feature article

Memories of Feynman

Theodore A. Welton

In this memoir, written in 1983, a contemporary and close friend of Richard Feynman's recalls the blossoming of Feynman's genius with vignettes from college, Los Alamos, and afterward.

After the adventures he describes here, **Ted Welton** (ginawelton@multipro.com) moved to Oak Ridge National Laboratory in Tennessee in 1951. He has since retired from his position there as a senior theoretical physicist in the physics division and now lives with his wife, Gina, in Crossville, Tennessee.

Almost 50 years ago, in September 1935, unnoticed among the as-yet-undifferentiated horde of entering freshmen at MIT were two ambitious, rather diffident physicists-to-be. One was Dick Feynman and the other was the author of these recollections. Initially unknown to one another, we remained so for the freshman year, since MIT grouped its students into classes by major. I was Course VIII (physics) from the beginning, while Feynman briefly vacillated, finding electrical engineering too practical after one semester and mathematics too abstract after another semester. My first hint of what was to come was on the occasion of the annual spring open house in 1936, when I found at one of the mathematics exhibits a fresh-faced kid (almost precisely my age, actually) who seemed to have a thorough comprehension of the concept of the Fourier transform and of the operation of the mechanical harmonic analyzer. Up to this point, I had nourished the fond, if secret, belief that I was the only freshman competent to handle such esoteric matters. Thus began my true education!

By the end of the summer of 1936, I had passed exams in the required mathematics courses for sophomore and junior physics majors and thus had available some interesting gaps in my schedule, which I promptly filled by signing up for the course Introduction to Theoretical Physics, taught from the book of the same name by John Slater and Nathaniel Frank. Before the first lecture, I had gone to the physics library and taken out Tullio Levi-Civita's book *The Absolute Differential Calculus*, which I hoped would reveal some further secrets of differential geometry not covered in Arthur Eddington's book *The Mathematical Theory of Relativity*, which I had read the previous year. Then on to class, where I discovered the mathematics whiz of the previous spring, apparently also prepared to do battle with theoretical physics. As I sat next to him, he glanced over at my books and immediately announced (in a somewhat raucous Far Rockaway version of standard English) that he had been trying to get hold of Levi-Civita and could he see it when I had finished. My interest piqued, I noticed that his stack contained Albert Wills's *Vector Analysis, with an Introduction to Tensor Analysis*, so he must be the reason I had been unable to find it in the library. Since we were, I think, the only two sophomores in that class, it apparently simultaneously occurred to the two of us that cooperation in the struggle against a crew of aggressive-looking seniors and graduate students might be mutually beneficial. Our friendship dated from that almost instantaneous recognition, and recollections from that period have enriched (and sometimes complicated) my life ever since.

Julius Stratton, who would later be chancellor or president or whatever they have at MIT, was the lecturer in that course, which we will both always remember with the greatest of pleasure. I was apparently easily accepted as a good student, but Dick was quickly recognized as truly superior. In fact, Stratton, who was certainly an admirable lecturer, would occasionally skimp on his preparation, with the usual consequence that he would come to an embarrassed halt, a little red creeping into his complexion. With only a moment's hesitation, he would ask, "Mr. Feynman, how did you handle this problem?" and Dick would diffidently proceed to the blackboard and give his solution, always correct and frequently ingenious. Stratton never entrusted his lecture to me or to any other student.

The mysteries of quantum mechanics

Dick and I quickly found that our interests, aspirations, and general state of physical knowledge were remarkably similar, or overlapped to such an extent that our conversations on such matters were enormously profitable to both of us. Later on, Dick would occasionally claim that I taught him quantum mechanics. The truth is that we learned it together, and the process was so pleasant that we never thought of it as work.

In our first conversation, on the afternoon of that memorable first class with Stratton, Dick announced that he wanted to learn general relativity. I already knew a bit and, with proper superiority, announced that I wanted to learn quantum mechanics. Dick promptly said that he had read a good book on the subject by somebody named Dirac and intimated that I should try my hand at it. Somewhat miffed at never having heard of Dirac or his book, I secretly resolved to get it and remedy this defect in my preparation. We talked for several hours about Albert Einstein's work on gravitation, of which, of course, neither of us had any deep appreciation. The mathematical manipulations of the general theory were, however, clear and interesting so that we considered ourselves real professionals. Dick had, by the way, ended his vacillation between too practical and too abstract and decided that theoretical physics was just right. I had previously come to the same conclusion, so we had no argument there.

