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THE MEMOIRS OF SAMUEL COLVILLE LIND

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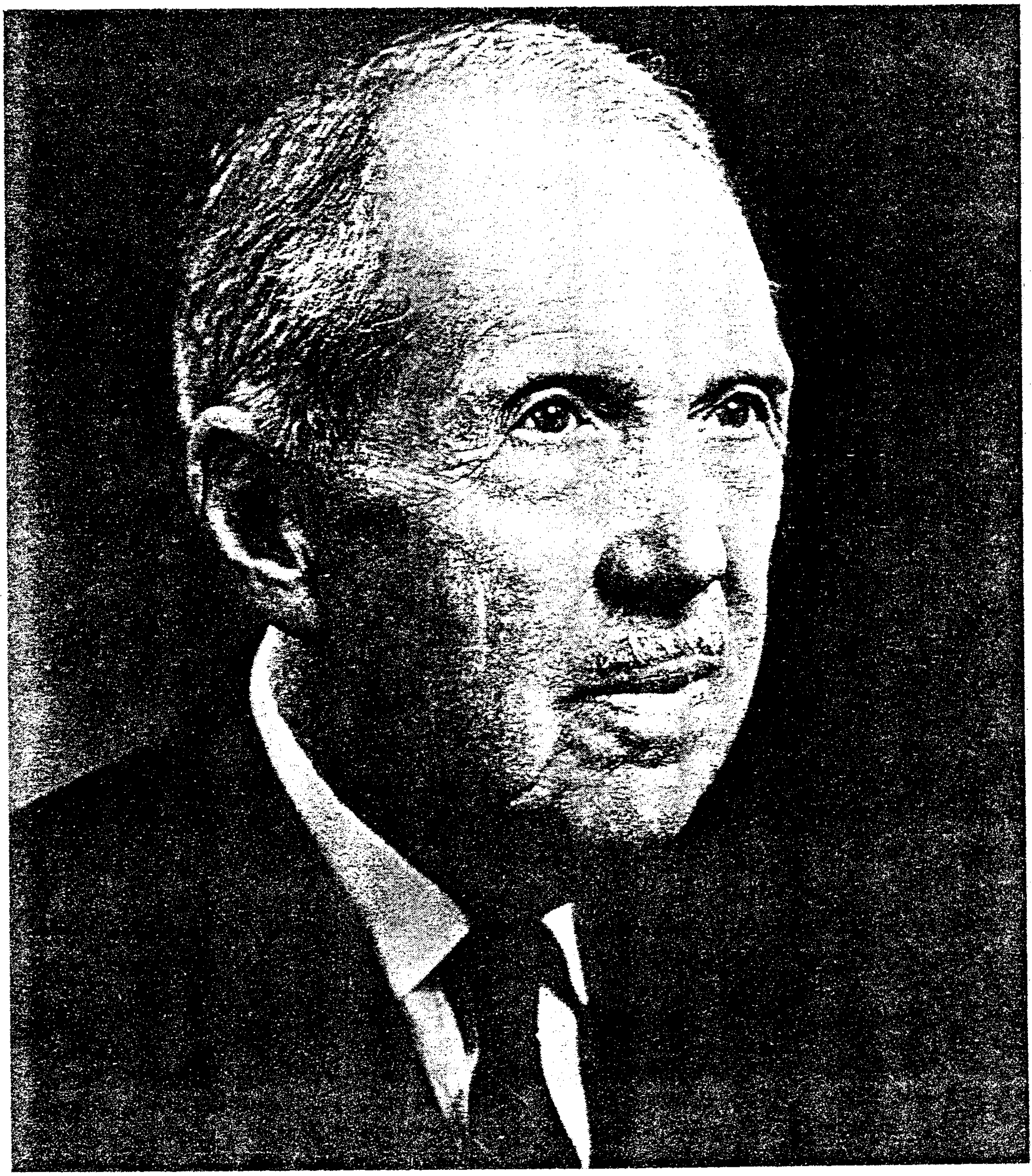

SAMUEL COLVILLE LIND

THE MEMOIRS OF SAMUEL COLVILLE LIND

PREFACE

Samuel Colville Lind, the only native Tennessean to be elected to the National Academy of Sciences, was until 1957 the only member of that august body to reside in Tennessee. Thus, it seems appropriate that the Tennessee Academy of Science publish the Memoirs of Tennessee's most famous scientist and one of this Academy's most eminent members.

Lind led a full, productive, happy, and healthy life. The patience and understanding of his gracious and charming wife contributed to the attainment of his high stature and recognition. On January 24, 1965, they celebrated their golden wedding anniversary in Oak Ridge, where the indomitable Mrs. Lind still resides.

During his long fruitful life, Lind had many interesting experiences. He recounts some of them in his Memoirs which he completed in 1963. A few bracketed notations have been added to Lind's manuscript to bring it up to date and to include more literature references.

Lind met his death, at the age of 86, on February 12, 1965, in the Clinch River below Norris Dam while indulging in his favorite hobby, trout-fishing. His love of trout-fishing is illustrated by the following anecdote. One evening, about three years before his final fishing trip, he asked me to check his automatic reel which was malfunctioning. Lind accidentally triggered the mechanism causing the hook which was still attached to the leader to pierce his finger. He absolutely refused to allow me to cut the hook so as to remove the barb and extract the hook. He vehemently protested, "This is my favorite fly—only yesterday I caught my limit on it." We drove to the emergency room of the hospital where the surgeon, a friend of Lind's, was waiting. The surgeon also met with the same resistance when he started to cut the hook. Exasperated, the surgeon said, "O.K., Lindy, to hell with the finger, we'll save the hook," and it was removed intact. This was the fly Lind was using when he lost the battle with the fast waters of the Clinch.

Many of the members of the Tennessee Academy knew Lind personally and I am sure that all with a chemical background are familiar with his publications. His doctoral publication (1906) on the synthesis of HBr is a classical example in chemical kinetics and his rate equation is still in use. The unusual form of the equation was interpreted by Christiansen (1919) and Polanyi (1920) on the basis of an atomic chain mechanism.

Lind's scientific career was concurrent with the development of modern science. He entered college the year Roentgen discovered x-rays. Before he graduated, Becquerel discovered radioactivity, Thomson the electron, and the Curies radium and polonium. It thus seems fitting and proper that his major contributions to science were in the fields of radioactivity and radiation chemistry.

After post-doctoral research in radiation chemistry at Madam Curie's Laboratory in Paris and at the Institut für Radiumforschung in Vienna, Lind recognized the importance of ions as reacting species in the radiation chemistry of gases and in 1911 published the first quantitative study relating products to ions. Some forty-five years later, high pressure mass spectrometric studies established the importance of ion-molecule reactions in the gas phase, substantiating Lind's postulate which had fallen from favor for about twenty years.

Lind and his group from the U. S. Bureau of Mines extracted 8½ grams of radium from Colorado carnotite, the first radium produced in the United States. During the production days, an accident (not of Lind's doing) caused him to inhale some radium chloride dust, some of which permanently remained in his body. No harmful effects of this accident were ever noticed. However, the thumb and index finger of his right hand, due to picking up radium capsules, were burned to almost half normal thickness. A decrease in sensitivity in these two digits was the only effect he noticed.

Of the radium extracted, ½ gram was retained by the Bureau of Mines and was made available to Lind for experimental purposes. This half gram of radium followed him wherever he went and was still in use at Oak Ridge National Laboratory, where he was a senior consultant, until his death. The radiolytic reactions most extensively studied by Lind were the synthesis of water from its elements, the polymerization of acetylene and the effect of additive gases on reaction rates.

He authored "The Chemical Effects of Alpha Particles and Electrons" which was originally published in 1921 and revised in 1928, and his more recent monograph, "The Radiation Chemistry of Gases" (1961). He co-authored the volume "The Electrochemistry of Gases and Other Dielectrics" (1939). In addition to his research and other duties he was editor of the Journal of Physical Chemistry for 18 years. Lind was the author or co-author of over 140 scientific papers, the first of which was published in 1903 and the last in 1964.

The following incidents are indicative of the man as I knew him. At lunch one Monday during his seventy-fifth summer, he was grumbling about getting old. He stated that on the previous day after playing only nine holes of golf he became too tired to continue. However, he neglected to consider that on Saturday he had spent most of the fourteen daylight hours wading the fast waters of the Little Tennessee to get his limit of trout.

Nonetheless his recuperative powers were amazing. One Saturday during that same summer we fished Hazel Creek in the Great Smoky Mountains for about twelve hours. When we got back to the boat to return to Fontana Village, we were both exhausted. He retired immediately after eating supper. At six the next morning he was at our cottage for breakfast and raring to go up Hazel Creek again. Although I was over thirty years his junior, I was not up to another day on Hazel Creek. We compromised and cast from the boat.

