

Francis E. Low: Coming of Age as a Physicist in Postwar America

by David Kaiser



Today, in the midst of discussions over particle accelerators that cost billions of dollars and journal-article author lists that run into the hundreds of names, it is easy to lose sight of just how recently the present scale and scope of physics took hold. During the first half of the twentieth century, physics had remained a backwater discipline within the United States, focused around handcrafted table-top experiments and budgets that only rarely surpassed the thousand-dollar mark. All of that changed, however—and changed dramatically—during World War II. Often dubbed “the physicists’ war,” the global conflict shoved American physicists onto center stage. *Harper’s* magazine claimed soon after the war that “No dinner party is a success without at least one physicist”—a claim that can only be read with a wry smile today.¹ My historical interests focus upon how the discipline of physics changed after the war—in its ideas, institutions, and interrelations with other activities. I want to understand what it was like to become a young physicist during

MIT Visiting Professor Francis Low, in the classroom of 8.04 (Quantum Mechanics I), during the 1956-57 academic year.



this period of rapid transition, and how these changes helped to shape physics as we know it today. MIT Institute Professor Emeritus Francis E. Low, born in 1921, came of age as a physicist during these times of transformation. As a specialist in theoretical high energy physics, he has taught in MIT's Department of Physics since 1956. I talked with Professor Low during the spring of 2001 about his career, and about some of the broader shifts in how physics has been

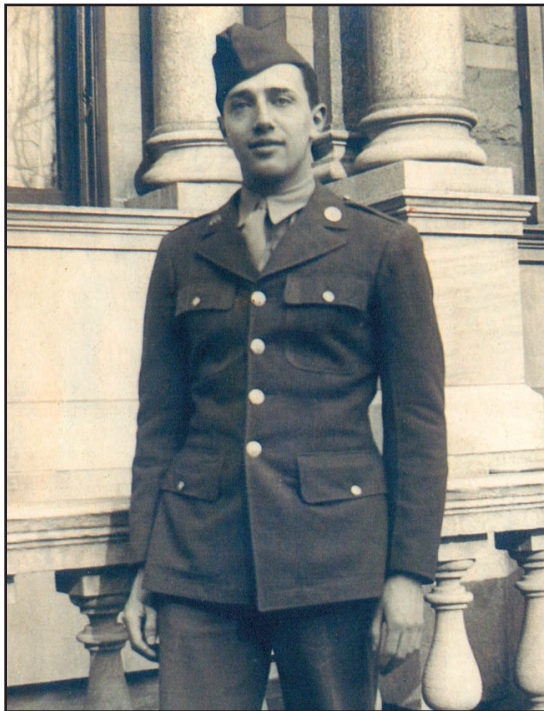
practiced during the past few decades.

Even before entering college, Low's interest in studying physics had been sparked by reading Albert Einstein and Leopold Infeld's book, *The Evolution of Physics* (1938). Twenty years earlier, Einstein had finished polishing his theories of special and general relativity, with their talk of time dilation, length contraction, and the curving of space-time. He had also made major contributions to the quantum-mechanical revolution, only to later emerge as its staunchest critic. Despite all of these famous efforts, however, Einstein and his co-author shied away from discussing these ideas of modern physics in their book. Instead, they focused on the edifice of classical field theory as developed during the nineteenth century. Even so, Professor Low recalls, "it was a very exciting presentation, very intellectually stimulating and interesting, and hard to pass up. The idea of eventually getting to see Maxwell's equations was very exciting."²

Thus primed with an interest in physics, Low entered Harvard as an undergraduate in 1939. He continued to focus on physics, in part because "this stuff was interesting," and in part because it seemed like the easiest course to follow:

"I was less strong in fields where there was no discernable objective criterion for quality. So I would write what I thought was an excellent essay, and it would come back with a B- or C, whereas, if I was doing physics and I got an answer, if I thought it was right, it was generally right." He recalls exciting lectures on mathematics from Saunders Mac Lane, along with classes on mathematical methods and theoretical physics from John Van Vleck, Edwin Kemble, and Wendell Furry.³

Low had an added incentive to concentrate on physics at Harvard. September 1939 was a difficult time to enter college: war had just broken out in Europe, and Low remembers thinking that "we would be in the war before long. I was quite sure it was going to happen." He was determined to complete his undergraduate studies as soon as possible, to have his degree in hand before wartime mobilization could interfere. Following along in physics, which he found both interesting and practicable, made the most sense. Having gone to high school at the International School in Geneva, Low had entered Harvard with a French baccalaureate degree and a strong Francophile inclination. From the start, he had been an interventionist, arguing with fellow classmates that the United



Pvt. Francis E. Low, in the dress uniform of the 616th Field Artillery Battalion of the U.S. Army, standing before his parents' home in Washington Square, New York City, summer of 1944.

States should enter the war. He graduated with his physics degree in three years, and immediately volunteered for military service.

Actually, he volunteered more than once. His first stop was with the Army Air Force, until a minor surgical complication interfered six months into flight training. (After the war, Low completed training for a pilot's license, and flew recreationally for many years.) Back home in Manhattan, he bumped into Elliott Montroll, a physical chemist, who asked what he was doing. Low replied, "Looking for work," and asked about Montroll's activities. "And he said, 'Well, I can't tell you. It's highly classified. But if you come work on it, of course, you'll know what it is.' I'd heard about uranium at Harvard," Low recalls. "[Kenneth] Bainbridge talked about uranium. So I went to work with Elliott." At the age of 22, Low thus joined the world's largest laboratory effort: the top-secret Manhattan Project, tasked to build an atomic bomb.

