paper to *Science* in February. By March, *Science* had decided to run the paper as one of its lead articles. However, before *Science* could send Miller an acceptance letter, Urey became concerned that the publication was taking their time with an article that he believed contained one of the most dramatic discoveries of the century. Urey sent a telegram to *Science* on March 10, requesting that the magazine return the paper at once, and stating that it had been sent with the understanding that it would be given speedy publication. He ended the telegram angrily: “it is being sent elsewhere cancel my subscription to Science as I regard it as a useless publication.”

Urey’s overzealous championing of his student’s work almost prevented Miller’s paper from being a lead article in *Science*. As Howard Meyerhoff wrote to Miller, he had just been dictating a letter to Urey and Miller about the acceptance of their paper when Urey’s angry telegram arrived. Luckily for Miller, Urey was not the lead author on the soon-to-be-famous paper. As Meyerhoff explained, he was happy to cancel Urey’s subscription to *Science*, but that since Miller was the lead author, he would only return the paper if Miller so desired. In May, Miller’s paper appeared in *Science*. Miller was the only author listed on the paper, while a footnote thanked Urey for his helpful

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567 Harold C. Urey to Edward U. Condon, January 29, 1953, Box 21, Folder 12, HCU.
568 Harold C. Urey to *Science* Magazine, telegram, March 10, 1953, Box 58, Folder 18, HCU.
569 Howard Meyerhoff to Stanley L. Miller, March 11, 1953, Box 58, Folder 18, HCU.
suggestions and guidance. As Urey explained, “Well, after this, I did very little and let Stanley Miller develop the project.”

Allowing Miller to claim the origin of life experiment as his own achieved multiple goals. Most obviously, it established Miller as a leading voice in origin of life research. Less obviously, it allowed Urey to shelter Miller from his own Cold War problems. The experiment took place in the midst of the Korean War. Miller was, as a graduate student, not necessarily guaranteed draft deferment. In the early 1950s, the Army was experimenting with draft deferment for graduate students. Miller, however, was classified 1-A, available for unrestricted military service. Only three months after Miller’s paper appeared in *Science*, Urey wrote to the local draft board of Miller’s hometown to plead Miller’s case. Miller, he told them, was a brilliant student whose experimental work on the origin of life had received a great deal of notice from scientists in many different fields. He also told them that if Miller’s work was interrupted before he graduated in the next year, it would not only be difficult for him to finish his dissertation, but other scientists would carry on his work without him: “This means a very definite disadvantage to this young man unless he is able to finish his work, and I believe it is a disadvantage to the country as a whole to interrupt the advanced training of a capable man.”

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571 Urey to Feinman, August 5, 1977.
574 Harold C. Urey to Local Board #48 (Alameda County), August 28, 1953, Box 58, Folder 18, HCU.
This was not the only way that the Cold War intruded on Miller and Urey’s work. Origin of life research was an international affair, and Russian scientists such as Oparin were in fact leaders in this new field. In 1956 Oparin invited both Miller and Urey to come to a Symposium on the Origin of Life in Moscow in the summer of 1957. At first Urey accepted the invitation and advised Miller to do the same. He wrote to Miller, “My advice after thinking it over for some time and talking to several people, is to accept the invitation. … One never knows what this will do to us sometime in the future but I think it is safe.” However, soon the violent Soviet reaction to the Hungarian Revolution changed Urey’s mind. Miller wrote to Urey that the subjugation of the Hungarians had shown the Russians to be “gangsters.” He speculated that it might not be career suicide to attend the conference with the endorsement of the State Department, but “Even with endorsement, there is the question of whether I want to have anything to do with people like the Russians. It is often said that science should be separate from politics, but in this situation I am not sure.” Urey agreed with Miller’s assessment: “I am very put out by the behavior of the Russians and do not feel like doing anything to indicate any acquiescence in their treatment of Hungary; but to the Russians I probably will not say this since I think it would do no good.”

Even after Urey’s death in 1981, the American Miller-Urey experiment continued to have Cold War relevance. In 1989, the International Society for the Study of the Origin of Life, on Miller’s initiative as President of the Society, instituted a Urey Medal for

576 Harold C. Urey to Stanley L. Miller, May 3, 1956, Box 58, Folder 18, HCU.
577 Stanley L. Miller to Harold C. Urey, April 10, 1957, Box 58, Folder 18, HCU.
578 Harold C. Urey to Stanley L. Miller, April 13, 1957, Box 58, Folder 18, HCU.
contributions to the field of origin of life studies. This medal would be awarded in alternative years to the Society’s already existing Oparin Medal. In addition to recognizing Urey for his contributions to the field, the new medal also afforded the Society a solution to a late-Cold War dilemma. The late-1960s had brought revelations by Zhores Medvedev and David Joravsky of Oparin’s alliance with T.D. Lysenko.\(^{579}\) As one disgruntled member wrote to the Society, citing both Medvedev and Joravsky’s work,

> More than 3 years ago, the street address of the Soviet Academy of Sciences has been changed to Vavilov str 32, Moscow… Vavilov is widely regarded as a martyr of science. Vavilov was hounded to death by Lysenko … and died of malnutrition in the Saratov prison. Oparin “joined the Lysenkoite movement, the only really eminent biologist to do so. From 1948 to 1955, he was in charge of Lysenkoite firing and hiring within the (USSR) Academy of Science. … He altered his speculations on the origin of life to suit the Lysenkoite creed. …” Oparin supported the notorious quack Lepeshinskaya, who claimed to have produced cells and tissues in vitro, and who was a close friend of Stalin. Now that the winds of perestroika are blowing, it is time that ISSOL revise the name of its major award. In my opinion, the change would be welcomed at Vavilov str. 32, Moscow.\(^{580}\)

Origin of life researcher and chair of the 1989 Oparin Medal selection committee, John Oro, in a letter to Miller, recognized the possibility that the winner might refuse the Medal based on its association with Oparin. In fact, this had happened in 1986 when Miller was the chair of the selection committee and Leslie Orgel had refused the award.\(^ {581}\) Oro concluded that the best strategy was to have the committee rank multiple candidates for the award, in case the top candidate refused.\(^ {582}\) The Urey Medal, approved


\(^{580}\) Thomas Jukes to Donald DeVincenzi, February 15, 1989, Box 160, Folder 6, SLM.

\(^{581}\) Stanley L. Miller to J.W. Schopf, April 3, 1986, Box 160, Folder 4, SLM.

\(^{582}\) John Oro to Stanley L. Miller, February 8, 1989, Box 160, Folder 6, SLM.
later that year at the Society’s annual meeting, allowed those who wished to refuse the medal on political grounds to accept the Urey Medal the following year.\(^{583}\)

**Leaving Chicago**

Not having more than a handful of scientists under him at any given time had its drawbacks. For example, when in 1955 Urey’s team lost five of its scientists, his research program experienced a severe setback. Furthermore, although Urey could give his scientists modest raises from his AEC funding, he could not give them university appointments. On instances when Urey attempted to keep a worker at Chicago, as he did with Emiliani, he was unsuccessful in getting the university to make satisfactory offers. One exception to this was Epstein, who was offered a lab of his own at Chicago when the Institute learned that he might be accompanying Brown, Patterson, and McKinney to Caltech. Epstein, however, turned down the Chicago offer with the rationale that Urey’s presence in the department would always overshadow him.

As Epstein’s case illustrates, the Institute’s unusual abundance of senior and eminent scientists was certainly one of the reasons that the younger members of the Institute (even those like Brown who had appointments) chose to seek out new positions in the west. In an oral history interview Epstein pointed out that, at the time he left in the early 1950s, none of the young, up-and-comers of the Institute thought that Chicago was a place they could stay: “Most of the young faculty were in their thirties. They chose the

\(^{583}\) Stanley L. Miller to Thomas Jukes, December 14, 1988, Box 160, Folder 6, SLM. Indeed, Orgel was the first recipient of the Urey Medal.
good people from a large pool of first-rate young scientists, and there was not much room for new, upcoming young people.” Epstein’s case also illustrates, as do the cases of Brown and Claire Patterson, that new positions in geochemistry and cosmochemistry were opening up in the younger institutions of the West Coast, where departments of geology were eager to bring in isotope chemists and the AEC contracts they could attract.

In the 1940s Urey had been unable to sell the idea of an institute of nuclear chemistry and physics to a West Coast university. That Brown was able to successfully migrate, and to take his three junior colleagues with him to Caltech as part of a package deal, shows just how much the Chicago program’s success had changed the way universities thought of nuclear research. Brown and his junior colleagues made their home not in Caltech’s Divisions of Physics or Chemistry, but within its Division of Geology. The Geology Division at Caltech, influenced by the flood of new isotope geochemical research, approached Linus Pauling and asked him what the best way would be to bring geochemists onto the faculty. It was Pauling who suggested that they look into trying to woo Brown away from Chicago.

Brown brought with him to Caltech not only the Chicago model of conducting research, but the corresponding model of funding research; the initial year of his AEC contract had a proposed budget of $195,230, $23,580 of which went toward overhead. No doubt this helped Brown to convince the university to accept as reasonable his estimate of the cost of constructing a world-class geochemistry laboratory: $56,000 for

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584 Samuel Epstein, “Oral History Supplement Interview,” 1997, Box 84, Folder 19, SE.
585 Robert Sharp, “Geology Division, Robert Sharp’s history of the,” 4, Box A10, CDG.
equipment and $20,500 per year for personnel. Furthermore, while the Institute had lost one of their greatest advocates within the university when Hutchins’s presidency ended, Caltech’s president Lee DuBridge had run the Radiation Lab at MIT during WWII and was well acquainted with the ways of contract research. Urey would find a similar situation at UCSD, where Herbert York – the first director of the Lawrence Livermore Laboratory – was the new university’s first chancellor.

587 Brown to Ian Campbell. June 1, 1951, Box 9, Folder 1, LD.
Chapter 4

Calling for the New Prophet:
A Skeptic Argues for the Importance of Religion

“[Harold Urey] shakes his head in his characteristic way over talk about ‘the technical problems of the international control of atomic weapons.’ There are no technical problems, he says, only political ones. So from time to time he lays down his papers on the origin of the world and indulges in oratory, that he may not have to witness the end of the world.”\(^{588}\)

Profile in *The New Scientist* (1957)

At the University of Chicago after WWII, Harold Urey moved away from work with military applications and kept his research contracts small enough to avoid taking on a purely managerial role. From the end of the war to the early 1950s, he assembled a small team of graduate students, postdoctoral researchers, and lab assistants, and with them developed a research program focused on the history of the earth and the origin of life. As *The New Scientist* reported in 1957, Urey had gone from the tedious and technical work of separating isotopes to the more rewarding work of “Tracing the Genesis of the World.”\(^{589}\) During this time, Urey’s public rhetoric also underwent significant changes. By the time Urey became involved with NASA’s lunar science program in 1958, he was advocating publicly the need for a “great prophet” who could unite the grand view of the universe provided by science with the traditional moral teachings of the Judeo-Christian religions.

Since winning the Noble Prize, Urey had grown accustomed to public speaking. Something of a debater and an orator in high school and college, Urey felt comfortable at the podium. As he joked in a letter to one of his fraternity brothers when he agreed to

speak at the Alpha Delta Alpha reunion, “I [make speeches] very easily, in fact, could
wake up in the middle of the night and start immediately to discuss some subject that is
important to me.” Although he could joke about his ability to speak spontaneously on
the subjects that interested him, he took the subjects of his speeches very seriously.
Delivered in the midst of a great economic depression, the dawn of the Atomic Age, and
the tensions of the Cold War, Urey’s public speeches display a longtime concern with the
improvement and preservation of society. This chapter examines Urey’s public rhetoric
from the 1930s to the 1960s in his public speeches, popular articles, and non-specialist
conference participation. These three decades are broken into three periods: the years
between 1934 and the beginning of WWII, the immediate postwar years up to Urey’s
1953 involvement in the Rosenberg Trial and his subsequent harassment by the House
Committee on Un-American Activities, and from 1953 on.

Examining Urey’s public rhetoric during these years reveals a shifting position
that was shaped by changing social and political contexts, as well as by a more personal
intellectual and moral crisis. Urey began his career as a public scientist as an optimist,
defending science from its Depression-era critics. These speeches expressed the
progressive spirit of the chemical profession, combined with the critical view of
American capitalism Urey had learned in Copenhagen. In the first few years after WWII,
Urey’s optimism remained strong. Although terrible new weapons had entered the world,
the fearsome capabilities of these weapons could be marshaled as an argument for world
government and a reordering of social, economic, and political life. The political reality
of the Cold War put an end to Urey’s world government dreams and pushed him toward

590 Harold C. Urey to Louis M. Dyll, July 25, 1962, Box 100, Folder 10, HCU.
support of the Truman Doctrine and the Marshall Plan, in combination with an alliance of the Atlantic democracies. Urey’s failed intervention in the trial of Julius and Ethel Rosenberg, along with the death of his mother, initiated a crisis of conscience for Urey. After this point he began arguing for the maintenance of traditional morals.

This was a turn to religion, and not a return to the Church of the Brethren. Urey clearly was influenced by his own memory of his childhood religious practices – including daily family worship and prayer – and his illusions to the Ten Commandments and the Sermon on the Mount privileged those texts that the Brethren found most useful. But Urey still rejected a personal God and Biblical literalism of any kind. His Brethren upbringing had laid the foundation for his understanding of religious life, and the Brethren understanding of Gemeinschaft – which held the proper expression of Christian faith to be a communal affair – likely did influence his understanding of the religious-like life of the scientific community. However, he seems to have been far more influenced by his experiences in New York City in the early years of WWII, where he became a member of a community of East Coast intellectuals interested in reuniting the specialized arenas of science, philosophy and religion and creating an American conscience out of this reunification. Meeting theologians who seemed interested in revising their faith to fit the contours of science, such as the diocese of Christ Church, Oxford, further influenced the development of Urey’s new prophet.
The Interwar Optimist

Urey’s earliest speeches, delivered in the context of the Great Depression, were delivered partly in defense against Depression-era attacks on science. American science had entered the 1920s on a high note, having contributed to victory in WWI, and had in the process developed strong ties to American industry. Chemistry’s claim to cultural relevance was thus tied in no small part to its ability to raise American industrial strength to a level that at least matched that of Europe.\(^{591}\) Within their own publications in the early 1920s, the American chemists congratulated each other for having demonstrated their self-sufficiency and centrality to the American economy.\(^{592}\) Now, in the early 1930s, critics wondered if science and technology had not grown American industry and changed the labor market too quickly.\(^{593}\) As Kevles points out, some of the harshest attacks on science were often articulated by religious critics who saw faith in the improving power of scientific research as faith that would be better placed in religion.

Many academic humanists of the era shared a similar sentiment. In 1933 the humanist Robert M. Hutchins, who within only a decade would become one of the leading supporters of atomic science as President of the University of Chicago, told the University’s graduating class that the nation’s moral crisis was due to an over reliance


\(^{592}\) The efforts of the editor of the Journal of Industrial and Engineering Chemistry during this time are described in Germaine M. Reed, *Crusading for Chemistry: The Professional Career of Charles Holmes Herty* (Athens: University of Georgia Press, 1995), chap. 5: “The Mouthpiece of Chemistry.”

\(^{593}\) An account of the attacks against science during the 1930s can be found in Kevles, *The Physicists*, chap. 16. Accounts of the defense of science by scientists during this time can be found in Kuznick, *Beyond the Laboratory.*
upon reductionist methods: “the keys which were to open the gates of heaven have let us into a larger but more oppressive prison house. We think those keys were science and the free intelligence of man. They have failed us.”\footnote{Quoted in Kevles, \textit{The Physicists}, 239.}

During the 1930s, Urey did not agree with Hutchins. With his introduction to chemistry during WWI and his subsequent training at Berkeley, Urey had inherited a professional ethic that combined a research ideal imported from Germany but tempered in American industry, with a public service ideal developed during America’s Progressive Era. According to Gilbert Whittemore, the American chemists’ research ideal “encompassed both pure and applied research, with the line between the two becoming less and less well-defined.”\footnote{Gilbert Whittemore, “World War I, Poison Gas Research, and the Ideals of American Chemists,” \textit{Social Studies of Science} 5, no. 2 (1975): 139.} Meanwhile, the profession’s public service ideal held that research was not only a means to an end in the laboratory or factory, but also “a powerful tool for the general welfare,” and that science was “not the physical laws governing nature, but a procedure or an orientation which could be applied to the problems of society.”\footnote{Ibid., 140.} The strongest interpretation of this ideal held that the chemist was the quintessential “public man,” equally qualified to conquer technical or social problems with his rational methods and his ability to reshape and reorganize the physical world.\footnote{Ibid., 145.} The nation that invested most in chemical research would by extension become the leader of the civilized world.

Urey was certainly not blind to the problems of the Depression, nor did he take science’s responsibility in solving these problems lightly. Urey’s speeches made it clear
that he believed science to be much more than a utilitarian pursuit; along the lines of the public service ideal, he believed that science had a clear social responsibility to improve the human condition, even when it was not the direct cause of human suffering: “After all each of us has but one life to live and that life is our most valuable possession. We should not overlook and excuse the sentence of poverty, privation and disappointment on an innocent fraction of the population on the grounds that the result is good for the greatest number, nor should we excuse our responsibility in the matter because of the indirectness of the method.”

While he reminded his audiences that “it is probably true that people live better today in both an intellectual and spiritual as well as a physical sense than they ever have before in all history,” he also admitted that civilization still had many imperfections “emphasized by many discourses and more by much discomfort and disappointment.”

Urey was even willing to admit that the rapid advance of science and technology might have something to do with the economic and social woes of the Depression. While in the past industry had advanced at a pace that allowed workers time to adjust to any changes in the demand for labor or the skills required to work in local industry, an unfortunate “by-product” of the application of science to industry during the second industrial revolution was that “Today industries are often born, grow to maturity in a year or so and are then often murdered. The change is so rapid that people cannot adjust themselves to the change and even though the end result is beneficial, the intermediate

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599 Ibid., 223.
situation may result in actual want and privation for many people.\(^600\) No doubt drawing upon his own personal experience on his family’s unsuccessful farms in Indiana and Montana, Urey described the problems that the introduction of synthetic fabrics might pose to cotton farmers in the south, noting that it can take an entire generation to adjust a region’s agricultural practices to a new crop.

To the ideals of his profession, Urey’s experiences in Europe had added a critical view of American capitalism. Although he never directly invoked socialism in his Depression-era speeches, he did come close. In Copenhagen Urey had learned not to fear socialism: “There was a country with many socialistic things that have been practiced for many years, and the country was as free and democratic as my own.\(^601\) Urey speculated that “some fatal defect” in the economy had caused its breakdown in 1929 and that one of the greatest contributions that chemistry might make to society would be “to profoundly modify our social and economic institutions” to match the “greatly increased productive capacity” that the practical application of chemistry made possible.\(^602\) WWI and the Depression, he believed, were both primarily the results of old economic relationships that had envisioned neither mass production nor the modern interdependence of the nations of the world. Urey understood history to be a story in which “the general progress has been toward the left,” and expected “that by the year 2000 very marked changes must come in our social structure.”\(^603\) He admonished one audience of graduating college seniors “to take with you some idea and philosophy of life that will enable you to

\(^{600}\) Ibid., 226.
\(^{603}\) Harold C. Urey, “Science and the Humanities”, June 3, 1935, 14, HC Urey Papers, Box 140, Folder 7, UCSD.
transcend selfish advantage which you might secure, and moreover, I should like to suggest that you take an open mind toward social changes which in any case are inevitable, and which we should all welcome rather than resist.\textsuperscript{604}

So even as Urey was willing to consider the social and economic problems of the Depression as indirect consequences of science applied to industry, he was also quick to point out that since the end of WWI “no country has arranged its internal affairs in such a way that anything approaching the maximum production of material goods for peaceful purposes has been accomplished.”\textsuperscript{605} Just as the desire to win the war had been matched by a productive and efficient war industry, the desire for peace and “for the good things which science and technology can bring” must be matched by an active “production machinery”: “Our people wish those things which chemistry can bring to them, but for some reason our chemical plants are partly idle, our chemists unemployed, and our workmen are on some form of direct or indirect relief.”\textsuperscript{606} This was a problem that chemistry could not solve. He hypothesized that the true cause of the problem, as well as of “most of the serious difficulties of this century,” including WWI, communism in Russia, and the worldwide depression, were caused by “the great production of goods by scientific methods, together with archaic methods of distribution.”\textsuperscript{607}

Still, Urey was hopeful that the increased productive capacity made possible by science would modify the social and economic institutions of Western countries in beneficial ways. He told his audiences that if properly applied science and technology

\textsuperscript{604} Urey, “Science and the Humanities.”
\textsuperscript{605} Urey, “Chemistry and the Future,” 135.
\textsuperscript{606} Ibid.
\textsuperscript{607} Ibid., 139. Although he listed Russia’s turn toward communism as one of the “serious difficulties” of the century, Urey (not unlike other observers of the depression) looked favorably upon the Bolsheviks’ stated commitment to distributing the fruits of mass production to all citizens of the USSR.
could aid in the elimination of war and poverty and help to extend comfort and wealth to all social classes of all nations. If mass production was to continue, Urey argued, it must be matched by mass distribution and consumption: “Whatever that distributive system will be, it must distribute an abundance to many people and not to the privileged few only. If this is not done, we must abandon these mass production methods.”

