N A R R A T I V E

To me, self-analysis is anathema; perhaps I am typical of scientists in this respect. I, therefore, approach this task with markedly little enthusiasm, and with considerable doubt about the value of these pages. However, another of my traits is a tendency to try to do what is asked of me. Besides, my wife says I have to do it, so here goes.

Some Generalities.

At this moment (mid-1965) I am an administrator and a textbook writer. This makes me, according to my peers, an ex-physicist. Indeed, I am increasingly apprehensive as the frontier accelerates away from me. Nevertheless, I choose to regard myself as an interim non-physicist, not an ex-physicist. I enjoy administrating and writing in the same way that I enjoy research. That is, in all three activities I groan at the frustrations, moan about falling short of perfection, and find solid satisfaction in the tangible evidences of success. I shall return at the end of this narrative to reflect on the relative merits of these activities.

One characteristic of my academic and professional life is that it has flowed along from point to point with very little conscious decision-making on my part. I see among many of my own students now long, hard processes of decision-making. Until very recently I had none, at least none that seemed at all difficult. I went from high school to Exeter, from Exeter to Harvard, from Harvard to Princeton, from Princeton to Los Alamos and back to Princeton, from there to Indiana, all on the recommendation of respected teachers. I did not question their recommendations then, nor do I now. Science as a career seemed natural and inevitable from the eighth or ninth grade, and this became refined to physics by the beginning of the twelfth grade with never a hint of agonizing choice. At the same time I have never been satisfied with my level of attainment. Dissatisfaction, self-doubt, and the struggle to achieve more have marked every step along my course, although at the same time I have been entirely satisfied with the places I have studied, the places I have worked, and the fields of work.

Only once have I wrestled long and hard with a professional decision. This was whether to accept a department chairmanship at Brandeis. I accepted it, and this decision was undoubtedly a turning point in my professional career. It will take another decade to see its implications. At the moment I think it was a wrong decision.

Preschool.

I was born in 1926, the second of three children, the other two being girls. The home environment was favorably oriented towards books and music, but was not self-consciously intellectual, nor tied to any academic or intellectual heritage. My father, the son of an Ohio farmer, was the first in his line to complete college. He was a civil engineer, advancing during my childhood from engineer-surveyor to construction superintendent with an interval of manual labor in a tobacco factory during the early thirties. My mother was one of five girls, only one of whom (not she) went to college. She was self-educated and read a great deal. She encouraged, perhaps pushed, my intellectual development. I was richly supplied with puzzles, blocks, maps, and alphabets at an early age, and could read before starting kindergarten.

Our family moved often, three times before I started to school, another six or eight times before I finished high school. I was born in West Palm Beach, Florida, and began kindergarten in St. Matthews, Kentucky.

Elementary School.

Perhaps in kindergarten, and in any event from first grade onward, I was always aware of being in the top one or two or three pupils in my class, and accepted this as the normal thing. I was highly competitive from the first. I regarded being number 1 as satisfactory, being number 2 (behind a girl) as unsatisfactory. I fretted over a single error of spelling or grammar or arithmetic. I was shy, but not wholly an outsider, except for one year in a small town in southern Goergia, when my fellow students gave me a bad time. Otherwise, I was tolerated as the brain, just as another was the athlete, another the comic, another the musician.

I skipped no grades, although this was discussed by my teachers and my mother in the second grade. Usually I was given extra work. In Georgia I sat at the back of the room working through books given to me by the teacher, for the most part ignoring the class. This was in the third and fourth grades. In the fifth through eighth grades, I was given no special treatment. In this period I started reading a great deal. Outside reading reached a peak in the eighth and ninth grades, when I went through every science book in the school library. I was particularly influenced then by Eddington and Jeans. In the eighth grade, a teacher told me I should be a lawyer. I have occasionally thought she was right.

Secondary School.

My educational fortunes were greatly advanced in the tenth grade by two good teachers and by a piece of luck. One of the teachers taught me mathematics. (This was in Highlands High School, Fort Thomas, Kentucky.) She decided that I ought to do two years in one. She set me down in the back of the room with the tenth grade text, a geometry book. When I had worked my way through this, she examined me on it, gave me some makeup work on what I had thereby missed of the eleventh grade class in advanced algebra, and then I joined the eleventh graders to finish the year with them. This was simple, and it worked. As a result of her attention, I had a good calculus course two years later before leaving secondary school.