THE MANHATTAN PROJECT forged unprecedented links between academic scientists, industrial engineers, and military officials. The multi-site effort eventually cost over \$2 billion and employed over 125,000 people during the war — a scale of funding and personnel the likes of which no scientist in the world had ever experienced. The project included four main laboratories, in addition to hundreds of additional contracting sites. At the University of Chicago, Enrico Fermi's group built the first self-sustaining nuclear reactor. With it, they probed the ways and means by which uranium nuclei underwent fission, releasing excess energy with each splitting. In Hanford, Washington, scientists and engineers scaled up the Chicago reactor one thousandfold, to produce micrograms of the highly fissionable (and highly toxic) man-made element, plutonium. In the tiny town of Oak Ridge, Tennessee, architects threw together row after row of prefabricated, factory-built houses to quarter the corps of scientists and engineers suddenly stationed there. The staff at "Y-12," as the secret Oak Ridge facilities were code-named, labored to separate the fissionable isotope of uranium — uranium-235 — from the much more common (and much less interesting) isotope, uranium-238. The fourth laboratory would become the most famous of the Manhattan Project's sites: the laboratory in Los Alamos, New Mexico, where J. Robert Oppenheimer led the scientists' efforts to build both uranium and plutonium fission bombs.⁴

Low joined the gaseous diffusion effort at Oak Ridge. Uranium was combined with fluorine to form uranium hexafluoride — a nasty, corrosive gas. The fissionable uranium-235 atoms formed molecules that were ever so slightly lighter than those which included uranium-238 atoms — a mass difference of only 0.85%. Once brought to equilibrium, the molecules in the gas would each have the same average kinetic energy; thus the less-massive molecules (containing uranium-235) would move with slightly greater average speed than the more-massive molecules. If the gas were heated and passed through a porous membrane, then, after a short time, the faster-moving molecules would be just barely more likely to pass through to the other side than the more sluggish molecules. After

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What’s new?”
Rabi asked.
“I’m married,”
said Low.
“Delusions of
economic
grandeur,”
came Rabi’s
quick reply.

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one of these enrichment cycles, the concentration of the sought-for uranium-235 would be increased by only a factor of 1.0043, so the process would have to be repeated hundreds of times — each time taking the slightly-enriched portion and passing it through another diffusion cycle. Low’s job was to help with some of the arithmetic calculations behind this enrichment process.

Yet with only an undergraduate degree in hand, Low quickly grew frustrated, thinking that he “didn’t know anything, and that I was pretty useless.” So he left Oak Ridge and volunteered yet again with the army. Sent to Camp Upton (now the site of Brookhaven National Laboratory on Long Island), Low was interviewed to find out where in the army he might make the best fit. “What do you do?” he was asked. “And I said, ‘Well, I did some calculations.’ He said, ‘What about?’ I said, ‘I can’t tell you.’ And he said, ‘You can tell me, I’m in the army.’” Sticking to his guns, Low simply told the interviewer that he had done some computations, so he was assigned to be a “computer” with the Tenth Mountain Division (the Ski Troops) — except that “I was in artillery, and artillery people don’t ski!” His job involved working with survey teams in the field to help aim artillery and maintain maps of their own and enemy locations. They were stationed in Italy during the Appenine and Po Valley campaigns.

After the war, with assistance from the G.I. Bill, Low entered graduate school in Columbia University’s Physics Department. Like his military service, even this wasn’t quite as straightforward as it might appear. After having volunteered for the Army Air Force but before being called up, Low had been admitted to the graduate program at Princeton. Princeton at the time was strapped for teaching assistants, so the Physics Department Chair, Henry DeWolf Smyth, asked Low to work as a teaching assistant while waiting for the call from the Air Force.⁵ Low thus worked in Princeton’s Physics Department after graduating from college, leaving within the year to join the service when called. Upon returning from the war, he was told he had to re-apply for admission to Princeton’s graduate program, and then he was turned down!

He chose Columbia instead, and hence entered what would become the fastest-growing academic field in the country. Starting immediately after the war — with news of physicists’ wartime efforts on the atomic bomb, radar, and other projects splashed across the nation’s newspapers — enrollments in physics Ph.D. programs within the United States grew at lightning speeds. The rate at which physics doctorates were granted rose at nearly twice the speed of all other fields combined. Within two years after the end of the war, the nation’s universities were producing more physicists per year than the prewar highs; two years later, American physics departments had outstripped the prewar pattern by a factor of three.⁶ Low remembers the new bustle within the hallways and classrooms of Columbia’s Physics Department: whereas only two other students had taken Wendell Furry’s course on electromagnetic theory with him at Harvard before the war, suddenly the classes at Columbia (as elsewhere) were overflowing with young physics students.