The many honors which accrued to Lind testify to his preeminence as a scientist, teacher and administrator. The esteem and affection with which he was held by all his associates and friends are mute evidence of his humanism, humanitarianism, and helpfulness. He had a delightful sense of humor and was never averse to a joke on himself. Although he enjoyed a good scrap, he never held a grudge. His only criterion for friendship was a common interest. His friends ran the gamut from backwoods natives that he met on his fishing trips to Nobel Laureates that he met at scientific meetings. To have known him was to be impressed; to have been a friend of his was an honor. His death was a great loss to his friends and associates. We do, however, have the consolation that, both literally and figuratively, Samuel Colville Lind died with his boots on.

Philip S. Rudolph

Oak Ridge National Laboratory
Oak Ridge, Tennessee
January 1971

AUTHOR'S PREFACE

The writer of a memoir is hardly the one who should attempt to characterize it in a preface, since he is not only the author but largely its subject. I therefore feel inhibited and shall restrict my remarks to a few factual statements.

In this modest undertaking I was quite aware that its readers will be interested less in me and my work than in the places where it was carried on and in the distinguished and interesting people with whom I became acquainted. The period of time covered is 1902-1963. It was physical chemistry that first attracted me and later nuclear chemistry, particularly that branch that has come to be known as *radiation chemistry*.

The text is interspersed with incidents and personal experiences. While many of these may seem to have little connection with chemistry I hope they will help to enliven the picture I try to give.

The personal background of the writer is offered in chapter one where the description of his family and of his birth place may be passed over by those not interested. The treatment in the succeeding chapters is largely in chronological order and is mostly limited to the time when I was resident in the scenes described.

A description of the places where I have lived and worked seems a natural setting for the main theme but sometimes may stray rather far afield as the reader will find.

CHAPTER 1

MY FAMILY AND MY YOUTH IN MCMINNVILLE, TENNESSEE

Before beginning to describe my scientific work in the field of chemistry it may not be out of place to give some personal background of myself and of my family. Born in McMinnville, Tennessee, June 15, 1879, I have wandered far from my native heath, imitating my Swedish father, but not my mother Ida Colville, whose family had lived in Tennessee before its founding.

My father, Thomas C. Lind, was born in Stockholm in 1842 where he received the usual education through Swedish high school, which like German Hochschule, includes subjects midway between American high school and the first two years of college. At the age of 16 he shipped as sailor on one of his uncle's small freighters and made a round trip to New York. On returning to Sweden he completed the required military training and performed his first term of service. At nineteen the wanderlust struck again and took him back to New York, this time as first mate of a sailing vessel. But instead of returning home, he enlisted in the Union Army just as the Civil War was beginning and troops were being actively recruited. With his Swedish military training, he rose rapidly to the rank of Captain and was put in command of a company of Negro troops, because as a Swede he had no race prejudice. After being wounded in the Battle of the Wilderness, he was hospitalized in New York City for recuperation, after which I have no information until he turned up in Tennessee in the 1870's, where he was sent by the old Pennsylvania Oil Company to hunt for petroleum in the Southern Appalachians.

McMinnville was chosen as prospect because the Barren Fork River there, at low water, shows a continuous bubbling of natural gas which was thought to indicate underlying petroleum. His drilling disclosed none, and to my knowledge none has been found in the Southern Appalachians. [Oil has recently been discovered in Scott County, Tennessee].

After this disappointment my father decided to settle in McMinnville and study law. As there were practically no law schools in the South at that time, law study consisted in "reading law" in the office of an older member of the bar, usually a judge, until admitted to practice by state examination. Judge Webb, with whom he began to study, was a wise choice since he was an able barrister with many years of experience both in practice and as a circuit judge. He took much interest in my father's training and gradually began to impose confidence in him as an assistant. This relation became more important to the judge as with declining years his health began to fail, especially his mentality. He would be seized with temporary aberrations which in time became more serious. He was obsessed with spells of violence and once imagined that my father was an enemy and made threats to kill him. Finally it was necessary for my father to sever their relation and set up law practice for himself.

Practice of law in those days was a respected, but by no means easy, profession. By custom and common consent, a country lawyer took cases in his own county and in the immediately adjoining ones. Many of the cases then were suits about ownership of land, arising from the aftermath of the Civil War and, of course, often involved lands extending into more than one county. Warren County, of which McMinnville is county seat, was named for the great hero of the Revolutionary War. It has the singular distinction of being the roundest country in the entire United States. On a map of the state showing counties, it looks like a penny, perfectly round except in the northeast corner where it is bounded with some irregularity by the Caney Fork River. The history of this circular boundary goes back to the time when Tennessee was separated from North Carolina and admitted to the Union in 1796. Many of the pre-existing counties were too large for horse and buggy travel to the county seat. As the counties were divided into smaller ones, a county seat would sometimes be placed far from the county center. To avoid this, a law provided that in the dissection of large counties, no boundary should come closer than 12½ miles to the county seat. Apparently Warren County was mapped by putting one point of the divider on McMinnville and describing a circle of just 12½ miles radius. There are vestiges of this procedure in a few other counties in Tennessee, but none in the entire United States is so nearly circular as Warren.

In going from one county seat to the next where a circuit judge would be holding court, it was necessary to travel by horseback, in an open buggy, or in a so-called "buck-board" without shelter. One must admire the fortitude of lawyer (and horse) who braved those twenty-five mile trips in the midst of winter over the worst of roads. Such was the practice of my father for thirty years until his death in 1903. Private automobiles had not then arrived, though he had once ridden in a taxi in Nashville.

Besides meticulous care of his law practice, he had always been an ardent member of the local "lodges," the Masons and the Odd Fellows. He was highly esteemed by all his acquaintances despite his foreign birth and the well-known fact that he had fought against the South. He was an Elder in the Presbyterian church and known for honor and upright behavior in all his relations.

I had an example of the affection in which he was held by the mountain people of Grundy County where he also practiced. Some of us "summer boys" were loafing around the Beersheba Springs Hotel trading with the mountain boys for watermelons when one of the boys slipped around to me and whispered: "We'll let *you* have it for less. For you are one of us." I was proud to be considered one of them despite my association with city boys, some of whom would have thought this a doubtful honor.

I once had another proof of the affection the mountain people had for my father and which they often sought to transfer to me. One of the old timers drew me aside at Beersheba and confided that he was about to leave for Texas. I did not ask him why, for it was not always wise to inquire too closely into the reasons for sudden departure for Texas. I had even heard a rumor that such might be so in his case. He told me he had long known my father and had a great fondness for him. They had the bond of fellowship in the Masons. For this reason he would like, before he left, to see me become a Mason. He explained that their rules strictly forbade solicitation of members, but he was violating them by making an exception for me on account of my father. I thanked him warmly for his solicitude and promised I would give it careful consideration. But the closest I have ever come to membership in any fraternal order is in the Oak Ridge Lodge of Elks.

Some time before the Civil War, Beersheba Springs, atop the Cumberland Mountains, became the mecca of summer refugees from Louisiana and other southern states seeking to escape the annual ravages of yellow fever, before mosquitos were brought under control. A large two-story hotel overlooking the valley housed the health seekers and afforded the usual recreations, dancing, horses, cards, billiards, etc. Cottages also housed summer residents from Nashville, Clarksville and other Tennessee cities.

Through his law practice my father had become acquainted with Mr. Hooneywaddle, a Swiss settler who had acquired American citizenship, and built a Swiss-type chalet one mile from the Beersheba hotel, which was humorously known as Dan, from the biblical association. I mention Dan because my family and I spent two summers there, renting from Mr. Hooneywaddle who was then serving as American consul in Maracaibo, Venezuela. Our rent was probably in lien of payment for legal service rendered to Mr. Hooneywaddle by my father.

With the advent of the automobile and extinction of the mosquito in other places, Beersheba has greatly changed. The hotel has been converted to a denominational summer school. Youngsters are now too healthy to need mineral water, which has also lost its appeal to older people. The spring and billiard parlors are closed. A few descendents of the original cottagers still maintain their places for summer use. But the glamor of the once famous summer resort has vanished.

