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ROBERT F. BACHER
1905—2004

A Biographical Memoir by
WARD WHALING

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Biographical Memoir

Courtesy of the Archives, California Institute of Technology.

Robert F. Bacher

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August 31, 1905—November 18, 2004

BY WARD WHALING

After Bacher retired from Caltech, he took part in 10 hour-long interviews (1981 and 1983) conducted by the Caltech Archives as part of its Oral History Project. He was invited to talk about the highlights of his career—events, people, places—and the result was more monologue than question-and-answer session. The transcript[1] of his recorded remarks, edited and amended by Bacher himself, amounts to an informal autobiography of 196 pages. I have drawn heavily from this source in preparing this memoir. Many passages, phrases, even single words appear below in quotation marks. Unless otherwise attributed it is Bacher speaking; they are his recorded words.

Another resource I used is the *Robert F. Bacher Papers, 1924-2004* in the Caltech Archives: 70 file boxes occupying 40 linear feet. Bacher spent several years of his retirement putting this vast collection in order before donating it to Caltech. It has been indexed by the Archives staff; the index to the collection is available online.[2]

ROBERT FOX BACHER WAS BORN in Loudonville, Ohio, on August 31, 1905. Three years later his family moved to Ann Arbor, Michigan, where his father was in banking services and his mother was a voice teacher on the University of Michigan music faculty. Young Bob attended the public schools in Ann Arbor but found little good to say about his early education: "It was boring, even though I managed to skip some grades."

He recalled that his decision to pursue a career in science originated with a book he read while a senior in high school: Frederick Aston's *Isotopes.* "His book was essentially a description of the work he had done over many years to measure the masses of isotopes of the elements. I found it fascinating. Within half an hour, looking at that book, I knew what I wanted to study. It was just that quick. I thought it was chemistry, because I'd found the book in the chemistry library at the University. There was no mention of atoms in the [high school] physics course I had."

By the time he entered the University of Michigan, just a few blocks from his home, he had learned from a neighbor that to follow Aston's lead he would need to major in physics instead of chemistry. That neighbor was Harrison Randall, head of the physics department at the university and the only role model mentioned in the oral history; Randall's important contribution to Bacher's career will come a few years later.

In spite of his plan to study physics, at the university he didn't take a physics course until his junior year, in part because he was immersed in nonacademic distractions. He had joined a fraternity (Kappa Sigma) and was living in the frat house instead of at home. By his sophomore year he had become the house manager, with a budget of some $20,000/year and six employees to supervise. "I've forgotten exactly how this happened, but they needed somebody to take on the job. I learned quite a lot out of that but it took quite a lot of time." He resigned at the end of his junior year and moved back home, and "spent all my time trying to get caught up and learn some physics during my senior year. My undergraduate education had a lot of holes in it and wasn't really very good."

GRADUATE SCHOOL, 1926-1930

On Randall's recommendation, "I applied to Harvard's graduate school in physics and was accepted and spent the academic year 1926-1927 there, mostly taking courses. The most interesting was a reading course with John Slater, a young assistant professor. It was my first introduction to quantum mechanics. This material was all new and not yet incorporated in the regular course. At that time *everything* in physics was just *changing like mad.*"

During the year Bacher was in Cambridge, his father suffered a heart attack. For financial and family reasons Bob was unable to continue at Harvard. He went back to Ann Arbor to help his mother, and he continued his graduate study at the University of Michigan with a teaching assistantship. In later years he judged this switch to Michigan as very fortunate. "That was the year (1927) that Uhlenbeck, Goudsmit, and David Denisson all came to Ann Arbor! Otto Laporte had joined the Michigan faculty in 1926, while I was away, and I actually started some work with Laporte that first summer I came back home. Michigan really took a quantum jump at that time, adding four young theoretical physicists that made it a fascinating place for me to be."

The University of Michigan had another new and unique asset. In 1927 Randall had started a summer school in theoretical physics as part of his plan to attract first-rank European scientists for his faculty. It was organized as a high-level international scientific congress for part of each day, and for the rest of the day as a summer vacation resort. This summer school attracted the most distinguished scientists in the world, and leading U.S. physicists came to meet the foreign celebrities—a veritable Who's Who in theoretical physics.

The Bacher family owned a summer cottage on Lake Cavanaugh, a few miles outside Ann Arbor, and it was at the lake that the Randalls and the Bachers were next-door neighbors. Harrison Randall directed the summer school, and he used his lakeside cottage as an entertainment center for the conference attendees. Graduate student Bacher, living just next door, found himself drafted to serve as chauffeur, waiter, lifeguard for the swimmers, etc. One way or another, Bacher appears to have met all the attendees. He recalled meeting Fermi for the first time when one night, long after dark, Fermi decided to swim across the lake—about three quarters of a mile—and Bob thought somebody should go with him. "We swam slowly and had plenty of time to talk and get acquainted, going over and back."

Over and over again in Bacher's oral history when a new name appears in the narrative, it will be accompanied by "I had known him since Ann Arbor Summer School days" or "I met him first in Ann Arbor in the summer of…." Bacher continued to visit Ann Arbor and the summer school long after he moved away. It is impossible to overemphasize the importance to his career of the contacts he made there.

Bacher's long association with Goudsmit began as soon as Goudsmit arrived to take a position on the Michigan faculty in 1927. "I signed up for Goudsmit's course on atomic structure. He began with the simplest elements in the periodic table and after about three weeks he said, 'Now somebody tell me an element and I'll show you how to figure out what the ground state configuration is.' I was sitting in the third row and I said, 'gadolinium.' I knew that gadolinium, in the middle of the rare earths, was absolutely the hardest element in the whole periodic table to figure out." Goudsmit gave Bob a hard look and started to figure it out, "and he had a terrible time. When the class was over he stopped me and said, 'How about coming around and talking to me in my office.' When I got there he wanted to know how I knew there was such an element as gadolinium, and how I knew it would be so tough. From then on, I worked with Goudsmit."

"Working for him was really wonderful for me. During the time I was a graduate student, I think I must have averaged two or three hours a day with him. He knew every physicist in the world, which was great for me." They worked on the Zeeman effect and hyperfine structure of atomic levels, analyzing spectra recorded by Ernst Back that Goudsmit had brought over. Their analysis of several levels in bismuth and thallium became Bob's Ph.D. dissertation: The Zeeman Effect of Hyperfine Structure (1930).

POSTDOCTORAL FELLOWSHIP, CALTECH, 1930-1931

After receiving his Ph.D. in May 1930, Bacher's next achievement was marriage to Jean Dow, an Ann Arbor girl he had known since grade-school days. The two Bachers then set out by car for Pasadena, where they would spend the first year of his National Research Council Fellowship, in 1931. He had chosen Caltech because "Ira Bowen was probably the best experimental spectroscopist in the country and he had done a lot of work on regularities in atomic spectra."

Although officially at Caltech, he spent most days in the library at the Pasadena headquarters of the Mt. Wilson Observatory, a better library for atomic spectroscopy than the one at Caltech. His library project had been conceived as he and Goudsmit were trying to fit theoretical models of the atom to the experimental spectroscopic information available from many laboratories over many years. As they searched one atom after another for regularities in spectral behavior, they began to assemble a systematic compilation of everything that was known about atomic states, listing for each term the term energy, J-value, parity, and electron configuration if known, with references to the source of the information.