With the application of scientific reason to the problem, Urey was confident that society could emerge from the Depression and enter a new and more economically just stage in its development. As was often the case in Urey’s speeches, his audience was faced with two mutually exclusive visions of the future: “if we act with courage our descendants will live in an abundance of necessities and luxuries the like of which we can not imagine. If we are not courageous, they may live with less than we have at present.”

As for the future of science, Urey was convinced that there were two fates possible. One was the further militarization of science. Here – foreshadowing his postwar concerns over atomic weapons – he imagined that science’s support of military activity could bring about “the complete destruction of our civilization.” Although scientists had lofty ideals, they could be diverted from these by “the stimulus of the belligerent and destructive human instincts.” This had been the case for German science leading up to WWI – and it had led to the development of work on the fixation of nitrogen for military purposes. He warned his colleagues at the American Association for the Advancement of Science that chemistry, contributing as it had to the design and construction of ever more explosive and effective weapons, “can and perhaps will destroy our European

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608 Ibid.
609 Ibid., 135.
610 Ibid., 134.
civilization.” He speculated that another war similar to WWI could not only destroy the wealth of nations, but could also “damage governmental regimes beyond repair and can make a world less safe for democracy or anything else.”\(^\text{612}\) But there was also the possibility that with the right encouragement scientists could instead work toward peaceful ends. Along these lines, Urey suggested to his colleagues that they should focus their activities on discovering chemical substances “which would stimulate such creative endeavor for the arts of peace.”\(^\text{613}\)

Because science could and often was diverted by the interests of the society within which it was practiced, Urey did feel that science prospered best when practiced in the proper political and social context. Science seemed to have blossomed in countries that had embraced liberal government and the development of humanism. Indeed, he suggested to a Pittsburgh audience that “the center of gravity of the sciences in the future will move with the center of gravity of liberal government.”\(^\text{614}\) This was perhaps why chemical research – so firmly established in German industry prior to the war – had now moved toward North America. Urey did not articulate any reasons for why science should align itself with liberal government, or for why his audiences should expect to see the western democracies embrace further social change. These things he took for granted as natural conclusions.

\(^{614}\) Ibid., 223.
Science and Religion in the Interwar Years

In these early speeches, Urey did not show much sympathy for his religious critics’ insistence that too much faith had been placed in science. There is no inkling in these early speeches that Urey felt any need to defend the traditional moral teachings of religion. Instead, he framed his description of the chemical and scientific profession in explicitly religious language, and at times claimed outright that science was a religion. In a speech delivered at the dedication of the new buildings of the Mellon Institute of Industrial Research, he told his audience that the “real purpose” of scientific activity was “to contribute something somewhere and at sometime to the sum of human satisfaction, as man lives for a brief span of time on this small planet.”\textsuperscript{615} While much of what the public would recognize as chemistry’s contributions to life were in the physical realm, the scientist’s highest aim was to contribute to the spiritual and intellectual satisfaction of mankind. In its applied form, chemistry contributed to these higher pursuits by reshaping the physical world of man in such a way that it allowed him to transcend the animal world, freeing him from preoccupation with his physical needs and wants. The ultimate aim of applied chemistry was to “abolish drudgery, discomfort and want from the lives of men and bring them pleasure, comfort, leisure and beauty.”\textsuperscript{616} In its purer form, chemistry and its sister sciences helped to broaden mankind’s intellectual horizons.

This religion of science was defined not in words but in actions, and was thus only meaningful to its practitioners. As for the men who practiced the religion of science,

\textsuperscript{615} Ibid.
\textsuperscript{616} Ibid., 227.
Urey assured his audiences that he and his peers were special members of the community. Their primary objectives were not “jobs and dividends,” and they were “satisfied with modest salaries,” knowing that their work will improve the human condition. In a moment of bravado, he told the crowd in Pittsburgh that “You may bury our bodies where you will, our epitaphs are written in our scientific journals, our monuments are the industries which we build, which without our magic touch would never be.”\textsuperscript{617} He went on to suggest that the plants where chemical processes were applied to industry should indeed “be regarded as national monuments,” although he admitted that “They shun the public eye, are located in small and isolated towns often with dingy surroundings and no multitudes make yearly pilgrimages to these Meccas.”\textsuperscript{618}

But the satisfaction of improving the lives of others through their contribution to industry was not the greatest satisfaction for the scientist – nor was it the only reason he was willing to sacrifice his own personal wealth. In one of his earliest public addresses after winning the Nobel Prize, delivering the commencement address at his alma mater the University of Montana, Urey introduced a higher form of satisfaction – direct communion with the laws of nature. He compared the work of the scientist to that of other intellectual workers who “find [themselves] engaged in a search for truth – let us not try to define it too precisely – as well as the beautiful.”\textsuperscript{619} This search for truth and beauty demanded more than a sacrifice of personal wealth and well being, it required complete supplication to the methods of science and objectivity. These searchers were defined by their willingness to regard their subject matter above themselves, and to make

\textsuperscript{617} Ibid.
\textsuperscript{618} Ibid., 224.
\textsuperscript{619} Urey, “Science and the Humanities.”
whatever sacrifices were necessary in order to take a disinterested and unprejudiced position. Thus he defined objectivity as a type of transcendence born of sacrifice – as an “attempt to dissociate ourselves from the subject and to consider it from a somewhat higher plane than our own individual desires, ambitions and fortunes would dictate.”

In his 1938 address to the American Association for the Advancement of Science, the rewards of this sacrifice and transcendence took the form of communion: “if [the scientist’s] postulates are in accordance with what we call natural law, nature unlocks her secrets with an amazing ease. When this occurs there is a communion between scientists and the eternal laws governing the behavior of this universe that is very intimate indeed. This communion represents the highest reward which a scientist receives for his services, and it is this that furnishes the major driving force for all his activities.”

While Urey did in these early speeches address the ethicality of scientists, he did not take the position that would define his Cold War speeches – that scientists were ethical only because they had been raised in a religious society. Rather, he took the position that one useful application of science to everyday life would be to educate the public about science’s ideals and ethics. He noted that in the sciences and the humanities, “these fields are dominated by an honesty of purpose, a sincere desire to find the best.”

Drawing upon his discussion of the sacrifices and the transcendence of research, he suggested that the scientist and the humanist were able to “submerge” their own individuality to a more important purpose. While they may remain concerned to an extent

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620 Ibid.
about their own personal well being, their demands were rarely beyond reason. And their ethics with regard to the truth were beyond reproach:

None of us would hold to an incorrect theory or experiment of our own in the face of proof to the contrary. We would not perpetrate a lie or an inaccuracy on our fellow-workers or on those to follow us, for our own selfish ends. Largely throughout all our science we have eliminated emotionalism. We do not try to win our debates by sarcasm or by any other tricks which might gain our immediate ends, but which would give us an inaccurate value of nature, and what is more, we will not tolerate any such attitude on the part of other scientists. If scientists do those things, they soon become complete pariahs whom all ignore. It is this sincerity of purpose and attitude found among the scientific groups, and also existing in the humanities, which I believe is the most important lesson science brings to these other fields of work.⁶²²

Thus the scientist was a shining example of moral integrity. This was a result of his sublimation of himself to his subject, not of anything brought into the scientific profession from its surrounding culture.

Aside from science being a type of religious experience for its dedicated practitioners, there was little space for religion in Urey’s early rhetoric. The progress of science – especially that of astronomy, biology, geology, and physics – had “driven clouds of superstition away” from the minds of man:

In the past many of our sister sciences have contributed notably to the broadening of man’s intellectual horizon. Astronomy gave him a proper perspective of his own importance and his own position in this universe. His earth is not the center of the solar system, nor is his solar system the center of this galaxy. At the same time that he was robbed of his central position in the physical universe what grandeur has been spread before him as our knowledge of astronomy grows! Biology has robbed him of his miraculous creation by one or another anthropomorphic god, and has placed him in the lowly position of one of the animals, that one, in fact, who in this particular geological age dominates the earth, but at the same time the grandeur of an organic evolution with time has been spread before him. Geology has shown him the long length of time that man and

⁶²² Urey, “Science and the Humanities.”
his lowly ancestors have existed on this planet, and in fact that life on this planet has been nearly coexistent with the planet.\textsuperscript{623}

Chemistry, meanwhile, had made the world more explosive but also more wonderful. There was no indication in Urey’s early speeches that he felt that these transformation away from traditional ways of life could have negative consequences. He seems, at least in the 1930s, to not have cared much at all if science robbed man of his gods. If anything, this removal of “superstition” was part of the progress Urey was celebrating, and it was a small sacrifice in the attainment of the grandeur of the narrative that was being produced. It was rational choices, not traditional morals, that would transform the world into one of leisure, abundance, and peace.

**Reordering the World for the Atomic Bomb Era**

“Among my scientific colleagues few have devoted themselves wholeheartedly to the cause of enlightenment as Professor Harold Urey. He has shunned no sacrifice of time and energy when it came to serving our important aim. Whoever is himself filled by the passion for scientific research knows how hard it is for a man of our kind to abandon his own aims for a length of time and to serve a social task, simply out of a felling of duty and of necessity. I may say in the name of all of us that we are thankful to him for his untiring unrelenting efforts and we hope that his words which are based on sound knowledge and on a feeling of responsibility will find fertile soil.”\textsuperscript{624}

Albert Einstein

NBC Radio Address (1946)

\textsuperscript{623} Urey, “Chemistry and the Future,” 134.

\textsuperscript{624} Albert Einstein, “Radio address by Professor A. Einstein” (NBC Network, November 17, 1946), Box 2, Folder 1, ECAS.
After WWII and the introduction of atomic warfare, Urey’s hope for the improvement of the world remained strong. Through the Atomic Scientists of Chicago, Urey and his colleagues attempted to consolidate the opinion of their fellow scientists on their role and responsibilities concerning atomic power so that they could present a united front before Congress in the hopes of influencing atomic policy. By November of 1945, the atomic scientists had formed a lobby that they named the Federation of Atomic Scientists (they later changed the name to the Federation of American Scientists), and in their first year attracted 2,500 dues-paying members, established offices in Washington DC, and hired former newspaper reporter Michael Amrine as press agent. Meanwhile, with his scientific hero, Albert Einstein, Urey helped to found the Emergency Committee of Atomic Scientists. The Emergency Committee, composed as it was of eminent scientists, was formed to assist the FAS in fundraising activities for the National Committee on Atomic Information, which had as its primary goal public education on atomic energy and its societal implications. Fearing too casual an acceptance of atomic weapons, the atomic scientists hoped that an educated public would fear the bomb’s destructive capabilities as they did. Thus within the first year after the dropping of the bomb, the scientists had developed a two-pronged approach that focused efforts on public education and political lobbying.

At the close of 1945, The New Yorker reported on the educational activities of the atomic scientists, and pronounced that seeing the scientists “pop out of their cloisters all

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626 The scientists’ movement after the war is described in Alice Kimball Smith, A Peril and a Hope: The Scientists’ Movement in America, 1945-47 (Chicago: University of Chicago Press, 1965); Paul Boyer, By the Bomb’s Early Light: American Thought and Culture at the Dawn of the Atomic Age (New York: Pantheon, 1985).
over the place in order to issue warnings” about the atomic bomb was evidence that the social responsibility of the scientists was indeed “well developed.” As a charismatic and articulate member of the Committee, Urey played his part. Urey made multiple visits to Washington DC, where he testified before the Senate Special Committee on Atomic Energy. While Einstein wrote letters from Princeton asking for donations on behalf of the Emergency Committee, Urey toured the country addressing various interested and influential organizations. The “heavy-water man and Nobel Prize winner” told reporters, “I know the bomb can destroy everything we hold valuable and I get a sense of fear that disturbs me in my work. I feel better if I try to do something about it.” When asked if he felt guilty for having unleashed the bomb upon the world, Urey told reporters that guilt didn’t enter into it. While he regretted helping to weaponize atomic energy, “Atomic energy is in nature… It can’t be concealed. Scientists can’t prevent modern war by refusing to do scientific work. The solution is political.” Urey was convinced that the possible peacetime uses of atomic energy would eventually eclipse the threat of the atomic bomb, but only if adequate controls could be placed on atomic weapons. He was determined to see controls imposed before returning to his own research program, and he hoped to see this happen soon.

Toeing the Emergency Committee’s party line in a Collier’s article that the Committee reproduced and distributed, Urey told the world that he was frightened of the bomb: “I write this to frighten you. I’m a frightened man, myself. All the scientists I

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627 “The Talk of the Town, Notes and Comment,” 23.
628 Quoted in Ibid.
629 Ibid., 24.
know are frightened – frightened for their lives – and frightened for your life.”

In the place of Urey’s typical descriptions of science as a search for truth and beauty, man’s potential for good and evil had taken up residence. While Urey in his pre-WWII speeches had speculated that science might one day through man’s belligerence destroy western civilization, this potentiality now took on the character of an impending apocalypse and moved into the foreground of Urey’s rhetoric. The invention of the atomic bomb had put humankind “face to face with the powers which, philosophically speaking, are supreme in our universe,” and unleashed once and for all the “limitless powers of the universe as developed by the limitless imagination of Man.”

This had accomplished nothing less than to reorder time and inaugurate a new calendar era: “This is indeed The Year Atom Bomb One,” Urey wrote, “It has opened most ominously. We must waste no time if we plan to be alive in A.B. 5 or A.B. 10. Atomic war could unleash forces of evil so strong no power of good could stop them. The main race, between man’s powers for evil and his powers for good – that race is close to a decision. You must think fast. You must think straight.”

When asked how far back in history one must search before finding a discovery as significant as that of atomic energy, Urey argued that the closest thing he could imagine was the discovery of fire: “If one thinks then that we have a discovery, made in the last few years, for which we cannot find an analogy except by going to prehistoric times, we must expect that we have before us a new source of energy that is likely to create an emergency that will last much more than a matter of months or a year. It will be an

630 Harold C. Urey and Michael Amrine, “I’m a Frightened Man,” Collier’s, January 5, 1946, 18.
631 Ibid., 51.
632 Ibid.
emergency that will continue for many years in the future.” Just as fire had helped fundamentally to reshape the lives and social structures of prehistoric humans, Urey was certain that atomic energy, too, would reshape modern society.\footnote{Proceedings: The Social Task of the Scientist in the Atomic Era - A Symposium, Emergency Committee of Atomic Scientists", November 17, 1946, 12, Box 3.015, Folder 15.4, LP.}

For Urey, and for many of the scientists involved, atomic weapons demanded that the world be reorganized.\footnote{The tendency toward world government among atomic scientists during this period is described in Boyer, By the Bomb's Early Light, chap. 3 “Atomic-Bomb Nightmares and World-Government Dreams.”} His prewar speculation that the many nations of the world might inevitably become a “world state” by the year 2000 now became a plea for world government.\footnote{Urey, “Science and the Humanities.”} With this aim in mind, Urey, Einstein, Edward Teller, Harrison Brown, and Leo Szilard all personally endorsed the United World Federalists. The prospect of world government was particularly attractive at the University of Chicago, where in 1946 President Robert M. Hutchins organized a committee of professors to debate and draft a “World Constitution.” After two years the committee produced a draft that began with the Preamble, “The age of nations must end, and the era of humanity begin.”\footnote{Committee to Frame a World Constitution, Preliminary Draft of a World Constitution (Chicago: University of Chicago Press, 1948); Joseph Preston Baratta, The Politics of World Federation: From World Federalism to Global Governance (Greenwood Publishing Group, 2004), chap. 15, “Robert M. Hutchins: Framing a World Constitution.”}

Urey found a precedent for world government within the scientific community. He told his Collier’s readers that he knew scientists from every corner of the world, and that he had learned before the war that all scientists spoke the same language. Now that the atomic bomb had entered the world, the world scientific community would become even closer, brought together as it was by “a common fear and a common pledge and a common hope.”\footnote{Urey and Amrine, “I’m a Frightened Man,” 51.} In this early plea for world governance, Urey even expressed
confidence that the Russians would want to be involved. After all, Russia had her own native scientists, and they would carry the message of fear to their leadership: “If you realize, as scientists do, that Russian science includes some of the best brains in the world today, I think you will understand, first, that Russian leaders must naturally be frightened of the possibilities of this power and, secondly, that it will not be long before they also are masters of it.” Urey pointed out that no country had been more devastated by WWII than Russia, and the Russians had come to understand this war’s great consequences as had no other country: “No one who understands atomic war wants anything but peace.”

In a moment of naiveté, Urey imagined a meeting of Russian and American leaders, along with their scientific advisers, in which the scientists acted as mediators between the two groups: “Scientists will have no trouble understanding one another. When they meet I think their recommendations will be almost unanimous.”

**The Russian Problem and the Atlantic Union**

Urey’s speeches from these immediate postwar years advocated strengthening the United Nations into a sovereign world government that had “adequate powers to prohibit atomic bombs” and “to police the world to see that such laws are obeyed.” The alternative, which Urey always claimed was the probable result of a lack of world government, was to cover the world in “armed camps” – outposts of American missiles matched by those of the Russians – and live “in constant fear that the other man’s itchy

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638 Ibid.
trigger finger would start something moving in a very short time.”

In addition to policing the world for atomic weapons, the world government would also help to insure that there was no war: “First of all, let us note that the atomic bomb is not the fundamental problem at all that we have to face. The fundamental problem is war. If there is another war, atomic bombs will be used.”

Urey’s hope that the Russians would participate as partners in the world governance of atomic weapons was short-lived. In March 1947 the Russians rejected the US-proposed Baruch plan. The plan was based on Secretary of State Dean Acheson and Chairman of the Tennessee Valley Authority David Lilienthal’s Report on the International Control of Atomic Energy (largely written by J. Robert Oppenheimer). The American statesman and financier, Bernard Baruch, presented it before a session of the United Nations Atomic Energy Commission on June 14, 1946. The plan proposed an exchange of basic scientific information among all nations, control of nuclear power that would limit its use to peaceful purposes, elimination of atomic weapons and other weapons of mass destruction, and international inspections to be performed by an international Atomic Development Authority. The Authority would also oversee the mining and use of nuclear materials, and would grant licenses to countries pursuing peaceful nuclear research.

Urey had supported the Baruch plan’s establishment of the Atomic Development Authority, writing to colleagues in the Atomic Scientists of Chicago that “Mr. Baruch’s proposal was that the ultimate decision in regard to this matter of the atomic bomb shall

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640 Ibid.
641 Ibid.
be made by the Atomic Development Authority, and not by the nation-states, and that proposal is the only one that makes sense from the standpoint of real prevention of the use of any weapon. We are not afraid of the state of New Mexico, in spite of the fact that atomic bombs are made there, only because New Mexico has no power whatever to use those bombs.” Furthermore, Urey was happy that the plan would force the US to dispose of its nuclear weapons: “Since I do not believe that atomic bombs should be made by anyone none should be possessed by any government of any kind, I cannot look with favor upon any other country’s learning how to make bombs, and I can only hope that arrangements can be made such that the United States will forget this art.”

