



Henry Eyring

MEN, MINES, AND MOLECULES

❖ 2636

Henry Eyring

Department of Chemistry, The University of Utah, Salt Lake City, Utah 84112

My ancestors were drawn together from northern Europe by the new Mormon religion. My mother's people, the Romneys and Cottams, migrated from around Preston, England, arriving in Nauvoo, Illinois in 1839. My grandfather, Henry Eyring, came from Coburg in Germany, and Grandmother Eyring came from German Switzerland. My Eyring grandparents met while crossing the plains and arrived in Salt Lake City in the same pioneer company in the fall of 1860. My mother's people had reached Salt Lake City 10 years earlier. During the next three decades, colonization of the intermountain area spread from Salt Lake City to Alberta on the North and to Chihuahua and Sonora on the South, wherever the water could be turned out onto the parched land. As a result of these migrations, all my grandparents ended up in the late 1880s in Colonia, Juarez, in northern Mexico, about 100 miles straight south of Columbus, New Mexico.

This part of Chihuahua lies in the foothills of the Sierra Madre Mountains at 5000-6000 feet of elevation in an excellent region for raising cattle. At that time Don Luis Terrasas owned about a third of the state of Chihuahua and had one of the largest herds of cattle in the world. My father owned the Tenaja ranch of 10,000 fenced acres where he raised high-grade cattle and had about 4000 acres of pasture and farmland nearer town where he kept 50-100 head of horses. The 10,000-acre ranch seven miles from town pastured about 600 head of shorthorn Durham cattle. The bull calves were sold to surrounding ranchers to upgrade the herds of Spanish longhorn cattle.

I was born into this well-to-do family in 1901 in Colonia Juarez. I was the third child and the first son. I have no memory of learning to ride, since I was riding as soon as my legs were long enough to straddle a horse. My earliest memories are of my father coming home from the ranch, putting me on his horse after the horse had been unsaddled, and leading him to the river to drink. Mother told a story of such an occasion when she accompanied us to the river. After drinking, the horse shook himself the way a horse wet with sweat often does, and I tumbled off into the river. After being fished out of the river, my first remark (as reported by my mother) was, "Put me back on the horse." This was at about the age of three.

At four I almost died of typhoid fever. Our prosperous little town had electric lights and telephones, but the clear water from the river was piped directly into our homes with no previous purification. After the first cases of typhoid appeared, the

practice was to boil the drinking water, but this was rather a case of locking the barn after the horse was stolen. One of my vivid memories of the spring of 1905 is of the magnolias in our front yard in full bloom while I was recovering from typhoid.

At about this time my father gave me two small goats, but they soon became a nuisance by getting into the neighbor's lot. My father's way of getting rid of them while leaving me contented was interesting. One day he came riding into the yard leading a pretty, little sorrel horse and asked me if I would be willing to trade my two goats for the sorrel horse. I was delighted with the proposition. My goats disappeared, and I started riding my sorrel horse, which was named Chivo, the Spanish word for goat. However, Chivo turned out to be too much horse for a small boy to handle, so Father traded me a dun horse, Grullo, for Chivo, and finally a pretty, little, black horse for Grullo. The black horse was called the Spanish equivalent of "black baby" and was the horse that I rode the most until we left Mexico in 1912. We left Mexico because of the unrest accompanying the Mexican revolution, which started in 1910. About 4800 colonists from our area left Mexico at this time. This migration to El Paso, Texas, occurred within about a week in mid-July 1912. Since everyone expected to return within a short time, everything was left behind except the requirements for a few weeks' stay. However, we, like most of the colonists, never returned as a result of the continued unrest. Our large, refugee family was thus suddenly transformed from affluence to humble circumstances. My first job in Calisher's department store paid \$2 for a six-day week. At that time five cents would buy a loaf of bread or a quart of milk. After a year in El Paso waiting for the Mexican situation to improve and a second year spent in Safford and Thatcher, Arizona, we moved to nearby Pima in southeastern Arizona, where Father had purchased a farm by making a small down payment. Part of the farm was under cultivation. Brush was cleared from the rest by hitching two teams, one to each end of a steel rail, and dragging it across the land after the mesquite had been dug up with ax, pick, and shovel. This involved a lot of hard work, but we were all healthy and anxious to get on our feet again.

I had finished the fifth grade in Mexico, missed a year's schooling in El Paso, and graduated from the eighth grade in Pima in 1914, having skipped the first and seventh grades.