The next day I got Paul Dirac's book, *Principles of Quantum Mechanics*, from the library and rather quickly found myself in over my head. Shortly thereafter, one of us located Linus Pauling and E. Bright Wilson's book *Introduction to Quantum Mechanics, with Applications to Chemistry*, and this was the level we chose for our entry into the mysterious realm of quantum mechanics. We wandered, without external guid-

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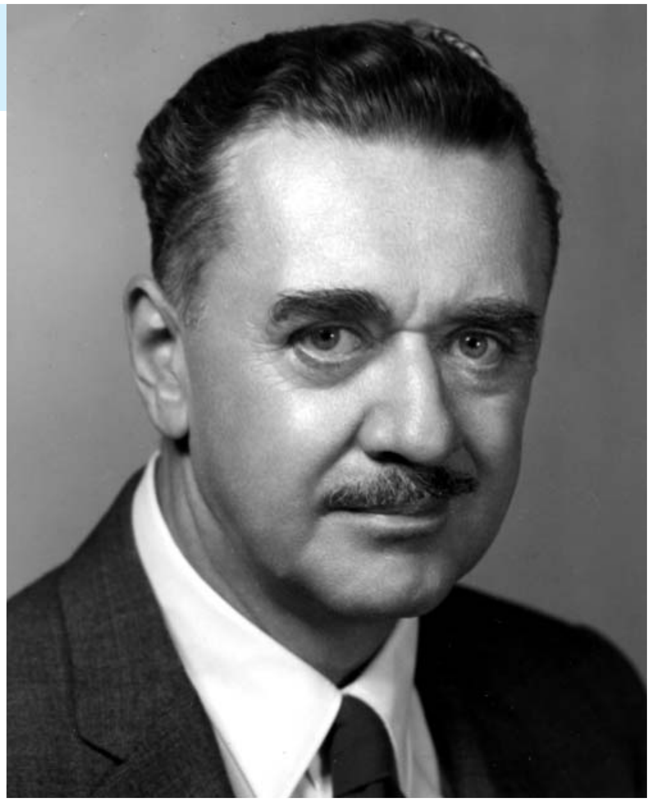
Philip Morse, a professor of physics at MIT, played an influential role in the education there of Richard Feynman and the author. (Courtesy of AIP Emilio Segrè Visual Archives.)



ance (except for the ideas of orthogonal functions and such that we were encountering in Stratton's course), through much of quantum mechanics, with naturally some enormous gaps in our understanding. Around Christmastime, one of our fellow students in Stratton's course announced that he had found a new book that seemed to have answers for most of the fundamental problem of physics. Its name was *The Relativity Theory of Protons and Electrons*, and its author was Eddington, whom we had already identified as a true prophet of the new physics from *The Mathematical Theory of Relativity* (not a bad book, but actually a bit pretentious). We got this book of promised revelation and took turns trying to make sense of it. We were both enormously impressed and far too immature to recognize it as garbage. It perhaps reflects faintly to our credit that we never fooled ourselves into believing we understood the book, but we did suffer through several agonizing months, tantalized by Eddington's claimed results, but convinced that we were somehow too dumb to understand his methods.

One amazing (in retrospect) quirk displayed by Dick in Stratton's course was his maddening refusal to concede that Joseph-Louis Lagrange might have something useful to say about physics. The rest of us were appropriately impressed with the compactness, elegance, and utility of Lagrange's formulation of mechanics, but Dick stubbornly insisted that real physics lay in identifying all the forces and properly resolving them into components. Fortunately, that madness appears to have lasted only a few years!

For the second semester, Introduction to Theoretical Physics was taken over by Phil Morse, who had been trained as a quantum mechanician and had worked extensively in several aspects of the field. Morse took proper advantage of his opportunity and included a section on elementary wave mechanics to complete the year. Dick and I already had acquired a fair grasp of the subject at that level, but Morse's systematic presentation did us no harm and further allowed Morse to see from our problem sets and questions that we were ready for greater things. Consequently, Dick and I were invited, with Al Clogston, who was a year ahead of us, to come to Morse's office for one afternoon a week the next year (our junior year) to be *properly* exposed to quantum mechanics. We accepted with alacrity and Morse thereby can claim an important role in our training.

"Real research"

After we had run through Dirac's book, Morse suggested we might be ready for a little real research and suggested some calculations of atomic properties using a rather convenient formulation of the variational method, which Morse had worked out in a previous paper. Dick and I set to work with a will, first learning how to use the "chug-chug-ding-chug-chug-chug-ding" calculators of those prewar days. Morse's scheme involved using kinetic and potential energy integrals calculated with hydrogenic radial functions and tabulated for various values of the coefficients occurring in the functions. For Dick, they were "hygienic" functions, and my efforts to convert him to "hydrogenic" were totally unavailing.