Realizing how useful their compilation could be to anyone working in atomic physics, they decided to prepare a publishable version that would list *all* known levels in *all* known atoms and ions. They worked full time on this during the summer before Bacher left Ann Arbor and agreed to continue the collaboration by mail. Bacher would assemble the experimental values from the literature; then Goudsmit would look over the pages that Bacher was sending him weekly (in longhand) and apply his expert judgment when conflicting experimental values required adjudication. The project was completed while Bacher was in Pasadena, and they signed a contract with McGraw-Hill to publish *Atomic Energy States, as Derived from the Analysis of Optical Spectra.* This well-known volume of 553 pages—nearly all tables—appeared in 1932, and Amazon.com still offers "new and used" copies. This was the first such compendium of atomic data and amazingly it is still cited today.

While concentrating on this monumental literature search, Bacher somehow found time to become acquainted with prominent scientists who visited Caltech or the observatory. With one such visitor William F. Meggers, the senior spectroscopist at the National Bureau of Standards, Bacher published a short paper (1931) on the nuclear spin of two isotopes of rhenium.

AT MIT, 1931-1932

For the second year of his NRC fellowship Bacher moved to MIT to work with Slater whom he had admired greatly since taking a course with him at Harvard five years earlier. Despite the long hours spent proofreading five hundred pages of tables from the printer, he enjoyed this second stay in Cambridge very much. He picked up ideas from Slater's quantum mechanical treatment of atomic structure that he would soon put to use in the next phase of his research.

The high point of his year at MIT occurred when Slater asked him to report to the weekly journal club on the 1932 paper by Chadwick reporting the discovery of the neutron. In 1931 the nucleus was believed to contain only protons, electrons, and alpha particles, and the MIT physicists were skeptical of Chadwick's claim. Slater told Bacher to "look into it and give us a report on this nonsense."

Bacher studied the paper and saw that Chadwick's neutron could clear up troublesome anomalies Bob had encountered in his study of nuclear spins and magnetic moments. For example, in Chadwick's view the Li^6 nucleus was composed of $\alpha + p + n$ instead of $\alpha + 2p + e$ in the conventional model. This would be consistent with the observed nuclear spin of 1 for Li^6 if the neutron had spin 1/2, just as the proton did. Furthermore, the new model would avoid the embarrassing fact that the magnetic moment of Li^6 is a thousand times smaller than the magnetic moment of the electron.

"When I had studied this paper a little I went to Slater and told him, 'This paper is one of the most revolutionary things that's come in physics for a long time. It's really correct.'" Slater's response was, "I'll wait and hear about it when you give your seminar."

"So I went in to face this seminar with all these well-known people in physics. I think almost the entire audience was skeptical, and here I was, just a young postdoctoral fellow, trying to espouse this work about neutrons. This really stirred me up a great deal, and at the end of a two-hour vigorous talk in the seminar, I think I convinced more than half of them that Chadwick was right. They had come in thinking it was absolute nonsense, and I think they all went out to read his paper afterwards." Bacher certainly convinced E. U. Condon who was visiting MIT. Four days after this seminar, Bacher and Condon submitted a letter to the *Physical Review* (1932) arguing that the spin of the neutron must be 1/2.

ANN ARBOR, 1932-1934

At the end of his NRC fellowship in 1932 Bacher returned to Ann Arbor for the summer. The United States was in the midst of the Great Depression and academic jobs were scarce, but the University of Michigan awarded him a postdoctoral fellowship for 1932-1933. When that appointment ran out, Bacher was unemployed and living at his family home in Ann Arbor, but he always maintained that those years were "two of the best years I ever had—I could spend all of my time working on things."

What he was working on was a "simple method for calculating the approximate energies of atomic levels" as a sum of *observed* energies of states of the ions of that atom. This method grew out of his MIT work with Slater. It also reflects his extensive work with Goudsmit on regularities between similar atomic systems and isoelectronic sequences. The long paper (1934) he produced at the end of those two "very good years" includes formulas for calculating the energy of an atomic level with a configuration of up to seven s- or p-electrons outside a closed shell. (The promise to include d-electrons in a subsequent part II paper was never fulfilled.) To demonstrate that his method was more accurate, easier to use, and applicable to more complex atoms than the full quantum mechanical treatment with Hartree wave functions, he compares experimental level energies in oxygen, nitrogen, and carbon with values calculated by the two methods.

He submitted the paper to the *Physical Review* just as he was leaving to take a new job at Columbia University, and one can imagine his dismay when the paper was rejected. Bacher convinced Goudsmit that as primary author, he should settle this matter without Goudsmit's intervention, and then mailed off a strongly worded complaint to the editor of the journal. "I got a letter back, almost by return mail, accepting the paper" (1934). In later years Bacher was pleased to see that the method set forth in this paper has found wide application, particularly in molecular chemistry; and it is still being cited in the literature. "This was probably one of the better contributions I've made to physics." And it marked the high point of his career as a theorist.

COLUMBIA UNIVERSITY, 1934-1935

Bacher obtained a position as instructor in the Physics Department at Columbia University for 1934-1935. He was attracted to Columbia by I. I. Rabi's atomic-beam studies of nuclear moments, the field that Bacher studied by atomic spectroscopy. But Columbia's immediate need was for help with teaching the introductory physics course, and that was the way he spent his first and only year at Columbia. He did manage to become thoroughly familiar with the research in Rabi's lab and with the brilliant group of young researchers working there, notably Jerrold Zacharias, Jerome Kellogg, Sid Millman, and, of course, with Rabi. He would in the future work closely with these men. Bacher enjoyed the stimulating scientific milieu at Columbia, but he found it difficult to live in New York City on an instructor's salary of $2,400.

In the spring of 1935 Hans Bethe, who had only recently arrived in the United States to become an assistant professor at Cornell, visited Columbia to talk about the research he hoped to get started at Cornell. Bacher met him for the first time and was tremendously impressed. In long conversations the two men found that they had many common interests, and that some of the research Bethe was planning might provide an opportunity for Bacher to move into nuclear physics, a change he had been contemplating. These discussions led to an offer of an instructorship at Cornell.

His friends at Columbia thought it was "a little wild to go up to Cornell, which was at that time not nearly as well known as Columbia." But Cornell had Bethe, and that settled it. Furthermore, Bob and Jean were expecting their first child (Martha, born December 17, 1935), and Ithaca, a university town like the one they had grown up in, was a more appealing place to raise a family, and less expensive, than New York City.

CORNELL, 1935-1943, MIT RADIATION LAB

When Bacher assumed his duties as instructor at Cornell in the fall of 1935 he right away set about assembling a laboratory to continue his study of nuclear moments by high-resolution atomic spectroscopy. He had an agreement with his department chair R. C. Gibbs that when this new lab was well established, Bacher would be free to shift his research activity to nuclear physics. In spite of heavy teaching duties (e.g., an introductory physics course with enrollment of about 500, so large that each lecture had to be given twice) Bacher and new postdoctoral fellow D. H. Tomboulian quickly set up the spectroscopy lab and were publishing experimental papers (1937) on hyperfine structure within two years of Bacher's arrival. Now he was looking around for opportunities to start up research in nuclear physics. He kept busy by contributing the section on nuclear moments and some material on neutrons to the first (1938) of Bethe's three famous papers on nuclear physics.

The major nuclear facility at Cornell was a 16 inch cyclotron built by Stanley Livingston after leaving Berkeley for Cornell in 1934. It could accelerate deuterons to 1.5 MeV, and Bacher thought it would make a good neutron source, but it belonged to Livingston whose principal interest was in accelerators, not in neutron physics. Then in 1938 Livingston left Cornell and Bacher inherited the Cornell cyclotron, along with Livingston's postdoc Marshall Holloway, whose primary assignment was to keep the cyclotron running.