My high school had an important bearing on my subsequent career. I went to a church academy in Thatcher, Arizona, six miles from Pima. My predilection for mathematics and the sciences showed up early. My science teacher, Alma Sessions, who had been a star basketball player at the University of Arizona, and was much admired, gave me career advice. He said I should study either electrical or mining engineering at the University of Arizona. I chose mining as the less hazardous. A \$500 state fellowship, won in the competition in Graham County, launched my mining career at the University of Arizona.

My father's advice upon departure for the university made a strong impression on me. He said the constraints our religion placed on one are "to be dedicated to the truth, wherever one finds it, and to live in such a way as to make one comfortable in the company of good people." This advice has had an appeal for me that has lasted through the years.

Arriving at the university in 1919, the same time as the veterans from the First World War were returning to school, made this an interesting time. The first day at the university, I saw President Rufus von Kleinschmidt driving his carriage, drawn by two well-matched black horses, through the campus grounds. This was still the customary way to travel. Hazing was also still in full swing. One resisted it, but the battle was often lost. The war veterans in the freshman class added a disciplined resistance to hazing which I found refreshing. I found it distasteful that, as a freshman, I was expected to wear a green "beanie." The sophomores would throw freshmen not wearing their beanies into the pool. However, it was against the rules to carry hazing into the dormitories. Since I thought I could outrun my tormentors, I left the dormitory without my beanie, believing they would have to quit the pursuit when I reached the door, or else they would be in trouble themselves. It was a miscalculation. I beat them to the dormitory and then to my room where I slammed the door, but they kept coming. Since my door was now locked, they tried to climb in through the transom, but I blocked their entry by working on their hands with a broom handle. By this time, they were annoyed and threatened to break down my door if I did not come out. I was pretty sure I would be paying for the door if they broke it down, so I came out. There were plenty of sophomores, and I was soon face down in the air with someone holding on to each arm and leg and another fellow enthusiastically swinging a large wooden paddle where it would do the most good. This did not engender love for authority in me, but it did engender respect for it when it is backed up by sufficient "lynch law."

The four years I spent studying mining at the university were very pleasant. I made my own way by assisting in classes and waiting on tables. I was able to send a little money home to help with payments on the farm. I enjoyed my studies and made high grades. I particularly liked mathematics and wrote a senior thesis with Professor Cressy of the mathematics department on the theory of the aerial tramway.

The summer after my junior year I worked as a miner in the Inspiration Copper Company in Miami, Arizona. The work was interesting and paid about twice as much as farming. Since I was a prospective mining engineer, they changed me rapidly from one job to another to give me added experience. After I had been underground for only a few weeks, I was given the job of timberman, with a man assigned as my helper who had been mining for 15 years.

The caving system used in the mine was economical, but extremely hazardous. One half of a square mile of rock would be blown up at one time and drained by gravity down raises (tunnels) into square sets where a man would be stationed to regulate the rate of flow to a "grizzly" 20 feet below. At the grizzly, a second man would break up the larger boulders with a sledgehammer, or with dynamite, until the fragments passed between the six steel rails of the grizzly, spaced about 10 inches apart. The ore then continued its downward course another 20 feet, where it was drained into cars of a train driven by compressed air. The train carried the ores to a tippie which turned the cars upside down, dumping the ore into a bin. The ore was drained out of the bin into elevators that carried it to the surface, where trains carried it to the mill. Fifteen thousand tons of ore and gangue would be taken from the mine in three eight-hour shifts. There were many separate operations paralleling

the one just described. On one memorable occasion a shift boss assigned my helper and me to repair one of the square sets, which had been all but destroyed by the tumbling boulders that had passed through it. We climbed up the raise to inspect the square set, and my veteran helper declared that he had no intention of being trapped in this particular hell hole and, if it was to be repaired, it was up to me. He would wait down at the grizzly, and I could call when and if I needed help. I had the usual ax and crowbar with which I could gingerly pry out the loose rocks until I had cleared enough space to put in new timber before it all caved in on me. I went about it as carefully as I could, but it was not long before a rock somewhat bigger than my head fell from the ceiling and hit my boot. I soon had a boot partly filled with blood, and I was taken out of the square set and brought up on top. When I returned to the mine some days later, I was assigned to another part of the mine.

I was once on another shift where there were three separate, fatal accidents. There were two other boys from Pima working in the mine that summer, and one of these had his arm crushed and later amputated. This accident occurred as he was driving his train onto the tippie. The customary indemnity paid to the family of a deceased miner was \$6000. This rapid method of mining born of wartime and economic needs has been replaced by the much safer opencut mining of today.