My mother, Ida Colville, came of Scotch-Irish descent. The family had been in America for more than two hundred years, migrating southwestward from Maryland, Pennsylvania, Virginia and Carolina to Tennessee. Her mother, Amanda Colville, was born in Athens, Tennessee, and married Samuel Lusk Colville of Warren County, who then made their home in McMinnville, where my mother was born in 1853. My mother was the third in a family of three girls and four boys. She was educated in public and private schools of McMinnville. She was about twelve when the Civil War ended and related that she and other girls would sit on the fence and "sass" the victorious Yankee soldiers marching by. Her father, however, was not a too ardent Confederate. Long before the War he had freed the slaves he held, and like many of the more conservative southerners he had not favored secession from the Union and believed a war between the states would be a great mistake and as disastrous as it later proved to be. His conservative leanings did not make him popular with some of the stronger "Rebels." One of my uncles in his boyhood was chased home from a neighbor's and told not to come back "until the War is over." Later they became good friends again.

I was the oldest of five children. One brother died at birth. Our only sister, Amanda, my brother Robert and I all had simultaneously severe attacks of virulent diarrhea. Amanda succumbed, while Robert and I pulled through. My attack, though severe and lasting a week, was lightest. Robert was desperately ill, but fortunately neither of us suffered after effects.

Robert, beloved by all for his genial spirit and liveliness, was our father's favorite. Even the truck drivers would pick him up to ride just to enjoy his engaging company. He knew and liked every Negro in town and they all liked him. When Robert was six a traveling circus came to town. He was up at dawn to see them drive the first tent stake and on hand at midnight to see the last one pulled. But somewhere in the crowd he must have picked up a bad germ. He was striken by what may have been the then unrecognized poliomyelitis and died of throat paralysis after a few hours illness. He left a sad family and his father never ceased to miss him.

My brother, Warner, ten years younger than I, grew up mostly after I had gone to college. He attended the Virginia Military Institute at Lexington after I had left Washington and Lee. Never married, he has spent most of his life in Kansas City, Missouri where he and our mother lived together for many years. Recently he retired from the Kansas City Structural Steel Company that he had served many years as Secretary. [Warner Lind died on January 3, 1968].

My own education was in the local schools beginning in my eighth year, previous to which my mother had taught me reading and arithmetic. Apparently she gave me up on penmanship, and I also became discouraged and have never mastered it. On completing high school in McMinnville at the age of sixteen, I took the entrance examination for Washington and Lee University which I entered in the fall of 1895. My going there rather than to the University of Tennessee, where three of my uncles had studied, was due to an influential school Superintendent who had graduated from Washington and Lee and who convinced my parents and me that it was the only respectable institution of learning in the entire South, if not in the country. But Washington and Lee began a new chapter for me and must also in this account.

But this chapter should not close without a word about McMinnville of 1895, typical of a small-town, county seat in Tennessee. Then already a century old, the town had a population of nearly 2000. One school building housed all grades, four years of high school including mathematics through algebra, geometry and trigonometry, two years of Latin, English, history, etc., with no organized athletics, gymnasium, nor ball teams. Those were the days of earnest study and three hours of homework at night—no movies—no autos—not a public eating place in town.

The population of McMinnville, like that of many other towns in Tennessee, was and still is largely of Scotch-Irish descent. This designation refers, I believe, to people of Scotch origin who, on account of religious or political persecution, took refuge in Northern Ireland. Many of them emigrated to the United States, principally in the South. About twenty percent of McMinnville's population consisted of Negroes liberated from slavery during the Civil War. There were no Jews then except one couple without children.

Seven churches—Baptist, Methodist, two Presbyterian and one Christian for whites, and two for colored worshipers—Baptist and Methodist, cared for the souls of McMinnville's population. Four saloons operated openly and legally but were not patronized by the more respectable citizens—at least not openly, though, of course, there were back doors. I was never in one of them after the tender age of four and, needless to say, only once then. I was playing alone in our front yard on a mild summer evening when a strolling band of gypsy minstrels passed by. I was so enraptured with their music that I followed them all the way down Main Street and right behind them through the swinging doors into the first saloon, naturally their mecca. Drinking only their music, I was oblivious of my surroundings until I was rescued and snatched back to the world of respectability by some friend who had heard of my wanderings.

There are now no more saloons in McMinnville (boot-legging has replaced them as in most other towns in the state). My grandfather and two of my uncles never tasted anything alcoholic. The other two uncles abstained until they had moved away to city life and were then content with a single cocktail or highball, at home before dinner or in a respectable cocktail lounge. As to my father—he of course never drank at home, but it was rumored that when away at court he would sometimes indulge his old Swedish sailor or soldier habits. My mother was oblivious, or wisely pretended to be, as long as he observed the rules at home, which were rather severe. I remember when fishing he liked to clench an unlighted cigar between his teeth for hours at a time. There was no public drinking among Southern ladies at that time. They would not have been admitted to bars and if occasionally one drank in solitude it was highly secretive. Neither my mother nor my two aunts ever knew the taste, nor allowed a drop in their homes. As to myself, I never drank in McMinnville nor at Washington and Lee. It was not until I went to M.I.T. in Boston that I was initiated, but I had little time or money and not too excessive taste for it. Later in Germany one drank mostly beer or wine. But Germany comes later.

CHAPTER 2

WASHINGTON AND LEE UNIVERSITY

The reason for my choice of this university was related in the foregoing chapter. Washington and Lee University at Lexington, Virginia was the outgrowth of Washington College, of which Robert E. Lee became President soon after the close of the Civil War, and did not become a university until after Lee's death in 1870. He was succeeded by his son, General Custis Lee, during whose administration I enrolled as freshman in the fall of 1895 at the age of sixteen.

Washington and Lee at that time was an old-fashioned, intensely Southern institution, particularly emphasizing the classics, but guarding against over specialization by the following wise rule. Out of the 66 total credits required for the A.B. degree, at least 16 had to be in each of three different fields, one of which was Mathematics and Science. Having spent most of my first three years in languages—Latin, Greek, French, German and even Anglo-Saxon—I came to my senior year faced with the need of six credits in the *science* group. Someone advised me that Chemistry would be a good way to gain the necessary six points, and so I was practically forced into the field that was to become my life work.

I found chemistry entrancing from the very beginning, owing to the inspiring teaching of Jas* Lewis Howe, a graduate of Amherst College (A.B. in 1880), who took his Ph.D. at Goettingen in 1882. He had held the chair of chemistry at the University of Louisville before coming to Washington and Lee. No praise could be too high for the teaching of Howe. His knowledge of the subject, his skill and enthusiasm in unfolding it were superlative. Although I had had a six-weeks course in so-called chemistry in high school, I had forgotten it wholly. Chemistry came as a new, fresh, and delightful subject. Also my three years of college work had developed study habits so that I had no difficulty in the first year's course which attracted me to pursue it further. The development of the Periodic System of the Elements of Mendeleeff had only recently reached a new height through discovery of the inert gas elements by Sir William Ramsay. I thought and still think it the most wonderful of the creations of chemistry, be it a short, compact, or a long, extended type of table. All the tinkering can neither improve it nor spoil it.

Howe at that time taught all the chemistry offered at Washington and Lee—general, qualitative, and quantitative, including laboratory supervision. How he managed research in his favorite theme, *ruthenium,* or to keep up so meticulously his well-known bibliography of that element is beyond my comprehension. His New England vigor and perspicacity must have been the factors. Upon my receiving the A.B. degree he advised me to return to Washington and Lee for a second year of chemistry—all that he gave. This I did without thought of any degree beyond the A.B. which I had received at the end of my fourth year. Washington and Lee did at that time offer an M.A. degree, but under very severe terms, 24 credits beyond the 66 required for the A.B., and all with grade *90* or above. Needless to say, it was seldom awarded. Even so, I might have qualified in five years had I not in my sophomore year spent too much time at the billiard table.

My five years at Washington and Lee were very pleasant, though some of my time might have been more profitably employed than in four years of Greek, three of Latin, and two of "Old English" (real Anglo-Saxon). My first year was rather Spartan. I lived at the so-called (and well styled) "Blue Hotel" with an older law student from McMinnville, Leslie Hoodenpyl, who later became Mayor of Pasadena, California. The cost of board and room was only $13 per month at this establishment patronized by students of small means. But in the spring, Hoodenpyl could stand the frugality of the Blue Hotel no longer and got us board and room in a private family, which was much more satisfactory. Strangely I have but little recollection of the Blue Hotel fare, whether good or bad. In my freshman enthusiasm I must have devoured it as well as my studies. Other men with straitened means, like my own, who lived per force in the Blue Hotel, were able students or assistants during their college years. Many of them later occupied high and distinguished posts in academic or official life.