Starting with essentially no experience in neutron physics, Bacher with postdoc Holloway and graduate students Charles Baker and Boyce McDaniel designed and built instrumentation to pulse the cyclotron ion source so as to accelerate deuterons in bunches, thereby producing repeated bursts of neutrons when a bunch hits the cyclotron target. A paraffin moderator surrounding the target spread the energy (and velocity) of the neutrons in each burst over a broad range from < 1 eV to a few MeV. By placing his neutron detectors at a distance L from the cyclotron, and by turning the detectors ON only for a short time following the initial pulse by the time interval Δt, his neutron counters became sensitive only to neutrons of velocity $v = L/\Delta t$. By placing a sheet of material in the flight path Bacher could measure neutron absorption and scattering cross-sections of the material as a function of neutron energy. "It was the first time this had been done, and the first time that neutron resonances were fully elucidated." This neutron velocity spectrometer required short (μ-second) time measurements with homemade circuitry using vacuum tubes available before World War II. Bacher always credited his graduate student Charles Baker for the success of the fast electronics that made this research possible (1941).

Just as Bacher's neutron research at Cornell was reaching full stride, preparations for World War II intervened. In December 1940 Lee DuBridge was recruiting staff for a new lab at MIT to develop radar. DuBridge had met Bacher at the Ann Arbor summer school, and now invited him to visit Cambridge to see if he would be interested in joining the radar work. For some time Bacher had felt that the United States might very well be drawn into the war raging in Europe, and the prospect of getting a head start on what could be a very important defensive weapon was appealing. But he had a family to consider; his second child, Andrew, was born in 1938. There were also his students to consider, and his responsibilities to Cornell.

Furthermore, it appeared that his current work with the Cornell cyclotron—on the neutron absorption cross-section in cadmium—was in serious disagreement with the accepted value of that cross-section in the literature, the value that Fermi was using as he tried to produce a sustained nuclear chain reaction in a "pile" of uranium plus graphite. It was important that the new Cornell measurement be checked, and if true, should be made known to Fermi without delay. He explained all this to DuBridge and promised to join the MIT Radiation Lab as soon as he could tie up all the loose ends.

Bacher worked out an arrangement with Cornell so that his research students could keep the cyclotron lab running to check their cadmium result. Every three weeks Bacher would go back to Ithaca for four days—Thursday through Sunday. (He was back in Ithaca taking data in the cyclotron lab on the Sunday afternoon that Pearl Harbor was attacked.)

After carefully checking the effective neutron absorption cross-section in cadmium, Bacher delivered a paper (1946) describing the Cornell measurement to Fermi. Convinced that the Cornell work was correct, Fermi urged him to publish the paper, and Bacher submitted the manuscript to *Physical Review.* On further thought, he decided that since the new value was useful to Fermi, it would be useful to anyone else trying to build a nuclear reactor and should not be published just yet. He asked the *Physical Review* to withhold the paper until after the war. It was eventually published in 1946, with the notation "Received February 13, 1942." Later on, Bacher would see dog-eared copies of this paper at Los Alamos marked TOP SECRET. Fermi's high opinion of this work launched Bacher's reputation as an expert at neutron experimentation.

The Cornell laboratory was shut down in 1942 for the duration of the war. The neutron velocity spectrometer equipment was taken to Los Alamos "where it was used to make a number of measurements of considerable importance to us."

At the Radiation Lab Bacher was put in charge of the division concerned with receiving and interpreting the incoming reflected signals. "We quickly concluded that the ultimate discrimination between signals reflected from a target, as opposed to noise from the transmitter, should be done finally on the cathode-ray tube. We had to develop the tubes, and then contract with GE and RCA to work together on producing them. I supervised the contracts myself, visiting GE one week and RCA the next, and the following week we would hold a joint meeting at the Rad Lab in Cambridge. I was getting into contract management."

LOS ALAMOS, 1943-1945

Bacher's first official contact with the Manhattan Project came in the spring of 1942 when Oppenheimer asked Bacher and Rabi (associate director of the Radiation Lab) for advice on setting up a new lab to work on a nuclear weapon, and whether some of the senior nuclear physicists working at the Radiation Lab might be released to join the new lab. They met several times "surreptitiously," with General Groves present.

"We were very disturbed to learn that Groves had ordered, and Oppenheimer had agreed, that the new lab would operate as a military project, with all the people on it members of the military service. Well, both Rabi and I took an extremely dim view of this. We told Oppenheimer this wouldn't work. We discovered to our terrific amazement that not only had he agreed to accept a commission as lieutenant colonel but had ordered his uniforms. We just made it very clear to him that if this was what he was going to do, we weren't going to have anything to do with the Manhattan Project, and we were pretty sure that nobody connected with our MIT laboratory would either…Count us out."

This strong negative reaction from two prominent Radiation Lab scientists "caused an uproar." Oppenheimer appealed to his boss, J. B. Conant, for help. He wrote[3] Conant that "the solidarity of physicists is such that if these conditions [of a civilian laboratory] are not met, we shall not only fail to have the men from MIT with us, but that many men who have already planned to join the new lab will reconsider their commitment."

Conant had initially supported the Groves plan, but he eventually agreed to the following compromise. In a letter[4] dated February 25, 1943, to Oppenheimer from Conant, and signed also by Groves, it was agreed that the lab would be operated in civilian fashion "until such time as the lab had considerable amounts of fissionable materials for the bomb. At that time the project would become a military project, with all engineers and scientists commissioned and in uniform, if they wished to remain." The transition, of course, never took place, but the Groves-Conant letter removed a critical obstacle in recruiting the scientists that the project would need, and Bacher felt that his part in this "uproar" and its outcome had made a significant contribution to the success of the Manhattan Project.

Oppenheimer was eager to recruit Bacher to join the Los Alamos lab and invited him to attend a two-week conference at Los Alamos in April 1943, the official—although secret—opening of the Los Alamos laboratory. Bacher took an active part in the conference discussions, and Oppenheimer privately invited him to join the lab. Bacher declined, and in a written memo explained his reasons: it was not clear what his assignment would be nor even why he was needed; the lab as described at the meeting was poorly organized; and it needed more engineers, not more scientists. He would prefer to remain at the Radiation Lab where it seemed sure that the mission would be successful in time to affect the outcome of the war, whereas a nuclear bomb appeared to be a long-term project, if indeed possible.

Oppenheimer's written reply[5] two days later, while Bacher was still at the meeting, reveals Bacher's importance to the project. Here are excerpts:

> I took up some of the points raised in your letter with [R. R] Wilson, [John H.] Williams, [Felix] Bloch, [Emilio] Segre, Rabi, [R. C.] Tolman, and Fermi, and I think that as a result of this I am in a position to make a few clear statements that may help to get your relation to the project better defined.
>
> (1). You know that I have been extremely eager to have your help in this work. I think perhaps you have not fully realized how much I appreciate your administrative experience and obvious administrative wisdom, nor how aware I am of our need for just this in the present project. Perhaps too you do not evaluate highly enough the fact that you have worked so much in neutron physics, and that you are so well informed about the last year's developments at MIT. These three qualifications make you, in my opinion, very nearly unique. In addition, I want to express in writing my own confidence in your stability and judgement, qualities on which this stormy enterprise puts a very high premium.
>
> (2). I would like to offer you the direction of the experimental physical work at Los Alamos. I know that you will so organize the work that the leaders of projects who are now here will have a real sense of responsibility and a maximum freedom compatible with effective coordination of the work. You would be responsible, as director of experimental physical research, to the governing board of the laboratory, and to the director.
>
> (3). The governing board of the laboratory does not yet exist, but I should like to start it by appointing you as a member.
>
> (4). You have my unqualified support in trying to develop an adequate physics-engineering group. . . I should like to have your help in bringing here a

group of men whom you would call physicist-engineers, and would want to give you a good deal of freedom in selecting these men.

