

THE YOUNG FEYNMAN

How a 21-year-old grad student met this 28-year-old assistant prof and went on to amaze the kids, wreak havoc in the lab and invent a new way to understand quantum mechanics.

John Archibald Wheeler

"This chap from MIT: Look at his aptitude test ratings in mathematics and physics. Fantastic! Nobody else who's applying here at Princeton comes anywhere near so close to the absolute peak." Someone else on the Graduate Admissions Committee broke in, "He must be a diamond in the rough. We've never let in anyone with scores so low in history and English. But look at the practical experience he's had in chemistry and in working with friction."

These are not the exact words, but they convey the flavor of the committee discussion in the spring of 1939 that brought us 21-year-old Richard Phillips Feynman as a graduate student. How he ever came to be assigned to this 28-year-old assistant professor as grader in an undergraduate junior course in mechanics I will never know, but I am eternally grateful for the fortune that brought us together on more than one fascinating enterprise. As he brought those student papers back—with errors noted and helpful comments offered—there was often occasion to mention the work I was doing and the puzzlements I encountered. Discussions turned into laughter, laughter into jokes and jokes into more to-and-fro and more ideas.

The busted bottle

One day our discussions led to Mach's principle. We knew of the inspiration Einstein had found in thinking of inertia as originating in acceleration—not relative to Newton's absolute space, but relative to Mach's faraway stars. Was it a problem in the junior course in mechanics that started us thinking about the familiar lawn sprinkler? Shaped like a swastika, it shoots out four jets of water. The recoil drives the sprinkler arms round and round. But where does the recoil act? Doesn't it act at the point where the stream of water suddenly changes direction from straight

out to straight transverse? But suppose the arm sucks water in instead of squirting it out. Surely, we said to each other, there is an identical change in direction and therefore an identical reaction. Surely the sprinkler will again turn round when water in the arms is sucked in rather than being shot out. Oh no, it won't. Oh yes, it will. We had a great time trying out both sides of this question on our colleagues. As the days went by, more and more colleagues up and down the corridors took positions. The debate grew more animated. No argument of theory was strong enough to still the disagreements. The situation called for an experiment.

Feynman made a six-inch miniature lawn sprinkler out of glass tubing and hung it from a flexible tube of rubber. He checked that it worked OK as a sprinkler. Then he wangled the whole dangling gadget through the throat of a great glass carboy filled with water. He got this outfit set up on the floor of the cyclotron lab, where there was a handy compressed-air outlet. He ran the compressed air in through a second hole in the cork at the top of the carboy. Ha! A little tremor as the pressure was first applied, as water first began to run backward through the miniature lawn sprinkler. But, as the flow continued there was no reaction. Then increase the air pressure. Get more backward flow of water. Again a momentary tremor at the start of this maneuver but no continuing torque. OK, more pressure. And more! Boom! The glass container exploded. Water and fragments of glass went all over the cyclotron room. From that time onward Feynman was banished from the lab.

Everything as scattering

I enlisted Feynman's help on one of the ever-expanding problems I had brought back to Chapel Hill and then to Princeton from my post-doc days. At the great Rutherford-centered October 1934 London-Cambridge International Conference on Physics, four puzzles stood out. Of them none excited me more than and in my subsequent Copenhagen year than the problem of the minishower, as I called it—the puzzle of the so-called "anomalous" back

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PRINCETON UNIVERSITY

The front steps of Palmer Physical Laboratory, where Feynman had his physics courses and got banished from the cyclotron. The statue on the left depicts Ben Franklin performing his famous kite-and-key experiment. The one on the right depicts Joseph Henry, discoverer of electromagnetic self-induction and professor of natural philosophy from 1832 to 1846, before he went to Washington to be first head of the Smithsonian Institution, America's first "National Science Foundation."

scattering of gamma rays by lead. Almost every elementary process of photon physics was needed to understand the 1930–35 experimental results of Louis Gray and Gerald Tarrant, of Chung-Yao Chao, Lise Meitner and H. H. Hupfeld and of Jacob Jacobsen: production of Compton electrons, photoelectrons and pair electrons by the incident 2.6-MeV gamma ray, and electron—and photon—scattering, both single and multiple. For each elementary process I had a symbolic diagram and a curve of cross section as a function of energy; but to combine these processes into a prediction about the spectrum of back-scattered radiation much numerical slogging was needed.