The second teacher taught me Latin. From her class I entered a national competition and won a prize. What is important, however, is only that she took a personal interest in my future. She learned through an Exeter graduate who lived in Ft. Thomas (the only one there--this was the piece of luck) that the Phillip Exeter Academy offered regional scholarships to boys from the south and west. I was only barely south of the Ohio River but I qualified geographically. She brought this possibility to the attention of my parents, she arranged for me to be nominated, and, when I was indeed offered a full scholarship (including train fare), she urged me to accept it. With my parents' approval, I followed her advice, although I had not previously heard of Exeter.

Nothing better could have happened to me. In two years at Exeter, I pursued mathematics through calculus, took two years of physics, an excellerated two-year German course, a good history course, and a demanding and rewarding course in composition. Also, thanks to Exeter's requirement of physical training, I learned that I was not a weakling after all, and gained strength and enthusiasm for sport that I would never have acquired in high school. I often think of my physical development as the most irreplaceable value of Exeter. Since then, too, I have come to appreciate the all-important connection between physical well-being and intellectual activity. Exeter was at the same time a wonderful challenge scholastically. I responded with competitive fire, worked far harder than in high school (with a corresponding reduction in outside reading and non-assigned projects), and ended at the top of the class. Still, it took more than prizes and success to give me poise or self-confidence. Although I won a German prize, a trip to Washington as a Science Talent Search winner, a scholarship to Harvard, and a prize for general excellence, I was nervous for weeks over the prospect of a valedictory address and was terrified when I stood up to give it. I was far more conscious then, as now, of imperfections than of relative success.

Elbert P. Little was not my best teacher at Exeter. Others (Major in Mathematics, Kesler in English, Gropp in German) stand out more clearly for their force in the classroom. But Bert Little, my physics teacher, was the most influential, and is the only one to have remained a friend and adviser in later years. His personal interest in me and his love for his subject proved to be more important than pedagogic excellence. Through him I was led into physics. On his advice I selected Harvard for undergraduate work, and again on his advice, I selected Princeton for graduate work.

A frequently expressed sentiment of teachers (to be found, among other places, in Feynman's preface to his recently published lectures) is that the good student will succeed without help, so that the teacher might better concentrate his attention on the less able. My experience in secondary school, and in higher education as well, has made me antagonistic to this point of view. My efforts and my rate of progress were always governed by two factors: the competition of fellow students and the interest and encouragement of teachers. My self-motivation apart from these external factors was low. It is a little higher now that I have experienced the satisfactions of discovery a few times, but the external factor, which is now the opinion of my colleagues, remains of dominant importance. At every stage of my education, from first grade to Ph.D., special attention from a teacher has resulted in greater effort and greater success from me. In every instance I have been grateful for it. As a teacher now, I see the brightest student as my primary responsibility. I certainly try to give every student a fair chance at something of lasting value to him. But if I have to choose where to put my extra time and extra attention, I choose the best student.

The War.

At Exeter from 1942 to 1944 I was scarcely aware that a war was in progress. When I did face the reality of military service upon approaching my eighteenth birthday in May, 1944, it was neither with a sense of mission nor with a sense of dread. It was simply what one did, and I went about the business of choosing an avenue of service purely in terms of self-interest. To avoid the infantry, I enlisted in the Navy. After failing to meet the perfect vision requirement for the V-12 college training program, I selected the Naval Air Force (which, remarkably, had less stringent visual requirements). Then I gave alternative consideration to an electronic technician training program. After some wavering, I picked the latter, not because it was closer to science, but because I judged the probability of death in the Air Force to be unreasonably high. Finally, after completing my Ph.D. work in 1953, I learned to fly, and I have been at it steadily since.

I worked at the electronics course as I had worked at school, enough to stay at or near the top. Monthly ranks in class were posted. Competition with fellow students provided the motivation, for to the instructors we were anonymous masses, or so I thought. In three months at Gulfport, Mississippi, the most memorable event was the occasion when an officer I didn't recognize stopped me ("What have I done wrong?" I thought) to ask in a friendly way why I had fallen down from first to second place that month. I readjusted my slide rule and redoubled my efforts.