LIFE IN GRADUATE SCHOOL proved to be an interesting time, both inside and outside of the classroom. During his graduate studies, Low married Natalie Sadigur, and they settled into an apartment together on West 29TH Street. “The apartment was full of life,” as Low remembered it in a brief autobiographical sketch he wrote in the mid-1960s. “On the floor above us lived a semi-professional prostitute, who received many interesting phone calls on the pay phone, which we answered since it was just outside our door. Also just above us was a mother with two hysterical adolescent daughters.” On one occasion, “we broke up a knife fight between them.” Meanwhile, “below us lived Leo Dubensky, a violinist who was formerly with the Philharmonic. I had a Steinway upright [piano], which my mother had bought for me, in our apartment and Dubensky and I played sonatas together mornings while his and my wife were out supporting us.” A few months after getting married, Low saw I.I. Rabi, at the time a recent Nobel laureate and one of the senior physicists in Columbia’s department. “What’s new?” Rabi asked. “I’m married,” said Low. “Delusions of economic grandeur,” came Rabi’s quick reply.⁷



Pilot Francis Low in the cockpit of a Cessna 172, during the late 1960s.

Rabi became a major influence on Low at Columbia. “Rabi really ran the department, though he wasn’t the chairman,” Low remembers. Rabi’s first order of business was to bring in visiting faculty to help train the suddenly large numbers of graduate student recruits. “The trouble was that theory was not well-supplied at Columbia,” or indeed at most American departments: with only a few exceptions, most American physicists before the war had worked in experimental physics, leaving the theoretical work to the Bohrs, Heisenbergs, Schrödingers, and Diracs of Europe. So after the war, Rabi arranged to get a steady stream of theorists from throughout the world to visit Columbia and work with the students. As Low recounts, “Students would sort of grab a visitor and shake him and get a thesis out. [Hideki] Yukawa was there, and Don Yennie shook him and got a thesis out of him. [Hans] Bethe was there, and I shook him, and started work on a thesis, and finished it with Aage Bohr, who had come. Norman Kroll worked with [Walter] Heitler.”⁸

LOW WORKED ON A TOPIC near to the hearts of Rabi, Bethe, and Bohr: the hyperfine structure of deuterium. Rabi, together with John Nafe and Edward Nelson, had been conducting extremely sensitive experiments at Columbia since the close of the war to measure the energy levels within simple

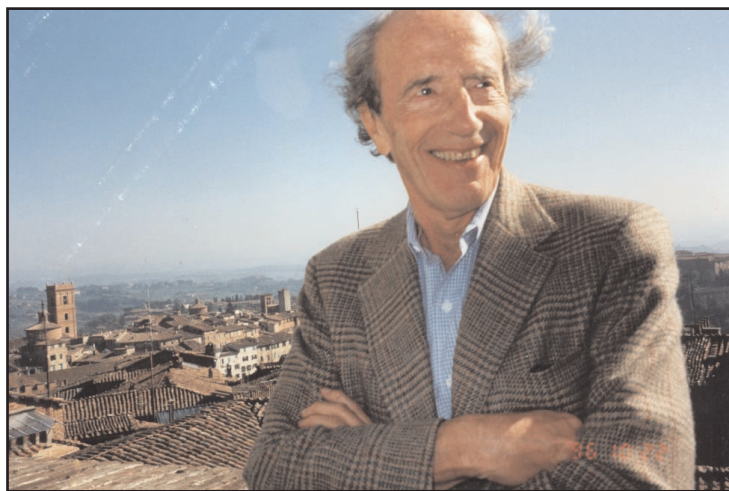
atoms. They used surplus electronics equipment from the wartime Radiation Laboratory at MIT, where Rabi had worked on radar along with thousands of other physicists and engineers. During the late 1940s, they used this equipment to probe the energy levels of hydrogen atoms to unprecedented accuracy. In a hydrogen atom, both its lone proton and its single electron carry some intrinsic angular momentum or “spin.” The spin of the proton can either line up in parallel with the electron’s spin or antiparallel — and the energy of the atom will be slightly different in these two cases, by just a fraction of a percent. This difference in energy between the two states became known as the “hyperfine structure” of the atom’s energy spectrum.⁹ Theorists such as Bethe and Gregory Breit struggled to calculate this tiny energy difference with the existing quantum-mechanical theory of electromagnetic forces, namely, quantum electrodynamics.¹⁰

The situation became even more complicated in the case of deuterium — an isotope of hydrogen, which has both a proton and a neutron in its nucleus, and only one electron. Aage Bohr had studied the structure of the deuteron, that is, the proton-plus-neutron nucleus of deuterium. Since the neutron also carries its own intrinsic angular momentum, the hyperfine structure of deuterium arose from the coupling between the combined spin of the nucleus and the spin of the

electron. Low investigated whether motions of the proton and neutron within the nucleus could account for some of the discrepancies between the measured and predicted values of the deuterium hyperfine structure — concluding at the end that the improvements in the theoretical value which he found looked promising, but uncertainties in both theoretical and experimental parameters remained too large to know for certain.¹¹

Certainty is rarely required in a dissertation, however. Having finished his dissertation, Low left Columbia in 1950 to embark on postdoctoral work at the presti-

gious Institute for Advanced Study in Princeton, New Jersey. Here again, Low’s career highlights some of the rapid changes within American physics after the war. Before the war, leading American physicists such as Kemble, Van Vleck, Rabi, Oppenheimer, and many others had traveled to the European centers for postdoctoral study. It was only in Cambridge, England, Copenhagen, Göttingen, or Zürich that these young Americans could, in Rabi’s famous telling, “learn the music” and not just “the libretto” of research in physics. After the war, these same American physicists endeavored to build up domestic training grounds for young physicists.



MIT Institute Professor Emeritus Francis Low, Italy, 1996.