The Russian rejection of the Baruch plan did not completely deter Urey from pursuing some form of world government. Urey became an ardent supporter of Clarence K. Streit’s Atlantic Union, and favored a nuclear alliance that would strengthen the West against the Russian threat, even at the high cost of an arms race. This led to a split amongst the atomic scientists over the best course of action after the Baruch plan. The other side of the split was held by Urey’s colleague and compatriot Leo Szilard, who argued that any union without Russia and the resulting nuclear arms race would lead to war; Szilard instead held out hope for a world-wide reconstruction of economic, social, and political relations. The split between the two men was obvious by the end of the

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643 Harold C. Urey to Atomic Scientists of Chicago, September 30, 1946, Box 11, Folder 5, HCU.
644 Ibid.
645 Urey and Szilard’s difference of opinion and their leadership roles in defining those positions for their scientific colleagues is described in Helen S. Hawkins, G. Allen Greb, and Gertrud Weiss Szilard, eds., Toward a Livable World: Leo Szilard and the Crusade for Nuclear Arms Control, vol. III, The Collected Works of Leo Szilard (Cambridge MA: MIT Press, 1987), 4; Streit had been proposing an Atlantic Union in order to preserve peace in the face of fascism since as early as 1939. After WWII, the plan was adapted to the problems of the Cold War. Clarence K. Streit, Union Now: A Proposal for a Federal Union of the Democracies of the North Atlantic (New York: Harper & Bros, 1939); Clarence K. Streit, Union Now with Britain (New York: Harper & Bros, 1941).
1940s, but had begun developing earlier. Urey claimed in a radio interview in 1949, after
the Russian’s had demonstrated their own atomic capabilities, that he concluded only two
months after the Baruch plan was introduced that the Russians would not agree to any
effective control measures and “I began immediately to revise my opinion as to what the
proper course for the people of the United States should be.” The Russians’ refusal of
the Baruch plan had strengthened Urey’s distrust of the Soviet leadership and his support
of the Truman Doctrine and Marshall Plan as steps toward an Atlantic Union:

I believe that the real foreign policy of the United States – that is, the
foreign policy dictated by the overwhelming majority of the people of the
United States – is approximately stated in the Truman Doctrine. We do not
intend to become a Soviet Socialist Republic and will accept atomic war
first. We are determined to fight the Communist dictators of Russia in any
way possible and in any part of the world. We as a people have adopted
this view because of our observation of the behavior of the cruel and
ruthless dictatorship of Russia, as we have observed it in operation since
1917, and particularly in light of discussions in the United Nations since
the war. I further believe that we have adopted this view because we
believe that the USSR has aggressive intentions toward her immediate
neighbors. This has been abundantly confirmed since the war. We further
believe that these aggressive intentions are probably not limited to the
European countries. The difficulty between the USSR and the United
States is partly a power conflict, it is true. But the power conflict is
founded upon a profound difference in philosophy.

This profound difference in philosophy between the US and Russia, now backed up by
atomic weapons on both sides, led Urey to conclude that war between the two powers
was more likely than ever – particularly if both powers felt that they could win such a
war. “The only course of action which will enable the United States to avoid war,” Urey
argued, “is one which will make the West stronger. I have maintained since 1946 … that
the most effective way to increase the strength of the West is through the formation of a

647 Ibid., III:58.
federal union of the Atlantic democracies. I believe that the Truman Doctrine, the Marshall Plan, the Atlantic Pact – all of which have been approved by Congress – are all steps leading in this same direction.”

Thus Urey eventually became a supporter of the development of the hydrogen bomb and of stockpiling atomic weapons. These measures, along with the adoption of the Atlantic Union, were the only way “the Western democratic powers [would] be able to maintain an overpowering political, commercial, military, and ideological strength. Only in this way can we have an enormous unbalance of power, so that perhaps one side does not attempt to start a war because it recognizes that it cannot win, and the other side does not need to start a war because it knows that the weaker side will not dare to attack.” As he explained in a letter in 1957, he had given up on a world constitution, believing that “those who engage in this sort of thing are completely unrealistic.”

The Cold War at Home

By the time political scientist Frederick Schuman concluded that “World government has become for this generation the central symbol of Man’s will to survive, and of his moral abhorrence of collective murder and suicide,” the Korean War was in full swing and the dream of world government was already all but dead. In addition to the Russians’ refusal to participate in world government, American political reactions to

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648 Ibid.
650 Harold C. Urey to Philip Iseley, August 9, 1957, Box 47, Folder 1, HCU.
the Cold War presented a Sisyphean challenge for intellectuals who questioned the wisdom of excessive security-loyalty measures or the perils of isolationism. As early as 1946, Urey had been attacked on the floor of the US House of Representatives for opposing the May-Johnson Bill that would have extended the absolute military control and clandestine character of nuclear research that had prevailed during wartime. Here, J. Parnell Thomas, a member of the Military Activities Committee and ranking Republican member of the House Committee on Un-American Activities (HUAC), attacked the alternative McMahon Bill (which advocated civilian control of atomic research and resulted in the formation of the Atomic Energy Commission) as being “the creature of impractical idealists.” Thomas likewise attacked Urey, labeling him a “one-world-minded” person who was “blinded” by an “intense ardor for a better world.”

As Jessica Wang has argued, such abuse at the hands of politicians and the anxiety of secrecy and surveillance in post-WWII America pushed many progressive left-wing scientists like Urey toward a more cautious and conservative Cold War liberalism.

Although the FAS succeeded in killing the May-Johnson Bill, the Federation and the other political organizations of the atomic scientists were put on the defensive almost immediately as their ideology of international cooperation and intellectual freedom clashed with what Wang has described as “the postwar preoccupation with national security and protection of the ‘secret of the atom.’”

As Walter Gellhorn observed at the time, “The world’s polarization into opposing forces has cast a shadow upon the

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652 Thomas’s statements on the House floor are found in Wang, *American Science in an Age of Anxiety: Scientists, Anticommunism, and the Cold War*, 48.
traditionally accepted values of scientists. In days gone by science was broadly viewed as an unselfish effort, international in scope, to expand knowledge for the benefit of all mankind. Today science has come to be regarded somewhat in the nature of a national war plant in which a fortune has been invested.\textsuperscript{655}

As they found their energies redirected toward the defense of their own civil liberties, the political lobbying efforts of the atomic scientists were severely hampered. In early 1947, Librarian of Congress Luther Evans accused the FAS of being “ham actors” who had used a “hell-fire and damnation approach” to convince the public that world government was the only thing to prevent the end of the world.\textsuperscript{656} The Emergency Committee spent much of 1948 rallying to the defense of physicist and head of the Bureau of Standards, Edward U. Condon, who was attacked viciously by HUAC. The Committee organized a dinner in support of Condon that was meant to “indicate to other scientists that we, as the scientific fraternity, will stand together.”\textsuperscript{657} The dinner expressed the scientists’ confidence in Condon’s loyalty, but also expressed their disapproval of HUAC’s activities. In his letters to prominent scientists, Urey justified the actions of the Committee on behalf of Condon. Urey explained that the Committee felt HUAC to be a disreputable and unconstitutional committee, and that it posed as great a threat to science as fascism or communism: “Attacks on scientists begin with scurrilous remarks but I believe that they end, both in Nazi Germany and in Communist Russia, by the murder of scientists – as well as other people.”\textsuperscript{658} In 1955, Urey was still defending Condon, and

\textsuperscript{656} “Higinbotham Answers ‘Ham Actor’ Charges In Atomic Research,” \textit{Washington Evening Star}, February 19, 1947 Box 1, Folder 7, ECAS.
\textsuperscript{657} Harold C. Urey to Charles A. Kraus, March 23, 1948, Box 18, Folder 33, ECAS.
\textsuperscript{658} Harold C. Urey to Frank Jewett, March 23, 1948, Box 18, Folder 33, ECAS.
still using the same argument, only now in print: “Twenty years ago, Nazism began its rise in Germany, and, with some notable exceptions, German scientists did not stand up very well to this rise of tyranny. It came so insidiously. Unpopular people and ideas were attacked, and it was so convenient to look the other way and be busy with one’s very, very, important tasks. … The state claimed that it was punishing criminals, but it became the chief criminal. Most of our scientific friends looked the other way.”

Urey refused to look away. In 1948, the Emergency Committee even considered challenging HUAC head-on by putting Harrison Brown up for Congress in the Second District of Chicago against incumbent Richard B. Vail, a Republican and a leading member of HUAC. The Committee believed that “Brown’s nomination would change the campaign from a local congressional election into a general fight of the atomic scientists of America against the methods practiced by the Un-American Activities Committee, for maintaining civilian control of atomic energy, and for a constructive foreign policy instead of Vail’s isolationist demagoguery.” This plan came to naught.

The world had changed, and world government advocates like Urey now found their loyalty to their country in question. By early 1947, the stress of working for world government while being attacked by HUAC had exhausted Urey; he wrote to Einstein that his doctors had ordered him to avoid outside activities: “I find that I am able to carry my university work and that is about all. Otherwise I become very tired, unable to sleep, and generally quite unable to take care of any of my work.” As one of the Emergency Committee’s most active public speakers, Urey knew that he could not walk away from

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660 Harold C. Urey, “Telegram to Trustees”, October 13, 1948, Box 1, Folder 16, ECAS.
661 Harold C. Urey to Albert Einstein, April 15, 1947, Box 1, Folder 13, ECAS.
the Committee without leaving it severely weakened. But he pleaded with Einstein to
consider adding “other outstanding men” such as J. Robert Oppenheimer to the
Committee who could add their prestige to the group while shouldering some of the
burden.

The attacks continued. As Sir James Chadwick summed it up in 1953, “Urey was
very badly treated by the American authorities.”\footnote{Quoted in Cohen et al., “Harold Clayton Urey, 29 April 1893-5 January 1981,” 643.}

When in 1950 he was scheduled to
give a speech in Helena, Montana, Montana Congressman Mike Mansfield contacted the
FBI requesting information on “the loyalty of Dr. Urey” after “a number of individuals
have questioned his Americanism.”\footnote{Memo, C.E. Henrich to A.H. Belmont, June 21, 1950, Harold Urey, Bureau File 116-18315, Vol. 1.}

Urey had by then already been investigated by the
FBI and had been found to belong to several Communist front organizations.\footnote{The “Communist front and Communist dominated organizations” to which the FBI determined Urey belonged included the American Committee for Democracy and Intellectual Freedom, the American Committee for Protection of Foreign Born, the American Committee to Save Refugees, the Coordination Committee to Lift the Embargo on Loyalist Spain, the Friends of Abraham Lincoln Brigade, the Greater New York Emergency Conference of Inalienable Rights, the League of American Writers, the National Council of American-Soviet Friendship, the National Emergency Conference for Democratic Rights, the Spanish Intellectual Aid Committee, the Veterans of Abraham Lincoln Brigade, the Medical Bureau and North American Committee to Aid Spanish Democracy, and the National Federation for Constitutional Liberties.}

In September 1948, Urey was implicated in the HUAC investigation of the analytical
chemist, Clarence F. Hiskey. Hiskey had been chosen to work on the Manhattan Project
based on Urey’s recommendation, and HUAC accused him of being an active member of
the Communist party and of giving atomic information to a Soviet espionage agent.\footnote{“Brooklyn Professor, Called a Red, Bars House Queries on Atom Data,” \textit{New York Times}, May 25, 1949, 1.}

Based on Urey’s associations and his outspoken political views, Howard Rushmore, a
reporter for the New York \textit{Journal-American}, argued before the Illinois Seditious
Activities Investigation Commission that Urey should be barred from continuing to work

on atomic projects, and that “an educator who could not ‘discern’ the true character of a
Communist-front organization should be prohibited by law from teaching.” Urey
responded to these allegations with the argument that secrecy and witch-hunts only hurt
American atomic research and, as a result, contributed to Russian success.

Urey’s confrontations with HUAC and the FBI were exacerbated when he became
involved in the highly publicized espionage trial of Julius and Ethel Rosenberg, who were
convicted of conspiring to pass atomic secrets from Oak Ridge and Los Alamos
employee, David Greenglass, to the KGB. The case was brought to Urey’s attention by a
woman who went to see him speak against a Congressional candidate at a Jewish Temple
in Chicago. The woman later appeared in Urey’s office at the university and left some
literature about the case with Urey. “Approximately two days later a transcript of the
[court] record appeared on my desk,” Urey later wrote to Joel Hildebrand:

I am a curious person so I read it. It took a week of evenings to go through
this. Half-way through I thought they were guilty as h---. I finished
reading the transcript. I was shocked at the type of proceeding that passed
for justice in a law court of the United States. I was shocked by what
passed for evidence. I was shocked by the complete lack of any pretense
of judicial objectivity toward the accused. I was frightened that such
proceedings could take place in the United States. You will remember this
was at the time of the McCarthy hysteria and the Korean War.

Urey had little success convincing his colleagues in Chicago of the weakness of the
state’s case against the Rosenbergs. Still, Urey was convinced that the Rosenbergs’
conviction and imminent execution were miscarriages of justice that would come to

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666 Rushmore is quoted in George Eckel, “Red Inquiry Hears Attack on Dr. Urey,” New York Times (New
York, NY, April 24, 1949), 30. Rushmore also listed several other University of Chicago professors,
including the philosopher Rudolph Carnap, as belonging to Communist front organizations.
667 “Secrecy on Atom Called Excessive,” New York Times, September 25, 1949, 8; William G. Weart,
668 Harold C. Urey to Joel H. Hildebrand, August 10, 1959, Box 43, Folder 6, HCU.
embarrass the United States. In December 1952, Urey sent a letter to President Harry S. Truman and asked that their death sentences be commuted, writing that “I believe the Rosenbergs are or have been Communists or very sympathetic to Communist ideas. I regard such people as unreliable generally, but I do not believe in punishing people unless they commit crimes.”

Urey did not allege that the Rosenbergs were innocent. As he wrote to Condon only one month after his letter to Truman, he was “most unhappy to be mixed up in the Rosenberg case, and they may be just as guilty as hell, too.” Rather, Urey felt that the trial itself had been a violation of due process, that the prosecution was based on perjured testimony, and that the death sentences were too severe. On June 12, 1953, one week before the Rosenbergs were to be executed, Urey sent a telegram to President Dwight D. Eisenhower. Urey had failed to secure an appointment with the Attorney General, and now pleaded for a personal audience with the President himself, during which he could present his understanding of the case and the improbability that the Rosenbergs could have passed atomic information to the Russians, as well as his skepticism as to the reliability of David Greenglass’s courtroom testimony.

The FBI and HUAC took a renewed interest in Urey after his support of the Rosenbergs. In 1953 the Bureau interviewed Urey’s Hyde Park neighbors only to find that he “apparently leads a very quiet scholarly life.” Interviewing his colleagues,

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669 Harold C. Urey to Harry S. Truman, December 16, 1952, Box 90, Folder 37, HCU.
Willard Libby told the FBI that Urey was not disloyal but simply “naïve and innocent concerning political matters and that this innocence combined with Urey’s renown as a scientist has caused many groups to seek him out either as a speaker or a supporter for their activities.”\textsuperscript{673} While Libby defended Urey’s championing of justice in the Rosenberg case, and was sure that he would “never knowingly reveal any classified information which was entrusted him,” he told the FBI that he “would conclude that Urey is so naïve and innocent as to constitute a security risk” if Urey joined a group to attempt to release the Rosenbergs’ co-conspirator, Martin Sobell.\textsuperscript{674} When HUAC published its report on the Rosenberg and Sobell affair, they characterized Urey and his many unwitting ties to Communist organizations as “a significant illustration of how the various Communist organizations, each created to accomplish a specific purpose, form separate but unified parts of the Communist conspiratorial system. … No clearer example [than Urey’s] could be desired of how the long-range plans of the Kremlin, and the individuals recruited to implement them, maintain their continuity despite a maze of forms and names.”\textsuperscript{675}

**A Crisis of Conscience**

The execution of the Rosenbergs in June 1953 was followed only three months later by the death of Urey’s moral compass, his mother Cora. Urey’s Royal Society biographers do not mention the death of Urey’s mother, but they do state that it was in

\textsuperscript{673} Ibid.  
\textsuperscript{674} Ibid.  
\textsuperscript{675} US House of Representatives Committee on Un-American Activities, *Trial by Treason*, 33-34.
this year that Urey’s hopeful attitude toward the improvement of society changed. They wrote, “But soon the execution of the Rosenbergs left him with a severe mental trauma. He did not consider the atomic secrets that he, himself, had helped to create sufficiently important to justify such a barbaric punishment.”

It was in this year, in the month that the Rosenbergs were executed, that Urey introduced a new speech to his repertoire that he titled “The Intellectual Revolution.” This speech, versions of which he presented for the next ten years, contained a more developed view of the history of science and its effects upon society than Urey had ever presented before. It was a story of two worlds – that of the scientist and that of the public. While the specialist scientist produced knowledge at an increasingly rapid pace, the public only slowly absorbed this new knowledge. This growth in scientific knowledge had produced a drastically different view of the universe and man’s place within it, and when this knowledge was fully absorbed might change “our philosophy of life and our ideas as to what men and women are” to a degree that matched that of the Reformation. It was a subject that needed to be addressed, Urey felt, because the failure to adjust properly to these changes had led countries such as Germany and Russia to adopt pseudoscientific societies based upon flawed understandings of science.

Urey’s intellectual revolution began with the introduction of the heliocentric theory of the solar system proposed by Copernicus, refined by Kepler, and systematized by Newton. The revolution continued with a summary of the great developments of the 19th century. First was the rise of chemistry “from almost one of the black arts to a great

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677 Harold C. Urey, “The Intellectual Revolution - Duke University Commencement Address,” typescript, June 1, 1953, 1, Box 140, Folder 5, HCU.
and exact science,” with the introduction of the elements and the exploration of their properties, and culminating with the emergence of the periodic system of the elements. Next came the discoveries in electricity and magnetism, the properties of light, and the laws of thermodynamics. Most revolutionary of all, Urey claimed, was the discovery of biological evolution. All of these new developments had helped to reorder the world of human beings, and some had even caused science and religion to come into conflict.

“[The] ‘conflict of science and religion,’” Urey wrote, “can be more nearly dated from the publication of ‘The Origin of Species’ in 1859 than from any other event.”678 However, Urey also claimed that “the authors of these ideas and developments were themselves mostly very sincere and devout followers of organized religion and in no way intended to disturb, much less destroy, the religious beliefs of their time.”679 He also claimed that these conflicts were only “storms in teakettles or at least not more than summer thunderstorms.”680

The great discoveries and developments of the 20th century, Urey believed, deserved much greater concern. First among these was the discovery of relativity and the development of quantum mechanics. Unlike the discoveries of the 19th century, which Urey believed were obvious truths obscured by superstition and hubris, these discoveries were “the result of the most careful and penetrating analysis into the ultimate structure of the universe in the regions of its smallest and largest manifestations.”681 The same was true of the development of the science of heredity and the discovery of DNA (reported by James Watson and Francis Crick only two months before Urey presented his speech).

678 Ibid., 4.
679 Ibid., 2.
680 Ibid., 5.
681 Ibid.
Perhaps most dramatically, the discovery of radioactivity had allowed geology to determine the age of the earth, and had confirmed that it was billions of years old and that human beings had existed on the earth for only a relatively short time. In addition, astronomy with modern telescopes had shown scientists that the Milky Way galaxy was vast, 50,000 light years across, and that the universe was infinitely vaster. Copernicus had ousted humans from the center of the universe, but these modern developments had shown humans that they were likely just one among millions of conscious and intelligent life forms that populated a universe where planets such as the earth might exist in great numbers.\footnote{682}

It bothered Urey, he told his audiences, that “these things have been accepted by the general public as the amusing speculations of scientists and as having little import for the ordinary human being.”\footnote{683} Meanwhile, another society existed within which the developments of 20th century science were treated as matters of fact around which were constructed new philosophies – that of the scientists themselves.

Just as in his Depression-era speeches, Urey presented scientists as a group that practiced a special way of life that demanded a rigid code of ethics. Urey explained, “Scientists must be honest people with respect to their scientific work and it is this rigid honesty that is responsible for the great advances of science. No misrepresentation of facts or dishonest interpretation of them is tolerated and men who engage in such practices become ostracized by respectable scientists.”\footnote{684} But what accounted for this upright behavior among scientists? Before the war, Urey had attributed it to the self-

\footnotetext[682]{Ibid., 6.}
\footnotetext[683]{Ibid., 5.}
\footnotetext[684]{Ibid., 9.}
sacrifice and sublimation that objectivity required of the scientist. After 1953, however, Urey’s ideas had changed.

In 1953, Urey turned to religion. “Scientific training is not responsible for this honesty of approach to scientific problems,” Urey said, “for a person’s scientific training is largely secured long after the foundations of character have been established. Scientific training only selects the objective, honest person.” Instead, the source of the rigid code of ethics that scientists exemplified was the community within which science was practiced. The ethical code that underlay the community in the West was that of Christianity. Urey explained,

It is … interesting to note that modern science has originated and has flourished in that part of the world where Christianity has been the dominant religion. Perhaps I am partial to this religion because of its great importance in my own life. This religion and other religions emphasize the greatness of men, their very great capacities to understand and assume responsibility rather than their mere animal characteristics, and the greatness of the universe, and they admonish men to think of great things and to act in great and noble ways. The Ten Commandments of the Jewish religion have been brought to our world by Christianity, and it is this religion which civilized the Roman world and the savages of ancient northern Europe who are the ancestors of so many of us. “Thou shalt not lie.” You must not lie about or misrepresent your data. “Thou shalt not steal.” You do not assume that you have done work which others have done. These two commandments are of paramount importance to science and I do not believe that science can originate nor be maintained in a community which does not generally subscribe to and practice them. It seems to me that science developed in Europe because of the important influence of Christianity on the people of that continent.