As Feynman and I reviewed that enterprise we found we didn't have the heart for it. It remains undone to this day. Instead, we found ourselves entranced by two issues peripheral to the original undertaking: What is the complete story of Compton scattering within the framework of the Fermi–Thomas statistical-atom model? And how could we understand, in terms of scattering and nothing but scattering, the propagation of a photon through a medium of variable refractive index, or the passage of an electron through a position-dependent atomic potential? How many wonderful aspects of physics came together in these two enterprises, especially the second one: Huygens's principle as concept of how light (and—in our day—matter) propagate; refractive index as the cumulative consequence of many individual scattering processes; spirals—Cornu and other—as tool to add up scattered waves; and as motto to inspire us, the phrase "everything as scattering." What fun it was, what jokes

along the way, what a happy mix of diagrams and equations, of the well known and the new! That work never got published but both of us went on in postwar years to capitalize on the insights we had won from it.

The tumbling can

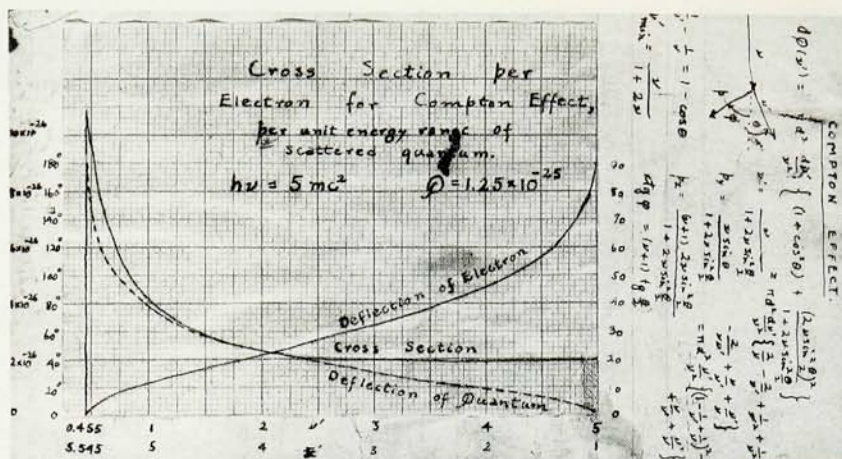
Sometimes we worked together in my Fine Hall office, three blocks east of Feynman's Graduate College room where he lived and worked—writing away hour after hour on one of those fan-folded pads of computer print-out paper, as enormous then as now. But for a long stretch of pow-wow, lasting two or three hours, we generally worked at my house two blocks west of the Graduate College. As we came downstairs from the work room for supper, Letitia, five, and Jamie, three, would follow him, hoping for one of the jokes or tricks he usually had up his sleeve. As those bright eyes tagged along, he teased, "A tin can." He came into the kitchen where my wife was cooking dinner and took off the counter a can not yet opened. "A tin can: I can tell you whether what's inside is solid or liquid without even opening it or looking at the label. Do you know how?"

"How?" came the response from the little people.

"By the way it turns when I toss it up in the air." And toss it he did, in an arc of wild precession. "Liquid," he announced. We could all see his prediction checked out right when the can was opened.

Hypnotized?

"Be my guest at the next Wednesday night dinner at the Graduate College," Feynman suggested one day. "There's



Letter and notebook page from 1935, subject of 1939–41 discussions with Richard Feynman. The letter (below), from Lise Meitner to the author, refers to the latest results on the “anomalous” backscattering of 2.6-MeV gamma rays. The page (left), torn out of the author’s workbook of the time, refers to the Compton effect, and is a sample of the data sheets of the many elementary processes that come into play in his minishower interpretation of the phenomenon. Standard diagrams of this type—the subject matter of consultations between Feynman and Wheeler—have a little of the flavor of ideological antecedents of the later, far more abstract, Feynman diagrams.

KAISER WILHELM-INSTITUT FÜR CHEMIE
Professor Dr. LISE MEITNER

PERNSPR. DA. BREITENBACH 1931 u. 1932

BERLIN-DAHLEM, DEN 23. März 1935.
THEILALLEE 41

Herrn I.A. WHEELER,
Institut für Theoretische Physik,

KOPENHAGEN.

Lieber Herr Wheeler,

Leider ist Dr.v.Droste, der die Streuungsmessungen an der γ -Strahlung bei 60° gesucht hat, derzeit krank, so dass ich Ihnen jetzt nichts über die Einzelheiten der Kurven berichten kann. Ich schicke Ihnen gleichzeitig die Arbeit von Dr.Köstern und hoffe, Ihnen nächste Woche, wenn Dr.v.Droste wieder im Institut ist, auch etwas näheres über dessen Messungen schreiben zu können.