(5). I believe it is essential, if you wish to undertake work in this laboratory, that you accept without further delay. I know you cannot leave MIT before the middle of June at the earliest, but I hope that your duties at MIT would leave you some time for this [recruiting engineers for Los Alamos], and that you and Rabi together will get to work on it at your earliest convenience.

Oppenheimer's strong letter was effective. Bacher agreed to join the Los Alamos Lab, but his letter of acceptance included this final sentence. "This letter is also my letter of resignation on the day the project becomes a military project, as projected in the Groves-Conant letter."

Bacher did not resign his position from the Radiation Lab. "They put me on leave for the duration, with the understanding that if Los Alamos became a military laboratory, I could return to the Radiation Lab." The Bacher family moved to Los Alamos in June 1943.

The Physics Division that he headed was charged with measuring cross-sections for the many neutron-induced and neutron-producing nuclear reactions that the weapon designers needed. This was just the sort of research he had been doing at Cornell; he fit the job perfectly. His oral history includes almost nothing about specific problems he worked on. All were initially classified and some were still classified at the time of his oral history. When asked about Bacher's work at Los Alamos, Bethe provided the following insights in a 1993 interview[6] with Judith Goodstein at the Caltech Archives. "Next to Oppenheimer, Bacher was the most important person at the lab…There were many famous experimental physicists in his Division, several were prima donnas. They were willing to work with Bob but probably not with anyone else…He held the Physics Division together…

Bacher had the full confidence of Oppie and, remarkably, of General Groves."

Beyond his technical and administrative duties Bacher was involved in emotional support of the director. "During that first summer (1943) Oppenheimer often expressed privately real doubts about his suitability for the Director's job, and he worried about how he was doing. I thought he was doing a fine job; in fact, I thought he was the only person out there who could conceivably be the director of the place. At any rate, during that summer I developed a very close relationship with Oppenheimer; I spent about two hours a day with him discussing things. Sometimes after work at night we'd talk for an hour or more." More on Bacher's admiration for Oppenheimer will be found in Bacher (1972).

When the laboratory was reorganized in July 1944 to focus on the use of plutonium, the Physics Division was split up, and Bacher became head of the G Division (G for gadget; the lab discouraged the use of the word "bomb"). The G Division was charged with engineering the means for assembling a critical mass of plutonium much more rapidly than the gun method used for the uranium bomb. The design eventually adopted included a large number of explosive charges distributed symmetrically in a spherical array that would produce, when all were fired simultaneously, a spherical shock wave moving inward toward the center of the array. A subcritical solid sphere of plutonium at the center would be compressed to criticality by this shock wave. G Division was charged with measuring the speed and symmetry achieved in test firings of various ways of shaping, placing, and firing the explosive charges.

Measuring the all-important symmetry of the implosion was very difficult, and as the summer of 1944 wore on, even Bacher needed encouragement. "I must say that in the fall I wasn't at all sure that this would work. I was just about ready to give up on whether we could get such a thing going, whether we could make a symmetric enough implosion to make the thing work as a bomb. We had to do experiments that were good enough to tell this, but we had very few explosive charges to work with, [and] we had trouble being able to measure things fast enough."

"Finally, [in the spring of 1945] we finally obtained confirming evidence. Three or possibly four different methods of quite different nature indicated that our implosion should work; the conclusions were pretty solid. But a wholly new development in the innermost core of the bomb was required to do this. This integral part of the implosion bomb [apparently still classified in 1983] hadn't even been imagined as being necessary when the G Division was set up. I won't give any names, but two or three of us independently thought of some of the different ideas of how to do this. And it worked."

Bacher was, of course, present when his confidence was put to the final test in the Alamogordo desert on July 12, 1945. He supervised the assembly of the bomb core at the site, "and after the core assembly, I drove it over to the test site where Holloway managed the assembly with the rest of the bomb."

"Well, the bomb went off and was even more impressive than most everybody thought it was going to be. Then came the job of getting successive bombs ready. Our instructions were that as fast as material could be delivered to us, bombs were to be fabricated and sent to the Pacific. This work of checking out the cores was done in a room across from my office, because I was directly responsible for it and I darn well wanted to see that I went over some of these things myself. We had just finished the check-out of another bomb core, and there was a car waiting out front to drive it down to Albuquerque airport where a plane was ready to fly it over to Tinian, when Oppenheimer came running down the hall and said he had a hold order from Washington. Well, we knew that meant this was the end."

But not quite. Many Manhattan Project scientists had already started thinking about how nuclear energy could be brought under international control. General Groves was interested, too, "but in a different way; he wanted to know whether control was *technically* feasible" The War Department ordered Groves to assemble a committee of experts to determine what kind and depth and frequency of international inspection would be needed to make certain that any future effort anywhere to construct a nuclear weapon would be detected. At Groves's request, Bacher remained in Los Alamos another five months to work on this problem as a member of a feasibility committee, chaired by Manson Benedict.

As hard as it must have been for the Bacher family to stay behind while everyone else went home, those extra months gave Bacher time to organize and polish his thoughts on the most effective and certain means for suppressing the proliferation of nuclear weapons. He became the arms control expert that the State Department would call on for advice again and again. Just as his extended stay in Los Alamos was coming to an end, on January 12, 1946, he was awarded the Presidential Medal for Merit, the highest civilian award for service to the nation in time of war.

CORNELL, U.N. ATOMIC ENERGY COMMISSION, 1946-1947

In January 1946 Bacher finally left Los Alamos and headed back to Ithaca to direct the Laboratory for Nuclear Studies at Cornell, rejoining Hans Bethe, Lyman Parratt, Trevor Cuykendall, Dick Feynman, Phil Morrison, Boyce McDaniel, Dale Corson, and Robert Wilson, all from Los Alamos. Bethe and Bacher had agreed that Cornell would now move into high-energy nuclear physics. Their goal was a 300 MeV electron synchrotron, but first they needed a building to house it, and Bacher went to work with the architects.

In the midst of all this planning Bacher found himself drawn into the national concern for control of nuclear weapons. At its first meeting, in 1946, the United Nations General Assembly created the United Nations Atomic Energy Commission (UNAEC), charged to develop a plan for nuclear disarmament and nonproliferation, and to foster peaceful applications of nuclear energy. Shortly after the UNAEC's first meeting in June 1946, Bernard Baruch (United States) and Andrei Gromyko (Soviet Union) presented diametrically conflicting proposals for nuclear disarmament, inspection, and control, and further negotiations ground to a halt. High-level discussions between senior U.S. and Soviet representatives went from bad to worse and were getting nowhere when someone proposed the appointment of a Scientific and Technical Subcommittee (STS) that would see whether scientists from both sides of the conflict could agree on details of just what inspections would be needed to ensure effective control of nuclear weapons. This proposal was adopted, and H. A. Kramers (Netherlands) was appointed STS chair. Each member nation on the Security Council could appoint representatives to the STS; the U.S. representatives were Richard Tolman (Baruch's full-time science adviser), Bacher, and Oppenheimer.[7] The UNAEC postponed further high-level talks while waiting for the report from the scientists on the subcommittee. "I realized that this was a much more important time than any I'd seen before, because getting international control of atomic energy and weapons was crucial."

"I knew Kramers [from Michigan summer school days] and Tolman wanted me to take a major part in the non-formal negotiations. Tolman was just swamped with work. We spent every weekday together for a period of two months, working as hard as we could. We had to go through all aspects of the problem of control." As Bethe put it, "At this time Bacher was spending four days a week at Cornell, and four days a week in New York."[8] Bacher's recent experience in dealing with this very same question as a member of the feasibility committee convened by Groves, and the fact that the United States was the only country with firsthand experience in this area, meant that the U.S. delegation dominated the STS meetings, "while the Russian delegate asked questions and listened carefully."