Although as a mining engineer I would not personally have to take these risks much longer, I would still have to send others to take them. Therefore, I reluctantly decided to graduate in mining but planned to change over to metallurgy after receiving my degree. The next summer, 1923, my younger brother, Edward, and I worked together as miners at Sacramento Hill in Bisbee, Arizona, for the Phelps Dodge Corporation. This work was interesting and a much safer type of operation than the caving system since it involved driving drifts into undisturbed terrain. This summer ended pleasantly, terminating my active mining career. In the fall I began my thesis on *The Separation of Heavy Sulfide Ores by Selective Flotation*. I had a Bureau of Mines fellowship and worked under the direction of Thomas Chapman, professor of metallurgy, at the University of Arizona.

After completing my master's degree, I spent the summer of 1924 working in the United Verde smelter at Clarksdale, Arizona. Again I had a favored position as a prospective metallurgist and was rapidly shifted among different phases of the smelting operation. After being there a few weeks I was assigned to take samples from the blast furnaces. The sulfur dioxide smoke was especially strong, and I was holding a handkerchief soaked in baking soda over my face when the smelter superintendent came by, slapped me on the shoulder, and said, "Eyring, I plan to put you in charge of the blast furnaces in a few weeks." The problems were intriguing, but the sulfur smoke made it easy for me to return to the University of Arizona as a chemistry instructor.

In 1925, toward the end of the teaching year, Professor Theophyl Buehrer recommended me to Berkeley, where he had taken his Ph.D. Dr. Lathrop E. Roberts recommended me to the University of Chicago, where he had worked under Harkins. I accepted the invitation to Berkeley, where I went in August 1925 and finished my Ph.D. in June 1927. This was a stimulating experience. The emphasis was on research and, since I was no stranger to work, everything went well. I started on a problem

involving the lowering of freezing points with Professor Merle Randall, but because Dr. Vanselow needed the equipment longer than expected to finish his Ph.D., I changed to working on an exciting problem with Professor George E. Gibson. A long vacuum tube filled with hydrogen at low pressure was bombarded with an 11 MV high-frequency discharge from a Tesla coil. The protons were expected to pass through a thin aluminum window at the bottom of the tube and strike a beryllium target, which we hoped would emit interesting radiation. Unfortunately, one got lots of unspecified radiation and a frequently punctured, evacuated tube. Nevertheless, I had gained useful experience from this study. We next turned to the study of the amount of ionization, the stopping power, and the straggling of alpha particles from polonium in various gases. We found that stopping power depends very little on how the atoms are bound together, but the total number of ions formed depends very much on the bonds being broken. This is readily understandable since the primary ionization induced by the alpha particles involves a large energy transfer to the electron being ionized, with the result that the fraction of energy lost by the alpha particle as a result of a difference in the molecular bonding of the atoms is negligible. Conversely, in the ionization by the fast secondary electrons only about half of the energy transferred from the secondary electrons goes into ionization, so that differences in the bonding energy are an appreciable part of the total energy expended in ionization.

In the fall of 1927, as a brand new Berkeley Ph.D., I became an instructor at the University of Wisconsin. During my first year there, I continued my research on ionization, stopping power, and straggling of alpha particles in different gases. The second year I took a full-time experimental research position with Professor Farrington Daniels, studying the decomposition of N_2O_5 in a wide variety of solvents. This was the beginning of my active interest in reaction kinetics, which has continued unabated.

In 1929 I was granted a national research fellowship to work with Professor Bodenstein in the University of Berlin. However, before my expected date of departure, I received word that Professor Bodenstein was going to be at Princeton at the dedication of the new Frick chemical laboratory. Professor Frumkin, who was visiting the University of Wisconsin at the time, suggested that, in view of Bodenstein's absence, I should work with Professor Michael Polanyi at the Kaiser Wilhelm Institute in Berlin. Acting on this advice, my wife, Mildred Bennion, and I sailed to Europe by way of Bergen and Oslo, Norway, through southern Sweden, to Copenhagen and Berlin. Arriving in Berlin, we found Professor Polanyi had likewise gone to the Frick dedication. Just at this time Bonhöffer and Harteck were front page news with their study of the rate of conversion of *para*- to *ortho*-hydrogen. Fritz Haber was directing his laboratory effectively. Workers in Haber's laboratory included Fritz London, Eugene Wigner, the Farkas brothers, and Hubert Alyea, along with many others. We had not been in Berlin long before I was visited by Professor Robbins from the Paris branch office, who was responsible for the National Research fellows. He was greatly disturbed to find that neither Professor Bodenstein nor Polanyi was in Berlin to greet me. Professor Polanyi, however, returned soon thereafter.