ONE OF THE KEY CENTERS for young theorists to complete postdoctoral work became the Institute for Advanced Study, then under the direction of J. Robert Oppenheimer. Having achieved worldwide fame for his role as director of the wartime Los Alamos laboratory, Oppenheimer was in constant demand. He left his Berkeley post in 1947 to become director of the Princeton Institute, in part to have a closer perch to his newfound consulting duties in Washington, D.C. Upon arriving at the wooded, picturesque Institute, Oppenheimer stirred up controversy by increasing the numbers of young theoretical physicists in residence, at the expense of other fields. The Institute quickly became a common stopping-ground for young theorists, who circulated through what Oppenheimer called his “intellectual hotel” for two-year postdoctoral stays.¹²

Low’s recollections of his time at the Institute are happy ones. “It really was wonderful. I loved the Institute,” he coos. “It was a place where you went and you met your contemporaries, you saw where you were and who you were in the hierarchy. You were at the place where important things were going on.” In addition to benefiting from Oppenheimer’s example, Low “met people who were special in my life.” He befriended other young theorists such as T.D. Lee, C. N. (Frank) Yang, Abraham Pais, and Murray Gell-Mann.¹³ There was an informal, social atmosphere among the postdocs — but also some self-applied tension. “It was a tense place, and I would go into my office every day. The tension was, you couldn’t imagine how likely an important advance was. And the fear was not being there when dynamite was uncovered or discovered.” While at the Institute, Low began a collaboration with Gell-Mann that extended well into the 1950s.

Following their postdoctoral visits in Oppenheimer’s “hotel,” Low and Gell-Mann both headed for teaching jobs in the midwest: Gell-Mann to the University of Chicago in 1951, and Low to the University of Illinois in Urbana in 1952. While still in close proximity, they completed a paper in 1954 on the small-distance behavior of quantum electrodynamics. The rudiments of quantum electrodynamics had been known for thirty years. For most of that period, however, the theory had suffered from a grave sickness: as soon as physicists tried to calculate basic physical parameters, such as the mass or charge of an electron, beyond the simplest, barest-bones approximation, the theory produced infinities instead of finite answers. During the late 1940s, young theorists such as Richard Feynman, Julian Schwinger, and Freeman Dyson (all working in the United States) and Sin-Itiro Tomonaga (working independently in Japan) found ways to evade these troubling infinities. The idea was that the electron’s “bare” mass and charge truly were infinite — but that no one ever saw, much less experimented upon, such bare quantities. Instead, thanks to Heisenberg’s

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Lecture Hall Lowisms

Throughout his years of teaching at MIT, Prof. Low’s unique style of getting his point across made him something of a legend amongst his students. Collected below are some memorable moments from the Francis Low classroom.

On Counting

“We have two cases: either these states are different, or they are the same.”

“If $n = m$, ... Good God, what happens?”

On Problem Solving

“You can do it or you can just understand it.”

“If you’re very good at calculus, you could probably figure out a way of doing it without thinking.”

“Since I know that is the answer, I might as well try it.”

“You don’t have to think — now that’s a tremendous advantage.”

“If we calculate it too exactly we will be talking nonsense.”

“You solve the electromagnetic wave problem one way or another — generally badly.”

“It’s not hard because we’re only going to do things we can do.”

“If it works, it’s probably right.”

On Teaching Technique

“I’m lying to you a little, but it’s OK.”

“And if I’m feeling sadistic, I may assign it as a problem.”

“I’m just mentioning it so you know I know about it.”

“You can count on it, but don’t rely on it.”

“It’s a good question, but I think it’s not meaningful.”

“I can’t answer that offhand — but I’d prefer not to anyway.”

“I think you have to be confused a little bit about these things.”

“It’s a little bit silly to describe uncertainties too exactly.”

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Low describes
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theoretical
physics as
“minimalist,
but not dirty.”

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uncertainty principle, which lay at the heart of quantum mechanics, ghost-like particles were constantly popping into and out of existence, all around the electron in question. Some of these “virtual” particles themselves carried negative electric charge, while others carried positive electric charge. Together, pairs of oppositely-charged virtual particles provided a cloud of charge around the original electron. Any observer would therefore only measure the electron’s charge as shielded or screened by this ever-present sea of virtual particles — and, moreover, the combined, effective charge of this system remained finite. The program for systematically replacing infinite quantities with these compound, finite ones was dubbed “renormalization.”¹⁴

In their 1954 work, Gell-Mann and Low returned to the hard-won terrain of renormalization, examining still more closely the structure of the theoretical expressions for quantities such as the electron’s mass and charge. What they found was that the value of these parameters varied with the distance scale at which they were being studied — that is, an electron’s charge when measured at a certain distance would not be the same as the charge when measured at a much closer distance. In particular, they found simple scaling relationships between the parameters at various distance scales.¹⁵ Twenty years later, this paper was seen by many as having laid the crucial groundwork for the renormalization group and investigations into effective field theories — topics and tools which moved to the center of both particle theory and condensed-matter theory during the 1970s and 1980s. But that’s not how the paper was greeted at the time. Low remembers that the paper “did not get great approval. One of my close friends, whose name I won’t tell you, said ‘Francis, you could really be doing something interesting’” — by working on the new meson theories and strong-force interactions, instead of this abstract-looking electrodynamics work!