Mit besten Grüßen

Lise Meitner

going to be a talk on hypnotism and a demonstration.” When the call came for a volunteer, it was Feynman who stood up and went up to the front of the crowded room. The hypnotist made his motions, spoke his abracadabra. In a sepulchral voice he intoned his instructions: “Walk to the corner of the room. Turn. Pick up the book that you will find lying before you. Balance it on your head. Bring it to me.” Feynman, looking like a sleepwalker, performed as commanded. He went on to fulfill further instructions. At last he was released.

Knowing Feynman, and having watched his performance, I came to an everyday, matter-of-fact theory of “hypnotism”: It’s acting. The Shakespearean player is animated to act his demanding part by the subtle pressure of the expectations of those around. So in hypnotism! Given an unfamiliar part to act, no one I know ever rose to the challenge more delightedly, more imaginatively and with more fun to his audience than Richard Feynman.

It would be tempting, if space permitted, to go on from the case of the tumbling can and the hypnotic trance to other stories of life with Feynman at Princeton: the black box electric circuit, the quaking jellyfish and the anodized-iron memory device. The last two are precursors, surely, of his lifelong interest in the mechanism of brain action. That interest showed never more clearly than in the Caltech seminar Feynman taught in his last years, first jointly with John J. Hopfield and Carver Mead, and then on his own (see the articles by W. Daniel Hillis on page 78 and by David L. Goodstein on page 70).

Precursor to a thesis topic

Richard Feynman is one of the many wonderful thesis advisees who, over the years, have done so much to teach me. In expressing indebtedness to him for many an insight, I testify also to the immense gratitude I feel to all students who have instructed me.

In 1939 Feynman had not yet decided what he was going to work on for his thesis or with whom. As a graduate student not yet committed to any particular topic or adviser, and being free—like all Princeton graduate students in physics then and now—of all formal course requirements, he had spread out before him all the richness in mathematics and physics of the university and the Institute for Advanced Study. He knew that I, on the other hand, was a divided man, torn between all the commitments that came in the wake of my fission work and an unquenchable curiosity about the foundation problems of physics. From more than one of my courses he knew my faith that whatever is important is at bottom utterly simple. But wasn’t my 1934–35 idea crazy: to out-Dirac Dirac and count the electron as the basis of

everything, of all particles, of the so-called “strong forces” of the nucleus, of even the electromagnetic field? Yet Feynman expressed some interest in this idea—and more in having even busy me as his adviser.

Interaction with the absorber

Animated by the concept of “everything as electrons,” I took time off from more immediate concerns one Sunday afternoon in the sunlit upstairs work room at home, and figuring on the back of an envelope I discovered that I could give a quantitative account of radiative reaction in terms of forces produced by the particles of the faraway absorber. The density of those particles and their distance—it turned out—cancel out provided only that there are enough particles around to guarantee complete absorption of the outgoing radiation. However, the strength I got this way for the force of radiative reaction was off by a factor of two from the well-known and often tested value.

The next morning, when Feynman came in with the homework to be returned to the students, I told him about my finding and my difficulty with the factor two. He jumped into the middle of this new game with his usual vim. He soon spotted the source of the trouble—I had undercounted the effective force exerted by the emitter on the absorber. Then all fell into place.

Not long afterward we gave a seminar report on our finding. At tea time a few days later Wolfgang Pauli said to me in a worried way that he felt that our result arose somehow from some mathematical tautology. However, Feynman and I went around to Einstein’s house at 112

Mercer Street to talk to him about our work. We found him both interested and sympathetic. He told us about a paper he had written with Walter Ritz to record their disagreement on the mechanism of radiation damping—to us a wonderful example of true collegiality and of responsibility in the realm of science. In this brief paper Ritz argued that the irreversibility of radiative reaction is a consequence of some irreversibility in electrodynamics itself. Einstein took the opposite position. In his view all truly basic equations for the dynamics of particles and fields are in and by themselves invariant with respect to reversal of the direction of time. The damping, in Einstein's view, originated somehow from asymmetry in the initial conditions. He expressed a strong interest in our work because we had at last given a concrete picture of what those initial conditions are and how they work.

Not until after the war, in stolen hours at conferences in Los Alamos and elsewhere, did we have the opportunity to present¹ an outlook so novel with some of the care it required.

A new method for a problem of a new kind

Our concept of direct action at a distance between charged particles, without the intermediation of any field: how to translate it from classical theory to quantum theory? How to capitalize for this purpose on the action principle of Adriaan Fokker? Feynman, with his wonderful zip, grabbed this issue and ran with it. A hint, in a paper of Paul Dirac, Feynman had soon magnified up into a complete prescription for quantization,² his famous method of "sum over histories" or "path integration"—also written up in full and published³ only after the war.