My first research was the study of light-emitting reactions. A sodium vapor jet meeting a jet of chlorine precipitated NaCl with the emission of a bright light. Spectroscopic examination of the light told the story behind the mechanism. Although we were intrigued by this subject, Professor Polanyi and I turned our attention to the construction of a potential surface for the reaction $\text{H}_2(\textit{para}) + \text{H} = \text{H}_2(\textit{ortho}) + \text{H}$. We made use of Fritz London's approximate equation for the potential energy as a function of the distance between the atoms. This involved using the Heitler-London-Sugiura exchange and coulombic integrals for the energy of attraction between atomic pairs. The results were disappointing. We then changed to a spectroscopic estimation of the attraction between pairs of atoms using Morse curves. The theoretical calculations gave us the needed guidance in apportioning the bonding energy between the exchange and coulombic integrals. This way we got an exciting, if only approximate, potential surface and with it gained entrance into a whole new world of chemistry, experiencing all the enthusiasm such a vista inspired. We perceived immediately the role of zero-point energy in reaction kinetics, and our method of using Morse curves made it possible to extend our calculations to all kinds of reactions. I continued this work with enthusiasm whenever opportunity permitted.

I received a disturbing letter from my father early during my stay in Berlin. He informed me that, contrary to the opinion of lawyers whom he had consulted when we returned to the United States, the State Department had ruled that my younger brother, Joseph, was a Mexican citizen. This meant that I, too, was a Mexican citizen. On that basis I held a passport and fellowship to which I was not entitled, and I had voted for Hoover. I spent a very restless night, to say the least. Early the next morning, I consulted the American consul, who ruled that I had acted in good faith and should continue my stay in Berlin. He recommended that when I returned to the United States, I should turn in my passport, at which time I could proceed with naturalization. This I did. Five years later, I took out my naturalization papers with a judge in Trenton, New Jersey, who had just recently naturalized Professor Einstein. I was then on the faculty at Princeton, and the judge was so interested in telling me about his earlier experience with Professor Einstein that I passed the tests without difficulty.

My year with Professor Polanyi was fruitful and altogether delightful. He was a very gracious and gifted human being. Toward the end of our stay in Berlin, Professor Wendel Latimer of the University of California at Berkeley visited me and, upon hearing of our involvement in applications of quantum mechanics to chemistry, suggested to Professor G. N. Lewis that I be invited back to Berkeley for a year as lecturer, to take over some of the duties that Professor Hildebrand's impending absence would leave open. As a result of Latimer's proposal, Mildred and I found ourselves back in Berkeley at the beginning of the fall quarter of 1930.

This was an exciting year. My duties at Berkeley were not heavy, so I was able to develop further the quantum mechanical attack on reaction kinetics. Our oldest son, Edward, was born in Oakland on January 7, 1931. By spring I had a paper to present at the Indianapolis meeting of the American Chemical Society, using potential surfaces to explain why iodine was the only halogen-hydrogen reaction to involve four atoms in the activated complex, while the other reactions all went by three-atom

complexes. The paper engendered much excitement, and Professor Hugh Taylor invited me on the spot to go to Princeton and present two lectures on quantum mechanical calculations of reaction rates. This visit led to an invitation to go to Princeton, which was the beginning of an exciting fifteen years to be spent in what turned out to be an ideal scientific environment. Hugh Taylor was an inspiring departmental chairman, incisive in chemical discussions and always generous in his encouragement of others.

It will not be possible to speak of more than a few of my 560 scientific papers and 9 books, but a few highlights may be interesting. Twenty years as editor of the *Annual Review of Physical Chemistry* were useful in keeping me conversant with the field. Also valuable was my coeditorship, with Douglas Henderson and Wilhelm Jost, of the 14 volumes of *Physical Chemistry: An Advanced Treatise*. Douglas Henderson and I are continuing to edit volumes entitled *Theoretical Chemistry: Advances and Perspectives*.

My second paper at Princeton was inspired by Professor Charles P. Smyth's suggestion that it would be useful to calculate the effective resultant dipole for a molecule having various dipoles lying along bonds that rotate with respect to each other. This is also essentially the same problem as calculating the distance between ends of a flexible chain, since lengths also lie along bonds. The procedure adopted was to choose coordinate systems such that the origins were at successive atoms along a chain, and the x axis coincided with the bonds of interest. One could then transform all coordinates to an initial set of coordinates and add vectorially the transformed dipoles, or lengths, in this final coordinate system. The rotations around axes could be appropriately averaged according to the potential energies of rotation. The procedure has since been extensively used by others for high polymers, both in calculating the mean lengths of a chain and in evaluating partition functions in thermodynamic calculations.