Urey was skeptical that science would continue to flourish in the Soviet Union, where Christianity had been abandoned and replaced by “apologies to dialectical materialism.” He did not expect to see the quality of science in Russia decline within his lifetime: “It

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685 Ibid., 10.
686 Ibid.
required centuries for Christianity to civilize Europe and it will require many years for
the false doctrines of communism to destroy the remnants of the western tradition within
that country.” However, his rhetoric in the years to come made it clear that he believed
science could only succeeded in societies where scientists were raised to act “as a good
Christian should behave.” If Christian morals were eroded or eliminated from society,
the decline of science was inevitable.

Scientists, unfortunately, could not help to carry on the Christian tradition.
Although they behaved as good Christians, most of them, Urey lamented, were skeptics
who worshipped the universe. Urey could act as a good Christian and could observe the
importance of Christianity, but he could not give non-scientists a reason to believe.
Moreover, he worried that the general public took the wrong message from the success of
science and engineering. The public, by his estimate, cared more for the practical
applications of science than they did for the grand view of the universe provided by
scientists, and were mistakenly becoming amoral materialists. This could leave them
susceptible to pseudoscientific creeds such as dialectical materialism, or worse: “The
Nazis were ‘scientific,’ or so they thought. They could discard Christianity and become
pagan. And within a decade they killed 6 million people as though they were animals.”
The solution to this dilemma could not come from science. Science could provide an
honest and even inspiring view of the universe, but “it gives little to the ordinary non-
scientific citizen which enables him to meet the spiritual needs of life.” Only religion
could meet these needs. The problem was potentially severe, given the uncertainties of

687 Ibid., 11.
the Cold War world, within which “it is so necessary that some inner well of strength be stirred and maintained for all men as individuals, for most occasionally, for some continuously.” Urey ended the first version of this speech with a plea to his audience: “The drift from that high and moral life taught to us in the past must be arrested and we must not think that this scientific and engineering century can be built strong and true in any respect without adherence to the virtues taught in the Ten Commandments and the Sermon on the Mount. I urge all of you to try to fit the new concepts of science into the ancient teachings of religion to which we owe so much.”

Judging from his speeches, the problem of retaining the traditional morals of the western religions continued to bother Urey for the rest of his career. By 1959 Urey had begun arguing, in addition to what he had outlined in 1953, that “One of the pressing needs of this age is a great prophet who can accept the facts of science and at the same time can give inspiration to fill this great void.” This prophet likely was modeled on the diocese of Christ Church, Oxford. Urey had spent the 1956-57 academic year in Oxford as the George Eastman Visiting Professor. Here, he had regular and pleasant exchanges with the Reverend Professor John Lowe, Dean of Christ Church, and his Canons. He told an audience after his return to the US, “Times have greatly changed when the Eastman Professor, without fear of being burned at the stake, can ask the successor of Dean Wilberforce whether he expects the second coming of Christ to occur within the next thousand years or the next million years and receive, what he did not

690 Ibid.
691 Ibid., 15.
692 Harold C. Urey, “Cooper Union for the Advancement of Science and Art,” typescript, November 2, 1959, 7, Box 21, Folder 19, HCU.
deserve, namely, a most courteous and thoughtful reply.” In the colleges of Oxford, Urey reported, he had seen “the enormous change in our religious attitudes” to a greater extent than he had seen anywhere else. “How does religion maintain the old values? Can it do so without the miraculous, and without illogical dogmas? Can it make use of the magnificent view of the universe supplied by science and the materialistic necessities and luxury supplied by its applications to give us a sound moral life and noble aspirations?” He assured his American audience that the religious men of Oxford were striving daily to find the solutions to these problems.

The scientists alone could not provide a religion for the atomic age. However, with the help of the new prophet – with religious thinkers who were willing to substitute science for superstition – they could still contribute. Because this new religion would require the most complete and inspiring view of the universe possible, the scientists in fact had an essential role to play. Not to participate in this vital task of giving life meaning could have dire consequences even for the most skeptical intellectuals: “It is necessary that the old fashioned morals be maintained for the daily well being of all men. It is necessary for science that these be maintained if it is to remain vigorous and active and if it is to continue to widen the horizons of men. It is necessary for intellectual pursuits of all kinds that the ancient moral teachings be maintained in an age of science.”

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693 Urey, “Religion Faces the Atomic Age,” 2.
694 Ibid., 3.
**Urey’s Path to Religion**

The execution of the Rosenbergs and the death of Urey’s mother made the year 1953 one of mental trauma for Urey. But what made him grasp for the handrail of religion? Had he found a renewed faith in the God he had given up on as a young man? Urey’s attitude toward God during this time indicates that he was no believer – at least not in any traditional sense.

Urey did not speak or write often about his own view of God. When asked whether his scientific education had eroded or strengthened his faith in God, Urey might respond as he did to one admirer’s letter that “I myself have my own definitions of God and things of this sort, but I would not like to discuss them in public at all.” In his autobiography he admitted that he did not believe in an afterlife, and believed instead that “we are all temporary, and are only part of this enormously complex universe that changes with time.” In a public forum, after reciting from memory the first verse of Genesis, Urey said that it was “beautiful poetry,” but that he was more convinced by the evidence for evolution.

When Urey did invoke God, it was a God that had done little more than set the universe in motion. When asked in an interview whether his disbelief in a personal God meant that he believed the creation of the universe was an accidental event, Urey answered, “No, not at all. … I follow the astronomers and their hypothesis of a Big Bang.

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696 Harold C. Urey to Winfield E. Little Jr., December 1, 1962, Box 54, Folder 9, HCU.
50 billion years ago.” Much to the dismay of his mother and stepfather, Urey preferred to believe in a “God [who] opened his hand and the universe was created.” As he explained to the editor of This Christian Century, it was simply “more beautiful to have a universe that is established in such a way that it takes care of itself completely by itself than it is to assume otherwise.” As he told one audience, the universe was like a God: “Now this God is a God that extends in all directions from billions of light years and has existed for billions of years and will exist for billions of years in the future and maybe all these numbers should be infinite. This God has left a true record in nature which we can read with exercising some diligence.”

It was the universe that inspired Urey. As he wrote in his autobiography, “To me, the enormous universe and all the things in it are the source of my wonder, and I need no God to increase this wonder at all.” Urey was happy believing that the universe, beginning with the Big Bang, had proceeded to unfold and evolve without the intervention of God, and that life had emerged on earth through chance chemical events. But he also questioned whether or not something might be at work underneath the seeming randomness of the universe. Urey wondered “whether anything in nature is chance at all. It seems to me that the properties of organic compounds are such as may have resulted in the spontaneous evolution of life.”

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701 Harold C. Urey to Harold E. Fey, April 8, 1955, Box 35, Folder 26, HCU.
704 Urey to Fey, April 8, 1955.
For the most part, Urey was an agnostic. He defined himself as an atheist because he did not believe in “a private God that listens to the prayers of anyone.” However, he also believed that nature was too complex to be fully understood by the human mind. “Now, what do we do with this enormous heart of nature which is beyond any possibility of our comprehension?” he asked. “It looks to me as though this is an unknown and uninvestigatable region and it will always be impossible to decide whether the things which we observe in nature are naturally so or whether they are guided by an outside intelligence.”

Despite this stated lack of faith in the God of traditional religion, Urey had begun making overtures toward religion as early as a decade prior to its appearance in his public speeches. In the early 1940s Urey contributed to an intellectual project intended to apply the method of “corporate thinking” to “see what scientists, philosophers, and theologians could do to unite the more abstract thought and thinkers of the present in defending democracy.” This was the Conference on Science, Philosophy, and Religion in Their Relation to the Democratic Way of Life, hosted at Columbia University and sponsored by the Jewish Theological Seminary. The Conference was only one of the many manifestations of the preoccupation with fascism and totalitarianism among New York intellectuals during this time. However, it also represented a special concern over the increased specialization of intellectual work and the “departmentalization of American

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706 Urey to Fey, April 8, 1955.
708 Descriptions of other such manifestations can be found in Hollinger, “The Defense of Democracy and Robert K. Merton’s Formulation of the Scientific Ethos”; Hollinger, “Science as a Weapon in Kulturkampfe in the United States during and after World War II.”
thought.”

One of the central tenets of the Conference, in the words of one participant, the historian Van Wyck Brooks, was the recognition that our failure to integrate science, philosophy and religion, in relation to traditional ethical values and the democratic way of life, has been catastrophic for civilization. … We know that democracy exalts the individual but that individualism as an end in itself means anarchy. We know that tradition can make slaves of men, but that the lack of historical perspective and rootage in the past makes their lives thin and unheroic. We know that technology can be and has been a great emancipator, but that it may also put tools in men’s hands with which to destroy their heritage.

The aim of the Conference, Brooks argued, was “to promote respect and understanding between the three disciplines involved and to create among them a consensus concerning the universal character of truth.”

The group was especially concerned over the threat of totalitarianism. The Conference adopted early on a model of totalitarianism within which the ruler derived authority because “the primitive identification of the state and the Deity has been re-established.” This “pseudo-religious” philosophy was incorporated into every part of the totalitarian empire’s government, economy, and society, making possible the “worship of power, and the contempt of truth, mercy, and justice,” and leading the followers of Hitler, Stalin, and Hirohito to worship them as “quasi-divine personages.”

The group feared that the totalitarians might take advantage of “Decreasing respect for ethical and religious values among the democratic peoples” and the resulting “confusion in their educational systems, in their literatures, and in organs of public opinion.”

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709 Louis Finkelstein, “Memorandum”, September 12, 1940, 3, RG 5, Box 1, Folder 5A-1-5, CSPR.
710 Van Wyck Brooks, “Statement Oct 1940 V.W. Brooks”, October 1940, 1, RG 5, Box 1, Folder 5A-1-12, CSPR.
711 Ibid., 2.
712 “Executive Committee - Press Release Draft”, August 9, 1940, 1, RG 5, Box 1, Folder 5A-1-5, CSPR.
713 Ibid., 2.
generally.” They worried that “A cynical, divided, hyper-individualistic America will necessarily become a doomed America,” and offered as remedy cooperation between leading scientific, philosophic, and religious thinkers. This, they hoped, would produce a “dynamic philosophy of American democratic life” that would “oppose any effort at deification of the state, or the suppression of individual liberty and sense of moral responsibility.”

The primary organizer of the Conference, the Seminary’s then-president, Rabbi Louis Finkelstein, began corresponding with Urey at the end of 1939 and soon convinced him to join the Conference as one of its eighty-four founding members. Others among the Conference’s founders were Brooks, Robert M. Hutchins, Albert Einstein, I.I. Rabi, Arthur H. Compton, and Harlow Shapley. Urey did not participate much in the planning or organizing of the Conference’s initial 1940 meeting, but once he was made a member of the Conference’s Executive Committee he began to take a more active role in the Conference’s activities. Although Urey warned the Committee early in 1941 that he and his fellow physical scientists were becoming increasingly preoccupied with national defense, he nonetheless agreed to preside over a public assembly at which Assistant Secretary of State A.A. Berle, Jr. presented an address about the wastefulness

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714 Ibid.
715 Ibid., 3.
717 Louis Finkelstein to Harold C. Urey, September 18, 1940, 1A-27-16, LF.
of the Nazi forces, and to participate as a discussant during a session on “The Natural and Social Sciences in Their Relation to the Democratic Way of Life.”

Although Urey did not in fact manage to contribute much to the Conference before his war work consumed his time completely, and he failed to keep up with the Conference’s activities after leaving for Chicago, he still approached the task of the conference seriously and seems to have been convinced of the importance of protecting the Judeo-Christian tradition in America. For more than a year, Finkelstein and Urey met and lunched on their common ground of Morningside Heights. Although no records of their personal discussions exist, their correspondence from this period displays mutual respect and agreement over the importance of the Conference and its mission. When it came time for Urey to participate in the Conference’s second meeting, he took his role as a discussant quite seriously. The unpublished notes from the Natural and Social Sciences session show that Urey had a great deal to say. It was in this session that Urey perhaps first articulated those ideas that would become so prominent in his 1950s speeches.

During the morning session he suggested that schools should do more to teach students the “Hebraic-Christian tradition that conditions all of our acts at the present time,” instead of focusing exclusively on the “Greek civilization”:

It is this religion that states that man is an important individual, regardless of his position in life, the color of his skin, or anything else that you wish to name, and it is that dignity of man which is established, I believe, as a result of this Hebraic-Christian tradition that is the most important thing in our democratic ideals today. It does seem to me that it would be well if this Conference could make a statement to the effect that more education in the field of the literature of our important religions in our common

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718 “Minutes of the Board of Directors of the Conference on Science, Philosophy and Religion in Their Relation to the Democratic Way of Life, Inc.,” January 13, 1941, 6, RG 5, Box 1, Folder 5A-1-2, CSPR; Bryson and Finkelstein, Science, Philosophy, and Religion. Second Symposium, 551-552.
schools would be well worth-while. It would be my hope that such a statement could be made by this Conference before it closes.\textsuperscript{719}

Urey even went so far as to wonder if science itself might be to blame for the erosion of the “permanent values” of western civilization, claiming that “I think it is time for us to consider the question as to whether science, with its materialistic point of view, may not be getting too strong a hold upon our ideals.”\textsuperscript{720} Margaret Mead, also present in the session, agreed with Urey that scientists must be made more aware of the social implications of their work, and stated that “If we look at the history of science, we find that the men who have meant most in the development of that science have been men who have been alive to the moral, religious, political implications of the points of view that they were helping to develop.”\textsuperscript{721}

Urey’s views did not go unchallenged. As Fred Beuttler had argued, the conference was meant to address American pluralism by creating an inclusive dialogue between Protestants, Catholics, Jews, and secularists, but its discussions also included non-Western traditions like Buddhism and Hinduism.\textsuperscript{722} Howard University’s Haridas T. Muzumdar, Mahatma Gandhi’s spokesman in America, questioned Urey’s neglect of “the largest segment of the religious and spiritual experience of the human race,” and asked if imposing the Judeo-Christian tradition upon the rest of the world was not a form of “spiritual fascism or totalitarianism.”\textsuperscript{723} Urey acknowledged that he had been speaking

\textsuperscript{719} \textit{“Monday AM + AFT Sessions,”} 37-38.
\textsuperscript{720} Ibid., 38.
\textsuperscript{721} Ibid., 19-20.
\textsuperscript{723} \textit{“Monday AM + AFT Sessions,”} 111.
from his own limited experience, and softened his position on the teaching of the Judeo-
Christian religions in public schools:

In my proposal this morning, I was quite partisan. … I am looking about for a way to insure [our way of life’s] continuance at the present time and, in so doing, have attempted to assess what the fundamental thing is that has given us this life of which we are so fond. I believe it is a Christian way of life and that Christian way of life is the mother of our democratic way of life. That is the way I personally look at it. … I propose that we study those religious beliefs, as literature in our schools, as a way to the definite end of combating totalitarianism in the world at large.\textsuperscript{724}

Still, the majority of Urey’s remarks that day remained committed to the role that the western religions had played in the development of civilization.

Toward the end of the day, Urey objected to the thesis of the psychologist Max Schoen that freedom, “this right of every man to his own life, on his own terms, is not merely a proclaimed right, but one that is deeply rooted in the very nature of animal existence in general and of human existence in particular,” and that therefore democracy was “neither a wish nor a hope, but the only mode of communal life in which there can be peace and which can have permanence.”\textsuperscript{725} Here Urey claimed that democracy was not something inherent to the human psyche, but something that historically had to be imposed upon the “savage” human from without:

… I would say it is surprising to me that such a large fraction of past history has consisted largely of despotism; in fact, history to me looks more like despotism with small interludes of democracy, rather than what I should expect upon the basis of this thesis, namely, universal democracy with an occasional despotism. … I think it is unscientific not to recognize facts from observation, and I believe that the statement Dr. Schoen presented fails to recognize facts. Most of us who are not descendants from the Mediterranean region are descended from savages as recently as

\textsuperscript{724} Ibid., 113-114.

1,500 years, savages who left nothing but the rudest stones as monuments. Superimposed on that background has been a past adoption of civilization from other regions, and those regions are Judea, Greece, and Rome. I think it is well to remember that it is this sort of tradition that has led to our democratic institutions and no reasoning about our biological characteristics at all, even if they are true; and that it this past tradition that has given us ideals that lift us above the level of the common, ordinary, primate.  

When *The New York Times* reported on the day’s discussions, they picked up on Urey’s contention that “it was no accident, that the totalitarian States were ‘anti-Christian,’” as well as his claim that the Judeo-Christian tradition was “the mother of democracy.”

James Gilbert has described the Conference as having “specified the terms for an enduring dialogue among members of America’s East Coast academic elite about the relation of science to religion during the postwar period,” and argued that “Although we might assume that the seeds of this modern dialogue about science and religion were sown in the midnight sun of atomic energy and raised in the glare of its aftermath, we are in fact looking at a harvest of ideas in the 1940s and 1950s, not their semination.” For Urey, this certainly seems to be the case. In 1953, Urey resurrected the argument of the importance of religion for democracy that he had made a decade earlier and applied it now to science and survival in the Atomic Age.

In addition to having an already articulated view of the importance of religion with which he could work, events of the Cold War do seem to have reinforced this view for Urey. His activities with the Emergency Committee had often brought him before religious audiences, and Urey had seen how readily religious organizations joined in the

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struggle against the proliferation of atomic weapons. Moreover, Urey had become increasingly convinced that religious courage was what separated heroes from villains in the Cold War. This issue came to the fore in a 1949 letter to Finkelstein, in which Urey compared two university presidents, Robert Maynard Hutchins and James Bryant Conant. These two men were, to Urey’s mind, polar opposites. Hutchins, a minister’s son, had as the President of the University of Chicago been an unwavering champion of the scientists’ movement. Conant, as the President of Harvard, had failed to defend scientists and educators from interference in the postwar years, and, compared to Hutchins, displayed a lack of intellectual courage: “[Conant] has consistently taken the line of least resistance in all of the problems facing education at present,” he wrote, “including the very difficult situation involving loyalty oaths and investigations for subversive activities, while [Hutchins] has consistently maintained a very courageous stand.”

Urey gave Finkelstein his account of the reasons underlying scientists’ complacency when faced with the loss of their political freedoms to the introduction of the loyalty-security system that would come to characterize the American Cold War science-state relationship. According to Urey, what Conant and these other scientists lacked was religion:

We are living in a time when it is necessary that people stand up and be counted. The trend toward Fascist-Nazi tendencies in this country is really alarming. In Germany and Italy only the religious groups had the courage to stand up and be counted, and it is also true today that only the religious groups exhibit this courage insofar as activities behind the iron curtain are concerned. I am exceedingly disappointed in scientists generally, but I could not possibly be in favor of giving awards now to people without this kind of courage, and I think Conant does not have it.

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729 Urey to Finkelstein, August 11, 1949.
730 Ibid.
Conant’s example illustrated to Urey that the loss of Christian character in the scientist or intellectual could have grave consequences.
Chapter 5

To Hell with the Moon:
The Cosmochemist’s Failed Quest for a Rosetta Stone

On July 29, 1958, President Dwight D. Eisenhower signed the National Aeronautics and Space Act, creating a new National Aeronautics and Space Administration (NASA) and signaling the beginning of an organized civilian program in space exploration. By this time, Harold Urey’s Cold War crises (in conjunction with his new Cold War patrons) had pushed his research program into the earth and planetary sciences. Meanwhile, these same crises had pushed his public rhetoric away from scientific utopianism and world government. Urey now advocated a new, modern Christianity that brought together the traditional moral teachings of the Ten Commandments and the Sermon on the Mount with a scientifically informed revision of Genesis that accurately portrayed the grandeur of the universe and the place of humans within it. In order to preserve both science and society, he told his audiences, a new prophet must combine the truths of western science with the truths of western religion.