Phase as it came into the scattering problems that we had been considering, phase as it comes into the time-dependent Schrödinger wavefunction, phase as seen in Feynman's wonderful new method of sum over histories! To see this central place of wave phase in the scheme of things was to see in a new light the central place of the action principle in classical mechanics. I was learning from these discussions with Feynman that the integrated action of classical theory, in a sense more precise than ever before appreciated, is—apart from a universal factor,

$\hbar = 1.054 \times 10^{-27} \text{ g cm}^2/\text{sec}$ —only another name for the phase of the probability amplitude associated with the classical history.

Visiting Einstein one day, I could not resist telling him about Feynman's new way to express quantum theory. "Feynman has found a beautiful picture to understand the probability amplitude for a dynamical system to go from one specified configuration at one time to another specified configuration at a later time. He treats on a footing of absolute equality every conceivable history that leads from the initial state to the final one, no matter how crazy the motion in between. The contributions of these histories differ not at all in amplitude, only in phase. And the phase is nothing but the classical action integral, apart from the Dirac factor, $\hbar$. This prescription reproduces all of standard quantum theory. How could one ever want a simpler way to see what quantum theory is all about! Doesn't this marvelous discovery make you willing to accept quantum theory, Professor Einstein?" He replied in a serious voice, "I still cannot believe that God plays dice. But maybe," he smiled, "I have earned the right to make my mistakes."

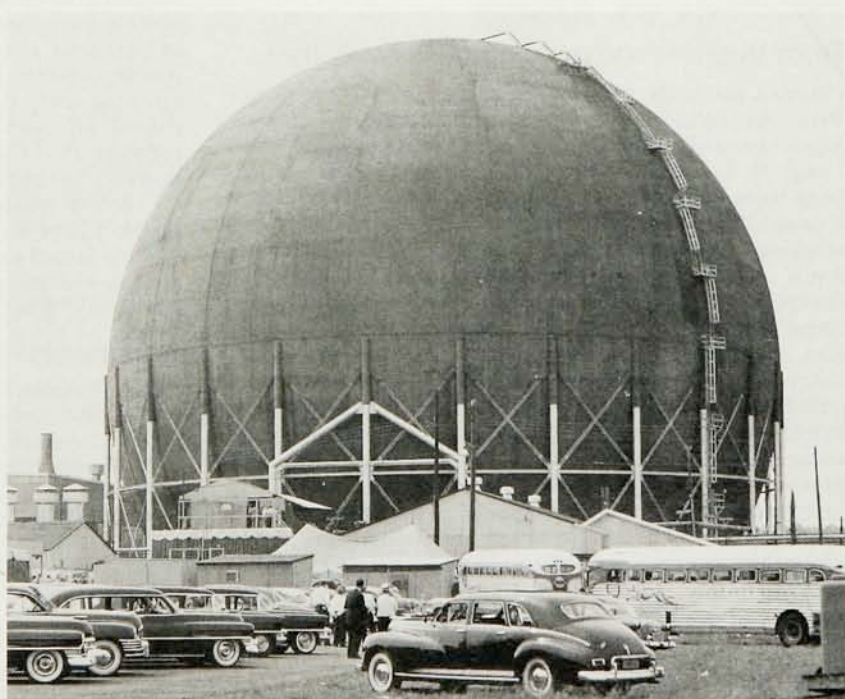
Undeterred I persisted, and still do, in regarding Feynman's PhD thesis as marking a moment when quantum theory for the first time became simpler than classical theory. I began my upcoming graduate course in classical mechanics with Feynman's idea that the microscopic point particle makes its way from A to B, not by a unique history, but by pursuing every conceivable history with democratically equal probability amplitude. Only out of Huygens's principle, only out of the concept of constructive and destructive interference between these contributions—and this only in an approximation—could one understand the existence of the classical history. Feynman sat there and took the course notes, of which I still have a mimeographed copy. On many a puzzling point he helped us both to find new light by discussions in class and out.

Any career for the kid from Far Rockaway?

While Richard was working on his thesis, his father, Melville Arthur Feynman, sales manager for a medium-

Dome of the first commercial nuclear power plant, West Milton, New York.

No feature of the modern nuclear power plant stands out more distinctively on the landscape than the familiar dome, proposed and made obligatory by the first Atomic Energy Commission Reactor Safeguard Committee, of which Richard Feynman, Edward Teller, Manson Benedict, Harry Wexler, Abel Wolman and the author were members.