Later in Chicago I was offered a second chance at college training (V-12). This time I passed the eye test, and in July, 1945, I joined several hundred sailors and a handful of civilians at John Carroll University in Cleveland, Ohio. By good fortune, my request for special consideration was approved despite a fixed required curriculum for all freshman. I passed the first year of physics and the first two years of mathematics by examination, moved into a course in thermodynamics taught by a lively priest, and received an excellent almost-tutorial course (with two other students) in differential equations under a fine teacher named Noetzel. I was fortunate to get started at this institution under a commanding officer willing to be flexible.

In October, 1945, with the war already over, the Navy moved the John Carroll group to the University of Michigan, where I recall in particular an excellent course in optics with Meyer. In his laboratory, this fine old gentleman taught me the meaning of precision work. Here, too, the

Navy proved flexible, allowing me to spend time in the optics lab when other sailors were practicing marching drill. I left the Navy in June, 1946, after one calendar year of college work that added up to two years of credit.

The Navy brought out in me a remarkable resistance to regimentation. I am not sure what this has to do with my career and work, but it may be related to my drive to do independent work. As a boy I had declined all opportunities to go to summer camp because I disliked the idea of a regulated life. At Michigan, my rebellion against rules that seemed silly led me to take an evening job as a waiter, which required that I break three rules--by earning money, by being out of uniform, and by being out after hours. I look back with horror on this lawlessness, but it happened.

Despite a uniform and the rules that went with it, I was for all practical purposes untouched by the war. Had I been born a year or two earlier, it might have been very different.

College.

I rejoined my Exeter friends at Harvard in the fall of 1946 and spent two enjoyable hard-working years there. The greatest excitement and expansion of my horizons came in courses outside of science, especially a course in philosophy by Demos and a course in language and thought by I. A. Richards. David Widder was an excellent teacher of mathematics. Herbert Goldstein was my best teacher of physics and I was lucky to have two courses with him. As a Harvard professor he did not publish and so perished. That was a loss to students.

In college I worked for grades. I had time for romance, for love letters, for outside reading and bridge games and squash games, and for a stint of service at a settlement house. But by and large, I was an efficient grade-getting machine. I paid little attention to the outside world, wasted no time on self-analysis, and stayed clear of extra-curricular activities (except briefly late in my senior year, when I joined a student group writing a critique of Harvard education). The results, in 18 semester courses, were 16 A's and A-'s, 2 B+'s (these in philosophy). In the class of about 1,000, I was one of seven to graduate summa cum laude. Two others were physicists: Richard Milburn, now at Tufts University and Herman Carr, now at Rutgers University.

Despite my efforts to succeed according to the measurable standards of the University, I was less impressed by my own accomplishments than were most of my friends. I was very conscious of the fact that my successes were still measures of potentiality, not actuality, and that the crucial tests lay ahead. I was aware, too, that other students had chosen other roads, that the student who devoted himself to the Harvard Crimson was perhaps gaining more from his college experience than I. My own immaturity outside the confines of the classroom was made plain when I came before a Rhodes Scholarship examining committee. I badly muffed their questions which sought to explore my knowledge and thought about the world in general. I was dropped from the competition.

Graduate School.

For the same reason that I did not go to Boy Scout Camp, I did not go to Caltech. Their catalog implied a more regimented program than did Princeton's. Also, Bert Little recommended Princeton for theory, and I had by this time decided to be a theorist. I was admitted and offered teaching assistantships at Caltech and Princeton. I had not applied to Chicago because they required a $5.00 registration fee. So I chose Princeton.

In graduate school I was for the first time merely average or slightly above average among my fellow students. I was no match for the pure intellect of Silvan Schweber or Richard Ferrell. I enjoyed teaching and worked at it. Along with my group of a dozen students, I pursued course work for two years. Almost all of us passed the qualifying examination in the spring of 1950.

In my second year of graduate study, I published my first paper (a Physical Review Letter, co-authored with David Bohm, then an assistant professor at Princeton). A small thing, yet more original than most of my later papers. It resulted from a calculation I did for a journal club talk. Bohm saw that it had some significance. He urged me to publish it. He added some thoughts and together we wrote it up. I omit from this narrative all details of this and later research papers, since I will attach specific comments to the reprints.