WHILE AT URBANA in the early and mid-1950s, Low also struck up an active collaboration and friendship with Geoffrey Chew, and through Chew, with Marvin “Murph” Goldberger. Chew was a young colleague of Low’s in Illinois, and Goldberger was working at Chicago; Chew and Goldberger had studied together both as graduate students and postdocs. Their varying styles of theorizing became complementary. Goldberger was fond of remarking that “the only rigor in theoretical physics is rigor mortis,” as he casually broke from formal developments and followed what looked to be promising phenomenological routes. Chew, meanwhile, had mastered the theoretical treatments of scattering phenomena — phase shifts, scattering lengths, poles in scattering amplitudes — long before these had entered most theorists’ toolkits. He brought these to the collaboration, while thanking Low for introducing him to the “fancy-ancy” techniques of Feynman diagrams, quantum field theory, and renormalization. These

were the skills that Low had honed while at the Institute, approaches that seemed to him much less “dirty,” or removed from the fundamentals, than some of the other theoretical approaches in play at the time. To this day, Low describes his approach to theoretical physics as “minimalist, but not dirty.”

With these varying strengths, the Midwest collaboration of Low, Chew, Goldberger, and Gell-Mann flourished for several years. They produced a series of papers trying to make sense of the new embarrassment of experimental riches pouring out of the federally-funded particle accelerators across the country. Low worked especially closely with Chew. “We were very good collaborators,” Low explains. “Geoff had lots of ideas and initiative, and I had a more critical view. We worked very well together, and we made a good team.” Even after Low left Urbana for MIT in 1956 and Chew left Urbana for Berkeley in 1957, they continued to collaborate over summers. The Chew-Low model for treating nucleon-meson interactions (in the limit that the nucleon remained static), published in 1956, soon led to even more useful results, such as the Chew-Low extrapolation technique, which they worked out in 1958-59. In this work, Chew and Low demonstrated how to extract information on interesting (yet experimentally unproduceable) interactions from the data on more humdrum experimental systems. The case they analyzed in their paper concerned elastic pion-pion scattering, some properties of which could be extrapolated from the by-then increasingly routine data on inelastic proton-pion interactions. Many years later, the Chew-Low technique was still guiding experimentalists who worked on similar scattering problems.¹⁶

AS HE STRUCK UP his collaboration with Chew in Urbana, Low’s attention also began to return to politics. Low, who had spent many late nights as an impassioned undergraduate arguing about foreign policy with classmates, now joined the budding Urbana branch of the Federation of American Scientists (FAS), which Chew had organized. The FAS lobbied against some of the worst abuses of McCarthyism, which were affecting physicists across the country. Even before the Atomic Energy Commission stunned the nation by denying Oppenheimer his security clearance in 1954, on allegations that he could no longer be trusted to act “loyally” in the service of his country, younger physicists had routinely labored under increasingly intrusive background checks when applying for jobs or fellowships. Politicians feared that physicists, as guardians of the “atomic secret,” required special scrutiny — even for the majority of physicists who did not work directly on military projects. Scores of young physicists found frustrating delays or denials when applying for passports for themselves, or for visas to host distinguished foreign visitors.¹⁷

Chew asked Low to join their local FAS group, and Low immediately agreed: “I was happy to do it. I thought it was a good organization. Geoff’s position was very good, and I was happy to take part in it. It was a serious time.” Part of their duties focused on their neighbors, as Low recalls: “One of the things we

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had to do was explain to the Illinois campus what the Fifth Amendment meant, and how one should listen to it.” They organized meetings on campus and hosted speakers. They also became a clearinghouse for other scientists’ complaints about unfair passport treatment. Chew eventually testified before the United States Senate to bring these abuses to wider attention.¹⁸

While working closely with Chew on both scientific and political questions, Low received an offer from MIT’s Department of Physics to spend the academic year 1956-57 in Cambridge as a visiting professor. It soon became an offer to stay on at MIT permanently. “It was a difficult decision for me,” Low reflects. “I enjoyed Illinois — it was a good place to live, a good place to bring up children. We made great friends who have lasted a lifetime.” On the other hand, at MIT “our children could be close to their grandparents.” And that wasn’t all — he had already gotten a taste of MIT and its environment during his year as a visiting professor: “Cambridge life is an exciting one, full of ideas, full of views. Driving down Memorial Drive along the Charles River is quite an exciting thing the first time you do it.” Although he considered his new geographical distance from Chew to be a major loss, he decided to stay at MIT.

Low became involved in another kind of political activity a few years later, when he joined the “JASON” group, organized under the auspices of the Institute for Defense Analysis (IDA). The IDA was a private think-tank with close ties to the military. The JASON group had been founded in late 1959 with the help of Goldberger and Kenneth Watson (another young theorist who had completed his postdoctoral work at the Institute for Advanced Study during the late 1940s).¹⁹ Low remembers how he became involved: “Ken Watson was very devoted to getting this group together, mainly theoretical physicists. And Ken convinced me that there were things happening, very dangerous things, and that if we didn’t solve some pressing physics problems, we, the United States, could be in real danger.” The group met for summer-study sessions, then divvied up problems that each member would work on during the intervening academic year.