The following year my family's finances had improved so that I was able to have room and board in the home of Professor Humphreys and roomed with his brother-in-law, Ewing Sloan, a student of Engineering, later retired in Jackson, Mississippi. Across the hall were two young law instructors who subsequently distinguished themselves—John W. Davis, Ambassador to England, candidate for the Presidency—Head of one of the foremost law firms in New York City, and William Reynolds Vance who became Dean of the Law School at the University of Minnesota and later of Yale. Davis delighted in identifying me with Tennessee mountaineers and good-naturedly twitted me about vaulting from peak to peak in my native habitat. During my earlier years in college I was very shy toward my elders and superiors. I remember I was embarrassed to appoach the professor, my host, to pay my board bill although I received the money monthly from home and had carefully kept it intact. After three months accumulation I mustered courage to present myself with the sum due and was received without reproach, though my roommate afterward told me that the grocer's and butcher's bills had mounted to an alarming extent. Later I paid regularly by the month.

The following year it was no longer convenient for the professor's family to have boarders. I moved to the home of a widow next door to the campus who took students. Here I roomed with a law student from Richmond, Henry W. Anderson, who later was once unsuccessful Republican candidate for the Governorship of Virginia.

I remember Anderson telling me stories about the

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* Not an abbreviation.

straitened finances of some of the old Virginia families soon after the War. One plantation owner said that he had been reduced to herding cattle and driving hogs but he would be damned if he would descend to trimming his cuffs! Times have changed. I do not herd cattle nor drive hogs, but confess to occasionally trimming my cuffs, which become chafed from wearing a wrist watch, a thing then unknown.

In the late nineties Washington and Lee was regarded as the second university of the South, second only to the University of Virginia founded by Thomas Jefferson. Lexington was the citadel of the old Confederacy. Both Generals "Stonewall" Jackson and Robert E. Lee are buried there—Jackson in the city cemetery, Lee in the Memorial Chapel of the University beneath his famous recumbent statue by the sculptor Edward V. Valentine. Also the Washington and Lee Campus adjoins the parade ground of the Virginia Military Institute, the "West Point of the South."

In my time Washington and Lee and Virginia Military Institute, with student bodies of 200-300 each, engaged in athletic contests with each other, football and baseball. There were occasional fights which sometimes involved the spectators; and such terms as "Rats" for the cadets and "Minks" for Washington and Lee students were exchanged. In later years the antipathies became so strong and the fights so serious that games between the two had to be abandoned and, I believe, have not been resumed.

Washington and Lee has grown in numbers to more than 1000 students. With no state support its finances are not adequate and many southern State Universities have far passed it in enrollment and diversity of curricula. Nevertheless, maintaining its unique position based on a noble background of precedent and tradition, it draws a select student body from all over the country and a surprisingly large number from states north of the Mason and Dixon Line.

The Virginia Military Institute has also expanded and developed engineering courses of high quality. Its contribution of high ranking officers in the two European Wars has enhanced its distinguished reputation. It is truly the "West Point of the South." My younger brother, Warner E. Lind attended it for two years after I had left Washington and Lee.

Lexington is the county seat of Rockbridge County, so named for the famous natural stone arch about twelve miles distant. The town is of staunch Scotch-Irish citizenry. In my time it had a population of some five or six thousand. It has since more than doubled. The Tucker family which dominated the Washington and Lee law school for two generations strongly supported secession from the Union and defended it on constitutional grounds.

The State of Virginia was named for the virgin queen of the British Isles, Elizabeth I. It was colonized early in the 17th century by English and Scotch settlers. The first colonies had a hard time becoming firmly fixed but when they did get settled, Virginia rapidly became foremost among the colonies along the eastern sea border. The Virginia settlers tended to divide into two groups due to racial and religious differences. The Scotch-Irish Presbyterians occupied the interior counties along the Alleghany Mountains, and Lexington was from the beginning a strong-hold of Scotch-Irish nationalists. I believe "Irish" did not indicate Irish descent but simply the Scotch driven from home by religious persecution who settled in the eight northern counties of Ireland, which today adhere to British government rather than to that of the Irish republic.

But the "Eastern Shore" of Virginia was settled largely by English stock, with a more worldly and freer outlook than the Scotch-Irish. The Lee family and that of George Washington may be regarded as typical East Virginia families. Dancing, merry-making, an occasional game of chance were not only tolerated but actually encouraged.

How greatly the national position of Virginia was changed by the so-called Civil War (or War of the States) can be appreciated by comparing various segments of its political life before and since the Civil War. Before the War Virginia had had by far the greatest number of national Presidents; since the War not a single one. And the latter is also true of all the other Southern states. The many able public leaders in the South cannot aspire to the highest office of their homeland. Lyndon Johnson, who became President by succession, is the first. The Democratic Party has long ago expanded beyond the South (e.g., President Kennedy from Massachusetts) but it was not strong enough to carry a Southerner to the White House before Johnson.

But did my education and residence in Virginia at the shrine of Robert E. Lee and the home of Jackson leave any permanent impression upon my outlook on life? My prompt transfer to Boston and then to Leipzig seemed to wipe out my prejudices and put me in balance. My loss of race prejudice is discussed in Chapter 4 and 12 and need not be mentioned here.

I am very grateful to Washington and Lee for my years there, especially to Professor Howe for bringing me under the influence of chemistry. If I had foreseen this in my freshman year I would have devoted more time to mathematics and physics with the hope of being a better chemist. But that is water over the dam that cannot be brought back.

Finally I can ask myself if today I would choose Washington and Lee for myself, or more pertinently for my grandson. Well, probably not, with no Meadors to send one there nor Howe to complete the capture.

CHAPTER 3

MASSACHUSETTS INSTITUTE OF TECHNOLOGY, BOSTON

When, after obtaining an A.B. degree, I finished the two years of chemistry offered at Washington and Lee and decided to pursue the subject further, Professor Howe advised me to enter the Massachusetts Institute of Technology which offered a *four year* course of required studies leading to the *S.B.* degree. This designation had been chosen to differentiate the four year course from *three-year* courses leading to the B.S. degree *in science*, offered at some institutions to stimulate enrollment in the sciences, and to compete with the traditional A.B. *four year* degrees (mainly in the classics). There were no graduate courses at M.I.T. when I entered in 1900, though they were instituted soon after I graduate in 1902.

M.I.T. was then located in "Back-Bay" Boston in the block between Berkeley and Charleston Streets on Boylston Avenue and was popularly known as "Boston Tech." Naturally it lost this title, to the dismay of former graduates, when it moved to its present stately site in Cambridge on the Charles River at Massachusetts Avenue where the very fine main building was made possible by the donations of the late George Eastman of the Eastman Kodak Company.

The four year course in chemistry at M.I.T. was then regarded as the best undergraduate chemical curriculum in the country. Its excellence was maintained by such professors as A. A. Noyes in physical chemistry, James Norris and Samuel Mulliken in organic chemistry, Henry P. Talbot and Henry Fay in analytical chemistry and Helen Richards in the chemistry of foods. Besides my regular courses in physical and organic chemistry, neither of which had been offered at Washington and Lee, there were so many required ones including mechanical and free-hand drawing in the freshman year, that it took every hour of the 5½-day week during my entire two years for me to catch up. I never became a "regular" student without back requirements until I passed the last examination a few days before my graduation. But I managed it somehow and was appointed to an assistantship under Professors Talbot and Fay for the following year at a salary of $500. My duties as assistant in the course of advanced analytical chemistry consisted in supervision of the laboratory work of the students required to take it—Chemists in their Junior year and Chemical Engineers in their Senior year. Analytical chemistry then was much more emphasized than now in chemical curricula, and the analytical methods have changed greatly. Operations were mostly by hand, both gravimetric and titrational. The modern application of physical principles had not arrived and some of the long and tedious procedures would not be tolerated today; for instance hand grinding of a hard silicate to less than 200 mesh before fusion to determine sodium or potassium in the resulting extraction. At the end of that academic year in 1903 I was granted a two-year Dalton fellowship for graduate study —also at $500 per year—which I decided to take at the University of Leipzig.

In the early nineteen-hundreds M.I.T. sought to differentiate itself as much as possible from the looked-down-upon academic colleges. The traditional cap and gown for graduation day was rejected in favor of high silk hat and frock coat. I believe M.I.T. has now adopted the usual academic costume. But I remember one incident of my graduation day in June, 1902. We were all lined up on the stone steps of the Museum around the corner in Charleston Street waiting to march, when a lone pedestrian came by. Having nothing better to do, we began to mark time by tapping our canes in unison with his steps. I shall never forget his nonchalance. He stopped, raising and adjusting his monocle, he deliberately and calmly surveyed this assembly of freaks as if they were inmates of the Museum, who presumed to mar the decorum of staid old Boston by invading the privacy of a passing individual. I take off my hat to him. He literally stared us down with scorn. When our tapping had subsided he resumed his unhurried walk until lost to sight. Surely he must have been an English actor to outface and quell 200 M.I.T. men on their own ground. Or perhaps we came to feel our conduct unworthy of high hats and canes, especially on our graduation day.