The task of developing methods of calculating potential energy surfaces for more than the four electrons treated by Fritz London was straightforward but tedious. In the April 1933 issue of the *Journal of Chemical Physics*, George Kimball and I presented a quick way of getting the secular equation for any number of electrons. This, together with the use of the Morse equation, enabled us to construct approximate potential surfaces for any molecular system of interest. It is interesting that, in the same issue, Pauling published a parallel procedure for getting the secular equation for any number of electrons. At this time George Kimball and I tried using difference equations to send a wave packet over the $H_2 + D = HD + H$ surface. This effort was premature. Joseph Hirschfelder carried out the first classical trajectory calculation in his doctoral thesis.

In 1932 Bethe's group theory treatment of orbitals in fields of different symmetry caught my attention. Arthur Frost, John Turkevich, and I incorporated group theory into our bond eigenfunction solution of the methane problem. From then on, group theory became standard procedure with us and naturally found its way, as a chapter, into the book, *Quantum Chemistry*, by Eyring, Walter, and Kimball. Interestingly enough, a contract for the book was signed in 1933. Publication, however, must always await completion of the manuscript, and the book did not come out until 1944. (Not unseemly haste!) This book has had wide use, there

having been over 20 printings of the unrevised first edition and a considerable number of translations into other languages.

Our second son, Henry Bennion, was born in Princeton on May 31, 1933. At about this time I was invited to participate in a symposium on molecular quantum mechanics held by the Physical Society, over which Niels Bohr presided. The three other participants were Mulliken, Pauling, and Slater. The audience was most attentive. I was immersed in all kinds of approximate quantum mechanical calculations that could be carried out at that time, without computers. Papers on the relative rates of isotopic reactions followed naturally from my first paper with Polanyi and from experiments with Professor Hugh Taylor. Very early in our experiments, Taylor and I prepared large amounts of heavy water by electrolysis. We found that 92% heavy water proved fatal to tadpoles of the green frog *Rana clamitans*, the aquarium fish *Lebistes reticulatus*, flatworms (*Planaria maculata*), and the protozoan, *Paramecium caudatum*. This work was reported by Taylor, Swingle, Eyring, and Frost in October 1933. During this period, my associates and I were investigating potential energy surfaces for a wide variety of reactions. In November 1934 I submitted my paper on "The Activated Complex in Chemical Reactions" to the *Journal of Chemical Physics*. I showed that rates could be calculated using quantum mechanics for the potential surface, the theory of small vibrations to calculate the normal modes, and statistical mechanics to calculate the concentration and rate of crossing the potential energy barrier. This procedure provided the detailed picture of the way reactions proceed that still dominates the field.

The activated complex has a fleeting existence of only about 10^{-13} sec and is situated at the point of no return or of almost no return. It is much like any other molecule except that it has an internal translational degree of freedom and is flying apart. This concept describes any elementary reaction involving the crossing of a potential barrier. If the activated state is really a point of no return, there is no perturbation of the forward rate by the backward rate, so that the rate at equilibrium applies unchanged to the rate away from equilibrium. For thin barriers, as in the inversion of ammonia, leakage through the barrier is faster than the rate of surmounting the barrier and so must be taken into account for cases involving light atoms and thin barriers. Of the nine books I have coauthored, all but *Quantum Chemistry* and *The Theory of Optical Activity* with Dennis Caldwell involve reaction rates. They deal with such diverse subjects as rates in plastic deformation, liquids, gases, solids, biology, physics, and engineering.

In 1937, E. U. Condon, W. Altar, and I, working at Princeton, published our paper on one-electron optical activity. This was precipitated by the general belief, current then, that a one-electron transition, even in a dissymmetric field, would not contribute significantly to optical activity. Werner Kuhn had shown earlier that Drude's calculation of the optical activity of an electron moving in a spiral neglected a term that reduced the outcome for his model to zero. The result was that Kuhn, Max Born, and other workers in the field adopted the coupled oscillator model as the sole source of optical activity. This seemed unrealistic to us in view of the success of treating spectroscopy as approximately due to one-electron transitions. Adding the simplest perturbing potential that would give optical activity, A_{xyz} , to the

potential $k_1x^2 + k_2y^2 + k_3z^2$, a system which could be readily solved, we found one-electron transitions were indeed optically active for reasonable values of A if the k 's were unequal. This treatment gave rise to what has later been popularized as the octant rule. Interestingly enough, theory showed that this one-electron optical activity also persists in the classical limit. These considerations were extended in my work with Walter Kauzmann, John Walter, Daniel Miles, Dennis Caldwell, and many others. This interest has evolved into a major concern of ours with absorption spectra, circular dichroism, magnetic circular dichroism, and, not surprisingly, nuclear magnetic resonance. These tools are of course invaluable in establishing molecular structure.