Nearly all of these issues were highly classified; Low’s own clearance level had to be raised. They worked on topics such as the purported “missile gap,” which several politicians feared had opened up between American and Soviet nuclear capabilities. Fairly quickly, Low became disenchanted with the group, years before the Vietnam War-era protests brought the group into open controversy. Part of the problem, as Low looks back on it, was that he couldn’t find anything useful to actually work on. “Classified experiments are very tough to work with,” he explains. “If you want to test a model or theory, you need to produce a range of experiments and data which will cover the idea. You need to get a long range of continuous data” — a point which had recently been driven home to him in his work with Chew on the Chew-Low extrapolation technique. “It’s difficult enough in particle physics. But when you get into classified experiments, it’s just awful. With these classified experiments, you just have a point here, a point there. They go out to Eniwetok or Bikini” — islands in the South Pacific where the

United States conducted tests of both fission and fusion nuclear weapons during the 1950s — “and they say, ‘There’s a good point; here’s another point.’ It mitigates against effective analysis.” In terms of scientific questions, therefore, he quickly lost interest. “It got to the point where when I saw a red ‘Secret’ stamp on a file, a feeling of boredom immediately came over me.”

Worse still, most of the problems were political problems, he began to realize, and not necessarily scientific or technological ones. This realization led Low to question what role he and his physicist colleagues should be playing in the first place. “I felt that I really shouldn’t be doing it,” he recalls, “because it wasn’t necessary. It was just using up money and making me and other people do things that didn’t have to be done.” Low quietly left the group in frustration.

A few years later, during the late 1960s and early 1970s, he experienced similar frustrations when trying to balance scientific and political matters. MIT physicist Herman Feshbach had helped to organize the Cambridge, Massachusetts-based Union of Concerned Scientists (UCS) in 1969. Soon after founding the organization, Feshbach stepped down and Low succeeded him as chairman. Once again, Low found scientific or technological questions — such as how safe or clean nuclear power could be — tangled up with hard-set political positions. “The group seemed too automatic, too rigid,” Low remembers. Certain questions, such as whether nuclear reactors could ever be made safe, clean, and reliable, seemed to be settled in the negative before all the evidence had come in; they would become “*idées fixes*” of the group, in Low’s estimation. Even worse, Low and his colleagues’ advice often obtained a cool and uninterested reception from various officials in Washington, D.C. “So I dropped out [of the UCS]. I don’t think the group had a formal constitution or anything like that in those early days, so there was no major resignation. It was very friendly; I just dropped out, as a personal decision. These days I support the group by making annual donations, but that’s all.”²⁰

LOW TOOK A MUCH MORE ACTIVE ROLE within MIT starting in the late 1970s. The first step was becoming Director of MIT’s Laboratory for Nuclear Science (LNS), a capacity in which he served from March 1979 until July 1980. “I was very involved in high energy physics, so I thought I could be of help with the LNS, and there was little administrative work involved. Fred Epling was the assistant director of the LNS; he’s a marvelous man and he did practically all of the work. It was very easy, working with Fred. So I didn’t think I could say ‘no’ when asked to direct the LNS.” This directorship proved to be but a short step into higher administration, as Low explains: “Paul Grey, who became President of MIT in 1980, came to me. He and the rest of the administration wanted a scientist to be Provost, after a long line of engineers in higher administration. I had served as a ‘fake’ administrator at LNS, since I really didn’t have to do much. I had to get money, go to Washington and talk with important scientists down there, but that wasn’t too much.” It had been enough to catch

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Grey's attention, however, "so he asked me to be Provost. When I was asked to be Provost, my friends" — people like Vicky Weisskopf and Herman Feshbach in MIT's Physics Department — "said 'you could do it, you'd be good at it,' so I did it." Low served as MIT's Provost from July 1980 until July 1985.

"In the large, it was very exciting to be at the center of this institution, and to see what was happening all around you," Low remembers. "In detail, however, it was rather painful. People would walk into your office every day with important concerns that you usually couldn't fulfill. Money was not as easy to come by as it had been before" — the fire hose of federal funding which had been directed toward science and technology during and after World War II had slowed over the course of the 1970s, with détente and an economic recession. "We used to joke that the administrators earlier on had woken up each morning and asked themselves, 'What new programs should I start today?'" By the early 1980s, such funds had become more difficult to drum up.

"I believed that the system at MIT was working well overall, so I didn't try to make many large changes." Two initiatives, however, do stand out. One was an attempt to strengthen the humanities program at MIT, and to boost undergraduate enrollment in those areas. "I thought it was very important for students to have peers pursuing other kinds of studies. In the end, though, I didn't actually accomplish much on this front." The other initiative proved to be more successful. "One thing which we did that was very important was getting the Whitehead Institute for Biomedical Research started. It wasn't easy to get the terms acceptable to both the Whitehead's attorneys and to MIT's faculty. But eventually we did, and that has proven to be very important."

OTHER HIGHLIGHTS include his teaching and interactions with students. "I've worked with some wonderful people, both undergraduate and graduate students." Several stand out in particular. "Alan Guth is hard to forget. I always thought he was the smartest student I ever had." Guth earned his Ph.D. in MIT's Department of Physics in 1972. A few years later, he invented inflationary cosmology, a model of the early universe infused with ideas from particle theory, which remains the front-running cosmological theory today. Guth is currently the Victor F. Weisskopf Professor of Physics at MIT. "William Weisberger was a real pleasure," Low remembers as well. "He did some very nice work on weak-interaction sum rules — they were very pretty, very interesting." Weisberger completed his Ph.D. in 1964, and is currently a professor of physics at SUNY Stony Brook, working on superstring theory. "Mitchell Feigenbaum, one of the inventors of chaos theory, also turned into a great student. He was clearly original from the start, but he just got better and better." Feigenbaum earned his Ph.D. from the department in 1970, and today is director of the Center for Studies in Physics and Biology at the Rockefeller University, as well as a professor of mathematical physics there. He continues to conduct cutting-edge research in nonlinear systems and chaotic dynamics. "Adrian Patrascioiu was a

very creative guy. He always focused on a mistake — the mistake would be accepting some conventional idea. He would always look into what people’s beliefs were really based on. He was very skeptical — and a great skier!” Having completed his Ph.D. in 1972, Patrascioiu is now a professor of physics at the University of Arizona, working on quantum field theory, statistical mechanics, and dynamical systems. “Susan Coppersmith was a wonderful undergraduate student. She was in a junior or senior-level undergraduate quantum mechanics class. If I remember correctly, she left an apple on my desk on the last day of classes.” She completed her S.B. at MIT in 1978 with a degree in physics, and today is a professor of physics at the University of Chicago, specializing in theoretical condensed matter physics.