Many students of M.I.T. lived at home in Boston or in its suburbs and either walked or commuted before the days of autos. Those from out of town roomed in the Back Bay section or across the railway lines, along Columbus Avenue and its side streets. Columbus Avenue was then on the edge of respectability. The color line coming up from the south side had not yet reached that far, though it later swept across the railway, across Huntington Avenue and Boylston Street on toward Arlington Street and the Charles River. I express here no prejudice against race or color. I am simply noting that even in Boston when the race of color moves in, the other moves out, and that if M.I.T. had not moved to Cambridge its more numerous students might now find housing impossible within walking distance because today commerce is also replacing much habitation in that district.

I found a room on the third floor of a red brick rooming house off Columbus Avenue and roomed for two years with a congenial student of electrical engineering from Alabama named Lawson. Both of us were entering the third year where specialization begins to separate underclassmen previously associated in common courses and in first year military training. Consequently we did not have the broad range of acquaintance so readily formed in the first two years. But I did get to know the fifteen or twenty specializing in chemistry and made good friends with my classmates, Bobby Edwards, of Portland, Maine and with the late Herbert Walker from Washington, D.C. At the instance of Professor Howe I was invited to join a fraternity but declined, as I had at Washington and Lee.

I really had no time for such little student life as there was then at M.I.T. I never entered the so-called "Chapel," the Brunswick Hotel bar across Boylston Street, seldom heard the Boston Symphony Orchestra or attended theatre. In my third year as Assistant, I had more leisure and lived in an apartment on Boylston Street with my youngest uncle, Warner Colville, with whom I spent much of my free time. He made annual trips to Europe and on one of them had met the Tolman Family of West Newton, parents of the distinguished chemist Richard C. Tolman whom I knew at M.I.T. and who graduated in the class one year behind me. Tolman Sr. was a member of the Board of Trustees of M.I.T.

On one occasion my uncle and I were invited out to the Tolman's in West Newton for dinner. When I got home from work my uncle was already dressing and began to worry for fear I would be late. I urged him to relax—there was plenty of time. But he became more worried as, with time passing, he found I had not even begun to dress. Finally he became so exasperated he put on his coat and hat saying he would go on and leave me to come in my own sweet time, but he would be embarrassed, as well as I, when I arrived after delaying dinner for half an hour. He departed in haste. There were plenty of suburban trains then before the days of autos, so I did not hurry. The result was, as he later recounted in merry self depreciation, that he arrived half an hour before dinner time. Mr. Tolman had just come from town. No one had dressed or come downstairs, and no one except a servant was ready to receive him. And to top it all I arrived later just on time.

In my third year at M.I.T. while I was assistant, I published my first piece of research, a small paper entitled, "The Constitution of K_2RuCl_5NO in Water" [J. Am. Chem. Soc., *25*, 928 (1903)]. It was a minor piece of research and the conclusion today for the structure of such a salt in aqueous solution would be taken as a matter of course. But then it was of interest to find that the NO radical acted like Cl in being attached to ruthenium in the anion. The freezing point lowering and electrical conductivity were found to be those to be expected from a salt of its character.

During my year as Assistant in Chemistry I was very fortunate in being awarded a Dalton fellowship for graduate study in chemistry. It seemed at M.I.T. at that time that there was but one place worth considering, the Laboratory of Physical Chemistry under the direction of Geheimrat Wilhelm Ostwald at the University of Leipzig, where Noyes, Whitney, Bigelow, Cottrell and many other distinguished physical chemists from America had recently taken their doctorates.

At the turn of the century when I went to Boston, it was as it always had been—one of the most important American cities. It was especially distinguished for its cultivation of the fine arts. Its handsome Public Library and its famous Symphony Orchestra had at that time no equal in the United States. The paintings by John Singer Sargent decorating the walls in the corridor and stairway of the Library were greatly admired and gave the building an air of cheerfulness and welcome. Although they may not be so much in vogue now as then, I can never enter the library without remembering the thrill they gave me the first time I saw them and without stopping again to renew my enjoyment.

Unfortunately my studies did not leave me much time to enjoy the cultural influences of Boston. Although the subway was already there I seldom got over to Cambridge to visit Harvard University and I believe there was not so much contact between the two institutions as now since M.I.T. has moved to the Cambridge side of the Charles, and some joint courses have been instituted.

I doubt if much of the Boston culture rubbed off on me. But I am still fond of the city and like to visit it and wander along Boylston Street and recall my student days.

CHAPTER 4

THE UNIVERITY OF LEIPZIG, GERMANY

In the previous chapter the Massachusetts Institute of Technology award to me of a Dalton Fellowship for graduate study was mentioned as well as my choice of the Ostwald Laboratory of the University of Leipzig and my reason therefor. In order to acquire some facility in speaking German before entering the University in October, I conceived the idea of spending the three summer months in a small town or village where I could meet people and have opportunities of talking with them. I had imagined there would be towns like the small county seats in the United States. I soon learned, however, that in Germany there seemed to be no such towns—nothing between a city and a small country village or dorf populated by peasants who work in the surrounding fields. Rather discouraged, I decided to go to the city of Kassel, where the purest German was supposed to be spoken, and try to find lodging and board in a family. On my first Sunday there, I went out to one of the country inns nearby where I could ramble about and have dinner. Perhaps I looked very lonely eating alone or maybe it was her instinct for business that inclined Frau König to invite me to join her party, consisting of her husband, two young Russian officers and a young German Fräulein. Frau König explained that she had a small "pension" in the city where she took a few guests, including the two Russians, for room, meals, and to learn German. She would have a place for me where I could have daily German lessons with her. I accepted at once and moved into the Pension. The lessons were mainly reading and pronunciation. She insisted that I avoid all contact with English speaking people and claimed she could detect deterioration in my German whenever I strayed into association with American or English people living in Kassel.

The chief summer amusement in Kassel was in the city park to attend evening concerts by the city orchestra. I found them so enjoyable I decided to buy a season ticket. On my offering a hundred mark note in payment, the official, after inspecting my note carefully, said it would be subject to a discount of 3% since it was issued by one of the German provinces that did not have full credit. Being rather suspicious of his claim, I found I had a note from one of the other German states which I then offered instead. The official looked rather embarrassed, agreed that my second note had full value and so accepted it. I had no trouble elsewhere in having the first note accepted fully. Incidentally, I heard later that the official said that the Americans seemed to have 100 mark notes in abundance like playing cards. I only wished it might have been so in my case. Those were the only two I had. At that time gold in ten or twenty mark coins instead of notes was in more common use in Germany, and many merchants had standard weights ready for balancing proffered coins to insure against counterfeiting or adulteration.

Kassel, the capital of the province Hesse-Nassau, I found a very pleasant place to spend the summer. I made acquaintance among the small colony of English people living or visiting there. Bicycling on the well kept highways was enjoyable, though on one occasion it resulted badly. Riding with one of the English ladies, I unfortunately turned too sharply in trying to pass to her left. My front wheel touched her rear wheel, but it was just enough to send her into a spin that grounded her and her bicycle. She complained of her ankle, but I think it was more the ruined silk stocking, which I of course replaced, though they were not easily come by in Germany at that time.

In September, after a pleasant and profitable summer in Kassel, when I was ready to leave for Leipzig, my host, Herr König, invited me into his tobacco shop on the ground floor below the Pension. In saying farewell he explained to me that all the house mail, including mine, was first left with him for distribution. He had taken the opportunity to use my name for his dealing with a pool in Berlin through which he placed wagers on horse racing. In order to insure that his wife would not discover him, he requested me to continue to have my mail come to Kassel and he would forward it to me after extracting his betting lists. Much to his disappointment, I explained I thought this would be an awkward arrangement that would delay my mail, might embarrass me otherwise, and I regretted I could not consent.

After a pleasant and rewarding summer's stay in the fine old city of Kassel I left it to take up my work in Leipzig that will be described below. I have never returned to Kassel but was much concerned that it was badly damaged during the bombardments by the Allies in World War I. The Spirit of Kassel was far from Prussian and it seemed unjust it should suffer so severely for the wrongs perpetrated by the Kaiser and his regime with which it had little sympathy.