When NASA’s Space Science Division decided to seek out ties with prestigious university scientists who could lend their scientific expertise and credibility to space projects, Urey was one of the first scientists the Administration approached. Without imagining that the moon would be reached within his own lifetime, Urey had by then developed a hypothesis of the satellite’s origin that made it a cosmochemical Rosetta Stone. Urey’s moon was a captured object, formed before the earth and the other terrestrial planets and largely unchanged since the earliest days of the solar nebula. His
moon had been born cold and had remained cold, never experiencing the chemical changes that occurred during the formation and evolution of geologically active terrestrial planets like the earth. With Urey’s participation, the Space Science Division capitalized on his claim for the moon’s scientific importance, using it to sell a crash lunar program to NASA administrators and Congressional committees.

NASA’s lunar program might have become the unifying project that Urey had been advocating. It was a heavily funded non-military project that tapped into what Urey felt were innate qualities of human beings – curiosity and the excitement of adventure. With the right encouragement, he also felt that it could be a stage for the public revelation of the grandeur of the universe. Urey became one of the space program’s most vocal advocates in the scientific community. He attempted to convince the public that the moon held on its surface the basic chemistry of the universe and an unaltered record of the early years of the solar system. If the moon’s capture had caused great enough tidal forces, then the lunar surface might also hold simple organisms transferred there from the oceans of the early earth. The most exciting and worthwhile contribution that the space program might make would be to illuminate the natural laws and evolutionary processes constantly at work in the universe that led to the formation of planets around stars and the emergence of life on those planets. This was the program Urey chose to support.

Evaluating Urey’s lunar work allows for an examination of just how linked Urey’s public rhetoric and scientific work were. Urey developed his model of solar system development while in Oxford, on the same grounds where he dined and chatted with the Diocese of Christ Church and developed his model prophet. Just prior to this, Urey rekindled an acquaintance from the early 1940s with the editor and philosopher of
science, Ruth Nanda Anshen. With Anshen’s editorial and “spiritual” encouragement, Urey began planning a book that would present a popular view of his lunar and planetary theories. While this project would not make Urey into the new prophet for whom he had called, he would at least be working toward the goal for which he felt science in the Cold War should aim – the production of an inspirational cosmic narrative. If this narrative could be presented to the public, perhaps it could stem the tide of materialism that an emphasis on the applications of science had produced.

Such an evaluation also demonstrates the limitations that a large scientific project imposed on the type of synthetic work Urey had in mind. After all, Urey was only one of many scientific participants in lunar exploration, and this itself was a project that already carried a good deal of political baggage of its own. Even as NASA justified its lunar program with Urey’s moon, another moon rose to prominence within the Administration. NASA’s mission planners found it convenient to treat the moon as an extension of terrestrial terrain, turning to geologists with earthbound experience in the mapping and selection of lunar landing sites, as well as the training of the Apollo astronauts. This approach was advocated by a small but dedicated group of geologists from the US Geological Survey who called themselves “astrogeologists.” Many of these geologists paid little attention to Urey’s capture hypothesis. That they seemed more interested in the theories of one of Urey’s bitter rivals, the astronomer Gerard P. Kuiper, led Urey to conclude that his theories of the moon’s origin were being suppressed within the lunar program.

Urey’s synthetic narrative of the universe and life’s place within it did not emerge from his lunar work, nor did he ever incorporate the Apollo findings into his public
speeches. Partly this was due to old age and poor health. Urey had entered into his relationship with NASA in 1958 at the age of sixty-five, as he energetically took up a new position at the University of California, San Diego. Although he had escaped retirement at the University of Chicago, Urey realized that he had come to an age at which the research projects he initiated in California would likely be his last. By the time the final Apollo mission returned to earth in 1972, the now nearly octogenarian’s health and characteristic youthful energy was fading. His hands now quivered, his heart had become unreliable, macular degeneration was diminishing his ability to read scientific papers, and he no longer felt well enough to travel. While he still went into his office nearly every day, he admitted to an old friend in 1975, “I do not do much science anymore.” 731 Not long after this, he would be diagnosed with Parkinson’s Disease and would spend most of his remaining days “sitting on my beautiful patio and viewing my flower/orchid garden” under the care of the still energetic Frieda Daum Urey. 732

Many of Urey’s struggles with NASA and with the astrogeologists seem in this light to have been wastes of the aging chemist’s time and energy. At times in this story, Urey appears to be a stubborn man holding relentlessly to an unpopular and unlikely theory. However, this stubbornness seems more reasonable when viewed against Urey’s Cold War ideal of science. Urey in fact never claimed to be right about the moon. He merely claimed that his hypothesis made the moon important, and that it was just as reasonable as any other hypothesis of the moon’s origin. As he liked to say, “All explanations for the origin of the moon are improbable.” 733 His criticisms were mostly

731 Harold C. Urey to Samuel S. Maclay, December 30, 1975, Box 55, Folder 3, HCU.
732 Harold C. Urey to Christian Abrahamsen, May 3, 1979, Box 2, Folder 6, HCU.
leveled at those whom he felt had made up their minds that the moon was an uninspiring extension of the earth on the basis of inconclusive photographic and telescopic evidence. If this was the case, what was the point? “I shall be sorry and disappointed if … the moon then will be an incidental object and not of fundamental importance. We can decide that it escaped from the Earth and then ‘to hell with it.’”\textsuperscript{734} In order to be worthwhile, a scientific study of the moon should be based on the moon’s potential importance, not on exploration for exploration’s sake. Thus it was not just important to be able to tell the moon’s story, but to tell the \textit{right} story with the moon.

\textbf{From the Earth to the Moon}

With his students and colleagues in Chicago, Urey had studied the isotopes of the light elements and introduced entirely new methods into the earth sciences. Once these methods were developed, and once Urey had trained a considerable number of students in their use, he decided that it was time to leave the field to his younger colleagues. Urey began searching for a research project of his own. The project he settled on would extend the methods developed in his Chicago lab to the cosmos, and would take on the daunting task of describing in chemical terms the formation and evolution of the solar system.

A few events conspired to push Urey in the direction of the cosmos. First was the 1949 publication of Ralph Baldwin’s \textit{The Face of the Moon} by the University of Chicago

This was a book that Urey might not have read had he not been a member of the university’s Committee on Publications in the Physical Sciences. Urey, who was new to the Committee, was sent a review copy of Baldwin’s book after publication. Dutifully, Urey took the book with him on a trip from Chicago to a speaking engagement in Canada, and by his account spent a good ten hours carefully reading the book on the train and trying to understand every detail completely. As he told one correspondent, the trip was to a Canadian nuclear power plant, and the book gave him some respite from the nuclear issues that plagued him: “On the way, I read Ralph Baldwin’s book on the moon and became immensely interested in the moon as a result of that. Then I forgot all about the Canadian power plants.” Indeed, although Urey would continue to engage in nuclear debates, in 1950 he “quit” the AEC, claiming that the work bored him, and told reporters that he was “more interested in my present work than in anything I was able to do in connection with the development of the bomb.”

Baldwin’s book summarized the history of selenography, described the different types of lunar features, evaluated the various hypotheses put forward to explain these features, developed a strong argument in favor of the meteoritic-impact hypothesis, and presented Baldwin’s own hypothesis for the history of the moon and its maria. Urey was not impressed with Baldwin as a scientist. He found him to be unfamiliar with standard scientific notation, and thought that his math skills left much to be desired. As Urey explained to Ian I. Mitroff, “Now I’m not an awfully good mathematician myself. There are just many people that are much better than I at this. … But [Baldwin] is far worse.

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736 Harold C. Urey to Harry G. Thode, August 4, 1975, Box 90, Folder 14, HCU.
than I am.” But Urey was able to forgive Baldwin his shortcomings because he felt that Baldwin treated the competing theories and hypotheses he presented in his book respectfully and did not neglect to mention theories or theorists with whom he disagreed. After returning to Chicago, Urey asked the press for Baldwin’s address and struck up a correspondence with Baldwin almost immediately. The two men continued to correspond and share ideas and frustrations with him about lunar issues until 1976.

Baldwin’s book compelled Urey to take a closer look at the moon. He requested pictures of the moon from the Harvard astronomer Harlow Shapley. He pasted these images together in his Chicago office and made a composite map of the moon that was “about a meter across and was a pretty good picture of the moon.” It was from this composite that Urey formed his initial impression of the moon as a cold and ancient object: “What impressed me about the moon was almost certainly the principal features of the moon were due to great collisions… The face of the moon predominantly was a picture of the final stages of the formation of the earth – a history that was not available from the rocks of the earth. It was for this reason that I became interested in the moon, and have remained interested in it since then.” Still, although his interest was piqued, and while he would continue to maintain that the moon held special relevance in the history of the solar system, the moon was not the first planetary body Urey tackled.

In the same year that Baldwin’s book was published, Urey was asked to co-teach a Summer Session course with Harrison Brown, “Chemistry in Nature.” Urey’s first lecture for the course was on the heat balance of the earth. In order to prepare for this

738 Harold C. Urey, “Tape 7a,” interview by Ian I. Mitroff, December 1, 1969, Box 1, Folder 7, IIM.
739 Harold C. Urey to Rollin D. Hemens, November 9, 1950, Box 12, Folder 14, HCU.
741 Ibid.
lecture, Urey turned to a review article by Louis B. Slichter. Here he learned for the first
time that the temperature of the earth had been rising, not falling, throughout its
history. Urey’s encounter with Slichter’s paper led him within a matter of months into
new considerations of the role of radioactivity in the movements of the earth’s crust and
mantle. In the autumn of that year, Urey presented his theory of the cold accretion of the
terrestrial planets, within which relatively homogeneous masses of material from the
solar nebula were slowly heated by radioactive uranium, thorium and potassium. This
radioactive heating melted the iron within each planetary mass, which further heated the
planet as the heavy metals flowed inward to form the core and displaced the lighter
materials that formed the mantle and crust.

Urey’s move into planetary science continued in January 1950 when he attended
the Conference Concerning the Evolution of the Earth in Rancho Santa Fe, California, co-sponsored by the NAS and the University of California’s Institute of Geophysics. Slichter arranged the Conference in reaction to Urey’s paper on the origin of the Earth’s core. Not many of the participants in the Conference were willing to accept Urey’s model in its entirety. Many were, however, eager to accept Urey’s method of initiating the gravitational separation of heavy and light elements through radioactive heating, thus

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744 According to Doel, this Conference was “hastily arranged in January 1950 in part to provide a forum for discussing Urey’s ideas”: Doel, *Solar System Astronomy in America*, 95, 97.
allowing for convection in the mantle.\textsuperscript{745} But even if Urey’s planetary hypothesis won few adherents, he was able to test out some of his early ideas about the formation of the Earth and other terrestrial planets at this Conference. As this was one of the first interdisciplinary planetary conferences – involving physical chemists, geochemists, geophysicists, geologists, physicists and astronomers – it also allowed Urey the opportunity to incorporate ideas from these disparate fields into his own thinking.\textsuperscript{746}

Another event that pushed Urey down the planetary path was the departure of Harrison Brown from Chicago. In 1951, Brown left the Institute for Nuclear Studies and moved his research program to Caltech. Urey had been interested in starting meteorite research earlier but had felt it was inappropriate to intrude into his younger colleague’s field. Once Brown had decided to leave, however, Urey felt that he would not be stepping on anyone’s toes if he picked up meteorite studies in his own lab. At this same time, the Austrian physical chemist Hans E. Suess, then with the US Geological Survey, spent the academic year of 1950-51 as a fellow at the Institute and as a guest in Urey’s lab. Suess had only recently (1949) begun writing scientific papers in English, but had nonetheless

\textsuperscript{745} According to Doel, Urey’s model intrigued those assembled because it solved what had become a crisis in the field – explaining how the iron core had formed in the first place, and how mantle convection was initiated. Earth scientists before the war, because it explained seismic and magnetic data, had largely accepted an iron core. However, wartime work on the behavior of solids and fluids under high temperatures and pressures, along with postwar inquiries into the viscosity of inner Earth indicated that gravitational separation of light and heavy elements would take several billion years. That Urey was able to jump start this system through radioactive heating and allow gravitational heating to then take over seemed to solve at least this one pressing problem: Ibid., 96.

already established a firm reputation in nuclear geochemistry. Together, Urey and Suess began working on the abundances of the elements.

Urey also in 1950 struck up a collaboration with Gerard P. Kuiper, astronomer at the University of Chicago’s Yerkes Observatory and President of the International Astronomical Union’s section on planetary research. Urey incorporated Kuiper’s model of the early solar system, within which a massive solar nebula formed large protoplanets through gravitational collapse of the nebular material. By the Spring of 1951, when he was to deliver the Silliman Lectures at Yale University, Urey felt that he had tested his ideas sufficiently and gathered enough evidence to contribute something substantial to the historic subject of cosmogony. In 1952 these lectures were published as a book-length chemical treatise on the origin and development of the solar system.  

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750 Urey, *The Planets*. 
“It is obvious that Congress is not appropriating five billion dollars a year for sciences. The object is adventure. People wish to put a man on the moon and bring him back. They talk about other things but they are not the true reasons for the program. Of course there is competition with the USSR, and as a matter of fact, this is very valuable. It even induces our own Nasa to do some interesting scientific work from time to time. … [My] enthusiasm for the space program is just one of trying to see that we learn something during the course of this enormous project.”

Harold C. Urey
“Affording the Space Program” (1967)

Despite Urey’s interest in the planets, he was not immediately an enthusiastic supporter of the US space program. When the successful launch of Sputnik shocked American citizens and politicians out of complacency, Urey’s response was not to call for an equally ambitious space program. Instead, Urey issued a response that “Sputnik is a salutary lesson for us if we learn from it. We can afford defense, education and scientific achievement if we wish to do so. We can drop our waste of resources, and manpower, on gaudy and oversized cars, for example, if we wish to do so.” In order to counteract American waste and put resources to good use, Urey perhaps drew upon his austere Brethren upbringing and proposed a ten-percent sales tax on all automobiles and luxuries in the next year, with twenty- and thirty-percent taxes in the years to come. Rather than putting this money directly into a space program, Urey believed that it would be better placed in new school buildings, increased teachers’ salaries, and basic research. He assured his American audience that his faith in democracy was unshaken, although he did

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752 Harold C. Urey, “[Response to Sputnik],” September 1957, Box 2, Folder 1, HCU.
take the opportunity to criticize McCarthy and HUAC: “It was not communism … that did these things, except insofar as people worked, as apparently they have under the system. And it is not democracy which is at fault in the west, except insofar as it seems to have bred a contempt for the intellectuals, professors of our universities and our schoolteachers and instituted witch hunts against some of our most intelligent and patriotic people. Let’s grow up!” The Russians had prevailed, he told his audience, because they “respect their educators and scientists and hold their intellectuals in high esteem. They support their schools and research establishments in a pre-eminent way according to reports which I believe to be reliable.” If Americans did not match the Russians in these respects, the Cold War was lost.

Urey’s lack of enthusiasm for the space race continued. Around the time that NASA came into existence, a local Chicago newspaper asked for Urey’s thoughts on the proposed space program. Urey responded with “a most negative interview.” According to Urey, he “did not believe that the program was worthwhile and I had no interest in sending men to the moon.” Deciding that he did not want to sound so negative of the program he felt would inevitably develop with or without him, Urey contacted the newspaper the next day and asked that they not run the story: “My reason for making this request was that I was sure that when men acquired the capacity to go to the moon they would go to the moon whether I thought it was worthwhile or not.” But Urey would

753 Ibid.
754 Ibid.
756 Ibid.
757 Ibid.
become very invested in the space program, and especially in missions to the moon, within only a matter of months.

Perhaps some of Urey’s lack of enthusiasm stemmed from the fact that the majority of those administrators and scientists who made up the new NASA were sky scientists. These people had joined the American space effort early on because of their interest in sounding rocket research and aeronautical issues. According to R. Cargill Hall, sky science clearly held the upper hand in the new space program.

However, planetary scientists began to gain some leverage within NASA by the end of 1958. Largely this was due to the efforts of Robert Jastrow. The new Assistant Director of Space Sciences, Homer Newell, brought Jastrow – a physicist and sky scientist from the Naval Research Laboratory’s upper air research group – to NASA in November 1958. When Newell tasked Jastrow with forming a Theoretical Division, Jastrow sought out the existing community of planetary scientists – many of who were represented at the 1950 Rancho Santa Fe conference – and brought them inside the NASA fold. Newell described Jastrow as “a prime mover in regard to academic ties,” an “imaginative theorist,” and a “superb speaker.”

Within NASA, Jastrow’s ability to “hold both lay audiences and professional colleagues spellbound with his descriptions of space science topics” was a boon when it came time for a representative of the agency to appear before Congress and defend the annual budget request. He also became one of the Space Administration’s greatest public advocates – appearing on television and publishing several popular articles and books about the ways in which NASA was benefiting mankind.

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760 Ibid.
Urey’s enthusiasm for constructing a cosmic narrative aligned with Jastrow’s interests. According to Jastrow, one of the very first things he did upon joining NASA was to seek out Urey. Jastrow had decided that instead of focusing on every scientific problem that could be studied by satellites and rockets, he should organize his Division around “a few important problems.”\textsuperscript{761} Jastrow decided that Urey, since he “had written a book on the moon and the planets, and was well known for the intensity of his interest in the scientific study of these objects,” would make a good ally within the scientific community.\textsuperscript{762} That he was a “great American man of science” and had written not “a dry discussion of the solar system, as such books usually are,” but one that “was enlivened by a sense of evolution in the Cosmos and the place of our planet in the larger scheme of things,” made Urey even more appealing as one who could sell the program to the public.\textsuperscript{763} By Jastrow’s account, he immediately saw the potential benefits of bringing Urey into his Division. When he met with Urey in person, Jastrow was impressed by his ability to explain his theory of the moon’s origin:

He sat me down, handed me his book on the planets, opened to the chapter on the moon, and began to tell me of the unique importance which this arid and lifeless body has for anyone who wishes to understand the origin of the earth and other planets. I was fascinated by his story, which had never been told to me before in fourteen years of study and research in physics. Harold Urey had the marvelous quality of an intense, almost childlike curiosity regarding all aspects of the natural world. This kind of curiosity is a rare quality.\textsuperscript{764}

\textsuperscript{762} Ibid.  
\textsuperscript{764} Jastrow, \textit{Red Giants}, 4.
Jastrow claimed that he recognized in Urey “that cosmological spark” that could make space science interesting.\footnote{Jastrow, “Exploring the Moon,” 46. According to a note in Urey’s papers, the two men met on December 4 and spent “a considerable time in discussion … that afternoon and the following day.” “[Jastrow meeting note]”, December 4, 1958, Box 47, Folder 15, HCU.}

In The Planets, Urey had in fact laid out one possible scenario within which the moon might be of unique significance. This was a scenario in which the moon formed independently of the earth during the early years of the solar system and was captured by the earth after accretion. This was a scenario that, Urey admitted in his book, was rarely given serious consideration because “[the moon’s] orbit is not retrograde, not highly inclined to the plane of the ecliptic, and not very eccentric.”\footnote{Urey, The Planets, 25.} But he also claimed that his own review of the chemical evidence did not rule out the capture scenario, and that if the moon formed from its own protoplanet and then was captured by the earth, this could explain the large angular momentum present in the earth-moon system as well as the chemical differences between the two bodies.\footnote{Ibid., 97-98.}

In the intervening years, Urey’s image of the origin of the moon and planets had changed. To Jastrow, Urey explained his new theory that the moon might hold the key to unlocking the secrets of cosmogony. As opposed to the earth, Urey believed that the interior of the moon was cold, and that it had been cold for most if not all of its history. Whereas most of the rocks on the earth’s surface are young, those on the moon would be much older. Furthermore, if the moon had been cold since accretion, it would be completely undifferentiated – meaning that the rocks on the surface of the moon would be just as old as any rocks in the lunar interior. As evidence that the moon had never been
hot, Urey pointed out that the moon had a frozen tidal bulge that would have collapsed had the moon ever had a soft interior.\textsuperscript{768} Jastrow quickly latched onto Urey’s conclusion that the surface of the moon would hold the record of its birth, or at least of the early years of the earth-moon system. It gave Jastrow a compelling way to sell a role for Space Science in missions to the moon: “It could tell us something we would never learn on the earth; it could help us solve the mysteries of the origin of the solar system and the origin of life.”\textsuperscript{769}

Jastrow resolved that he would use Urey’s moon in order to make the case for the inclusion of planetary research amid the already existing emphasis on Van Allen belts, orbiting telescopes and manned missions into earth orbit. Jastrow invited Urey to come to NASA in January 1959 to give lectures on the moon and planets to an audience of space scientists and NASA administrators. Afterward, Jastrow and Urey met privately in Jastrow’s office, where Jastrow complained to Urey that “the Russians were wiping up the floor with us in space.” According to Jastrow, it was Urey who then suggested that NASA step up its existing plan to land a craft on the moon in 1963. He reportedly asked Jastrow, “why don’t we get on it before then and show the world we can do something scientifically important in space.”\textsuperscript{770}

Jastrow and Urey approached Newell about advocating a crash project to land on the moon by 1961. Newell requested that the two men write a memo to NASA Administrator Abe Silverstein. Jastrow wrote the political part of the memo while Urey handled the scientific aspects. The memo argued that a crash lunar project with a real

\textsuperscript{768} Jastrow, “Exploring the Moon,” 46.
\textsuperscript{769} Ibid., 47.
\textsuperscript{770} Ibid.
scientific agenda would “enhance the reputation of the United States to a degree that cannot be achieved by the execution of a conventional scientific program on a normal schedule,” and that “A soft landing with performance of the experiments listed below will capture the imagination of the scientific community and the general public to a greater degree than any project of comparable scientific value.” The memo also stated emphatically the authors’ belief that a lunar study was of greater scientific importance than a study of Earth’s other near neighbors, Venus and Mars, which was based on their claim that “there is written plain to our eyes on the surface of the moon the history of the origin of the solar system.”