The result of our work was, of course, a gradual realization that quantum mechanics was more than a romantic dream and might have something to do with life outside the college years. By this time, I had learned one lesson well, namely, that I was not going to be able to compete with Dick in his chosen field. Unfortunately, some years would pass before I was to learn that life could be lived, perhaps even pleasantly, without that possibility. Fortunately, the first realization did not embitter me, and I continued to enjoy Dick's company and support, although we began to grow apart as he matured in his chosen field and I wandered a bit intellectually. Of course, we learned much together in addition to quantum mechanics, and I have always recognized elements in his later work that clearly came directly from some of the well-organized and well-taught MIT courses, particularly the physics but also the elementary electrical engineering courses we were required to take. Many of the chapters in *The Feynman Lectures on Physics* stem directly from material we were subjected to—rather pleasurably, I should add—during those years.

In our senior year, we had a nice experimental physics course given by George Harrison and featuring a well-organized lab. Harrison's lectures were a pleasure to attend, with a wealth of ingenious applications of physical principles. The laboratory required each student to select a project from a list, think of a way to execute it, and carry it through. Dick chose to make a mechanism to read out the ratio of two shaft speeds. He thought of a mechanism consisting of a turntable turned by one shaft, with the second shaft threaded and oriented parallel to a radius of the turntable. Threaded onto the shaft was a wheel whose rim was in contact with the turntable. As the two shafts rotated, the wheel would advance itself along the threads to a distance from the center of the turntable that was obviously proportional to the desired ratio. We all admired this simple and ingenious principle, and Dick went energetically and skillfully to work with the lathe. (In those days every physicist was supposed to know such things.) Then came the great day of the test. Everything fitted and rotated, but the accursed little wheel stubbornly refused to move to the correct location. The friction between the wheel and the turntable was too small in comparison with that between the wheel and the threaded shaft. I remember Dick's (and our) disappointment vividly. Another student was attempting to solve the same problem by charging and discharging condensers with commutators attached to the two shafts. His device did not simply fail on test; it blew up with gratifying electrical fireworks as all the capacitors let go at once. Very educational for all!

We also signed up that year for a nuclear physics seminar that was occasioned by the desire of Morse and Nat Frank to absorb the recently printed review articles by Hans Bethe and Stanley Livingston. They were a useful introduction to material needed later, but our attention was being largely absorbed by the required senior thesis. Dick did his under the direction of Slater and made a real contribution (the Feynman–Hellman theorem) to the understanding of molecular structure. My thesis, with Morse, was undistinguished, and I was left without an offer from a graduate school, while Dick accepted one from Princeton. We left MIT in 1939 not really expecting to meet again. Fortunately, Morse was able to locate an assistantship for me at the University of Illinois, but that is another story.

Los Alamos reunion

My Feynman saga resumes in the early spring of 1944. Bob Serber, Sidney Dancoff, and Phil Morrison had successively left Illinois (and me) to go to war research laboratories with purposes supposed by army intelligence to be unknown to us. (Actually, we knew a great deal.) I had gotten my degree, was married with a child on the way, and was quietly trying to retain my civilian status by teaching defense-oriented courses. This finally wore a bit thin as the draft regulations kept shifting, and the acting department head called me in and informed me that my best chance was to go up to Chicago where at a certain time, in a certain hotel room, a mysterious stranger would reveal to me a possible nonmilitary future. The stranger turned out to be Arthur L. Hughes, personnel director of the Los Alamos project, and he, in fact, offered me a spot working on his mysterious project (which I, in fact, already knew of in broad outline). It was the classic offer impossible to refuse, and I duly embarked in Bedroom B, Car 192 of the Santa Fe Chief for a strange-sounding destination in Santa Fe. Finally arriving on the mesa, my bus was met by a very familiar Dick Feynman set in a wildly unfamiliar context.

It turned out that he had saved my pitiful civilian life by noticing my name on a list of available physicists and filling out a suitable requisition. We went for a long hike that evening and began again the friendship that had first begun in 1935. His first question, "Do you know what we're doing here?" was answered truthfully, "Yes. You're making an atomic bomb." A momentary, slightly thunderstruck expression was followed by a second question, "Well, did you know we're going to make it with a new element?" That was answered in the negative, and he proceeded with the job of bringing me up to date.