In 1903 the University of Leipzig was one of the four or five best known universities of the twenty-nine then existing in the German Reich—some of the other best known ones being Berlin, Heidelberg, Göttingen and München. Leipzig was financed by the state of Saxony, one of the larger provinces of the German Empire, and was governed by the Ministerium of Saxony. Like most of the other German universities, Leipzig did not have a centralized location similar to an American campus. The various institutes occupied buildings scattered about the city. Wilhelm Ostwald's laboratory, situated some distance from the Augusteum on Augustus Platz in the center of the city, enjoyed the same independence of management as did Organic Chemistry under Professor Des Coudres in its quarters a half-mile distant or Applied Chemistry under Professor Beckmann in yet another location.

Ostwald and his family of several sons and daughters lived in a house belonging to the University, adjoining his laboratory. He had many interests beside chemistry, painting, violin playing, philosophy, etc. His family also had tastes and talent for music and art. Ostwald had invented a method of pastel painting. Through these talents he brought together those with similar tastes and frequently invited the older students and assistants for concerts or entertainment in his home. But in the Laboratory he no longer had much direct contact with the students and their work. He left their instruction and the supervision of their research almost entirely to his two very able Privatdocents, Drs. Robert Luther and Max Bodenstein, while he had a private assistant who carried on work for him on subjects in which he was personally interested. He invented a method for the catalytic oxidation of ammonia to nitric acid from which he was said to derive quite an income.

Ostwald by 1903 had developed a great interest in what he called "Naturphilosophie." He gave a course in that subject, not for chemists but for a general audience attracted to his lectures in the Augusteum downtown. I think few chemists attended. He was a prolific writer and published many books not only on chemistry but on "Naturphilosophie," art and anything that interested him.

In his latter days in Leipzig he confined his chemistry lectures to one semester only, each year a two-hour course per week in Thermodynamics. In 1904 he had the great honor of being invited to Harvard as the first of the American-German exchange professors. This must have renewed his interest in chemistry—or there may have been a contractual requirement. In any case, at Harvard, instead of giving one lecture course he gave three. But that turned out to be unfortunate. On returning to Leipzig, Ostwald petitioned the Ministerium to relieve him of the one course he had been giving. The Ministerium however declined—on the ground that if he could give three lectures at Harvard he should continue to give at least one for a half-year at home. Ostwald was not a man to take "No." He forthwith resigned and retired to an estate at Grimma, fifty miles from Leipzig, where his chemical activities ceased entirely except for writing—principally renewal of older texts and editorial work.

On the whole, Ostwald was one of the four great figures in Central Europe who created Physical Chemistry and gave it to the world as a lusty youngster. It is still growing 70 years later. The others were Van't Hoff, Arrhenius and Nernst. Ostwald by his teaching, writing and research gave Physical Chemistry the interest that launched it on a great future. Yet it has been said *sub rosa* that when the Committee decided to give him the Nobel Prize which he received in 1909, it had difficulty in finding an outstanding experimental achievement that would justify the choice. And one can today pick out a few flaws in Ostwald's teaching which contrast with an otherwise brilliant background. He taught the indivisibility of all matter—that even gases on expanding are being only stretched like 3-dimensional rubber—and this some time after Rutherford had proved the individual existence of helium atoms by the study of alpha particles. This reminds me to note that *radioactivity* made at first so little impact on the Ostwald laboratory that during my two years there, seven years after the discovery of radium and polonium by the Curies, I never once heard the subject mentioned.

One of Ostwald's most valuable contributions was the founding of the Zeitschrift für physikalische Chemie in 1887 of which he was editor until 1922, when Bodenstein succeeded him. It remained an outstanding organ of physical chemistry until disrupted by the Second World War. It now appears using the same title under two different managements, the one in West, the other in East Germany. Both sadly reflected the influence of the war on German chemistry and chemists, but both have begun to regain their former prestige.

One may conclude from the foregoing that I reached the Ostwald Laboratory when it had passed its zenith. That might well be true so far as Ostwald's influence was concerned. But fortunately he had put the guidance of the doctorants into two extraordinarily able hands. Robert Luther was the senior of the two—Max Bodenstein a relatively newcomer from Heidelberg. Most of the candidates went to Luther first for guidance. He advised me to seek Professor Bodenstein, which I did at once. But in making acquaintance I made a most embarrassing mistake. By introduction I said: "Herr Dr. Luther hat *mich befohlen* (has commanded me) Sie zu besuchen" instead of: "hat mir empfohlen (advised me) to visit you." Bodenstein smiled understandingly and soon we had arranged for me to do my thesis under his direction.

But first let me explain how I had got that far. At that time in the United States the leading universities interested in graduate work had organized the American Association of Postgraduate Universities. It then had fifteen, now has some forty members. In studying the means of promoting graduate work in the United States it had eyed suspiciously the strong lure of German universities which took so many Americans abroad. The Association had decided that attaining the doctorate in Germany was too easy, that students not well prepared were being allowed to attain the Ph.D. in two years instead of the three-year minimum at home. Therefore the Association petitioned the German universities to stiffen their requirements, to grant no Ph.D.'s with less than 3 years beyond the bachelor's degree, and *of course,* to accept no graduate time from the United States except from one of the fifteen members of their Association.

Four of the German universities, Berlin, Leipzig, Göettingen and Heidelberg granted the petition at once and raised their requirements, one of which was an entire semester of pre-doctoral studies before undertaking a thesis. In chemistry this included laboratory tests and a preliminary examination. Upon arrival at Leipzig I learned this from two fellow students from America, William C. Bray and Victor Sammet, both of whom had already run afoul of the new rules.

Believe it or not, Bray, who later became a world authority in analytical chemistry, had failed his "unknown" mixture, either by reporting an element not there or by failing to find one. Sammet had failed his "preliminary" from lack of knowledge of the "phase rule," a rather new subject at that time. Both of them later recouped and went on to Ph.D.'s *summa cum laude.* Warned by their experiences I put in a very full fall semester of preparation both by study and experiment. For the "unknown" in qualitative analysis I did several practice ones until I felt I had mastered the system of Professor Bötger, who was in charge. He gave me a mixture with about six bases and two acid components—with a two-day time limit. When I reported he shook his head and said I had correctly found everything present, but was mistaken in reporting *nickel,* which was not supposed to be there. I asked for a new sample, and again found nickel. When he became convinced of it, he blandly admitted it was probably an impurity in the cobalt component and looked almost aggrieved that I had found it.

In the quantitative test he usually gave a mixture of two metal salts and asked for their separation and determination. He gave me iron and manganese and I happened to have taught their separation by the basic acetate method at M.I.T. So they caused me no trouble. I also passed my oral preliminary examination by Luther and was ready for my thesis. It was then that I introduced myself to Bodenstein as I have already recounted.

During my vacation in the summer of 1904, midway in my work at Leipzig, I took a trip to Sweden to visit the land of my father's birth and to meet some of my kin. Being rather tired, I decided on a leisurely trip north through Denmark to cross to Sweden from Helsingör to Hälsingborg.

Among the notable sights of Copenhagen, including its majestic cathedral, is its fine Botanical Gardens. There a simple thing has left a lasting impression on me, as apparently violating the laws of nature, but at the same time illustrating vividly their application. A beautiful fountain more than head high played sparklingly a few feet before my admiring eyes, and yet I could hear not a sound. This would not surprise me today, but at that time my hearing was still excellent, making the apparent miracle all the more unbelievable. Of course, the explanation is that the fountain was enclosed in a glass case with double wall, providing a vacuum so soundproof that the beautiful water nymph seemed to be making the effort to speak but had been struck dumb, though her face was still smiling naturally and showed no abhorrence of the vacuum.

Leaving Copenhagen after a few days during which I had visited her famous beach resort, I took the train toward Helsingör. I had planned to visit one of the two provincial cathedral towns en route. By stopping off between trains at the second one I could see the cathedral and still reach Helsingör in time for the afternoon boat to Sweden. But I must have dozed en route. Nothing is more tiring than sight-seeing. When the train stopped I awoke to see a cathedral on a hill and crowds of tourists walking in that direction. I followed to the cathedral and dutifully inspected it. Whether two cathedrals could be so much alike or whether I paid too little attention to the details of my Baedeker I do not know. But I do recall that the lack of correspondence sometimes puzzled me. When I went back to the railway station to continue my journey I wondered at the failure of the train to arrive. Trains in Europe were surprisingly dependable. After waiting some time and noticing that no other passengers were waiting, I looked up the Station Master. He could not speak a word of English nor I a word of Danish, which made communication difficult. Finally by showing him my time table and the train I intended to take he made it clear to me I had got off at the first station instead of the second, and that my train had already gone. There would be other trains but I could no longer make my boat and would have to spend the night in Denmark. So I took the next train, stopped off to see the second cathedral and then on to Helsingör where I spent the night before crossing to Hälsingborg the next day.