Lunar proposals were nothing new. Since even before the founding of NASA, proposals for missions to the moon had been circulated among the leadership of America’s then mostly military space effort. The earliest of these was a 1957 proposal by William H. Pickering, Director of the Jet Propulsion Laboratory (JPL) in Pasadena, California. Pickering and Lee DuBridge, President of Caltech, agreed that lunar flights were the most appropriate response to the Soviet threat. With DuBridge’s support, Pickering proposed “Project Red Socks,” outlining a series of nine rocket flights to the moon. Director of the Advanced Research Projects Agency (ARPA), Roy Johnson, agreed that lunar missions would be an appropriate response to Sputnik, and announced in March 1958 that ARPA would make the evaluation of American capability to explore space “in the vicinity of the moon, to obtain useful data concerning the moon, and to

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771 Memo quoted in Ibid.
772 Ibid., 48.
773 Hall, Lunar Impact, 5.
provide a close look at the moon” one of its primary activities. The rationale for such a program was clear; the moon represented a tangible goal, and one that the Russians had not yet reached. The scientific benefits of such a program, on the other hand, were not specified.

Urey’s was the first statement drafted within the Space Administration that put forward a scientific rationale for lunar exploration, and it was certainly the first to rank the moon above Mars and Venus in scientific importance. Newell remembered that Jastrow and Urey used their memo to “[undertake] a small campaign to sell the idea to NASA.” For his part, Newell saw the potential of such a campaign, and passed the memorandum up the chain of command to Silverstein with his endorsement. According to Jastrow, “Harold Urey was the trigger, I was the bullet, and Homer Newell fired the gun.” The memo reached the highest levels of NASA and eventually made its way to Congress. By Newell’s account, “Urey’s story provided good ammunition for moving the proposal on up the line. The persuasiveness of the argument carried the day at each stage, within NASA, in the Administration, and finally in Congress, and in due course investigation of the Moon was formally and officially a part of the NASA space science program.” Throughout the program’s existence, Urey would continue to provide much of the scientific justification for lunar exploration.

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774 Ibid., 6.
776 Jastrow, “Exploring the Moon,” 49.
Romancing the Planets

Jastrow was not alone in noticing Urey’s move into lunar and planetary work. Others also appreciated the “epic” nature of this turn from atomic work to cosmogony. After unveiling his 1949 cold accretion hypothesis, the *New York Times* applauded Urey, who “knows all about radioactivity and makes proper allowance for it,” for taking on cosmogony – work they labeled “scientific romancing.”\(^{778}\) Scientists, the *Times* wrote, “are supposed to concern themselves only with facts”; the history of cosmogony, however, from Kant and Laplace to T.C. Chamberlain and F.R. Moulton, proved that “when it comes to wild romance they eclipse the most extravagant fancies of those who contribute to ‘pulps’ given over to science fiction.”\(^{779}\) The *Times* understood Urey’s compulsion to provide an accurate and scientific description of what had historically been the realm of religious folklore. About Urey’s “dream which is concerned primarily with the earth,” they wrote:

Here we have a sample of the folklore of a scientific age. A primitive savage could explain the wind only be supposing it was a blast from the mighty lungs of an invisible demon. The sun and the moon were similarly personified. Today we tell the same tale with improvements. We have the old stage, meaning the heavens, but the characters of the play, the stars, wear different costumes and talk a different language. Electrons, protons and neutrons strut about where once there were spirits. Instead of Greek gods on Olympus we have Greek symbols in equations. The wonder of how it began, the dreaming, is still there. And why not? Creation – there is no theme so stupendous. Only a bloodless dullard would fail to speculate about it. Let’s have more fiction of the type that Dr. Urey has given us. There is something epic about it.\(^{780}\)

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\(^{779}\) Ibid.

\(^{780}\) Ibid.
If Urey was in 1949 already considering the importance of the cosmic narrative that would become central to his concept of the new prophet, he had hit his mark. If he had not yet considered it, reactions like that of the Times certainly may have pushed him toward this understanding.

Along these same lines, Urey may have been encouraged by the stated purpose of the Silliman Lectures themselves – “to establish an annual course of lectures designed to illustrate the presence and providence, the wisdom and goodness of God, as manifested in the natural and moral world.” While Urey no longer believed in any kind of personal God or Creator, the idea that “any orderly presentation of the facts of nature or history contributed to the end of [the Silliman Foundation] more effectively than any attempt to emphasize the elements of doctrine or creed…” certainly was compatible with his own emphasis on the universe itself as a source of religious-like inspiration.\footnote{Urey, The Planets, vii.}

Ruth Nanda Anshen – philosopher, author, and self-described “intellectual instigator” – also noticed Urey’s move.\footnote{Anshen is a neglected 20th century intellectual. Short biographical treatments of Anshen can be found in Susan Wyckoff, “Anshen, Ruth Nanda (b. 1900),” ed. Paula E. Hyman and Deborah D. Moore, Jewish Women in America: An Historical Encyclopedia (New York: Routledge, 1997); Joann Giusto, “Ruth Nanda Anshen,” The Publishers Weekly, January 9, 1978; Mark Teich, “Editing Einstein,” Omni, July 1988. The description of her as an intellectual instigator comes from Wyckoff’s encyclopedia entry.} If anyone could have taken on the role of the prophet that Urey described in the 1950s, Anshen was a fine contender. And if anyone may have reinforced for Urey the notion that his planetary work might be valuable to the religious thinkers of his time, it was Anshen. Anshen had been a graduate student of the philosopher of science, Alfred North Whitehead at Boston University in the late-1930s. Under Whitehead’s guidance she had developed a preoccupation with what she decried as
the “atomization of knowledge.” She retrospectively described her life’s work as being
guided by a desire to “lead man from an age of fragmentation to a new plateau of
consciousness.”

As a response to atomization, Anshen developed a “lifelong obsession with ‘the
unitary structure of all reality.’” In order to illuminate this unitary structure, Anshen set
out in search of a “thematic hypothesis” or “unitary principle” under which all aspects of
life and knowledge could be organized. In the early 1940s, Anshen initiated a series of
edited volumes entitled the Science of Culture Series with an editorial board of Einstein,
Bohr and Whitehead. Within this series she collected and edited texts from some of the
20th century’s most influential scientists, philosophers, theologians, and authors. Einstein
reportedly was very enthusiastic about the project, telling Anshen “[It is] a very good
plan. You want the future to come sooner.” By the late 1970s she had founded five
additional series and through these had published over 130 edited volumes and
monographs. These series became “a crusade to ferret out kindred spirits, those European
and American thinkers who shared her vision.”

Many of those whom she ferreted out
were former participants in the Conference on Science, Philosophy and Religion, at
which Urey had first expressed his view of the civilizing force of Christianity and its
importance in preserving democracy. As Anshen herself described the purpose of these
endeavors, they were similar to those of the Conference (although not limited to the
American way of life): “We were heretics, burning not at the stake but in our hearts and

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785 Wyckoff, “Anshen, Ruth Nanda (b. 1900),” 54.
787 Anshen recounted this discussion with Einstein in Ibid.
minds with one unending plea for unity. … We hoped to provide the core of a cultural Magna Charta for the guidance of our species.”

When she was first planning the *Science of Culture Series*, Anshen contacted Urey and asked if he would contribute to what would become her second edited volume, *Science and Man*. Eager to participate, Urey at first promised Anshen an essay that he said would be titled “Chemistry and the Physical Foundation of Civilization.” By January of 1941, however, Urey asked “to be excused from doing so,” explaining that he had been so busy with his war work that he had even had to cancel all of his scientific lectures for the year. Instead of writing a new essay, Urey contributed the transcript of a speech he had given in 1937, “The Position of Science in Modern Industry.” When the volume appeared in 1942, Urey’s essay was published in a section entitled “Science: Its Materials, Methods, Ends,” which was headlined by Arthur H. Compton’s essay, “The Purpose of Science” and also contained essays by Waldemar Kaempffert and Brand Blanshard that addressed the relationship between science, democracy and values. Also included were essays by the likes of Reinhold Niebuhr, Jacques Maritain, Bronislaw Malinowski, Julian Huxley, Walter B. Cannon, Lewis Mumford, and Jean Piaget.

After the war, Anshen shared Urey’s concern that a “fact-based, materialistic world” would be “perilously devoid of ethics and values.” It was her contention that this very “threadbare” worldview was responsible for the destruction of Europe during

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789 Anshen quoted in Ibid.
790 Harold C. Urey to Ruth Nanda Anshen, May 21, 1940, Box 5, RNA.
791 Harold C. Urey to Ruth Nanda Anshen, January 6, 1941, Box 5, RNA.
792 Ibid. This speech appeared in the previous chapter, as it was also published under the title, Urey, “Accomplishments and Future.”
794 Teich, “Editing Einstein,” 110.
the war. Not long after *The Planets* had appeared in print and Urey’s public rhetoric had shifted in the direction of his comments at the Conference, Anshen approached Urey a second time in the hope that he would be willing to publish a monograph in her new *World Perspectives Series*.

Anshen recognized a kindred soul in Urey. She told him that she was approaching a select few scientists who shared her viewpoints on the importance of integrating science with spirit:

> Only those spiritual and intellectual leaders who possess full consciousness of the pressing problems of our time are invited to participate in our Series: those who are aware of the truth that beyond the divisiveness among men there exists a primordial unitive power that we are all bound together by a common humanity more fundamental than any unity of doctrine; those who recognize the error of the environmentalist who forgets that man is an element of every experiment and that the most important element in man’s environment is his fellowmen; those who realize that the centrifugal force which has scattered and atomized humanity must be replaced by an integrating process and structure giving meaning, purpose and dignity to existence.

For his contribution, Anshen suggested that Urey publish an abridged version of *The Planets* that would be understandable to the general public. She tentatively titled this hypothetical volume *The Origin of the Solar System*.

Urey was at first enthusiastic about the possibility of publishing such a book. He instructed Yale University Press to send Anshen a copy of *The Planets* and agreed to a deadline of September 1955. He told Anshen that he was in the middle of attempting to revise the book for Yale and bring its discussion of the available data up-to-date, but that he felt this would not take long; he was confident that the general arguments he had made

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795 Ruth Nanda Anshen to Harold C. Urey, March 5, 1954, Box 5, RNA.
in the book were still valid. Anshen was also enthusiastic to have the distinguished chemist onboard. After she had read the copy of Urey’s book that Yale had sent to her, she wrote to him that “Perhaps the new consciousness of our epoch consists in the increasing awareness of the cosmic influences on man. How reassuring it is to know that you contribute to this consciousness with so much lucidity and integrity.”

Urey never met the 1955 deadline. Although he and Anshen continued to correspond for the next thirteen years, and even met in person twice to discuss the importance of bringing Urey’s new work to the public, the book that they envisioned never appeared. Over the next thirteen years, Anshen continued to ask Urey for a monograph about the planets. In language that sounds quite similar to Urey’s own public rhetoric from this period, Anshen suggested that “Our series is dedicated to the definition of what may be considered a revolution in thought and a widening of horizons comparable perhaps to the beginning of the new scientific era… This revolution seems to be taking place in spite of the intransigence of nationalisms and in spite of the spiritual and moral erosion of our time.” She suggested that his contribution would help “to restore sapientia to scientia and to articulate the integrating forces which are moving in the two hemispheres of humanity.” She implored him that “it is indeed indispensable to the spiritual and intellectual community that your seminal thought be represented.”

Urey continued to tell Anshen that he needed more time to revise his original work, and provided her with new estimated dates of completion. Finally, unceremoniously, Urey

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796 Harold C. Urey to Ruth Nanda Anshen, March 16, 1954, Box 5, RNA.
797 Ruth Nanda Anshen to Harold C. Urey, April 4, 1954, Box 5, RNA.
798 Ruth Nanda Anshen to Harold C. Urey, August 3, 1954, Box 5, RNA.
799 Ibid.
800 Ruth Nanda Anshen to Harold C. Urey, March 7, 1955, Box 5, RNA.
wrote to Anshen in 1967 to tell her that “I have not thought seriously of writing a book at all, but if I were going to I would try to revise my book on the planets. This of course would have to be published by Yale University Press and would not give you any help.”

Promising the Moon

Urey did try to revise *The Planets* for Yale in the mid- to late-1950s, but with little success. In addition to wishing to bring the book up-to-date with work that had appeared since its initial publication, Urey wanted to develop a new framework for the formation of the solar system that would allow him to replace the framework that he had borrowed from Kuiper. In Kuiper’s model, planetary systems were formed in special cases of failed binary star formation. In the place of a companion star, the stellar nebulae in these instances produced protoplanets. It was in order to solve the chemical problems in his model that Kuiper struck up a collaboration with Urey in late 1949 after the Rancho Santa Fe Conference. Urey had at first adopted Kuiper’s binary star and protoplanet model, and Kuiper in turn adopted Urey’s hypothesis of the cold accretion and subsequent radioactive and gravitational heating of the terrestrial planets. But in the early 1950s Kuiper began moving away from Urey’s cold accretion model and toward a hot model of planetary formation within which the terrestrial planets had all gone through a completely molten phase.  

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801 Harold C. Urey to Ruth Nanda Anshen, January 5, 1967, Box 105, Folder 3, HCU.
802 The failed collaboration between Urey and Kuiper is described in Doel, *Solar System Astronomy in America*, chap. 4 “Consensus, then Controversy.”
That Urey had to depart from Kuiper’s model in order to keep his model of the moon intact became clear to him as early as 1955, when Kuiper published a paper in which he claimed that the moon had at one point been completely melted. It also became clear to him in this year that the planetary community was split between the cold and hot models. Urey began writing to colleagues in astronomy and geology to try to convince them of Kuiper’s errors, in addition to publishing criticisms of Kuiper’s model. He wrote to the astronomer Dinsmore Alter that “[Kuiper’s] I do not believe is a tenable position at all and I think that all his conclusions are colored by his theory, so I doubt his observations very much.” In a critical review of Kuiper’s work, Urey claimed that Kuiper had based his arguments on new telescopic observations of the lunar surface, but that in fact the astronomer had not observed “anything markedly different from what has been previously observed and recorded in the extensive literature on the moon.” As in his letter to Alter, Urey claimed that what Kuiper now saw was actually derived from his theory, which dictated that the moon must at one time have been completely melted. Urey felt that his own theory, on the other hand, was based upon unbiased observations of the moon. The most compelling of these observations was still the moon’s lack of isostatic equilibrium, which he interpreted as evidence that “the moon cannot now or ever have been in the past as plastic as the outer parts of the earth are today. This means a cold origin for the moon, and it means that it has remained cold up to the present time.” Urey also was convinced that the age of the solar system as determined by Clair C.

803 Ibid., 148.
804 Harold C. Urey to Dinsmore Alter, February 9, 1955, Box 3, Folder 6, HCU.
806 Harold C. Urey to Dinsmore Alter, June 9, 1954, Box 3, Folder 6, HCU.
Patterson together with his own determinations of the abundances of the radioactive isotopes presented “serious difficulties” that the hot model could not overcome.\textsuperscript{807}

The split with Kuiper was not limited to differences of theory. Urey also was at this time feuding publicly with Kuiper, whom he accused of using his ideas without citing them properly and, in Urey’s view, attempting to pass them off as his own. In 1953 he had begun arguing in his public speeches that the Ten Commandments laid out clear norms for the practice of science: “‘Thou shalt not lie.’ You must not lie about or misrepresent your data. ‘Thou shalt not steal.’ You do not assume that you have done work which others have done.”\textsuperscript{808} He told his audiences that any scientist who violated these commandments would be driven out of the scientific community. Now, only two years later, feeling that Kuiper had transgressed these norms, Urey began a letter-writing campaign to have the astronomer removed from the presidency of the International Astronomical Union’s section on planetary research and to have the NSF subsidies for his editorial projects cancelled.\textsuperscript{809}

Urey began forming his own model of solar system development in 1956. In this year, Urey decided to work backwards from the physical properties and chemical composition of the meteorites to determine what kinds of processes and objects had formed them. From this exercise, Urey concluded that two types of objects had been created and destroyed before the accretion of the planets. These objects, which he termed “primary” and “secondary” objects, were of lunar and asteroidal size, respectively. From the ages of the meteorites, Urey determined that the primary objects must have

\textsuperscript{808} Urey, “The Intellectual Revolution,” 10.
\textsuperscript{809} Doel, \textit{Solar System Astronomy in America}, 142.
accumulated 4.5 billion years in the past. In order to melt the metal and silicates within the primary objects, they had to be heated by some means to the melting point of these materials, and then cooled to 500° C for several million years. After cooling, the primary objects were broken into very small fragments (ranging from a millimeter to a centimeter) and then reaccumulated to form the secondary objects. The breakup of these secondary objects then formed the meteorites.\footnote{Harold C. Urey, “Diamonds, Meteorites, and the Origin of the Solar System,” \textit{The Astrophysical Journal} 124 (November 1956): 623.} The primary and secondary objects thus predated Kuiper’s protoplanets, and it was the pieces of these original objects that accreted to form the planets.

Urey continued developing his model while in England as the Eastman Professor at Oxford University. Here he completely abandoned the binary star model and made the primary processes in the formation of the planets chemical rather than astrophysical. On the same grounds where he dined and chatted with the Christ Church theologians, Urey developed a model of planetary system formation that made planets a normal result of the birth of stars. Unlike Kuiper’s, Urey’s model did not require special astronomical circumstances. Instead, Urey postulated that as any great rotating mass of gas contracted to form a new star, the need to preserve angular momentum caused it to throw off a rotating cloud of dust and gas. This cloud would flatten into a disk rotating in a single plane about the newly formed star. The formation of an opaque cloud near the sun then cooled the disk by absorbing the radiant energy of the star. It was from this disk that the primary objects were formed and then underwent their subsequent transformations.\footnote{Urey described these ideas developed in Oxford in Harold C. Urey, “Statement before Committee of the House of Representatives on the Objectives of the Exploration of the Moon and Planets,” typescript, March 11, 1965, HCU NASA.}
By the time Urey sat down with Jastrow for the first time in 1958, he had begun revising his theory of the origin of the meteorites. The general story of the primary and secondary objects remained intact. However, the secondary objects were now the parents of only the chondritic meteorites. The stoney meteorites, on the other hand, Urey now believed must have come from the moon or other moon-like objects. This led Urey to the conclusion that the moon was in fact a primary object, older than the earth, that had been captured by the planet after accretion. As he noted in a 1959 paper, “No simpler process seems feasible for accounting for the facts [about meteorites].”\textsuperscript{812} Urey speculated that there were likely far more remnant primary objects in the solar system in the past, and that they may have played an important role in the evolution of the solar system. As for the moon, Urey argued that it should be “composed of the more nearly correct solar average material of the less volatile kind than the earth and other terrestrial planets.”\textsuperscript{813}

These developments in the latter half of the 1950s would seem to indicate that Urey’s revisions of \textit{The Planets} were going well. Indeed, in September 1959 Urey reported to his editors that he had completed a revision of the chapter on the moon, was in the process of revising the chapter on the terrestrial planets, and had written an entirely new chapter on meteorites. He wanted to distribute the revised chapters on the moon and meteorites immediately in mimeographed form to his colleagues at NASA. He felt that this was essential because, as he put it, “There have been a number of very bad reports put out by various people … who have not studied the subject at all carefully and merely report that so-and-so says this and so-and-so says that without any attempt to evaluate

\textsuperscript{813} Ibid., 1729.
ideas. … [My ideas] may not be right but at least one person has thought about the subject for 10 years and has attempted to put down his ideas in systematic form.”\textsuperscript{814} At this point, Urey still felt that his model of the moon would be the driving force of NASA’s lunar program.