Although I was 7000 feet above my normal altitude, my body postponed its demands long enough so that we walked the length of Los Alamos mesa, descended into Omega canyon, walked back the length of the mesa to Omega Site, home of the criticality experiments, and then climbed the more-or-less precipitous canyon wall back to square one. All this was done with a steady stream of excited conversation. Feynman was, of course, thoroughly adjusted to the altitude, but I could not have done such a feat again for perhaps a month following.

After giving me a thorough briefing on the work of the project and of his group—Julius Ashkin, Fred Reines, and Dick Ehrlich, until the addition of myself—the talk degenerated to a description of our interests in nonmilitary physics. I quickly perceived that Dick's former distaste for the work of Lagrange had undergone a certain alteration. I was already familiar with the Fokker–Tetrode Lagrangian (yielding the half-advanced, half-retarded electromagnetic interaction between charged particles), and Dick proceeded to explain in some detail how he and John Wheeler had attempted to go this route in avoiding the self-action problem. That work impressed me at the time as extraordinarily ingenious and hopeful. It was only after much hard work following the war that I was to realize that it was an untenable formulation.

As an important tool in the attempt to domesticate Fokker and Tetrode, Dick had discovered his later-to-be-famous formulation of quantum mechanics in terms of a summation over all possible spacetime trajectories of the system. He showed me how it worked by a simple illustration, but at that time neither he nor I had any idea of the use to which the method would eventually be put. Another interesting lecture was delivered on the iniquity of displacing integration paths for real integrals into the complex plane. Dick's view was that if you are going to do a real integral, you should do it and not mess around with the complex plane. He told how a fellow student at Princeton had challenged him to do all the definite integrals in the back of William Smyth's *Elements of the Differential and Integral Calculus* without setting foot off the real axis, with a 24-hour time limit. Dick didn't get much sleep, but he succeeded. I think this part of his thinking also had to be changed a bit to allow the work of 1947–48.

Much had also transpired at the personal level. Dick had married his high-school sweetheart, then discovered that she had a mysterious malady, finally and very belatedly diagnosed as tuberculosis. She was hospitalized in Albuquerque, where Dick visited every weekend and where she died, late into the project. The news was particularly sad for me to hear since I had met her in the spring of our last year at MIT in the course of a well-planned and executed joke by Dick on his shy friend. My wife was forced to remain in Illinois until after the birth of our son, at which time she came to Los Alamos and met Dick also. In this connection, I came in for some good-natured ribbing from Dick on my new responsibilities.

Dick had established a reputation for energy and ingenuity that was unmatched on the mesa. The four of us in his group (T-4, under Bethe of the theoretical division) were worked unmercifully but were proud to carry out our parts. The morale of T-4 was as high as that of any of the groups. We knew from experience that if an idea was needed, it would quickly come with suitable help from Dick. Some ideas too complex or bizarre for us to originate would emerge fully formed from Dick's fertile imagination. He would carefully describe what was to be done; we would skeptically ask how such a thing could work; he would patiently reply, "Let's try it and see!" We would all pitch in and do a prototype calculation with our faithful Work Projects Administration math tables and Marchant mechanical calculators. An essential feature would always be a preexisting special-case calculation with which comparison could then be made. We knew that a new method worked and what its error limits were when we were through with it. This game of innovation, played many times over, was perhaps the most stimulating time of my adult life. I know that it honed Dick's talents for what was to come.

Many others have detailed recollections of Feynman at Los Alamos, each viewpoint tinged with personal feelings.

LOS ALAMOS NATIONAL LABORATORY

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Los Alamos badge photos for Richard Feynman (left) and the author. (Feynman photo courtesy of AIP Emilio Segrè Visual Archives.)

We all saw him diplomatically, forcefully, usually with humor (gentle or not, as needed) dissuade a respected colleague from some unwise course. We all saw him forcefully rebuke a colleague less favored by his respect, frequently with definitely ungentle humor. Only a fool would have subjected himself twice to such an experience.

Puzzles

Dick loved to solve puzzles. Once presented with a clearly formulated physical paradox, mathematical result, card trick, or whatever, he would not sleep until he had the solution. Shortly after I arrived, I presented him with a problem (later immortalized in his *Lectures*, volume 2, section 17-4). I had gotten it from a friend at the Naval Research Laboratory and had immediately solved it. I stated the problem and Dick asked if I had solved it. I said yes, but truthfully and a bit strangely, the answer had slipped away. He promptly set to work on it, with me steadily demolishing his attempted solutions but still not remembering my own. We parted to get some sleep (I thought), but the next morning Dick showed up at the office a bit the worse for wear yet triumphant. This sort of thing happened over and over again, frequently with important matters rather than trivia. Incidentally, part of my education consisted of watching the already great of physics struggle unsuccessfully with that little problem as it spread like a plague through our close-knit community.