In the autumn of 1937 I went with my wife, Mildred, and two oldest sons for a four-month visit to Manchester, England, where Professor Polanyi headed the chemistry department. This visit was prompted by invitations to address the Faraday Society and the Chemical Society, and included lectures at various English universities. The trip included memorable envelopments in the famous English fog, but was altogether delightful.

Returning to Princeton in December, I continued my active involvement in research and teaching. Our third son, Harden Romney, was born in Princeton on August 20, 1939.

In early 1942, I was talking to Professor Newton Harvey, head of the biology department at Princeton, about problems of shock brought on by broken bones when Professor Frank Johnson came into the room. Harvey immediately began to discuss the problem that Johnson, Brown, and Marsland had encountered. They had been studying bioluminescence of bacteria as affected by temperature and pressure. Bioluminescence was a field in which Harvey was preeminent. Bioluminescence is absent at ice temperature and becomes maximal about halfway to blood temperature. It then drops to a very low value at blood temperature due to inactivation of the enzyme at higher temperatures. The problem that interested Johnson could be stated as follows: Why, when the bacteria in a suitable solution are subjected to 200 atm pressure, is there a marked decrease in luminescence in the low-temperature range with a rise of luminescence in the high-temperature range? Since, as Braun and Le Chatelier pointed out long ago, increased pressure shifts an equilibrated system toward lower volumes, and since the activated complex is in equilibrium with reactants, the luminescent response must follow the same laws governing other equilibria. Hence, the reactants are less voluminous than the activated complex in the low-temperature range, and the reverse is true in the high-temperature range.

Soon after this discussion John Magee and I published a paper that quantitatively explained this behavior. This discussion started a collaborative investigation with Frank Johnson into biological reactions that has continued and eventuated in our writing two books dealing with reactions important in biology and medicine. Study of the pressure, temperature, and narcotic effects on bioluminescence has been a powerful tool in understanding many physiological problems.

Prior to the Second World War, I was able to show that gases that penetrated gas masks more readily than those used in the First World War were not to be expected. This conclusion derived from the study of various freshly prepared and aged smokes

with the new electron microscope available in Camden, New Jersey. Fairly frequent conversations with Dr. Irving Langmuir at this time revealed that his was a brilliant, determined mind uncompromising on principle.

During the war I worked with the Navy as a consultant on high explosives. Dr. Stephen Brunauer was in charge of high-explosive research for the Navy and suggested that we consult with Professor Einstein, who also lived in Princeton. After a very pleasant morning in discussion with Professor Einstein at the Institute for Advanced Study, we walked together at noon through what had been a rose garden but was now planted with a field crop. I plucked a sprig and asked Professor Einstein what it was. He did not know. We walked a little farther and encountered the gardener sitting on his wheelbarrow. His reply to the same query was, "It is soybeans." Even for a first-rate mind, what gains attention is not just propinquity but interest. Professor Einstein's mind was too busy with more important things. Einstein's manner was never ostentatious but, indeed, on the very infrequent occasions when I talked to him, always kindly.

I also talked frequently with Professor Hugh Taylor about the nickel barriers to be used for separation of uranium isotopes, and I occasionally discussed the properties of uranium with Professor Wigner. I must confess that I did not expect the atom bomb to materialize soon enough to influence the war's outcome.

During the war the study of detonations took up a lot of my time and eventuated in a 112-page report entitled *The Stability of Detonations*, written with Richard Powell, George Duffey, and Ransom Parlin. The article was published in the August 1946 issue of *Chemical Reviews*. The curved front of the detonation was related to the diameter of the cylindrical explosive and the thickness of the reaction zone behind the wave front. The curved front theory, which established these relationships, is still widely used. The fact that the individual solid particles in an explosive burn from the outside, in layer after layer, accounts for the slow burning rate. The consequent outward burning of layer after layer of the bubbles in liquid explosives gives this same type of delayed reaction. This is a case where reaction rate is slowed down by delay in heat conduction and is to be expected, generally, at very high temperatures.