The STS met for first time in July 1946 and promised their report by September 1. "The meetings went on at great length. When we first started we didn't appreciate that, from the Soviet standpoint, this was a matter of digging for extra information, and that they had no intention of making any formal agreement. At the end, when it came time to decide this question [of the feasibility of international control], the Soviets asked for a postponement to the next day. And then for another postponement, and another. I told Kramers that our delegation would insist on a meeting whether Dmitrii Skobeltsyn [the Soviet delegate] came or not. The other members of the STS supported this demand, and Skobeltsyn *did* attend the next meeting and signed the final report of the Scientific and Technical Committee. We were surprised to receive a message from Skobeltsyn a few hours later saying goodbye, and that he had been called back to the Soviet Union."

"So [the STS report] was approved unanimously. As far as I know, this was the *only* substantive thing that was actually agreed on in the [UNAEC] negotiations." Bacher took considerable pride in the success of the scientists on the STS in reaching agreement in the face of Soviet obstruction.

U.S. ATOMIC ENERGY COMMISSION, 1947-1949

A few weeks later, in October1946, David Lilienthal called Bacher and asked him to come to Washington to consider joining the U.S. Atomic Energy Commission (AEC) that was just being organized. "Well, I told him immediately on the phone that I really didn't want to do this, but he was insistent that I at least come talk to him about it, right away. He would send a government plane to pick me up."

In Washington Bacher learned that Lilienthal had agreed to chair the AEC, and three businessmen had agreed to serve on the five-member commission. "I would be the only scientist. Furthermore, I was told directly that if I didn't take the position, there would be no scientist on the AEC. This was a pretty rough way to twist my arm, because they knew I felt strongly that science should be represented on the AEC."

"Well, this whole proposal hit me pretty hard. This was a very hard thing for me to do, because I'd just come back to Cornell. We were building a new lab and had barely gotten started. I hated to leave, I really did. It was very sad for our whole family to leave Cornell and close friends there, but we went to Washington."

The commissioners' two-year terms commenced on November 1, 1946. First came a tour to get acquainted with the widely scattered facilities they were responsible for. During their tour, they also worked on choosing members for the General Advisory Committee (GAC) as required by the 1946 Atomic Energy Act. The GAC was composed of nine scientific and engineering heavy-hitters that met quarterly and provided the real intellectual muscle of the AEC. Bacher, as the only commissioner with wide scientific acquaintance, played a major role in putting together the list submitted to President Truman for appointment to the GAC: Oppenheimer, Rabi, Fermi, Glenn Seaborg, Lee DuBridge, Cyril Smith, and James Conant.

The commissioners learned on their tour that the operation they would soon inherit was in bad shape: hastily built production facilities were wearing out, and the personnel—especially the senior scientists—had largely returned to their peacetime jobs as soon as the war was over. "During its first year, the AEC had the problem of building up personnel in the various laboratories, and getting proper management for the laboratories where some of the contractors that had managed them during the war wanted to be relieved."

As soon as they returned to Washington, Bacher and Commissioner Sumner Pike (former chair of the Securities and Exchange Commission) left again for a more thorough inspection at Los Alamos and Hanford, the two facilities that appeared to be in the deepest trouble. The first three bombs had been, quite literally, handmade, but those hands had since gone back to their peacetime employment. The Los Alamos staff was so depleted that it had been unable to assemble a complete bomb for several months, "and we learned that only when we went there in November on our tour. This was a real emergency, because the U.S. was going around acting internationally as if we had many atomic bombs, whereas in reality we'd almost lost the art of making one of its critical parts." What Bacher had learned was that the United States had produced nine bombs in 1946, but only four in 1947.[9]

At Hanford they found that the reactors were showing signs of old age and could be run only at reduced power, with consequent lower production of plutonium. This problem would be left for the GAC to deal with as they ordered permanent replacements for production facilities thrown together in a rush during the war. Restoring and eventually raising the production of nuclear weapons would be the highest priority of the AEC during its first few years. It would have been hard to find someone better qualified than Bacher to handle this assignment. Lilienthal was wonderfully lucky in finding his fifth commissioner.

The AEC formally accepted title to the far-flung operations of the Manhattan Engineering District on January 1, 1947. Three weeks later their confirmation hearings began before the senatorial members of the Joint Committee on Atomic Energy. Four commissioners (all Republicans) were approved quickly—Bacher by a vote of 8 to 0—but the questioning of chair-to-be Lilienthal (a well-known New Dealer) dragged on into April while Senator McKellar (Tennessee) attacked Lilienthal relentlessly, charging among other things that Lilienthal had led a local communist cell at the TVA.

The commissioners tried to attend all the hearings but still managed to get some work done during Senate recesses. During one such break, Bacher was able to conduct an inventory of the fissionable material the commission had inherited from the Manhattan Engineering District, in the form of completed weapons, supplies of raw materials, and nuclear cores and other radioactive parts not yet assembled. "This may have been the first real physical inventory, actually checking selected fabricated parts to see if they were plutonium or enriched uranium. I did this myself with Norris Bradbury who had succeeded Oppenheimer as Director of the Los Alamos laboratory. We spent two days going through the great safe where radioactive material was stored. I don't think there'd ever been a physical check of the fissionable material before this."

"We briefed the President [on April 3, 1947] on the weapons we had in the stockpile. But we didn't even write the number of bombs in the stockpile on the copy we gave the President. I read the report to the President myself, and when we came to the point where it said how many of such-and-such a thing we had, I put the numbers in from memory—they weren't written down anywhere." Also in

April 1947 Bacher was elected to the National Academy of Sciences.

"The AEC had enormous prestige in Washington right from the start. The AEC budget was several hundred million dollars per year—a lot of money in 1947—one of the biggest single items in the federal budget." Bacher insisted that the 1948 budget include funds for research as contemplated in the 1946 Act. "In the first budget we put in several million dollars for basic research. The AEC wasn't yet prepared to distribute these funds, so we turned the funds over to the Office of Naval Research for them to distribute through their contracts supporting basic research. I felt it was important to set a policy from the very beginning that the AEC would have a budget for scientific and technical work."

By the spring of 1948 weapons production at Los Alamos had recovered to such a degree that the testing of new designs was justified. Under the 1946 Act only the AEC was authorized to dispose of fissionable material. "Legal counsel advised that this authority could not be delegated, but a single member of the Commission could represent the AEC, and I can still remember that meeting where they all looked around at me." So Bacher spent three weeks on a ship at Eniwetok in charge of the whole show. "The tests [known as Sandstone, April-May 1948] went very well, and we got quite a leg up in those three tests."

As the second year of his term on the AEC drew to a close Bacher could see good progress in managing the emergencies in declining weapons production that he had encountered on his first days with the commission. He now had time to think about the nonexplosive uses of nuclear energy. After soliciting ideas from all the labs and throughout the AEC, he proposed that the first peaceful applications should be: (1) a high-flux materials-testing reactor to see how materials stand up under prolonged exposure to neutron bombardment; (2) a land-based prototype of a reactor suitable for ship and submarine propulsion; and (3) a breeder reactor that would produce heat for power generation while creating plutonium, with the goal of producing more fuel than the reactor consumes.

The submarine propulsion program was understandably popular with the Navy but led to trouble with the Air Force, which wanted a corresponding program to develop nuclear-energy-powered aircraft (NEPA). Essentially all the scientific advisers gave this NEPA request very low priority, but somehow Bacher became the particular target of Air Force wrath. "I've never really understood why they thought I had been the one who killed it."