I have seen Feynman unsuccessful only once with one of these challenges. It was a card trick for which he was warned in advance that he could see it only once. The trick was duly performed with a result much to his surprise. He promptly said, "Do it again!" and was reminded that he had had his one permitted view. Muttering angrily, he went off to attempt to solve the problem. He failed, to his considerable annoyance. To make matters worse, the rest of his group solved it. Actually, we cheated, since we compared our separate experiences as we remembered the trick and thus effectively had the trick performed more than once.

Shortly after the start of the project but considerably before my arrival, the senior people (they actually weren't very old, being roughly 10 years older than we were) were chewing over the problem of calculating critical masses in a system with spherical symmetry and monoenergetic, isotropically scattered neutrons. The problem was approximately that formulated by Eberhard Hopf (the sun's limb darkening) and so beautifully solved by Norbert Wiener and Hopf, but the mathematics involved—integral equations—was thoroughly unfamiliar to all of us. There was a clear necessity to devise useful and general approximations, Wiener–Hopf being of no use except in an idealized situation. Dick, who was never shy about proposing physical ideas and had long admired the variational method in quantum mechanics, piped up with the suggestion that the criticality problem might be usefully approximated by a variational formulation. Bethe (probably among many) considered this to be nonsense and said so. Dick rose immediately to the challenge and when next seen had performed a simple, elegant, and convincing calculation. As an important result, Bethe, who combines a certain fair-mindedness with his talent for physics, correctly decided that Dick was a man to be trusted with complex physical problems, and Dick shortly found himself in great demand.

As another illustration, the explosion of a fission weapon would seem to be a matter of great complication, which it certainly is when viewed in detail. There was a clear need for reliable estimates of the yield of a given configuration, and some unprofitable attempts to make estimates that took the complications into account. Dick shortly produced Feynman's Famous Formula (one line, with only a few key parameters) for the efficiency of a fission explosion. There would be other famous formulae, but this one achieved a unique honor. When atomic spy Klaus Fuchs was passing information to courier Harry Gold, he included FFF. The KGB (or whatever the Soviets had in those days) would pass Gold's stuff to a group of their own scientists who could then send back queries by way of Gold to Fuchs. It is known from Fuchs's confession that his opposite numbers in Russia could not believe that the efficiency could be given reliably by such a simple formula, and Fuchs had to give Gold a detailed derivation. My understanding is that this query was one of maybe two that Fuchs had to answer.

Another type of puzzle had absorbed part of Dick's spare energy for several years. He had learned to pick Yale locks and had become very proficient. His method, which he willingly described, was the standard method of locksmiths and burglars the world over. At Los Alamos a related challenge appeared in the form of the combination safes. Dick quickly found that a straightforward testing of all possible combinations would consume about 8 hours, so the safes were actually *safe* since they would be viewed by a guard much more frequently than that. He did some private research on the safes in use and found he could quickly open them. He did not reveal his secret method to anyone at the time (he has

Young Richard Feynman (center), talking with Stanislaw Ulam (left) and John von Neumann in New Mexico's Bandelier National Monument around 1947. (Courtesy of AIP Emilio Segrè Visual Archives.)

since told all in a most interesting interview), but it did become widely known that in case of necessity, Dick could be depended on to open any safe. He developed another skill, namely the ability to dissect a sick Marchant calculator and restore it to perfect health. Bethe, thinking of all the good theoretical man-hours going down the drain, finally had the wit to get the army to procure enlisted men with the appropriate skills and to forbid Feynman from spending any more of his valuable time on such activities. Of course, the new locksmith couldn't easily open a safe whose combination he didn't know, so Feynman was left a portion of his old territory.

The boss does it again

Edward Teller, in his prime, was a most remarkable theoretical physicist. He produced ingenious ideas at a great rate, many of them potentially very useful. Unfortunately, he was also a great salesman and he could sell an ultimately unworkable idea with great enthusiasm. One of his ideas, aimed at an eventual thermonuclear weapon, was to fabricate the available fissionable material as the hydride, rather than the metal. Teller argued that the critical mass would be much reduced and that the performance as a weapon would not thereby be fatally degraded. Bethe accordingly set Feynman and T-4 the task of evaluating Teller's suggestions with great care.