In 1944 I was asked by Professor Taylor to head up research for the Textile Research Institute, which was coming to Princeton. The position was vacant because Dr. Milton Harris, who had made distinguished contributions in this field, had accepted a position in industry. The position carried with it the responsibility for appointing 15 fellows who would work for their Ph.D. at Princeton University, and I would have Professor Eugene Pacsu, Professor John Whitwell, and Dr. Robert Rundell as my associates. This activity required 40% of my time; the remaining 60% was devoted to my regular duties at Princeton. This was a rewarding experience and drew on my engineering as well as my chemical experience.

The Institute was housed on the shore of Carnegie Lake, with extensive grounds whose graveled roads were never meant to carry the increased traffic. As a result, we would receive an occasional call for help from a stalled motorist, which we would answer with enthusiasm and dispatch, even though the consequences were often detrimental to our shoes and trousers. I enjoyed sharing in these rescues as much as my fellow workers did.

The study of spinning, weaving, dyeing, and the measurement of the physical properties of fibers and fabrics involved us in new problems of physical chemistry. My active interest in deformation kinetics dates from this time. Professor A. S. Krausz, of the University of Ottawa, and I coauthored the book, *Deformation Kinetics*, which was published in 1975. George Halsey made notable contributions to the study of the physical properties of fibers, as did Howard White and many others. I carried on parallel work with Arthur Tobolsky and others in the chemistry department. I became a member of the National Academy of Sciences in 1945.

In the spring of 1946, Ray Olpin, newly installed president of the University of Utah, visited us in Princeton to offer me the job of dean of the School of Mines or of the graduate school. My wife said that I should be the one to decide what we should do. Since at Princeton I had nine graduate students working with me, had been a professor since 1938, and had what was considered a high salary at that time, the choice was difficult. At Utah I would have to start the doctoral program and build up my own research program. As a result I decided not to go and wrote a letter to President Olpin to that effect. The next day Mildred asked what decision I had made. I told her, and she was crushed. We had lived away from her family and her Salt Lake mountains for 19 years. Although Princeton had been most pleasant, she wanted to go home. She said nothing at the time I told her of my decision, but prepared a nice letter that she asked me to read when I got to the university. When I realized her need to return home, I naturally agreed.

I immediately told Professor Taylor of the change in plan. His response was, "I told President Dodds the storm wasn't over. We have more money than the University of Utah. What do you want?"

"Nothing," I replied. "We're going."

He asked, "Do you want the Jones Professorship?"

"No. My wife wants to go, and we are going."

"One can't do business with a crazy man," he exclaimed. "Do you mind if I talk with your wife?"

"Help yourself," I said.

He and Mildred were well acquainted, and they had a pleasant conversation, but nothing was changed. So in August 1946 we started our new adventure at Utah, where I was dean of the graduate school and professor of chemistry.

Although the University of Utah had not granted Ph.D.'s before my arrival as dean of the graduate school in 1946, the transition went smoothly since the university already had a strong master's program. I found that two general administrative policies are possible: One involves strict administration from the top, the other encourages individual initiative as long as it is successful. When appropriate procedures are well understood, most decisions can be made at the departmental level, and routine decisions can be made by capable secretaries in the dean's office. Following such a policy of decentralization left me free to devote the required time to administration without seriously curtailing my research during the 20 years I served as graduate dean.

One of my first graduate students at Utah was Tracy Hall, who, after studying chromium complexes at Utah, went on to solve the problem of making diamonds at General Electric in Schenectady. Bruno Zvolinski, who had commenced his work

on the transmission coefficient of reaction kinetics at Princeton, finished up at Utah and got his degree from Princeton. Thirty-five of my collaborators completed their doctorates at Princeton and over a hundred at Utah.

Most of the avenues of research I began at Princeton, as well as new ones, were carried out at Utah. In 1935 it was clear to me that the law of rectilinear diameters was understandable in terms of fluidized vacancies in liquids that mimic the behavior of molecules in the vapor both in concentration and behavior. That this is reasonable follows from the fact that to form a molecular-sized vacancy without vaporizing a molecule requires the same number of broken bonds as volatilizing a molecule without leaving a vacancy. Accordingly, the heat required to form a vacancy is the same as the heat required to vaporize a molecule. Further, molecules falling domino-like into a vacancy move the vacancy much as a gas molecule moves. Thus, the fluidized vacancy converts three molecular vibrations into translations. A molecule behaves like a gas for the fraction of the time, $V - V_s/V$, in which there is no neighbor to prevent its free fall. The remaining fraction of the time, V_s/V , it behaves like a solid. Here, V_s and V are the molal volume of the solid at the melting point and the volume of the liquid, respectively. These considerations led to the development, with Professors T. Ree and N. Hirai, of the significant structure theory of liquids, which has evolved into a quantitative treatment of the thermodynamic and transport properties of many types of liquids. Since a liquid is intermediate between solid and vapor, it seems natural to treat it as a mixture of solid-like clusters intermingled with vapor. A quantitative description of a liquid in terms of solid and vapor properties actually rests on first principles insofar as solids and liquids can be so described. The result is a single partition function for solid, liquid, and vapor.