Just one year later, however, Urey was reporting to Yale that “During the last three months I have gotten nothing done on the book. In fact, I have been pulled by the space program and its various meetings in one way or another until I have gotten nothing at all done. I am now trying to curtail these activities and if need be I will resign all connections with it in order to finish the job.”\textsuperscript{815} Urey’s experience at NASA was on the whole “discouraging,” and eventually completely soured him toward the idea of revising and publishing an updated edition of \textit{The Planets}. As he reported to his editors in 1961, trying to convince his fellow lunar scientists of his theory’s merits was futile, not because his ideas were unreasonable but because everyone had a theory of their own: “I find that I have worked for years on this problem of the origin of the solar system and I have advanced ideas which seem so reasonable to me, but other people do not believe them at all. In the last year a number of other people have undertaken to write similar things; I have no doubt but what they think they are very reasonable ideas, yet I do not believe their ideas at all. In fact, I have a feeling that no one working in the field believes what anyone else does.”\textsuperscript{816}

\begin{flushright}
\textsuperscript{814} Harold C. Urey to Yale University Press, September 16, 1959, Box 105, Folder 16, HCU.
\textsuperscript{815} Harold C. Urey to Yale University Press, February 3, 1960, Box 105, Folder 16, HCU.
\textsuperscript{816} Harold C. Urey to Yale University Press, January 26, 1961, Box 105, Folder 16, HCU.
\end{flushright}
The Cold Moon Crusade within NASA

Although Urey’s moon provided the scientific justification for NASA’s lunar program, Urey felt that he had great difficulty within NASA’s lunar working groups getting a fair hearing for his ideas. He had succeeded in forming his own model for the formation of the solar system and the origin of the moon as a primary object. However, he had succeeded neither in doing away with Kuiper’s model, nor in winning over many converts within the planetary science community. While the community was still split over whether or not the moon was hot or cold, whether it was captured or was ripped from the earth, and whether or not its features were due to volcanism or bombardment, Urey’s chemical model was not the go-to model for the cold moon contingent. Furthermore, much to Urey’s dismay, Kuiper was making inroads within NASA and it seemed to Urey that the astronomer’s ideas were being treated more favorably than his own.

Kuiper’s 1959 theory of the moon’s origin, Urey thought, was not substantially different from that which Kuiper had presented in 1955, and Urey was annoyed that Kuiper had done little to take his criticisms into account: “I criticized this theory in 1955 and his reply was quite unsatisfactory. Kuiper may not like my criticisms but he has an alternative, namely, to stop talking about it.” A few years later, Urey wrote to Jastrow to complain “What a dreary business the space program is in many ways. … One must be around a certain astronomer much to one’s discomfort, etc.”

817 Harold C. Urey to Robert Jastrow, June 10, 1959, Box 47, Folder 15, HCU.
818 Harold C. Urey to Robert Jastrow, December 12, 1962, Box 47, Folder 15, HCU.
It cannot be overemphasized just how annoyed Urey was with Kuiper’s presence and favored position within NASA. By the time Jastrow met Urey, the chemist had moved his research program from Chicago to the newly formed University of California, San Diego. In August of 1957, before the option of moving to La Jolla had even occurred to Urey, he had written a letter to Willard Libby detailing the reasons why he no longer wished to remain in Chicago, and asked his colleague if he knew of “a possible ‘out.’” First among the reasons that Urey listed for wanting to leave was that Kuiper had recently been appointed the director of Chicago’s Yerkes Observatory: “This means no satisfactory contact with the astronomers from now on. This has been the case from 1952 and was somewhat the case since 1949. … If I had been a younger man, I would have left the university and my present field of research long ago.” Libby wrote to the oceanographer Roger Revelle, who was then recruiting for his new university in La Jolla, and explained Urey’s unhappiness. Within a matter of months Urey had accepted an appointment as Professor of Chemistry-at-Large in the University of California. Within a year, without having yet found a buyer for their Chicago home, Harold and Frieda Urey had moved to La Jolla.

The move to La Jolla represented a clean break with the Chicago astronomers. Thus it is not surprising that Urey was unhappy with NASA’s in-house staff, their dismissal of his theories, their unfamiliarity with his own publications on the subject, and their uncritical acceptance of Kuiper’s work. In one particularly discouraging exchange, Urey reported that a scientist from JPL visited him in La Jolla, “[sat] at my desk and

819 Harold C. Urey to Willard F. Libby, August 26, 1957, Box 52, Folder 45, HCU.
820 Harold C. Urey to Willard F. Libby, November 22, 1957, Box 52, Folder 45, HCU.
821 Harold C. Urey to Willard F. Libby, November 18, 1958, Box 52, Folder 45, HCU.
[told] me essentially that he does not believe anything I say and apparently does not even think my arguments worth considering."822 This prompted Urey to write an angry and severely critical letter to Al Hibbs, head of the Laboratory’s lunar projects, stating that he would rather not have his time wasted in this way.823 In a later letter to Hibbs, in which he apologized for his initial outburst, Urey admitted that the true nature of the problem was the uncritical acceptance of Kuiper’s theories at JPL:

> It does seem to me that someone connected with the program might try to evaluate a theory that is ten years old. Of course, I am to blame also because I too accepted the theories rather blindly. My only excuse is that I am not a trained astronomer and I assumed that the work being published was being critically reviewed by others in the field and that I could trust the results of such studies … Of course, part of my difficulty is that I am very much annoyed at myself for being taken in, but I am even more annoyed at the fact that there is no critical review of this subject.824

These complaints to Hibbs continued. In November of 1959, Urey wrote to Hibbs about the lunar symposia, stating that “the people that run them do not know good men from bad. Therefore, the program always has poor papers and good papers mixed without any discrimination whatever.”825

Urey’s frustrations with Kuiper’s resilience only escalated. In early 1960 Urey became aware that Kuiper was not only gaining favor within NASA but also planning to move west to take a position at the University of Arizona, where Urey feared he would come to dominate the newly constructed Kitt Peak National Observatory. Urey wrote to Albert Whitford at the Lick Observatory to see if he could enlist his support in an effort

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822 Harold C. Urey to Al R. Hibbs, July 3, 1959, Box 43, Folder 3, HCU.
823 Ibid.
824 Harold C. Urey to Al R. Hibbs, August 10, 1959, Box 43, Folder 3, HCU.
825 Harold C. Urey to Al R. Hibbs, November 16, 1959, Box 43, Folder 3, HCU.
against Kuiper.\textsuperscript{826} On the same day, Urey sent a similar letter to Kitt Peak’s Director, Aden B. Meinel, asking if he did not feel “a little concerned about the possibility that the instabilities of Yerkes Observatory might be transferred to Kitt Peak.”\textsuperscript{827} He told Meinel, “I think it is no great secret that many, many people who have done work anywhere near that which interests this man have had considerable trouble. Possibly it is always the fault of the other fellow but I doubt this to some extent.”\textsuperscript{828} Urey emphasized that his concern was not due to any personal feud that he had with Kuiper, but about securing a “stable regime” at the country’s newest and most advanced national observatory.

Urey’s overture to Meinel likely fell on deaf ears. Meinel was one of Kuiper’s colleagues at Yerkes before coming to Tucson and was already well aware of the conflicts in Chicago. Meinel had headed the study that selected Kitt Peak as the location of the new observatory. Once he became Director of the observatory, Meinel helped Kuiper to extricate himself from his problems in Chicago. According to the account of one of Kuiper’s collaborators, these problems arose not from any problems with Kuiper’s personality but from “undercurrents of discontent [that] were circulating among some of the nonlunar-oriented personnel,” and from Kuiper’s “generally strong-arm tactics” in promoting and favoring lunar work over more traditional astronomical work.\textsuperscript{829}

Meinel brokered an agreement with the University of Arizona’s Coordinator of Research that would allow Kuiper to establish a NASA-supported Lunar and Planetary Laboratory at the university. Meinel viewed such an addition as a great opportunity for

\textsuperscript{826} Harold C. Urey to Albert Whitford, February 1, 1960, Box 51, Folder 7, HCU.
\textsuperscript{827} Harold C. Urey to A.B. Meinel, February 1, 1960, Box 51, Folder 7, HCU.
\textsuperscript{828} Ibid.
\textsuperscript{829} Ewen A. Whitaker, The University of Arizona’s Lunar and Planetary Laboratory: Its Founding and Early Years (Tucson: University of Arizona Printing-Reproductions Department, 1985), 16.
the university and the observatory, and worked to smooth over any reservations about Kuiper that existed in Tucson.\footnote{Ibid., 18.} By the time Urey wrote to Edwin F. Carpenter, Director of Arizona’s Steward Observatory, explaining that Kuiper was the reason he had left Chicago, it was too late.\footnote{Harold C. Urey to Edwin F. Carpenter, January 19, 1960, Box 51, Folder 7, HCU.} Kuiper had already visited Tucson and laid the groundwork for his new Laboratory. Although Carpenter was no great fan of Kuiper, Meinel had convinced him that the astronomer and his NASA ties were worth whatever trouble he might bring with him.\footnote{Whitaker, \textit{The University of Arizona’s Lunar and Planetary Laboratory: Its Founding and Early Years}, 18.}

Urey’s crusade against Kuiper was entirely unsuccessful. By the end of the summer, Kuiper and his staff were in Tucson building their new Laboratory. Also in that same summer, Kuiper was invited by NASA’s Deputy Administrator Hugh L. Dryden to serve as a consultant on NASA’s Planetary and Interplanetary Sciences Subcommittee.\footnote{Ibid., 25.} The die was cast. Urey’s complaints continued, but he no longer seemed to expect that anything would result from his criticisms. Late in 1962 he wrote to his friend the Dutch astronomer Jan Oort about a recent lecture Kuiper had given in Leiden on the structure of the moon. Urey had heard about the lecture through a colleague in attendance and was not happy with the report he received: “[As] usual he neglected the work of other people. … In this case even the technique he uses to produce the pictures you will note was invented by myself several years before he undertook to use it.”\footnote{Harold C. Urey to Jan H. Oort, November 9, 1962, Box 73, Folder 7, HCU.} He sent Oort a reprint of the 1956 article in which he had originally criticized Kuiper’s theories and asked Oort, as a fellow “product of The Netherlands,” to account for Kuiper’s behavior: “Dr. Kuiper
is a product of The Netherlands and perhaps you understand why it is that he spends so much effort in attempting to give the impression that previously published work is indeed original with him at much later dates.” Urey concluded his letter with one last expression of his exasperation with Kuiper, asking Oort, “Why don’t you take him back to Holland?”

Urey’s enthusiasm for his own relationship with NASA seemed to wane at this time. Even within Jastrow’s theoretical group, where he felt he could get a sympathetic hearing, Urey didn’t have to look far to find unsympathetic responses to his capture hypothesis. He wrote to Jastrow toward the end of 1962 to complain about the behavior of his junior colleagues: “Recently O’Keefe and Cameron published a paper in *Icarus* on the moon in which they disagreed with everything I have said about the moon during the last 10 years but they thanked me profusely at the end. This makes me thoroughly angry. I do not like to be thanked for being their ‘whipping boy.’ … I know your intentions are good but I do not like being ‘made over’ and then have my ideas ignored.”

By the summer of 1963, Urey seemed even more demoralized than ever. He wondered “why I bother to do anything for NASA,” and complained that the Space Administration was “the most unsatisfactory contact I ever had … in my life.” At every committee meeting he attended, Urey felt that his fellow lunar scientists only wanted to “clobber” his theories: “It seems to me that people think that everything I do is wrong; in fact,

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835 Ibid.
836 Ibid.
837 Urey to Jastrow, December 12, 1962.
everything that I say in one of these committee meetings is wrong immediately as soon as it is said without any doubt."\textsuperscript{838}

**Manned Missions and the Problem with Pictures**

Although Urey’s disappointments were in many respects personal, they were also brought on by the political nature of NASA’s mission. NASA’s Cold War mission was to establish a strong American presence in space, not to provide an inspiring view of the universe. This became increasingly clear after President John F. Kennedy’s announcement in 1961 that the United States would land a man on the moon before the end of the decade. NASA Administrator James E. Webb had convinced Kennedy that “To be pre-eminent in space we must conduct scientific investigations on a broad front … in the minds of millions, dramatic space achievements have become today’s symbol of tomorrow’s scientific and technical supremacy.”\textsuperscript{839} Manned missions to the moon became the US response to Soviet manned flights into space, and manned spaceflight became synonymous with scientific exploration. NASA was reorganized around the Apollo program in November 1961, and Homer Newell now became director of the Space Administration’s new Office of Space Sciences.

According to David DeVorkin, Newell interpreted the new structure and priorities of the space program to mean that he had no choice but to link space science to Apollo. The robotic lunar missions that Newell had overseen now became precursors to the “real

\textsuperscript{838} Harold C. Urey to Robert Jastrow, June 28, 1963, Box 47, Folder 15, HCU.
science” of having human observers on the surface of the moon. This in turn led to a redefinition and reorganization of the priorities of existing lunar missions. Project Ranger, for example, had been designed to be a multi-functional lunar probe. After the announcement of Apollo, however, Ranger’s scientific program was cut and the remaining probes’ missions were repurposed as support for Apollo. Landing site selection now became a top priority. Any “pure science” that flew onboard Ranger or the later Surveyor spacecrafts was now tasked with providing engineering information for Apollo.840

This decision limited much of the activity of the pre-Apollo missions to the photographic reconnaissance of possible lunar landing sites. Urey did not respond favorably to this decision. Throughout the remainder of the 1960s he argued that the pictures taken of the moon, while he “found them very interesting” were “rather difficult to interpret.” In them, he explained, “Each person sees … exactly what he expected to find there – evidence of volcanism, of movement of dust, for fragmented material, liquid water, and so forth.”841 Some evidence of the chemical composition of the moon had been gathered on robotic missions prior to the reorganization of the program, and it was this evidence that Urey preferred to privilege. He told his colleagues that he reserved final judgment about the moon’s origins for the day that laboratory analysis could be performed on returned lunar samples, and he encouraged them to do the same.

840 Ibid., 102.
Much to Urey’s dismay, however, this reliance on photography and telescopic observation allowed for a new group of scientists to rise through the ranks of NASA – the planetary geologists. These geologists would later congratulate themselves, in the words of Donald Wilhelms, on making the moon “a world of rock” and dethroning “the physicists and other quantitatively minded scientists who once dominated space science.”

Indeed, a study of the field geologists within the United States Geological Survey (USGS) who took up the study of the planets (and who labeled themselves “astrogeologists”) does show that some of the same Cold War patrons and pressures kept field work alive within lunar and planetary science even as the geosciences as a whole moved away from the field and formed strong prejudices against observational methods. These same forces allowed field geology to carve out a prominent place for itself within the newly forming planetary science community – a place from which it was able to reinvent its methodologies via new remote sensing technologies.

The USGS Astrogeology Branch began its existence in 1960, under the direction of the geologist Eugene Shoemaker. Although it did incorporate geochemistry, the Astrogeology Branch was strongly defined by its field component. Shoemaker and his colleagues could not do field work directly on the moon. However, using telescopic observations and NASA photographs, they attempted to produce the types of geological maps that a field geologist would construct from field observations. In their previous work, many of the astrogeologists had learned techniques of photogeology, and they had become accustomed to making maps from aerial photographs. The astrogeologists began

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842 Wilhelms, To a Rocky Moon, x.
making geological maps of the moon using the Lick Observatory’s 36-inch refractor and the Lowell Observatory’s Clark refractor. The derivation of stratigraphic maps from photographs and remote imagery was further reinforced when NASA built the Astrogeologists their own observatory, staffed by USGS personnel and used exclusively for lunar mapping. The trend of course continued with the lunar images returned by the robotic Ranger and Surveyor missions, all of which carried cameras and returned thousands of black and white images via television signal.\textsuperscript{844}

The astrogeologists tended to treat the moon as an extension of terrestrial terrain. Brown University stratigrapher Thomas Mutch, one of the few geologists outside of the USGS to take up astrogeology during this period, wrote the first textbook of planetary geology.\textsuperscript{845} Here he described the history of terrestrial geology as the exciting story of how fundamental questions had been asked and answered through the slow and persistent study of the earth.\textsuperscript{846} That the same type of work could now be done on the moon, and that it might even uncover a stratigraphy comparable to that found on earth, meant that fundamental questions could be asked once again. It is in this context that Mutch introduced photogeology as the successor to traditional field work. In the absence of actual ground truth, Mutch advocated the use of earth analogues in the stratigraphic interpretation of telescopic and spacecraft images, allowing astrogeologists to bring terrestrial field experience to bear upon lunar surface features. For his frontispiece, Mutch

\textsuperscript{844} The photos were actually developed and scanned onboard the spacecraft, then returned to Earth via video signal where the images were regenerated and transferred to 35mm film. As one might imagine, this technique did produce a number of artifacts in the returned images.

\textsuperscript{845} Thomas A. Mutch, \textit{Geology of the Moon: A Stratigraphic View} (Princeton: Princeton University Press, 1970). Mutch’s text was written after spending a sabbatical at the USGS Astrogeology Branch, and was completed with the assistance of the USGS astrogeologists.

\textsuperscript{846} In Mutch’s version of the history of geology, the discovery of plate tectonics was not a revolution, but part of this same steady advance of knowledge.
chose an Ansel Adams’ photograph, “Autumn Moon, High Sierra from Glacier Point”; portraying the moon as it might be viewed by a field geologist on a clear night, the photo highlighted one of the book’s main themes – the moon was unexplored territory, but it was connected to something familiar and mapped.

Well into the 1960s, Urey would clash continuously with the astrogeologists, claiming that they were “second rate” scientists who knew little basic science and had few publications to their names. In 1961, for example, Urey wrote in a letter to Al Hibbs at JPL:

I believe that geology is the worst training that can be given to a man for the investigation of the Moon and planets. The reason for it is this. Geology … is largely a purely descriptive science with a very minimum training in the more mathematical aspects of chemistry and physics. It largely deals with description of rocks, sedimentary and igneous, and the gross features of the earth. I have been associated with geologists now for some 10 years and I have met a few who are good exact scientists, but the training which students get in the usual geology department is a very descriptive training.847

Urey explained that the processes and theories geologists were familiar with would be of no use on the moon, which he felt had a very different past than the Earth. Thus they would be blinded by their preconceptions and their allegiance to such theories as uniformitarianism. Urey then revealed his Manhattan Project biases as he went on to suggest that space scientists should be chosen mainly from those disciplines that attracted the best and brightest scientific minds – nuclear physics, solid-state physics, and physical chemistry. Geology was not one of these disciplines; as Urey explained to NASA

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847 Harold C. Urey to Al R. Hibbs, September 26, 1961, Box 43, Folder 3, HCU.
Associate Administrator George E. Mueller, “[We] all know that geology attracts the less brilliant type of scientists.”

To be clear, Urey knew he could not do the work of a hard rock geologist. With Lowenstam he had made trips to the American south and had watched his geologist colleague “spot exactly the beginning of the Eocene period in the top of the cretaceous in the rocks which I could not recognize at all.”

On a later vacation with his family in Colorado, Urey tried his hand at geological collecting, only to find that he could not determine what was important and what was not: “I just did not know where I was in geological collection.” From these experiences, Urey concluded that geology did in fact “require an enormous amount of careful personal observations,” and that “A mature scientist finds it difficult to become expert in fields of this kind.” This type of observational work did not suit Urey, as he admitted, “I had studied geology as a student in college and had found it to be an exceedingly boring subject. I just couldn’t get particularly interested in all of the rocks that were laid out for us to study at the time.”

Urey did admit that geologists were essential for lunar exploration. As he told Mitroff, “I’ve always said the geologists must be used in the interpretation of the moon. They must be used, they are the ones that know what igneous rocks are like. I don’t. Other people don’t. They know when they pick up a rock, that this belongs to such and such a class of rock, I don’t.”

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848 Harold C. Urey to George E. Mueller, October 7, 1969, HCU NASA.
850 Ibid.
851 Ibid.
852 Ibid., 23.
853 Harold C. Urey, “Tape 89: Urey, Harold; Green, Jack, undated,” interview by Ian I. Mitroff, n.d., 6, Box 5, Folder 20, IIM.
Flight that sample collection to determine the origin of the moon would require that “all astronauts be well trained hard rock geologists.” However, Urey felt that, as it was, the lunatics were running the asylum. Geologists were valuable as observers and collectors, not as theorists. Instead of performing the service role to which they were best suited, the geologists were actually guiding NASA’s mission planners.