Up to that time, problems involving slowing of neutrons had also involved relatively large systems so that the slowing could be reasonably handled by Enrico Fermi's age approximation. Now we were faced with the necessity of making a real advance in techniques for criticality calculations if a genuine critique of this new proposal were to be made. Group T-2, under Serber, had been charged with bomb criticality calculations and had done an excellent job of the Wiener–Hopf-type calculations. Bob Marshak had come from Montreal, bringing with him a generalization of diffusion theory, the spherical-harmonic method. T-2 had thus carried through a variety of calculations of known accuracy for two-region, infinite spherical systems, with only elastic isotropic scattering and monoenergetic neutrons.

This available variety of results was not yet enough to satisfy the clear needs of T-4, so we began a somewhat sneaky invasion of T-2's territory. This started with one of Feynman's seemingly mystical insights to the effect that the general problem (omitting only inelastic scattering in the outer reflector region) could be solved very simply and accurately if only a table or, preferably, a simple formula were available of the eigenvalues defining criticality for the general spherical, infinite, two-region, monoenergetic, isotropic problem.

We displaced our usual incredulity at his bold assertion, following which our usual testing period convinced us all that the boss had done it again! Unfortunately, the required table or formula did not exist, nor did it seem likely to. Only a short period of reflection was, however, required before Feynman announced that we were going to take the accumulated computational results from T-2, put them through the meat grinder, season them with some further insights (yet to be produced), and extrude this mixture as a handy interpolation–extrapolation formula. (Its name would be FFF #2, I suppose.)

We quickly set to it, and after a very hard day's work, we had the formula, a full rationale, and a thorough check against the available data. We then had the job of persuading T-2 that our result was (a) correct and (b) useful. This took a little longer, but it is worth noting that when Serber eventually produced an ingenious method of rather directly approximating to the solution of the general integral equation, he took as his test of success the quite good agreement he found with FFF #2.

We had definitely scored, but not yet quite on Bethe's required target because we could not yet handle a moderating reflector. Feynman muttered and mumbled, but nothing coherent came out for awhile. Finally he announced that he thought he had a beautiful general solution building neatly onto what we had already done. Ashkin and I pressed him for details, but he pled lack of time since he was about to leave on a vacation trip. We subsided but agreed we would make a concerted effort to steal a march on Dick in his absence. We did work at top speed for a week and probably showed a considerable excess over our normal ingenuity. On Feynman's return we proudly showed what we had accomplished (a potentially useful special class of problem) and in turn asked him about his new invention. He looked at us in (feigned?) surprise and asked us for further information. We assured

The Shelter Island Conference of 1947 gathered 23 physics luminaries to discuss the foundations of quantum mechanics. Willis Lamb's report there of the first measurement of the 1058-MHz level splitting in the hydrogen spectrum was a landmark in the development of quantum electrodynamics. Shown here from left to right are Lamb, Abraham Pais, John Wheeler, Richard Feynman, Herman Feschbach, and Julian Schwinger. (Courtesy of AIP Emilio Segrè Visual Archives.)

him that we knew only that he had promised a general solution to the problem we had attempted. In some (feigned?) astonishment, he assured us that he would go off and attempt to resurrect his supposed inspiration. Somewhat skeptically we waited, until after a few hours Dick burst in to give us a lecture. This one was less transparent than any he had thus far delivered, and we reacted with the usual skepticism. Dick told us how he proposed to test it, and we got to work. Within a couple of hours we were convinced, but we still did not understand why it worked.

Dick had to spend several days in the hospital, and I took the opportunity to attempt some alternate, understandable (to me, that is) derivation of this mysterious new result. To my gratification, I did get the germ of an idea. To my annoyance, when Dick returned and I explained my idea, he took the ball and ran very fast with it. Within an hour he had a different formalism on the blackboard. At least we all understood this one, but we were confused as to how both could be correct. It took only a short time to convince ourselves that both worked well, although the results were not identical. Much later Dick resolved the problem and showed how each method was a plausible approximation to the exact solution.

Finally, we had sharpened our computational task to the point where a reliable judgment could be made on Teller's hydride proposal. The answer was negative, in that the presence of the hydrogen grossly lengthened the generation time so that the efficiency formula effectively gave zero.

At about this time, the glory days of T-4 were past. Feynman was asked to take on a major new responsibility for the group doing hydrodynamic calculations, and the rest of us took on smaller tasks, for which the master's touch was not essential. He continued to support us and was always available for advice, but we had no more of those crash developments that had so enhanced our days.