Francis Low’s career highlights the sea-change that physics and physicists have undergone during the past fifty years. A member of the first generation of American physicists to “grow up” amid the new institutional arrangements forged during World War II, Low and his peers embarked on home-grown training in theoretical physics, capped by domestic postdoctoral study. His research began with the new age of renormalizable quantum electrodynamics. Then he, along with his colleagues, helped to sharpen those hard-won tools for use in other areas of nuclear and particle physics, such as the strong-force interactions — tools that would eventually undergird the standard model of particle physics. Along the way, he has seen the discipline enter the broader public sphere, becoming involved with political questions — both as a classified inside consultant with JASON, and as a concerned outside critic with the FAS and the UCS. He has served as a research scientist and as a scientist-administrator, helping to direct the vast institutions of scientific research that sprang up in the wake of World War II. Only by charting careers such as Professor Low’s can we begin to deepen our understanding of what it has meant to become a physicist in the recent past, and what it can mean today.

DAVID KAISER, *Assistant Professor in MIT’s Program in Science, Technology, and Society, and Lecturer in the Department of Physics, works in both the history of science and in early-universe cosmology. His historical research concerns the establishment of theoretical physics in the United States after World War II. He is currently writing a book about the early spread of Feynman diagram techniques during the late 1940s and 1950s. In physics, his research has focused on post-inflation reheating, and on brane-world cosmology. Honors include the Leroy Apker Award from the American Physical Society, the Ivan Slade Prize from the British Society for the History of Science, and the Levitan Prize in the Humanities from MIT. He has recently been named the Leo Marx Professor in the History and Culture of Science and Technology at MIT for the term 2001-4.*

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- 1 See Daniel Kevles, *The Physicists: The History of a Scientific Community in Modern America*, 3rd ed. (Cambridge: Harvard University Press, 1995 [1978]). The quotation from *Harper’s* appears in *ibid.*, pp. 375-6.
- 2 Unless otherwise attributed, all quotations are from my conversations with Professor Low during April and June 2001.
- 3 Mac Lane specialized in modern algebra, co-writing an influential textbook on the topic with fellow Harvard mathematician Garrett Birkhoff, *A Survey of Modern Algebra* (New York: Macmillan, 1941). Beginning in the late 1910s, Van Vleck and Kemble spearheaded the first generation of American physicists to do any research on quantum theory, which at the time was developed predominantly by European physicists. See Katherine Sopka, *Quantum Physics in America: The Years through 1935* (New York: American Institute of Physics, 1988). Furry entered physics a little later, working as a postdoc under J. Robert Oppenheimer at UC-Berkeley in the mid-1930s, before joining the physics department at Harvard.

- 4 There is a tremendous literature on the wartime Manhattan Project. For an introduction, see Richard Rhodes, *The Making of the Atomic Bomb* (New York: Simon and Schuster, 1986). Details of the research conducted at Los Alamos during the war are treated in Lillian Hoddeson et al., *Critical Assembly: A Technical History of Los Alamos during the Oppenheimer Years, 1943-1945* (New York: Cambridge University Press, 1993). See also Peter Hales, *Atomic Spaces: Living on the Manhattan Project* (Chicago: University of Illinois Press, 1997); and Martin Sherwin, *A World Destroyed: Hiroshima and the Origins of the Arms Race*, 2nd ed. (New York: Vintage, 1987 [1973]).
- 5 Smyth later penned the famous “Smyth Report” on the atomic bomb, the only official information on the new atomic projects released by the government at the close of World War II: Henry DeWolf Smyth, *A general account of the development of methods of using atomic energy for military purposes under the auspices of the United States government, 1940-1945* (Washington, D.C.: U. S. Government Printing Office, 1945). This was very quickly republished as Smyth, *Atomic Energy for Military Purposes* (Princeton: Princeton University Press, 1946).
- 6 The sudden postwar expansion in enrollments within U.S. physics departments is discussed in David Kaiser, *Making Theory: Producing Physics and Physicists in Postwar America* (Ph.d. dissertation, Harvard University, 2000), CHAPTER I.
- 7 Francis Low, “Autobiography, ca. 1965,” deposited in the History of Physics Manuscript Biography Collection of the Niels Bohr Library, American Institute of Physics, College Park, Maryland. On Rabi’s career, see John Rigden, *Rabi: Scientist and Citizen* (New York: Basic Books, 1987).
- 8 Yukawa, a Japanese theorist, had been active in theoretical nuclear physics since the 1930s. In the mid-1930s, he predicted that a new particle, never before detected, might be responsible for carrying the strong nuclear force, that is, the force that keeps protons and neutrons bound together within atomic nuclei. Within a year of his prediction, physicists thought they had found Yukawa’s particle within cosmic ray tracks. Instead, they had found a different, unexpected particle, the mu-meson or muon. Only much later, in 1947, did physicists identify Yukawa’s predicted particle as the pi-meson or pion, although its role in the strong force remained murky for another twenty-five years. Bethe was born in Germany but emigrated to the United States in the mid-1930s; he has been teaching physics at Cornell ever since. During the 1930s, Bethe penned what quickly became known as “Bethe’s bible”: a series of review articles on nuclear physics. He won the Nobel Prize for his work on the nuclear fusion reactions which fuel stars, and served as head of the theoretical physics division at the wartime Los Alamos laboratory; he remains active in nuclear and particle theory. Aage Bohr, son of the famous Danish architect of quantum theory, Niels Bohr, himself became a Nobel Prize-winning physicist, working on various aspects of nuclear theory. Heitler, an émigré from Germany, worked in Dublin during the war. He penned a famous and influential textbook on quantum electrodynamics before the war: Walter Heitler, *The Quantum Theory of Radiation* (Oxford: Clarendon Press, 1936). For more on the work of each of these physicists, see, for example, *The Birth of Particle Physics*, edited by Laurie Brown and Lillian Hoddeson (New York: Cambridge University Press, 1983).
- 9 The designation “hyperfine” was meant to distinguish this splitting between energy levels from the so-called “fine-structure” of the energy states. The fine-structure arises from the coupling between the electron’s orbital angular momentum and its spin, whereas the hyperfine structure comes from the coupling between the spin of the nucleus and the spin of the electron.
- 10 On the early hyperfine-structure experiments and efforts to make sense of them theoretically, see Silvan Schweber, *QED and the Men Who Made It: Dyson, Feynman, Schwinger, and Tomonaga* (Princeton: Princeton University Press, 1994), pp. 188, 211, and 220.