Bacher's initial two-year appointment to the AEC expired on January 1, 1949, "but in 1948 the President asked me to accept a renewal and I did, with the understanding that I might resign before my new term was up. I began to realize how tired I was, and I began to talk to David Lilienthal about leaving." After consulting the GAC, Bacher recommended that Lilienthal invite Henry Smyth to take Bacher's place on the AEC.

When Bacher submitted his resignation, "the President saw me alone and first tried to get me to stay on. It was hard to say no, because he had backed us every time and was dedicated to civilian control. But he understood my situation and did not pressure me too hard. He was extremely cordial in saying goodbye and thanks, and it was arranged that I would leave early in May [1949]."

Bacher had resigned his position at Cornell before accepting the AEC appointment, and he decided not to go back there. Robert Wilson (who had been a group leader in Bacher's division at Los Alamos) had become director of the synchrotron lab at Cornell "and was doing a first-rate job. It would be awkward to go back there, as much as I loved

Cornell and admired Wilson." Meanwhile, Lee DuBridge renewed his invitation that Bacher come to Caltech.

Three years earlier, when Du Bridge was leaving the Radiation Lab at the end of the war to become president of Caltech in 1946, he persuaded Bacher to visit Pasadena and offered him the chair of Caltech's Division of Physics, Mathematics, and Astronomy. At that time Bacher was fully committed to his plans for Cornell, but now with the changes at Cornell the Caltech invitation was more appealing. After consulting with Charlie Lauritsen and with Oppenheimer, who had been on the Caltech faculty briefly right after he left Los Alamos, Bacher accepted the Caltech offer, to start in the fall of 1949. "I accepted with the proviso that we would start new experimental research in high-energy physics and would strengthen theoretical research in this field."

With that critical decision settled the Bacher family set out for a much needed and well-deserved vacation in northern Michigan, amid quiet surroundings where Bacher, now 45, expected to plan the next stage of his career: inaugurating a program at Caltech in high-energy particle physics, both experimental and theoretical. He had arranged for Caltech to hire two new Ph.D.s to start roughing out the design for a 600 MeV electron synchrotron, and Feynman had accepted Bacher's invitation to give some lectures at Caltech. It looked as if Bacher's return to private life was getting off to an auspicious start. There is no way he could have anticipated the distractions with which the AEC would wreck his vacation.

Two weeks after Bacher left the AEC, Senator Bourke Hickenlooper (Iowa) charged Lilienthal with "incredible mismanagement," culminating weeks of bad press for the AEC: the loss of 4 grams of uranium at the Argonne Lab; "shocking cost over-runs" at Hanford; and the award of an AEC Graduate Fellowship to a student who admitted he was a communist. Lilienthal responded by inviting an investigation of "the AEC's stewardship of weapons production, research, and security. Our record in these respects is a proud one." Hearings before the Joint Committee on Atomic Energy began on May 26, 1949. The first Bacher heard of this was "when they called me in northern Michigan and said they would send a plane up to the nearest town so that I could come down and testify."

"I thought that what Hickenlooper was doing was outrageous; there was no basis for his charges. . . This attack came only a few days after Smyth joined the AEC as my replacement, and it certainly wasn't fair to put Smyth into this. I thought the AEC had a very good record, and I felt impelled to go back and testify, much as I hated to go back into that atmosphere." Lilienthal was finally exonerated, but the hearings lasted through the summer.

That wasn't the only distraction in the summer of 1949. The Bacher family arrived in Pasadena in August and were just getting settled "when sometime in the first week in September I got a call before breakfast saying that the Air Force had detected radioactive particles in air filters flown over the northern Pacific Ocean, and would I come to Washington to help evaluate what we've found and report to the President on what the situation is."

Arriving in Washington, Bacher joined Oppenheimer, Compton, and Admiral William Parsons on a panel headed by Vannevar Bush to see what could be learned from the atmospheric samples collected by the Air Force. "It was clear to us that they had detected debris from an atomic bomb explosion. It was absolutely, completely clear and we wrote a report to that effect. The interesting part was that Mr. Truman doubted it. He was apparently so shaken by the fact that the Russians had the bomb that he wanted all of us, individually, to sign the report that Van Bush had written. [Truman] found it very difficult to believe, from all that he had been told, that it would be possible for the Soviets to do this so fast."

John Cockcroft was present for the Bush panel discussions because British observers had also collected atmospheric samples as the radioactive cloud passed over Britain. An AEC security officer took Bacher aside and asked if he would be willing to speak to his good friend Cockcoft privately and find out if he had any misgivings about Klaus Fuchs, who had returned to England from Los Alamos after the war. Bacher, who had at one point worked "rather closely" with Fuchs, replied that he didn't want to do this "unless you have some really solid evidence against Fuchs," and was told, "It's pretty solid." Bacher consented and asked Cockcroft "if they'd had any suspicions that Klaus Fuchs might really be working for somebody else." Cockcroft: "No, we haven't had any indications of that at all. I shall look into it as soon as I go home." That was the first Bacher, and apparently Cockcroft, had heard of the Fuchs affair. Five months later Fuchs was arrested in Britain and confessed to spying for the Soviets.

CALIFORNIA INSTITUTE OF TECHNOLOGY, 1949

When Bacher was finally able to leave the AEC behind and get acquainted with Caltech's Division of Physics, Mathematics, and Astronomy (PMA), he found an educational operation that had not yet revived after World War II. The PMA faculty had 17 professors in 1949 (nine in physics, six in mathematics, two in astrophysics) but no division chair since Millikan retired in 1945. In the physics department there were two world-class research groups financed by the Office of Naval Research (ONR): the cosmic-ray lab started by Millikan and now directed by Carl Anderson, and the Kellogg Lab directed by Charles Lauritsen, with two Van de Graaff accelerators to investigate energy levels in light nuclei.

Other physics faculty carried out individual research—some quite distinguished—but the institute provided little financial support.

In the negotiations that brought Bacher to Caltech, DuBridge had agreed that Bacher would build an accelerator for research on high-energy physics and add to the physics faculty as needed for this mission, including theorists active in particle physics. Bacher had discussed this plan with both Lauritsen and Anderson before taking the job and found them both enthusiastically in favor of this growth, which supplemented and extended their own research interests. Lauritsen had hired Robert Langmuir, an electrical engineer and Los Alamos alumnus who had just completed a 70 MeV electron synchrotron at General Electric Research Labs, and put him to work designing a 600 MeV version of his GE machine even before Bacher's arrival. Lauritsen had also arranged a faculty appointment for Robert Christy, a former Los Alamos theorist much admired by Bacher who judged Christy to be "the department's only tie to modern theoretical physics."

Another strong supporter of Bacher's move into high-energy physics was E. O. Lawrence at the Berkeley Radiation Lab. When he learned of Bacher's plan to build an electron accelerator, he offered the magnets and vacuum chamber from the quarter-scale model of the 6 BeV proton accelerator (Bevatron) being built at the University of California, Berkeley. The Bevatron's race-track design was innovative but unproven and the model had been built to check out the new design.

Bacher enjoyed more good fortune in finding space for the new accelerator. The PMA had inherited a permanent, hangar-size building adjacent to the other physics buildings, built originally for grinding and polishing the 200 inch mirror for the Palomar telescope. Vacant since 1948, this building was "ideally suited to house the synchrotron." And free.