GE HALL OF HISTORY



Joe Fowler and team to witness a high explosive implosion test. And at the lab with what enthusiasm he explained to me how he had found out that heat can't be hidden. Over and over on one of the best of the card-controlled IBM computers of that time he had calculated the same hydrodynamic implosion run. The resulting motions and pressures here and there in the metal came out filled with frightful irregularities. These irregularities, moreover, came out totally different from one run to the next. What was wrong with the computer? Suddenly he had grasped its message. The program had failed to include a term for heat. The machine knew better. If the stupid equations weren't going to include heat, the computer would have to impose its own way to represent heat: motion varying chaotically from point to point and from instant to instant. With what zest he explained this, and what delight he had in the nighttime fireworks! But all the time I knew the burden that lay close to his heart.

His wife, Arlene, lay slowly dying in the hospital at Albuquerque.

Arlene

On one of these trips from Hanford, Washington, to Los Alamos, New Mexico, I took the occasion to visit Arlene in the hospital. My wife and I had first come to know Arlene Greenbaum when Richard invited her down from his town, Far Rockaway, New York, to Princeton for one or another of the occasional Saturday night dances at the Graduate College. Auburn-haired, Arlene was not attractive; she was *very* attractive. Two lively watercolors by her we gained as souvenirs of these special weekends.

Those Princeton dances were respites from her overtaxed life. She was a full-time art student in New York by day. By night she taught piano, earning the wherewithal to pay for those art lessons. The strain of the double life in time, I believe, proved too much. She picked up an infection. Months went by with divided doctors dithering with their diagnoses. When finally she was coughing blood and it was unmistakably tuberculosis, it was too late. Feynman's latest book, which he did not live to see, tells the affecting story of how the two young people, against the advice of family and friends, and knowing early death was certain, nevertheless married, shared all their deepest concerns and stood devotedly together until the end. A week after I said good-bye to her hospital, her oxygen line and Arlene herself, she was gone.

Arlene was a strong character. She was one of the few people I have known who could stand up to Richard. She and his father and mother were guides he trusted. It was Arlene who gave him the advice that forms the title of his last book, "*What Do You Care What Other People Think?*"

References

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3. R. P. Feynman, *Rev. Mod. Phys.* **20**, 367 (1948). R. P. Feynman, A. R. Hibbs, *Quantum Mechanics and Path Integrals*, McGraw-Hill, New York (1975).
4. Nova, "The Pleasure of Finding Things Out," broadcast 25 January 1983.
5. R. P. Feynman, as told to R. Leighton, "*Surely You're Joking, Mr. Feynman!*" Norton, New York (1985). R. P. Feynman, as told to R. Leighton, "*What Do You Care What Other People Think?*" Norton, New York (1988). ■

sized uniform company, made a brief call on me in my office one day. How important he had been in Feynman's upbringing many of us saw in a Feynman television program,⁴ and more of us can read in his two autobiographical best-sellers.⁵ The father was concerned whether his son had any future. "A brilliant one," I assured him. "But won't he be handicapped by his simple background, or maybe even by some kind of anti-Jewish prejudice?" "No," I replied, and went on to describe the career histories of several close colleagues. I did not tell him that in college days in Baltimore I had been one of the founders and first president of the lively Federation of Church and Synagogue Youth!

From student-teacher to customer-supplier

Concern about the imminence of war drew some of our Princeton colleagues to the MIT Radiation Laboratory. Simultaneously the uranium work at Princeton grew: Heinz H. Barschall, Morton Kanner and Rudolf Ladenburg were doing controlled-neutron-energy experiments; Edward Creutz, Lewis A. Delsasso and Robert Wilson were working with the cyclotron; and Henry De W. Smyth, Louis A. Turner, Eugene Wigner and I were doing theoretical analysis. We brought Feynman into this work. Some months after Pearl Harbor some of us, including him, moved to Los Alamos, where Turner's plutonium concept was destined to win an ever bigger place. Before going Feynman took his final PhD oral. I was sad to have to miss it. However, I had already been called to Chicago to forward the uranium project. By fall, the West Stands pile—the first nuclear chain reactor—was on its way to final assembly, and Arthur Compton asked me to take Chicago know-how to Du Pont, manufacturer of the plutonium for our customer, Richard Feynman's Los Alamos. More than once he and I had to meet at Los Alamos to help formulate meticulously reliable safety precautions for the chemical separation of the plutonium at the Hanford plant.

One night Richard and I went out on the mesa with