- 11 Francis Low, "On the Effects of Internal Nuclear Motion on the Hyperfine Structure of Deuterium," *Physical Review* **77** (1950): 361-70.
- 12 See Ed Regis, *Who Got Einstein's Office? Eccentricity and Genius at the Institute for Advanced Study* (Reading, MA: Perseus Books, 1987), pp. 137-40.
- 13 Lee and Yang became most famous for their 1956 announcement that parity — that is, the symmetry between interactions and their mirror-image reflections — might be violated in certain particle interactions. Parity non-conservation, quickly confirmed in various experimental studies, earned the duo the Nobel Prize the very next year, in 1957. Imprisoned in a Gestapo jail during World War II, the Dutch physicist Pais escaped narrowly with his life when the war came to an end. He went on to postdoctoral work in theoretical physics with Niels Bohr in Copenhagen before moving to the Institute for Advanced Study in 1946; he made a series of major contributions to particle theory. Gell-Mann's most famous contributions to particle theory include the introduction of SU(3) symmetry for strongly-interacting particles, and his hypothesis that particles like protons and neutrons are made up of more fundamental particles called "quarks." See Abraham Pais, *A Tale of Two Continents: A Physicist's Life in a Turbulent World* (Princeton: Princeton University Press, 1997); and George Johnson, *Strange Beauty: Murray Gell-Mann and the Revolution in Twentieth-Century Physics* (New York: Knopf, 1999).
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- 15 Murray Gell-Mann and Francis Low, "Quantum Electrodynamics at Small Distances," *Physical Review* **95** (1954): 1300-12.
- 16 Geoffrey Chew and Francis Low, "Effective-range approach to the low-energy p-wave pion-nucleon interaction," *Physical Review* **101** (1956): 1570-9; *idem*, "Unstable particles as targets in scattering experiments," *Physical Review* **113** (1959): 1640-8. The Chew-Low extrapolation technique, as introduced in this 1959 paper, was invoked in practically every one of the contributions by experimentalists in the conference proceedings, *π - π Scattering – 1973 (Tallahassee Conference)*, edited by P. K. Williams and V. Hagopian (New York: American Institute of Physics, 1973).
- 17 See Alice Kimball Smith, *A Peril and a Hope: The Scientists' Movement in America, 1945-7* (Cambridge: MIT Press, 1965); Ellen Schrecker, *No Ivory Tower: McCarthyism and the Universities* (New York: Oxford University Press, 1986); Jessica Wang, *American Science in an Age of Anxiety: Scientists, Anticommunism, and the Cold War* (Chapel Hill: University of North Carolina Press, 1999); and Silvan Schweber, *In the Shadow of the Bomb: Oppenheimer, Bethe, and the Moral Responsibility of the Scientist* (Princeton: Princeton University Press, 2000). On the Oppenheimer hearing in particular, see Barton Bernstein, "In the matter of J. Robert Oppenheimer," *Historical Studies in the Physical Sciences* **12** (1982): 195-252.
- 18 Chew's testimony was published in Geoffrey Chew, "Passport Problems," *Bulletin of the Atomic Scientists* **12** (January 1956): 26-8.
- 19 On the founding of the JASON group, see Finn Aaserud, "Sputnik and the 'Princeton three': The national security laboratory that was not to be," *Historical Studies in the Physical and Biological Sciences* **25** (1995): 185-239.
- 20 Despite ups and downs over the past three decades, the UCS remains active today, producing technical studies and policy statements on a variety of topics from the environment and health to energy production and global security. See the group's website at <http://www.ucsus.org>.