Postwar diaspora

Finally came the test and the two bombs on Japan and then the six months' job of winding down the project. I took on the major task of writing up the many-velocity criticality methods, and the rest of us were similarly occupied, but the war was now over and urgency had departed. We had time to begin to think about postwar activities. Each senior scientist, of course, already had a spot lined up: Bethe back to Cornell University (he briefly planned to go to Columbia, I. I. Rabi being a major attraction), Victor Weisskopf to MIT, Serber to the University of California, Berkeley, and Rudy Peierls to the University of Birmingham. Each had been commissioned to bring one or more junior people with him, and a number of us then had the problem of deciding where to go. Feynman had it easy, having decided some time before to cast his lot with Bethe. I was torn between Weisskopf, whose style in physics seemed to be more of the kind I might reasonably aspire to, and Bethe, who made a strong case for the fact that he could perhaps teach me to calculate, a skill that was not fully apparent in my work up to that point. I was too unsure of myself to consider Peierls's offer to take me to England with him, and since a goodly part of my graduate training had been with Serber, it was time to seek further experience with someone new.

Aside from the clear fact that Bethe's facility with theoretical calculations tended to frighten me rather than fill my youthful head with visions of how I might emulate him, there was a practical fact militating against Cornell (and unfortunately, therefore, Bethe also). Feynman would be there, and it was clear that I had to strike out on my own or be a pale shadow of my friend for the rest of my life. Dick was understanding when I explained my feelings after he had entreated me to go along and teach him field theory. I smiled at that one! I had learned field theory under Serber's tutelage, and I assumed that Dick had learned it with Wheeler and was just buttering me up a bit. Later on I came to realize that Dick had somehow been insulated from quantum electrodynamics (or at least from the more general aspects of quantum field theory) at Princeton and that this strange innocence had probably played a crucial role in allowing him the freedom to innovate as he did a few years later.

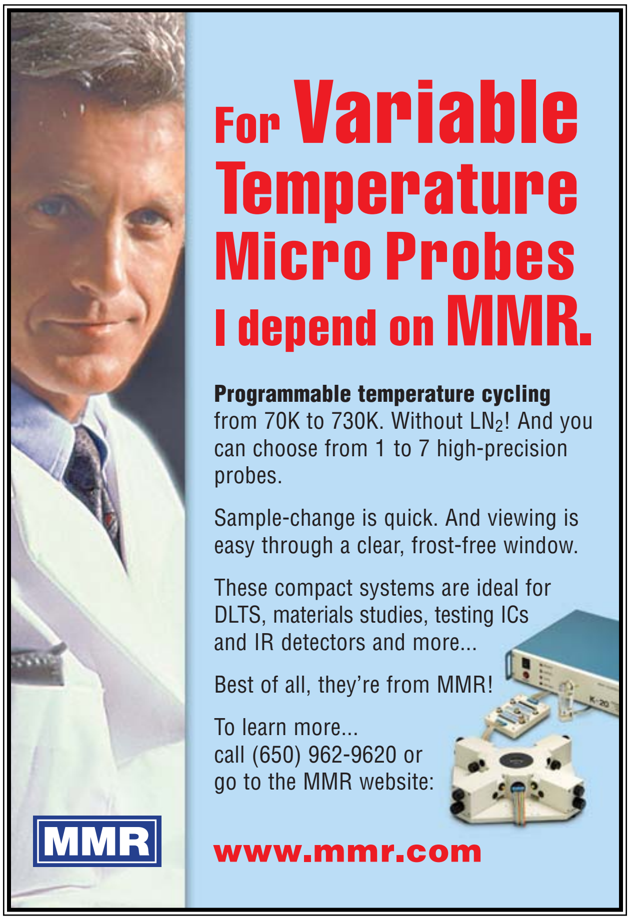
So it was to be Weisskopf and MIT for me. I bade farewell to Los Alamos and Feynman, knowing this time, however, that we would meet again. Weisskopf had opted for MIT because they had made him a fine offer and because Julian Schwinger was to be only a mile or so away at Harvard. We had frequent joint theoretical seminars with the Harvard people, and I quickly acquired a healthy respect for Julian. For a time the burning issues seemed to be quite apart from the quantum electrodynamics that consumed much of my energy without yielding any visible fruit. I encountered Feynman at meetings and was entranced as always by the flow of ideas, but it was clear that his mind was not really where it properly belonged.