After an excellent course in classical mechanics with John Wheeler, I decided I wanted to work with him. He had a flair and originality and a way of getting behind the formalism that appealed to me. He agreed to take me on, and suggested studies fo collective motion in nuclei as a topic. Thanks to the Russian atomic explosion in late 1949, my association with John Wheeler turned out to be much closer than it otherwise might have been. That association proved to be by far the most important and the most beneficial influence on my subsequent scientific career.

I can not remember when I first approached Wheeler. Perhaps it was by mail in the spring of 1950. Things moved swiftly thereafter. By June 1950 I was working in Los Alamos. Urged on by Edward Teller, Wheeler cut short a leave in Europe in the spring of 1950 and joined Teller in Los Alamos to work on the development of a thermonuclear bomb. At one point in that spring, Wheeler and Teller were both in Princeton and I recall sitting on a stairway in the Institute for Advanced Study while Teller urged me to come, too. I accepted without hesitation. I wanted to work with Wheeler, and that is where he would be. I planned to split my time between thesis work and bomb work. It turned out that bomb work was dominant, but the beginnings of a thesis also percolated slowly at Los

Alamos. The applied work, aside from whatever merit it may have had in contributing to the H-bomb development, was a vitally important part of my overall training. I consider it to have been of enormous value as part of my education.

John Toll, another Princeton student, also accompanied Wheeler to Los Alamos. In Wheeler's home was a study equipped with three desks, and there, four or five evenings a week, John Wheeler, John Toll, and I worked on research problems apart from the laboratory work. During the day "Wheeler's boys" included, besides John Toll and me, Burton Freeman as a regular and sometimes one or two others. We started out five in an office. Fortunately, no one smoked. These were exciting times. The laboratory went on a six-day week. There was a sense of dedication and a sense of progress. The year 1950-51 at Los Alamos deserves a long history of its own, one that I fervently hope will be written as soon as the secrets of those days can be openly discussed (or even before, for later publication). In T-Division alone were a galaxy of major contributors--Ulam, Teller, Wheeler, Nordheim, DeHoffmann, Longmire, Rosenbluth (I think he arrived during this year) full time; Fermi, Bethe, Von Neumann part time; and Carson Mark as synthesizer and coordinator with the vast range of talent in other divisions all pointing to a single objective.

Wheeler's boys were a very able group of assistants and we greatly extended his powers of accomplishment at the same time that we learned a lot about the art of doing physics from him. Teller was a separate nucleus with Freddie DeHoffmann as his chief assistant.

It is tempting, indeed, at this point to discuss the work and the personalities and the spirit and the life at Los Alamos, but I will stick to my personal narrative here. In the summer of 1951 John Toll and I followed John Wheeler back to Princeton (I on a motorcycle) where we formed the core of one part of Project Matterhorn. The other part had a one-man core, Lyman Spitzer. It has since grown into a major enterprise. Our part grew rapidly to 20 or 30 people, flourished briefly until its job was finished, and then terminated. During 1951-52 I was deep in programming and numerical calculation, first at the primitive IBM card programmed calculator (CPC) in New York, then at the SEAC at the Bureau of Standards in Washington. There, for several months, we calculated from midnight to 8 AM six or seven nights a week. I was straw boss of this operation, directing the work of several people hired temporarily for this work in Washington, reporting results almost daily by telephone to Princeton, and never sleeping enough.

I phased out of this work in the summer of 1952 and moved back to Princeton to work full-time on a thesis. On November 1, 1952, the first thermonuclear explosion destroyed an atoll, in the Pacific. At Project Matterhorn we received a cablegram from Wheeler announcing success so cryptically that we were not completely sure until classified confirmation arrived. Matterhorn's work continued another year, but I made no further significant contribution to it.