In order to see as much of provincial Sweden as possible on the way to Stockholm I had decided to take the Göta canal boat via Jonköping. Passing through the many locks gave me time to stroll about and see the small towns and the interesting sights and quaint country people.

After two days of this kind I arrived in Stockholm and put up at the Grand Hotel. I was resolved first of all to visit my aunt, my father's older and only living sister, who lived some 25 miles from the city on one of the many lakes that surround Stockholm. I knew very little of the family. My father had kept contact with his sister through occasional letters. But in forty years he had so completely forgotten his native language that he had to have her Swedish letters translated to English. So I knew nothing of her except that she was a widow living alone. I had written her I would come on a certain day to visit her. For the past several weeks I had spent some spare time in trying to learn a little of the Swedish language so that I could make an effort to converse with my kinsfolk. What little I acquired proved quite helpful during my visit. But like my father I have forgotten it completely.

One morning I took a steamer which made its tortuous way through many lakes and canals, stopping frequently to disembark passengers and freight. At first we passed islands or promontories with fine estates and noble architecture. I wondered if my aunt would be living in one of these grand houses and what she would be like. Gradually as we got farther from the city the houses became less ornate and smaller, the scenery more country-like. And finally we drew up to a small dock where my aunt was waiting to meet me. She picked me out at once and made herself known. I don't think I have ever had a warmer, more heart-felt welcome, nor myself had more immediate affection for anyone. She seemed at once to transfer to me all the love she had borne my father, and soon told me he had been her favorite brother, and I sensed at once I might become her favorite nephew.

We walked up the hill to her simple little cottage which she had surrounded with flowers and a small garden. After the evening meal she brought out all my father's letters which she had treasured through the years, even his first to his mother when he went to sea at sixteen, telling how he had saved his money to buy a heavy sweater. She told me how my father, after finishing the country schools, had lived in Stockholm with relatives and thus received his further education, quite beyond that of his home schools.

After spending the night I went with my aunt back to Stockholm to the home of her son Carl, my cousin. But my aunt was not content for me to stay there and got me quartered across the street. I think she feared I might be corrupted. My cousin had the reputation, I learned, of occasionally or frequently imbibing too much. But he behaved well while I was there. His wife and cousin who lived with them were both likable and cordial. After a few days my aunt bade me farewell and returned home. I never saw her again. But we exchanged letters until her death a few years after my visit.

After the usual tourist sights of Stockholm I went to visit one of my uncles, who had a substantial farm at some distance in the country. I spent one night there and was glad to become acquainted with him, his wife, and two daughters. The younger one (twenty-one) was particularly charming and made it rather plain she would have no objection to living in the United States. But I cautiously made no overtures. She married soon after and I recently had a letter from her son which I will refer to below. I did not visit my other uncle, who also had a farm—but at quite a distance from Stockholm.

After returning to the United States I kept up correspondence with my aunt until her death. I then lost contact with all my Swedish relatives until a few years ago I received a letter from the son of one of my cousins who said his mother had spoken of my visit to Sweden after seeing some account of me which gave my address. She had asked him to write me. It was a friendly letter giving me news of the family and requesting a reply. Promptly I wrote at some length telling of my wife, son and grandchildren and of my various activities since I was in Sweden. Having had no reply I suspect they had not known of my marriage and lost interest in me.

During my visit to Sweden I took advantage of the opportunity to make a side trip to Oslo (Christiania it was then called) and at least have a glimpse of Norway. Even in early September it was getting too cool for a farther northern trip to be pleasant. In the late afternoon in a public garden overlooking Oslo, a light blanket was supplied the guest so that he could wrap up and be comfortable while having tea.

The sight of the city from this height was very impressive, especially the extent of the large docks and the number of Norwegian and foreign vessels indicating the international character of their shipping.

The Norwegian architecture in its sturdy outlines without too much ornamentation gave one the impression of the strong and rugged background of its people. Visits to its library and public buildings, including its cathedral, showed a high degree of cultural development reaching back over many centuries.

I also took boat trips into some of the deep fjords surrounded by steep, towering mountains from which poured the streams that have since been harnessed to give Norway its stupendous supply of water power.

It was with regret that after a week I returned to Stockhold on my way back to Germany where I landed in the great port of Danzig and continued on through Berlin to Leipzig, back to finish my work in the University.

Bodenstein's specialty was gas kinetics. He had formerly done a brilliant piece of work under Professor Victor Meyer at Heidelberg on the thermal combination of hydrogen and iodine which was accepted for his doctor's thesis and has become an all-time classic. In the meantime it had been suggested that if one knew the rate of combination of hydrogen with each of the three halogens, iodine, bromine, and chlorine, and if each reaction proceeded by the same kinetic law, a relation similar to Ohm's law might exist between rate, chemical affinity, and "chemical resistance," later disproved as will be shown below. Bodenstein wished to complete the series and had already had a candidate work on hydrogen-bromine. He therefore assigned me the hydrogen-chlorine reaction to be attacked by his technique of introducing mixtures of the two gases into a series of hard glass tubes connected by narrow capillaries. After filling, the tubes were separated by sealing the capillaries. Each tube was then heated at a desired temperature for a given time and later opened under water to measure the amount of hydrogen chloride produced.

At once I found that it was impossible to duplicate results by this method. Two tubes, when filled and handled identically, might show rates of combination differing by as much as 12-fold. After a month's experiments we became convinced of their futility and Bodenstein asked me to repeat the hydrogen-bromine thermal synthesis of HBr. He did this because he was doubtful of the results of one of his former students, which had never been published except as a Ph.D. dissertation. The former data seemed to agree with Bodenstein's bimolecular formula within a given series but when the experiment was made at the same temperature with a different initial proportion of H_2 to Br_2, again a rate constant was attained, but not the same one as before. This did not make sense, so I was given the task of trying to straighten it out.

My first approach was to make a large supply of pure bromine by the Stas method consisting of preparing $KBrO_3$ by recrystallization, converting five-sixths of it to KBr by heating, adding the KBr to the remaining $KBrO_3$ and treating with 50% H_2SO_4 by dropping gradually onto the mixture and distilling to collect pure bromine. It is a slow process, and one day as Bodenstein stopped beside me to watch the liquid fall slowly in beautiful red drops, I grew impatient and facetiously exclaimed: "Well, if making beer were as slow as this, there would be great thirst in Germany!" Then, remembering that his father had made a fortune in brewing beer, I feared he might be offended. But instead, he patiently explained to me that brewing is a slow operation which takes at least six months to make a good product.

This reminds me of another time when Bodenstein came into the room where his private assistant, Dr. Kuhl, and I worked. And this visit did not turn out so well. Dr. Bodenstein had on a new spring suit, worn for the first time—very elegant, evidently quite expensive and one which he was wearing with some pride. He was not talking with me at all but with Kuhl, while standing near a gas burner above which I was heating a large beaker full of hot chromic acid, cleaning some of my hard-glass reaction bulbs for re-use. The hard-glass roughened tips had evidently scratched the bottom of the beaker and weakened it. Suddenly it collapsed and most of the hot, highly corrosive liquid landed on Bodenstein's new suit. This time he did not smile. He was not even patient—but railed at me as cause of the disaster until he realized I was not responsible and could not have known it would happen. After quickly dousing himself with water so he would not be burned, he went home in a taxi and showed up later for his morning lecture in a fine white suit. Mrs. Bodenstein told me later the acidified suit was collected in shreds from the bathtub—a complete loss. I did not feel my responsibility extended to the offer of more than sympathy—certainly not of a new suit, which I knew he could afford much better than I.

This incident recalls one in which I was more personally involved. One of our friends from the United States who owned one of the first autos even in Germany had invited Bray and me to drive with him down to Grimma to visit a friend. I was hurrying to finish a piece of work before getting off. I stood on top of the table to do some glass blowing and inadvertently placed the blast lamp so that it played on a thick glass cylinder containing 30% caustic soda. It immediately broke and spilled the caustic solution. This in itself was not serious, but in my haste I sprang to the floor into the pool of caustic, than which nothing is slicker. I came down on my seat in the caustic. Like Bodenstein I had to don a laboratory coat and ride home to change clothes for the trip to Grimma.