Urey was right. An internal NASA memo from the Director of Apollo Lunar Exploration responding to Urey’s criticisms confirms Urey’s suspicion that the astrogeologists were having a great influence on the Apollo missions: “We have turned strongly to astrogeologists for advice on site selection in the past because, with our present paucity of knowledge, the topography and stratigraphy of the lunar surface has been our key input. … The astrogeologists were of major assistance in [Ranger, Surveyor, Orbiter and now Apollo] and have been responsible for a major portion of the data analysis.” The astrogeologists had successfully translated NASA’s priorities into the development of their own niche within the Space Administration.

854 Harold C. Urey to Jay Holmes, January 10, 1964, HCU NASA.
856 For more on the success of the astrogeologists within NASA corridors, see Donald A. Beattie, Taking Science to the Moon: Lunar Experiments and the Apollo Program (Baltimore: Johns Hopkins University Press, 2001). Beatty describes how he and his geologist colleagues were able to convince NASA that geological field work would “optimize those aspects of exploration that humans do best: observing, describing, manipulating complex equipment, and responding to the unexpected” (41). This was essential in making their case, as Apollo’s managers were trying to strike a delicate balance between automated functions that minimized human error and manual tasks that took advantage of the astronauts’ human abilities.
So many of these people would like to have the moon be something ordinary like the rocks of the earth. I am prejudiced. I would like to have the surface of the moon be something unusual – something that would tell us about the early history of the solar system 4 ½ billion years ago. But many of the people would be glad to have the moon be exactly like the earth. If that is the case, here is one taxpayer that does not think that it is worthwhile.  

Harold C. Urey  
Letter to George de Hevesy (1960)

The moon that Urey had helped Jastrow to sell to NASA and Congress was scientifically unique, and it was this uniqueness that made the moon valuable in Urey’s eyes. Urey was comfortable stating in the memo he co-authored with Jastrow that a lunar mission would be of greater scientific importance than a similar mission to Mars or Venus because of his belief that the moon would hold evidence of the formation of the solar system that had long since been erased on the other terrestrial planets. A high profile scientific mission to such an important object was what Urey imagined would “capture the imagination of the scientific community and the general public to a greater degree than any project of comparable scientific value.”

His version of the moon put the secrets of the solar system in close enough proximity that samples could be gathered, and put the isotope geochemist in the position to unlock those secrets. Furthermore, that this science could be done in the eyes of the world, in the midst of a highly publicized space race with the USSR, would allow him to perform publicly the type of inspirational

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857 Harold C. Urey to George de Hevesy, December 29, 1960, Box 42, Folder 27, HCU.  
858 Memo quoted in Jastrow, “Exploring the Moon,” 47.
work that he had described in his public speeches. He and his colleagues would perform a public communion with the laws of the universe.

Any other moon would not fit the bill. This became clear to Jastrow’s dismay at the end of 1959, in a press conference held by his Theoretical Division. One reporter asked Urey why he was so insistent that NASA should answer important questions on the moon, and not just show the world that the US can reach it: “I was under the impression that there was some objective in reaching the moon, other than simply learning about it.” Urey responded that although he was personally “thrilled by a feat of exploration,” such as when explorers had reached the South and North Poles or when the Russians had photographed the dark side of the moon, from a scientific point of view there was no justification for the exploration of the moon if not to determine its origin: “Our primary concern as scientists is to try and understand this universe. I am much more interested in the origin of the solar system … than I am in making a trip somewhere.”

When it came to the moon, Urey thought he knew exactly what NASA should find. If it found something different, it was hardly worth looking at. Jastrow, on the other hand, while acknowledging the place of existing theories as a starting point for exploration, painted lunar exploration in the colors of territorial exploration. The moon might hold the answers to Urey’s big questions, but it was also the next great frontier to be explored.

The moon as a frontier was not a moon that appealed to Urey. In a 1967 argument in favor of space exploration, Urey countered Max Born’s criticism that space travel, and especially the space race, held little benefit for mankind. Rather than being “a common

859 National Aeronautics and Space Administration, “Press Conference on Lunar Science” (Washington DC, December 1, 1959), HCU NASA.
undertaking of all peoples which would act for the reconciliation of antagonisms and the maintenance of peace,” Born claimed it was “a symbol of a contest between the great powers, a weapon in the cold war, an emblem of national vanity, a demonstration of power.”860 It was not “a lightning conductor of our inborn aggressiveness and violence” that would prevent war, but “a preparation for war, a dangerous game.”861 The relatively small scientific gains from exploring the moon and planets were not worth the illusions of superiority and the resulting increase in nationalism. Urey rallied to the defense of space science and lunar exploration, but in a very limited fashion:

Of course if the moon escaped from the earth all we can conclude is that an insignificant planet, Earth, made a mistake, got too much angular momentum, and solved its problem by throwing off an insignificant cinder in space. If this is the case, I shall be immensely disappointed and shall feel that my attention to this subject in the last years has been wasted time and effort. But if the moon is a primitive object captured by the earth and has on its surface at least a partly very ancient surface that enables us to say something about the events during the very early history of the solar system, it will be an enormously interesting object in connection with the origin of the solar system, stars, etc. It might even give us a sample of material that was indeed the primitive material from which the solar system evolved…862

Put in stronger terms, Urey later told O’Keefe, “I shall be sorry and disappointed if … the moon then will be an incidental object and not of fundamental importance. We can decide that it escaped from the earth and then ‘to hell with it.’”863

As for the lunar mapping that was so important for the Apollo program and for the professional development of astrogeology, Urey was certain that selenography would be a dull subject: “There are those who fully expect to map the moon in great detail. But

861 Ibid.
862 Urey, “Affording the Space Program,” 25.
the mapping of the earth has been important because of its very active history and because men live here. … One cannot expect that this sort of interesting phenomenon will be extended to a cinder such as the moon. When he spoke before the public, he often cautioned his audiences that the moon might in fact be disappointing, but that he hoped it would be “interesting” enough to make the twenty billion dollar Apollo program “worthwhile.” Urey’s gamble was that the moon would be of fundamental importance. As he told Congress, “I think that it has no real interest to us except as a way of understanding the origin of the solar system, and I hope very much that it will be important in that connection.”

**Inconstant Moon**

With so much institutional resistance to his ideas and his inability to exert his influence upon NASA in the planning of lunar exploration, it is no wonder that Urey never managed to revise *The Planets*, let alone to produce a popular work on the origin of the solar system for his prophet. The only hope Urey held out was that the moon might yet prove to be as important as he hypothesized. In the end, however, even the moon would not cooperate with Urey’s expectations.

The first lunar rocks brought back by Apollo 11 were distributed in 1969 to the Apollo research teams that performed various forms of age measurements and mineral analysis. In some ways, Urey and his fellow cold moon advocates were vindicated by the

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findings from the Apollo 11 rocks and those that followed – all of the measurements agreed that the rocks were billions of years old. Some rocks proved to be 4.5 billion years old, almost as old as the solar system itself, just as Urey had predicted. This prompted some lunar scientists to tell The Washington Post, “You know that Harold is the grandfather of us all … Uncle Harold is the real modern father of lunar science … Don’t let anyone tell you he isn’t as sharp as he ever was.”

Still, the moon turned out not to be anything that likely predated the formation of the planets. It lacked many of the materials that Urey predicted a primary object should contain. It was completely depleted of volatile elements. The hot moon advocates were also vindicated. Although the youngest rocks on the moon were roughly 3.8 billion years old, meaning that it had been geologically inactive for most of its history, it had experienced significant melting in its early history.

Urey was wrong about the uniqueness of the moon. It did not represent a fossilized remnant of the early solar system. While the moon could still be considered a Rosetta Stone of sorts, as it did record the history of a period of bombardment in the inner solar system that had been erased from the earth’s surface, it was not the geochemical Rosetta Stone that Urey hoped it would be. As Newell explained:

> … Harold Urey has said many times that he expected the Moon would be most interesting – that he hoped the Moon would turn out to be an interesting object – that he feared the Moon would not turn out to be interesting after all. Now that Ranger, Surveyor, Lunar Orbiter, Luna, Lunokhod, and Apollo have flown, providing samples of lunar material for laboratory analysis and vast quantities of photographic and instrumental data on the physical and chemical characteristics of the Moon, many of the Moon's secrets are being exposed to view and scrutiny. The Moon is very old, as old as the solar system, as Harold thought and hoped it would be. It

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is not, however, in its primitive state, having clearly undergone considerable evolution in the first 1500 million years after its formation… Nevertheless, many parts of the lunar surface are appreciably older than most of the Earth's surface, and the Moon may yet prove to be the Rosetta stone of planetary origins, as it was dubbed in the early 1960's. In any event, it is an important and illuminating link with the remote past, and its study will have much to reveal about how the Moon and planets formed and evolved. I hope that Harold Urey has decided that the Moon is after all a most interesting object, that has been worthy of all the competence and insight that he has brought to bear upon the study of it.  

But was Urey happy with this Moon?

Despite all of the conflicts and unhappiness that NASA’s lunar program had brought him, Urey had enjoyed the adventure of lunar exploration. However, the moon did disappoint him. If a letter Urey wrote in 1976 is any indication, he was indeed not happy with the moon that Apollo had revealed: “Yes, I think the moon has been quite a disappointment to me. I thought it would tell us something unique about the solar system. However, it seems to be an incidental object of some kind with no theory for its origin that is generally accepted.” Indeed, no generally accepted theory of the moon’s origins would emerge until just after Urey’s death. Not only was a great synthetic narrative no longer possible, but even the story of how science had unlocked the moon’s secrets could not be told.

But by then, Urey was disappointed by much of what had once energized him. He wrote in an annual birthday letter to Raymond T. Birge, “I am feeling my old age every day. My hands quiver, I wobble when I walk, my eyesight is bad and I can’t remember things so that I cannot keep up with the literature, hence I can do no scientific work at

869 Harold C. Urey to Douglas Allan, September 21, 1976, Box 2, Folder 33, HCU.
all.” Science had fulfilled many of its promises: “We have lived through very interesting
times in science. Think of the things that have happened in this century. Almost the entire
development of radioactivity has happened in this century. The whole development of
isotopes also, industrial things have developed immensely. Biology has done very
well.” But at the same time, Urey was plagued by the uncertainties that the great
developments of the century had unleashed. In his next birthday letter, Urey asked Birge,
“Do you think people will be here a million years from now, or even a century from now?
… I wonder if we have not lived through an exceedingly interesting time and just before
a very dreadful time when problems and disaster will plague men on the earth.”

Urey’s attempts at intervening in the politics of atomic weapons had failed. In one
of his final public speeches he told the graduating seniors of McGill University that
although he and his colleagues had had “fifty-three years of comparative professional
success and great prosperity,” he did not feel confident that they would leave the world
better than they had found it. There had been triumphs, but “one great cloud” still hung
over their heads. Already at the end of his own career, all he could do was wish the
younger generation luck at clearing that cloud away. While he had successfully
transformed his research program into one that was focused on earth and planetary
sciences, and had managed to become the “grandfather” of lunar exploration, he had
failed to find a way to use this research to provide the world with what he felt it most
needed.

871 Harold C. Urey to Raymond T. Birge, July 13, 1977, Box 14, Folder 1, HCU.
872 Harold C. Urey to Raymond T. Birge, March 10, 1978, Box 14, Folder 1, HCU.
873 Harold C. Urey, “War and Peace”, June 4, 1970, 14, Box 144, Folder 12, HCU.
Conclusion

This dissertation has used biography as a window into the social, cultural, and political forces that helped to define Urey’s final position on science and religion in the latter half of the 20th century. The first conclusion we can draw from this study is that such positions are not purely intellectual constructions, but are incredibly dependent upon context. In Urey’s case, he had several intellectual traditions upon which he might have drawn, and which did form parts of his personal understanding of science and religion. His Brethren upbringing, for example, might be credited for his view of the Bible and of the Judeo-Christian tradition as civilizing forces in the history of Western culture. Growing up amongst the Brethren might also be responsible for Urey’s understanding of Christianity as a communal practice aimed at self-sublimation, expressed in outward forms and enforced by powerful taboos. A very similar understanding of community, or Gemeinschaft, seems to have been at work within Urey’s vision of the ideal scientific community. For that matter, Urey also may have been influenced by the Brethren position on the two truths of the Bible and nature. But even this was context dependent, as Urey’s understanding of these positions and the roles they had played in his own development were related not to intellectual or spiritual satisfaction, but to community structure and lived social experience. It was Urey’s memory of the quality of social life, not the attractiveness of the positions themselves, that seems to have driven him back to these positions.

The extent to which the Brethren influenced Urey was limited. Urey’s ideas passed through several social and cultural filters at different times and places. Although
he remained personally connected to his former religion through his relationship with his mother, and after her death may have tried to compensate for her absence with a newfound enthusiasm for religion, he did not adopt every part of the Brethren position (or any other religious position). His interaction with the texts of the Freethinkers during his time away from home among his more “sophisticated” high school peers led him to doubt the Biblical literalism so important to the Brethren. Later, advised by a Cambridge scholar and as a member of an Anglophilic fraternity at the University of Montana, he traded his German-American identity for an English view of education. During World War I, as a novice chemist working for the American war effort, and after the war as a student at the University of California, Berkeley, Urey adopted the optimism and triumphalism of his new profession. In his postdoctoral year as a junior member of Niels Bohr’s Institute in Copenhagen, he added to this optimism a commitment to internationalism and an admiration for socialism. Again, this adoption seems to have had less to do with the positions themselves than it did with the invigorating experience of being a part of Bohr’s international coterie of physicists – not to mention his own personal movements around the modern Danish city. Bicycling around Denmark, Urey saw an alternative way of life to the poor agricultural world of his parents, the mining camps of his students’ families in Montana, and the industrial cityscape of Philadelphia in the 1910s. Urey inhabited worldviews, and each seems to have left its mark upon him.

It was an amalgamation of worldviews that was on display in Urey’s Depression-era speeches, in which he heralded the coming of a world that would be better both materially and socially. Although in this world Urey imagined that scientists behaved as though their science were a sacred religion, the superstition and supernatural elements
that for Urey characterized traditional religion had no place. This better world was short lived, however. While Urey experienced six happy years of scientific fame between winning the Nobel Prize and taking up war work once again for the Manhattan Project, the years of war and the ensuing Cold War would all but crush his optimism and force him to adopt more acceptable liberal positions. Only after Urey’s attempts at political intervention had failed did he turn to religion in the early-1950s. By this time, Cold War forces had also helped to reshape Urey’s research program into an investigation of the history of the earth and solar system.

But it was not just the world outside of science that had changed. The Cold War and the introduction of the contract research system and “Big Science” had also transformed the size and structure of science. New professional identities had been formed within science during Urey’s lifetime. He had managed to move with the major trends during his lifetime, had risen from farm boy to scientific star, had participated in many of the great scientific achievements of the 20th century, and had even managed to create new fields on the boundaries of disciplines. However, the scale of research after WWII did not suit Urey’s personality. This is obvious in his demoralizing experiences as the Director of Columbia’s SAM lab and as a participant in NASA’s lunar exploration program. He was eager to see America finish the Manhattan Project, but he could not stand managing his part of the project. He was excited by lunar exploration, but he could not exert the type of authority he felt he deserved. Urey’s “life geography” navigates a changing society and a changing scientific community. The world Urey inhabited after the war was not the world with which he was familiar. It no longer seemed to contain the
hope of a better tomorrow. Or, at least, the tomorrow that seemed possible in the interwar years no longer seemed likely to materialize.

Urey’s story is in some ways tragic. In his final years, although he still enjoyed a good scientific discussion and the company of close colleagues, he found little in the world about which to be optimistic. It is possible that Urey could not imagine a future for humanity because he could no longer understand the present. It also seems likely that his frustration over not being able to give society something to believe in stemmed from the constraints imposed by the bureaucratization and commoditization of science. Science and technology were now valuable resources for the national defense, tools of foreign policy, and forms of statecraft. Like his friends J. Robert Oppenheimer and Edward U. Condon, Urey had entered the Cold War with a particular prewar vision of the scientist as a special type of citizen with a special type of expertise – not a politician in his own right but a valuable adviser and mediator in times of crisis. They had seen their attempts to intervene in Cold War politics met with hostility and claims of disloyalty. The politicians were not interested in their advice, only their service.

After political intervention failed, Urey attempted to reinvent himself as a cosmic storyteller only to be met with the resistance of a great scientific and professional bureaucracy. This was a much different world than the one that had celebrated him in the 1930s. Between the wars, Urey had represented a burgeoning American greatness in science. He had fashioned himself in the personae of the American chemist, the internationalist, the generous benefactor, and the politically engaged visionary. In the Cold War, however, Urey’s sense of his place in science and of science’s place in the world were more ambiguous.
## Works Cited

### Archives

**ADA**  
Kappa Sigma Fraternity, Delta Omicron Chapter Records, Archives and Special Collections, Maureen and Mike Mansfield Library, The University of Montana, Missoula.

**CGD**  
Papers of the Geological Division, Caltech Archives, California Institute of Technology.

**CHF**  

**CSPR**  

**ECAS**  
Emergency Committee of Atomic Scientists Records, 1946-1952, Special Collections Research Center, University of Chicago Library.

**HCU**  

**HCU INS**  
Harold C. Urey Papers, Special Collections Research Center, University of Chicago Library.

**HCU NASA**  
Harold C. Urey Files, Archives, History Division, National Aeronautics and Space Administration, Headquarters, Washington, DC.

**IIM**  
Ian I. Mitroff interviews with lunar scientists, 1969-1972. American Institute of Physics, Niels Bohr Library and Archives, College Park, MD 20740, USA.

**LDB**  
Lee A. DuBridge Papers, 1932-1986, Caltech Archives, California Institute of Technology.

**LF**  
Record Group 1: General Files, 1902-1972, Special Collections Reading Room, Jewish Theological Seminary Library, Jewish Theological Seminary, New York, New York.

**LP**  
Ava Helen and Linus Pauling Papers, 1901-1994, Special Collections Library, Oregon State University.
MJE Morton J. Elrod Papers, 1885-1959, MSS 486, Archives and Special Collections, Maureen and Mike Mansfield Library, The University of Montana, Missoula.

RMH University of Chicago, Office of the President, Hutchins Administration Records, Special Collections Research Center, University of Chicago Library.

RNA Ruth Nanda Anshen Papers, 1938-1986, MS 35, Rare Book and Manuscript Library, Columbia University.

RPI Institute Archives and Special Collections, Rensselaer Libraries, Rensselaer Polytechnic Institute.

SE Samuel Epstein Papers, 10159-MS, Caltech Archives, California Institute of Technology.

SKA Samuel King Allison Papers, 1920-1965, Special Collections Research Center, University of Chicago Library.

SLM Stanley Miller Papers, 1952-2010, MSS 642, Mandeville Special Collections Library, Geisel Library, University of California, San Diego.

WHS Historical Society Archives, Walkerton, Indiana.

Oral History Interviews


Alfred O.C. Nier, interview by Michael Grayson and Thomas Krick, University of Minnesota, Minneapolis, Minnesota, April 7 and 8, 1989, Chemical Heritage Foundation, Oral History Transcript #0112.


Harold C. Urey, interview by John L. Heilbron, March 24, 1964, Niels Bohr Library and Archives, American Institute of Physics, College Park, MD USA.
Published Sources


Brickwedde, Ferdinand G. “Atomic Models as Proposed by Bohr”. MA, Baltimore: Johns Hopkins University, 1924.


———. *The Story of the Natural Sciences at Manchester College*. Manchester College, 2005.


Norton, Thomas H. “The Significance of the National Exposition of Chemical Industries.” *The Chemical Engineer* 25, no. 8 (October 1917): 282-316.


Reinoehl, Paul E. *A History of the Fairfield Cemetery: The Most Famous Man Buried Therein, Dr. Harold Clayton Urey, the Fairfield Township Schools, the Hamlet of Fairfield Center*. Ashley: Reinoehl, 1998.


———. “Chemical fractionation in the meteorites and the abundance of the elements.” *Geochimica et Cosmochimica Acta* 2, no. 5-6 (1952): 269-282.


———. “Discussion.” *Journal of Chemical Education* 6, no. 2 (February 1929): 258-259.


Urey, Harold C., and Michael Amrine. “I’m a Frightened Man.” Collier’s, January 5, 1946.


“War Profits of Chemical Corporations.” *The Chemical Engineer* 25, no. 8 (October 1917): 281.