When I got down to serious thesis work late in 1952, I began with a preprint from Aage Bohr discussing the dynamics of a single extra-shell nucleon in a deformable nucleus. I generalized this to several nucleons and was led at once to the idea of rotational states in even-even nuclei. I rushed to Brookhaven to discuss it with Gertrude Scharff-Goldhaber and then submitted a paper that was published in 1953. Then I pursued various other implications of the collective model for nuclear properties, and by the spring of 1953 had compiled a thick thesis. My Ph.D. was awarded in June, 1953. In the meantime, in Copenhagen, Aage Bohr and Ben Mottelson were pursuing the same lines and doing it better. (My thesis suffered in particular from the use of Condon-Shortley methods rather than Racah methods of spectroscopy.) The Bohr-Mottelson paper on the unified model became a classic in nuclear physics.

I gained in my thesis work some power and some research confidence, and had a head of steam for nuclear calculations that provided impetus for several years more. In my overall professional career, I look on the Los Alamos-Matterhorn interruption as a positive contribution to my development, not a self-sacrificing loss of time. Moreover, my own contribution was perhaps greater then than at any other time--certainly it was so per unit salary. In that intensive period of work, I earned less than $5,000 per year. In every subsequent job I have felt overpaid.

Career.

I went to Indiana University just after getting married in 1953. I went there because John Wheeler recommended it and because it was the first job offer I had. I could not have been treated better. I was promoted rapidly and was granted leaves in 55-56 and 57-58. My conscientiousness made it impossible for me not to devote a lot of time to teaching. I ran at once squarely into the problem of research vs. teaching. My reaction was to try to escape frequently to environments of pure research, yet it did not cross my mind--at least not seriously--to leave the university permanently. My first leave in 55-56 was to the Max Planck Institute in Göttingen as a Fulbright Fellow. My second in 57-58 was to Los Alamos where I spent most of my time on pure research.

In 1958 I treated Indiana particularly shabbily by accepting an offer from Brandeis while on leave, and rather late in the academic year. The youth, vitality, and eastern location of Brandeis were the attractions. Rank and salary were not factors. Again at Brandeis the mounting pressures of teaching plus now some administrative responsibilities made me seek a leave. I was fortunate to win an NSF Senior Postdoctoral Fellowship and in the fall of 1961 went to Imperial College in London. On my first leave to Göttingen, I had no student with me. On my second, in Los Alamos, I had one (Norman Glendenning). On my third, in London, I was accompanied by two students (Alfred Kaufman and Wayne Mills). In six months in London I wrote a paper predicting possible super-heavy almost bare particles and wrote a non-mathematical book on elementary particles. I had gone to London on the heels of a divorce and left two children behind. The elementary particle book has been much better received than I could have hoped. I don't know to what extent my combination of misery, freedom, and ill health in London contributed to its success. Most of it was written in lonely evenings and weekends in a state of considerable mental and physical discomfort. The paper on heavy particles made no stir and will surely be soon forgotten unless some such particles are discovered.

The London sojourn lasted six months instead of a year, the second half of the leave being spent at M.I.T. Three months after returning I was remarried. At M.I.T. I worked on Regge analysis of high-energy scattering but never brought the work to publishable conclusion. Since 1962 my rate of publication has dropped drastically, owing to deeper involvement in administration and book writing.

In 1963 I accepted the department chairmanship at Brandeis, a poor institution in which to hold such a job. In no other year had I ever had such a sense of hard work without accomplishment. By the spring of 1964 I was ripe for escape from the job. The offer to join a new campus of the University of California--even as a chairman--was irresistable. I have thrown myself into this job with enthusiasm. It is going well.

Even in this mobile society I have jumped around a good deal. Yet not at every opportunity. In the dozen years since my Ph.D., I have said no to one or two approaches per year. I do not anticipate moving again soon. The next few years will be crucial in my career, for they will determine whether I can get back on the track of regular productivity in research.

Research.

As mentioned before, I shall annotate my reprints instead of discussing them in this narrative. Here I shall mention only the broad lines of work. After pursuing nuclear physics until 1955, I felt a need to broaden my base and to tackle something more fundamental. In Göttingen I worked mostly on field theory, teaching myself with the help of the Lee Model. I returned mostly to nuclear physics and mu-mesonic atoms and scattering theory, pursuing field theory again when on leave in 1961-62. Somewhat by chance and somewhat on a lark, I joined an experimental search for magnetic monopoles in 1962 which led to some work on monopole theory. I shall probably concentrate next on phenomenological elementary particle theory, although it is easy for me to be lured in the direction of anything that looks truly fundamental if it is within my capabilities.