Thanks in part to this time- and money-saving good luck, Bacher had the needed ONR-AEC financing ($1 million for construction plus $300,000/year for operations) in hand by May 1950, and he had an electron beam of 500 MeV by 1952. Bacher's proposal to the AEC specified an electron synchrotron to be built in two phases. Phase I would accelerate electrons to 500 MeV, well above the energy of any existing electron accelerator. After exploiting the opportunities at that energy, new pole pieces could be added and the power supply upgraded to produce 1500 MeV electrons in Phase II.

At the same time he was adding to the staff. Already in 1949 he had arranged a faculty appointment for Robert Walker, a 1948 Ph.D. from Cornell who had impressed Bacher as an experimentalist at Los Alamos. With the AEC contract approved, he added experimentalists Alvin Tollestrup (a new Ph.D. from Lauritsen's lab), and Mathew Sands (a Los Alamos alumnus, expert in accelerator electronics). And he was working on theorist Richard Feynman.

When Feynman left Los Alamos in 1945, he followed Bethe to Cornell, much as Bacher had moved from Columbia to Cornell in 1935, following Bethe. But the cold winters and the relative isolation of Ithaca were strong negatives for a young widower who was, in the fall of 1949, just returning from a six-week visit to the Brazilian Center for Nuclear Research (BCNR) in Rio de Janeiro and the warm, sunny beaches nearby. Bacher became aware of Feynman's restlessness in Ithaca and saw an opportunity to invite him to spend the coming winter in sunny Southern California as a visiting professor during Caltech's winter quarter. Feynman accepted and spent January-March 1950 in Pasadena, offering a lecture course on his new graphical approach to quantum electrodynamics.

This visit went well for both the visitor and the host institution, and Bacher invited him to stay longer, with a full professorship starting in the fall of 1950. The conversation apparently went something like this:

Feynman: Thanks for the invitation, Bob, but I have a sabbatical from Cornell for the coming year (1950-1951), and I have arranged to spend my sabbatical at BCNR again.

Bacher: If you will accept a professorship here, you can spend 1950-1951 at BCNR on sabbatical from Caltech.

Feynman: OK.

Thus Bacher accomplished a recruiting coup that earned the admiration and envy of physics department chairs worldwide.

Bacher (and DuBridge, too) found Pasadena in 1949 far removed from the centers of intellectual activity on the East Coast and sought to remedy this isolation with a stream of visitors from the East, especially during the winter. "I went after this problem with a vengeance, right away. Just off the top of my I head I can remember that in the early days we had Bohr, Fermi, Oppenheimer, Bethe, Purcell, Rabi, Weisskopf, McMillan, Alvarez, Felix Bloch . . . Gell-Mann came sort of out-of-the-blue one year, just before Christmas. We'd heard of him, of course, but this visit had not been arranged. We heard him give one or two very interesting talks, and we immediately broached the subject, among ourselves, of doing something about it. That's how he came to Caltech."

ASTRONOMY AND MATHEMATICS

As chair of the PMA, Bacher was ex officio cochair of the observatory committee that directed the combined Mt. Wilson and Palomar observatories; Ira Bowen was the other cochair. Bowen had been a Caltech professor for many years before he resigned to become director of Mt. Wilson Observatory, which was built by, belonged to, and operated by the Carnegie Institution. The Palomar Observatory was built by, belonged to, and was operated by Caltech for the Rockefeller Foundation. This management arrangement for the combined observatories was never satisfactory. "Overall we [Bacher and Bowen] got along fine, but it wasn't an easy thing. There were always some observatory problems between Caltech and the Carnegie Institution and they became worse as the years went on."

Bacher's interest in astronomy was ignited by the 1951 observation and identification of the 21 cm radiation from atomic hydrogen and the opportunity it afforded for studying interstellar space. Possibly this transition between hyperfine levels took him back to his thesis days with Goudsmit. At any rate, Bacher thought the combined observatories should get into this new field of radio frequency astronomy. Bowen disagreed, "because he felt they didn't know enough to take up this field; it looked too hard and complicated." Bacher appealed to Vannevar Bush, director of the Carnegie Institution, who suggested that we "start with a little 10-foot dish in the park across the street [Tournament Park]. Frankly, that didn't seem to me enough of a start."

With enthusiastic backing from DuBridge, Bacher hired two British radio-astronomers: John Bolton and Gordon Stanley, both from Australia (Commonwealth Scientific and Industrial Research Organisation). With a grant from ONR in 1956, Bolton built a radio observatory in the Owens Valley (OVRO), about 300 miles north of Los Angeles, with two 90 foot dishes that could be coupled for interferometry. This instrument could determine the position of a radio source with such precision that the Palomar telescope could then pick out its optical radiation and find its distance, composition, and temperature. This OVRO-Palomar collaboration played a leading role in the study of quasars. The radio astronomy program has flourished and is still growing, far beyond anything Bacher could have imagined. It would give him great satisfaction to see what has become of his venture into radio astronomy.

Concerning the mathematics component of the PMA Division, Bacher's feeling was quite different: "As I look back over the years, I feel almost worse about not having been able to accomplish more in building up our work in mathematics than anything else." The math faculty was heavily involved in teaching the required two years of math to every Caltech undergraduate. Bacher felt that Millikan had treated the mathematicians as a teaching service. "Millikan quite consciously did not try to develop a leading mathematics department, and the mathematicians were aware of this. I spent a lot of time talking to the mathematicians about this, but found they did not agree fully among themselves about what should be done. Although we had people in theoretical physics who were very accomplished in mathematics, their contacts with our mathematicians were not great. The mixing of physics and mathematics did not happen. . . I tried very hard to get Mark Kac out here, because he can talk to physicists as easily as he can to mathematicians, and it's been one of my greatest regrets that I couldn't make it go. We were able to attract some people who were very good, but someone would steal them away from us."

CALTECH SYNCHROTRON, 1950-1969

The first research with the new synchrotron was a search for excited states of the nucleon. Earlier work at the synchrotron labs at Berkeley, Cornell, and MIT had hinted at a possible resonance (or peak) in the probability (cross-section) of producing pions when protons are bombarded with photons: $p + \gamma \rightarrow \pi^+ + n$, but their photon energy ($\leq$ 300 MeV) was not high enough to see the resonance clearly. Bacher and his coworkers set out to study this reaction with photons of energy up to 500 MeV.

The peak in the yield of neutral pions from the parallel reaction $p + \gamma \rightarrow \pi^0 + p$ was found right away—the peak was sharper than expected—and a letter[10] reporting this result was submitted to *Physical Review* early in 1953. This success was quickly followed by measurements confirming that this resonance appears in the photoproduction of $\pi^+ + n$. Next followed a series of measurements of the angular distribution of charged pions at a series of bombarding energies below and above the peak energy (300 MeV) to confirm that the reaction behaves as predicted by nuclear theory, thereby determining the energy, lifetime, angular momentum, and parity of the compound state. This was the first resonance or excited state involving pions that could be produced abundantly with an accelerator, instead of one at a time as in a cloud chamber photo, and the hope was that detailed information about excited states would help make sense of the many "strange particles" seen in cosmic-ray photos. No other accelerator at that time could match the Caltech synchrotron in this research.

Bacher's name rarely appeared as author on subsequent publications from the synchrotron. In his retiring address as president of the American Physical Society he turned again to this first synchrotron experiment (1965). "This pion-nucleon system with angular momentum 3/2 and isobaric spin 3/2 has a total mass of 1238 MeV [938 MeV proton mass + 300 MeV]. It is called $\Delta(1238)$ and fits into the same SU(3) multiplet with the recently discovered Ω^-, and is regarded as just as much a particle as the Ω^-." This is the only synchrotron experiment that Bacher refers to in his oral history.