After studying the self-energy problem in about every way I could imagine, the light briefly dawned, and I obtained an interesting-looking formula for the level shift in an atom. Weisskopf was interested and wanted to present it at the Shelter Island Conference, but I was convinced I would have a relativistic result in a few weeks and asked him to desist. On returning from vacation, I encountered Bethe at General Electric and asked how things had gone at Shelter Island. Since the atomic level shift had now clearly become a hot item, I thought it time to fill him in on my ideas. To my considerable dismay, he pulled a manuscript from his drawer with the crucial 1000 megacycles already calculated. So much for my idea, but where was Feynman? I was desperately working, hoping to regain some lost ground, but unfortunately by this time so was everyone else! Schwinger was delivering magnificent weekly lectures in which he developed a formalism with beauty and power I could not dream of matching. Weisskopf was starting a full-dress relativistic calculation of the Lamb shift and traveling each weekend to Ithaca to keep Bethe informed of progress (and, presumably, to warn him off from the race). Still no Feynman!

Feynman arrives

Finally—I think it was at the New York American Physical Society meeting in January 1948—I had my usual talk with Dick, and he told me that Bethe had finally gotten him pointed in the right direction and explained what he was up to. I went back to Cambridge and gave my colleagues their first coherent account of the rumblings then beginning at Ithaca. Dick had succeeded in doing a relativistic mass-renormalization using what he would later call a modified photon propagator, but he did not yet actually have the full, beautiful, clearly covariant formalism, which is today learned and used by everyone. Feynman had finally arrived! From then on for several years, I watched with interest and admiration while the Feynman machine built up power and overtook the already highly perfected Schwingerian approach. I was, of course, still struggling to get something out of my own clearly outclassed efforts, and finally came to appreciate the full beauty of what Dick had done only several years after he handed me his two crucial reprints with the quiet advice, "You may find these interesting."

And so the story ends. I have seen Dick a few times since leaving MIT and have talked extensively with him a very few times. He has had a profound influence on my life, and I hope to have reciprocated in some degree. He will smile if he reads this, but I feel as I imagine Marcel Grossmann must have felt about Einstein or Christopher Wren about Newton: amazed at having been given the privilege of knowing so interesting a character. After long reflection, I would think it apt to compare the young Feynman with the young Newton. Of course, Newton had it easy; he had a new science to invent. Feynman could only perfect something already existing, but the ingenuity and energy with which he went about the job have been seen only rarely since the plague years. ■



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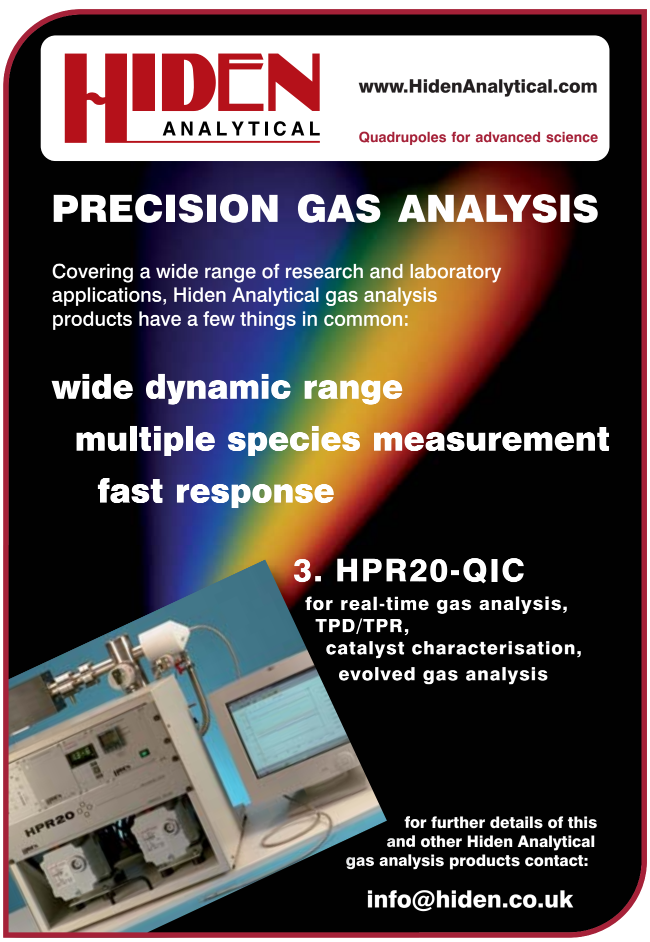

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