Teaching.

I am by no means a natural teacher, but I work at it and am probably above average among physicists. By now I have taught at most levels from non-science freshmen to second-year graduate. Teaching has been a constant frustration because I enjoy it but find it quite time-consuming.

My interest in teaching non-science freshmen at Brandeis led to my book-writing efforts. The book on elementary particles appeared in 1963. A general text for beginning students should appear in 1967.

Consulting.

My greatest contribution to practical goals by far was at Los Alamos and Project Matterhorn. In later years, I consulted for Lockheed Aircraft, Convair, Aeronutronic, and United Aircraft. These jobs were not highly satisfying because they were concerned with projects so visionary that one could get little sense of real achievement. A summer job in industry full-time for a month or two I find more rewarding than long-term consulting at the rate of one or two days per month. To me it is much more satisfying to do a job than to look over the shoulder and give advice to someone else doing the job.

Administration.

At Indiana, I, along with other junior department members, fretted because we had no voice in departmental affairs. This was a factor in my decision to leave Indiana. At Brandeis I gravitated year by year into successively more administrative responsibility, becoming director of summer institutes, department chairman, and computer center director. Now I am the chairman of a new and potentially outstanding department. My tendency always has been to say yes when asked to do something (such as write this narrative). Given that trait, my present position and the way I spend my time seems more the result of the inevitable flux of events than of conscious choice on my part. For me administration brings some satisfactions, but less than individual personal achievement.

On the one hand, I regret that competence in scientific administration wins no respect from the physics community. On the other hand, I myself have the most respect for physicists such as Eugene Feenberg and Leslie Foldy who have kept clear of politics, glamorous projects, and major administration, pursuing their research at a high level of competence year after year.

The Jobs of a Physicist.

Research is a full-time job. Not even the greatest physicists have achieved important results without dedicated hard work, and a lot of it. As a graduate student I had the opportunity to watch the Princeton physicists at work, also occasionally Bethe, Fermi, and Von Neumann. I reached the conclusion early that brilliance alone was not enough. These men worked hard. In my own experience there has been a definite threshold effect. Unless I devote very nearly full time to research, I accomplish little or nothing. (At the same time, I believe strongly in the importance of recreation and diversion. An hour of concentrated work plus an hour of play produce more than two hours of half-hearted work.)

Research and teaching are more than a full-time job. Nevertheless, they can be combined successfully because a man can work more than a full-time schedule, and most productive professors do. I succeeded at this for awhile. Research and teaching and administration are, in my opinion, more than anyone can do well. The physicist faced with all three should make his choice and not apologize for it. Still, he may regret his choice, as I do. It is impossible for me to decide what fraction of that regret comes from the loss of outside respect and what fraction from the loss of inner satisfaction. Both factors are present.

In my own career, I have been on the one hand single-mindedly devoted to my profession; on the other hand, constantly at war with it. The single-mindedness means that I measure the value of recreation in how it contributes to my ability to work; that I sacrifice the wishes and needs of others to the demands of my work; and that I am impatient with self-analysis and with discussions of unanswerable questions, preferring to get on with a specific job. I have known the keenest pleasure, too, in professional success--not rank or salary, but a calculation that works, a paper in print, a favorable book review, or kind words from students. At the same time, my war with myself has taken two forms; first, the doubts about my own ability to do significant research--do I really have the makings of a physicist at all?--and second, the war of priorities--how should I distribute my time over the many tasks always waiting? I do not expect this war ever to end. It is in my nature to struggle and to be dissatisfied with my achievements. Whether I shall ever make a major break away from the present track of my career is doubtful, but it always seems possible.

I consider my greatest contribution to society to have been in teaching, the next greatest in applied work for Los Alamos, with administration in third position, and pure research last. The order of relative satisfaction has been different. Research, both pure and applied, has been personally most rewarding, then teaching, and last administration.

I have omitted my immediate family almost entirely from this narrative, not because they, too, have not had an important bearing on my work, but because it is simply too difficult to write intelligently about it. I will save that for the second installment of this document, perhaps to be written in some future year.

Kenneth W. Ford
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