Two further resonances were discovered as the Caltech group explored the pion-nucleon system at photon energies up to 500 MeV, the limiting electron energy in Phase I. In 1956 the accelerator was upgraded by installing new pole faces that raised the magnetic field strength, hence the maximum electron energy, to its Phase II value of 1.2 BeV (now called GeV). At 1.2 GeV Caltech's electron synchrotron was again the highest-energy electron accelerator in the world. But not for long; during the period 1956-1959, six other accelerators went online with energy above 1 GeV, and eleven more were under construction. The Caltech accelerator continued in operation until 1969 but its days of "highest energy in the world" were past.

"As more and more excited states of the nucleon were discovered, we could see that we would need to go to higher energy to get nucleon structure straightened out, but we didn't know where we wanted to go. One summer [1961] we ran a study of what a *really* high-energy machine would be like, and we tried to get other West Coast schools to join a Western Accelerator Group, without much success. The AEC did not like our making studies in this direction, and in fact ordered us to stop. They felt it complicated their political problems with universities in Northern California. But the report of that summer study contained a lot of good ideas that have persisted and were incorporated in later accelerators."

By the early 1960s Bacher had decided that "if we were going to continue in high-energy physics, we'd better get started sending people away to work on the really big machines, and eventually all our work turned in that direction. Our people are now (1980) working at most of the highest energy labs around the U.S. and some in Europe. I continued as PI on the contract but Bob Walker took over its actual management."

Bacher seemed to have a natural talent for rising to the highest levels of any activity he joined. He was a valued committee member, known and trusted by the other members, always present at the meetings, took copious notes. An example: intense regional competition for the next large accelerator led the AEC to appoint in 1965 a committee to select a site for the National Accelerator Laboratory (NAL). Gerald Tape, the only scientist on the AEC in 1965, was influential in naming the members of the site committee, and Bacher and Tape were old friends; Bacher had given Tape his first job, at Cornell before the war, and they had worked together at the MIT Radiation Lab. So it was not surprising when Bacher was asked to serve on the small site selection committee. After examining "some hundred proposed sites, we recommended four," and the AEC chose Batavia, Illinois, for the next big accelerator.

In parallel with the site selection, Fred Seitz, president of the National Academy of Sciences, was pushing the organization of the Universities Research Association (URA), made up initially of 34 universities with an interest in high-energy physics, the potential users of the NAL. The URA would manage the operation of the NAL for the AEC. "Lee DuBridge played a role in the URA right from the start (1965). He served on the Council of Presidents, and I was his alternate and almost always attended Council meetings." The URA was organized into regional groups, and each group named a trustee to the governing board. "I represented the Southern

California Group on the Board of Trustees for almost ten years, part of the time as vice-Chairman. I was even Chairman of the Board and president of the URA for one year." Thus Bacher maintained his involvement with high-energy physics even as his direct participation in Caltech's synchrotron lab diminished.

He was also drawn back into public service on the President's (Eisenhower's) Science Advisory Committee (PSAC) for two terms: November 18, 1953-June 30, 1955 and December 9, 1957-December 31, 1959. By 1957 Britain, the Soviet Union, and the United States were all testing both nuclear and hydrogen weapons, and Eisenhower wanted a treaty that would limit these tests. The PSAC was asked to study whether violations of a test ban could be detected, the same problem that Bacher had worked on twice before. And Bacher also had experience negotiating with Soviet scientists for the UNAEC. So Bacher was appointed (with James Fisk and E. O. Lawrence) to represent the United States on a committee of scientists called "Experts to Study the Methods of Detecting Violations of a Possible Agreement on the Suspension of Nuclear Tests," with representatives from Britain, Canada, and France, Soviet Union, Czechoslovakia, and Romania. The experts met in Geneva for two months during the summer of 1958. Although they reached agreement on the number (150) of monitoring stations needed to detect a violation, Bacher was disappointed with the committee's work: "It really didn't accomplish anything,...but if it hadn't been for those negotiations, it would not have been possible subsequently to get ahead in this area."

Eisenhower was sufficiently reassured by the committee's work that he proposed a one-year moratorium on all testing while the United States, Britain, and the Soviet Union worked toward a permanent treaty to ban nuclear testing. Following the meeting of the experts, all above-ground testing of nuclear weapons ceased from November 1958 to September 1961.

CALTECH PROVOST, 1962-1970

Bacher's years as Caltech's first provost coincided with a famously contentious period on U.S. campuses. Following the drawn-out and damaging loyalty oath controversy at the University of California, Berkeley, that led to the firing of 31 professors (1949-1951), Bacher played a prominent role as the Caltech faculty established its own Academic Freedom and Tenure Committee (AFTC) and the policies to guide it. "I took some time to go up [to Berkeley] and see the people there and find out why it had gotten into the situation it ended up in." He chaired the AFTC for its first four years, a period that included the investigation of Linus Pauling by a committee of state legislators searching for communists. Working behind the scenes, Bacher was able to prevent a noisy, front-page encounter between the two sides, which seemed to be aiming at confrontation. Even so, "[at least] two…trustees resigned as a result of this episode."

Provost Bacher's most notable improvement in Caltech's governance was the streamlining of Faculty Board procedures by creating a small (seven members) Steering Committee for the Faculty Board, with the provost as an exofficio member. In hindsight it's hard to imagine how the full Faculty Board functioned without a Steering Committee to prepare items in advance for consideration by the full board. This was an example of the administrative talent Bacher was famous for throughout his career.

Other lasting educational accomplishments during his tenure as provost were: (1) pass/fail grading for all freshman courses; (2) admission of undergraduate women; and (3) broadening the Humanities Division to include social sciences. The first two innovations were conceived and nurtured in discussion groups, often seated in a circle on the floor, initiated by Carl Rogers, a psychotherapist Bacher brought to the campus for two years to critique Caltech's undergraduate education. Well-known for his client-centered therapy, Rogers advocated student-centered learning, with pass-fail grading as a prime example. Bacher was pleased that this innovative (for Caltech) excursion into new-age psychology had a "lasting impact on faculty opinions and ideas."

When DuBridge resigned as Caltech's president in the fall of 1968 to become science adviser for the Nixon Administration, Bacher served as acting president until Harold Brown arrived in February 1969. Bacher knew and admired Brown from many contacts in Washington, notably on the PSAC. Bacher remained in the provost's office to smooth the new president's first year in his new job, and then resigned so Brown could choose his own provost.

On August 31, 1970, on his 65th birthday Bacher left the Provost's Office but remained a professor of physics on a half-time appointment (at his request because he was not teaching) until he joined the emeritus ranks in 1976. In 1988 he and Jean moved to Santa Barbara, where their daughter, Martha Bacher Eaton, lived. Jean, an ardent advocate[11] of nuclear arms control and limitation throughout her life, died in 1994; they had been married 64 years. Bob continued the task of putting his papers in order up to the time of his death on November 18, 2004, at age 99.

NOTES

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2. The index to the Bacher papers is at http://www.oac.cdlib.org.
3. Oppenheimer letter to Conant (February 1, 1943). In *Robert Oppenheimer, Letters and Recollections,* eds. A. K. Smith and C. Weiner, p. 247. Cambridge, Mass.: Harvard University Press, 1980.
4. At http://www.lanl.gov/history/road/pdf/Conant-Groves.pdf.
5. Smith and Wiener, op. cit. p. 254.
6. H. A. Bethe interview by Judith R. Goodstein. Pasadena, California, February 17, 1982, January 28, 1993. Oral History Project, California Institute of Technology Archives.
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8. Bethe, op. cit., p. 47.
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10. R. L. Walker, D. C. Oakley, and A. V. Tollestrup. Photoproduction of neutral mesons in hydrogen at high energies. *Phys. Rev.* 89(1953):1301-1302.
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