My wife, Mildred, died of cancer June 25, 1969 after a lingering illness that she endured gracefully with a serene faith in happier times to come. During the five years and four major operations spent fighting cancer, we sought help in every quarter where there seemed any prospect of curbing the disease. The most that could be accomplished was to slow, somewhat, its relentless course.

In 1970 Dr. Betsey Jones Stover came to me with some of the results of radiating beagles with radium and plutonium, which caused them to die of bone cancer. She pointed out that the survival curve reminded her of the curve for Fermi-Dirac statistics. The equation that fitted her data for the fraction of a population surviving, S , plotted against the age, t , is

$$S = [1 + \exp k(t - \tau)]^{-1}. \quad 1.$$

The death rate, $-dS/dt$, is given by

$$-\frac{dS}{dt} = kS(1 - S) = \frac{d(1 - S)}{dt}. \quad 2.$$

The fraction not surviving is, of course, $(1 - S)$. From Equation 1 we see τ is the age at which half of the population still survives. From Equation 2 we see also that $-k/4$ is the slope of Equation 1 at age $t = \tau$, where $S = \frac{1}{2}$. Chemists will recognize that the equations for S are those for an autocatalytic reaction. If S is the fraction of healthy cells and $(1 - S)$ the fraction of sick ones, one would expect Equation 2

to represent the rate of spread of the disorder as it likewise should represent the growth of any ecological population.

Dr. Stover and I also pointed out that under certain conditions Equation 2 would also represent the rate of mutation. This would be true if the rate of mutation were proportional to the product $S(1 - S)$, where S is the probability one gene is undamaged and $(1 - S)$ is the probability that a neighboring gene is damaged. By use of absolute reaction rate theory, a meaning was given to the parameters k and τ . The last chapter in *The Theory of Rate Processes in Biology and Medicine*, which I coauthored with Frank Johnson and Betsey Stover, develops these considerations further.

In 1971 I married Winifred Brennan and added her daughters Eleanor, Patricia, Joan, and Bernice to our family. This has been a rewarding experience. At the time of writing, January 1977, I still have my regular professorship at the University of Utah and am enjoying excellent health. My research and teaching are going ahead at an undiminished rate with almost 20 collaborators.

As I look back over my efforts, I would characterize my contributions as being largely in the realm of model building. To test a model it is usually advantageous to cast it in mathematical form so that quantitative predictions can be used to compare calculations with experimental findings. Ideally, agreement should be quantitative and complete. Unfortunately, this never happens in the real world. Even Newtonian mechanics must be amended in the realms of relativity and quantum mechanics, and Maxwell's electromagnetic theory fails to predict stationary electronic orbits. The usual statement that the entropy always increases is not mended very successfully by Boltzmann's proposal of known theories of fluctuations. The observed cosmological departures from equilibrium boggle the imagination when considered in terms of fluctuation theory. A better statement of the second law of thermodynamics would seem to be that living things never exist in an environment where there are large decreases in the total entropy. This statement seems to include all we really know about entropy. We still need to find the gigantic Maxwellian demon that winds up worlds and consequently exists.

In model building it is convenient to start out with the following hypotheses:

- (a) There is always a model that will explain any related set of bonafide experiments.
- (b) Models should start out simple and definite enough that predictions can be made.
- (c) A model is of limited value except as it correlates a substantial body of observable material.
- (d) Models that suggest important new experiments, even if the theory must be modified, can be useful.

To be a Newton or a Maxwell it is very convenient to be stimulated by a Kepler or a Faraday, but if one were gifted enough he could still be a Gibbs with very little outside interaction.

Self-analysis is always hazardous but can be amusing. I perceive myself as rather uninhibited, with a certain mathematical facility and more interest in the broad aspects of a problem than the delicate nuances. I am more interested in discovering what is over the next rise than in assiduously cultivating the beautiful garden close at hand. In any event, the study of chemistry is still both exciting and rewarding to me.