The trip itself was full of incidents. I think it was my first in an auto—certainly of that length, 100 miles round trip. I do not remember the make of the car. There can have been few like it—one cylinder—no fender nor mud-guards, as they were then most suitably called,—solid rubber tires, etc. We wore head gear but the dust was awful on the cobble roads. Fortunately the one cylinder continued to function and we made the fifty miles to Grimma in about two and a half hours. We repaired at once to the city swimming pool and were badly in need of a dip. I remember a diving tower with three stages. I confidently climbed to the topmost, thinking I could, as in my teens, dive from any height. But when I looked down at the pool so far below, my knees seemed to buckle. I tried in vain to get up courage to renew my youth, but finally settled for the lowest board, about 8 feet above the water. And today I shamelessly climb down the ladder and slip into the water without even wetting my head.

The trip back from Grimma began about midnight and took at least five hours. The chaussee had filled up with ox-drawn carts, plodding their slow way into Leipzig with the morning milk supply. The stolid oxen, although they had probably never seen an auto, were not the least frightened, and their drivers walking beside them were equally unconcerned. We had to thread our uncertain way among them in the dust which they and the carts stirred up, far worse than on the trip going down. We were glad to be nearing home as day broke and the sun appeared.

But to return to my work at the University, the intermediate examination was passed without difficulty. It was held in the office of Ostwald while he devoted himself to proofreading, turning only occasionally to interject a question between those of Luther, who confined his questions mostly to the published works by Ostwald. Incidentally, there were no written examinations in German universities. The preliminary and three finals of one hour each in three subjects, the major and two minors, were all oral. In course lectures no record was kept of attendance. One paid a fee for each lecture course, depending on the number of hours per week, and received a record which the lecturer signed, usually the Department head (on account of the fees), but he had no way of knowing in a large class whether you had ever attended a single lecture. It was assumed you would not be willing to cheat yourself by missing such fine lectures and experiments.

The final examination of one hour for the doctorate, after your thesis had been approved, was held in downtown Leipzig in a drab building having small rooms in which most exams were held. Professor Credner of geology examined me in his own office, although he adhered strictly to the old formality of a previous courtesy call to request the examination—with the candidate appearing in full regalia, tails, white tie, high hat, gloves and cane—all at 11:00 a.m. The Germans lacked the English and American prohibition of wearing full dress before evening. Ostwald and probably some of the other department heads examined two candidates at once, thus saving time and doubling their fees. I was examined by Ostwald, in company with a Polish student of chemistry. The first half of the examination was on subjects in which I was better prepared, but midway in the hour Ostwald switched to other subjects and the situation was reversed. The Pole had the better part, so we came out about even. Both of us passed but both I believe with *magna* instead of *summa cum laude*.

In my experimental thesis work I fared much better. Soon after I switched to the reaction of hydrogen and bromine I found that the key to the measurements depended on a more accurate method of measuring the reaction product hydrogen bromide, which on dissolving in water becomes hydrobromic acid. The acid content in each experiment was determined by titration with standard barium hydroxide, using phenolthalein as indicator of the end-point. However, if one allows carbon dioxide of the air to enter or remain in the dilute acid solution, it gives a vanishing and variable end-point. To prevent this I adopted the system of boiling the HBr solution for a few minutes to remove CO_2, closing the flask with a tube containing soda-lime to prevent reabsorption of CO_2 while cooling the flask in tap water, before carrying out the titration quickly without access of air. This gave sharp end-points and reproducible analyses. The experiments yielded data which gave the same velocity constant at a given temperature for all three types of gas mixtures: (1) excess hydrogen; (2) excess bromine; (3) equivalent mixture of the two.

The results, however, did not fit a bimolecular reaction formula, as is the case with the iodine-hydrogen reaction of Bodenstein. The rate is proportional to the hydrogen concentration, but not to that of the bromine, although they react in equal proportions. The square root of the bromine concentration fits kinetically and indicates that it is the Br atoms not the Br_2 molecules which react. Another anomaly differentiated the bromine reaction, namely, that the HBr formed retards or inhibits the reaction by a mechanism not interpreted at that time. Perhaps I may cite Professor Robert Livingston's recent opinion. "In this thesis he demonstrated that the formation of HBr from its elements is not, as was expected, a bimolecular reaction but conforms to a complicated rate equation containing a fraction exponent (½), an additive term, and two empirical constants. This work was completed in Leipzig in 1905, and now, fifty-four years later, his equation, including the values of the constants, still stand as essentially correct. It is quoted in every monograph on kinetics and in practically all texts of physical chemistry as "the classical example of a reaction whose kinetics are at once complex and understandable." (Radiation Research, *10*, 605 (June, 1959)). Further association with Bodenstein in Minneapolis and in Rome is cited in Chapter 11.

As to the city of Leipzig itself, how sad the changes that came at the end of the Second World War. It was assigned to Russian domination which still exists. But I cannot imagine that its sturdy and independent citizens are happy under Communistic regime. The University is also affected. The notorious Klaus Fuchs, who betrayed the United States and its allies was unbelievably given only twelve years imprisonment in England. Upon completing his sentence, a position was open for him in the University of Leipzig where his father is also a Professor. The former Ostwald Laboratory is no longer sought by physical chemists from all over the world and can accept only Bolsheviks from within the realm of communism. How long this deplorable situation will last no one can know, but I do not doubt but that some day the worthy Saxons will arise and cast out the treacherous invaders.

CHAPTER 5

THE UNIVERSITY OF MICHIGAN

Soon after I had received the Ph.D. degree at the University of Leipzig I was recommended by the Massachusetts Institute of Technology for an Instructorship in chemistry at the University of Michigan. I gladly accepted the appointment at this well-known institution, although it was made for but one year at the standard small salary of $900, but not to include summer school salary. Appointments to instructorships were reviewed each year for the first three years, and if renewed were at the same salary. On continuation beyond three years, the salary was then increased by $100 per year to a maximum of $1200, where it remained until the University should decide to extend permanent tenure by appointment to an Assistant Professorship at $1600 per annum. I left before attaining to that high honor and little thought that thirteen years later I would be offered and decline the headship of the Chemistry Department.

Compensation for work in the Summer School was equal to about one-fourth the annual salary. The head of the division of physical chemistry, Professor S. Lawrence Bigelow, also a Leipzig doctor, was a man of means who did not care to work in summer. Consequently I was permitted to teach in summer but did not draw my salary, in order to take advantage of an unusual and most beneficial policy. At the end of four summers if one left his summer pay with the University, he would be given a year's leave of absence at his then annual salary so that he might take postgraduate study at any university of his choice. I refer later in Chapter 6 to my selection. I have always regarded this as a most wise policy and hope it is still in force.

The life of a young faculty man in Ann Arbor in the early part of the century had many attractions. Most of the faculty men in the higher ranks had come from the eastern states and had brought with them urbane standards of life and society. High hats and frock coats were *de rigeur* for Sunday afternoon calls on the married families or daughters. Families which did not entertain in sufficient style to be thought worthy of such calls were not considered to have highest social rank and accordingly were neglected by the snooty young bachelors.

A faculty bachelor's club, the "Apostles," so named by President Angell's wife for originally having been twelve, had, by my time, expanded to eighteen. Election could be made only to fill vacancies. I was nominated by my fellow Instructor of Chemistry, William J. Hale, a Harvard Ph.D. Upon election in the fall of 1905 I took a bedroom with adjoining study in the house where all the Club members had meals, though only one other, Dr. Elmer Butterfield, lived in the house. Not until later did the Apostles attain to their own house on Hill Street, where I lived to the end of my stay in Ann Arbor. The Club idea became quite popular and two similar Clubs were organized to care for the numerous faculty bachelors. The Apostles later moved to a yet larger house and were still vigorous when I last visited Ann Arbor. Later, however, the bachelor clubs at Michigan folded up. I believe none exists now. The reasons were: earlier appointment to positions with higher salaries, and consequent increasing ability to marry and establish private homes.

My friend Hale, while still at Michigan, married Margaret Dow, the daughter of Herbert Dow, founder of the Dow Chemical Company of Midland, Michigan. Two years after their marriage his wife succumbed in the flu epidemic and left him with a daughter one year old. In order that the baby, the first granddaughter of the Dows, might be near the family and be cared for by his mother, Hale moved to Midland where he made his home until he died there in 1955 at the age of eighty.

Hale, after identifying himself with the Dow Chemical Company as Director of Research, made two great contributions to its success. The company had been founded at Midland by Herbert Dow in 1897 on a slim financial basis. The Midland location was chosen because of the high iodine content of the salt brines underlying that region. As a student at the Case School of Applied Science, Dow had become interested in the Ohio brines as a source of iodine, but found them too low in iodine for commercial exploitation. What little financial support Dow had initially came from his Cleveland friends, who later profited greatly as the company prospered and widely expanded. But it was not until World War I that the company really found itself and moved into large production. Up to the time of Hale's advent the company had produced *inorganic* materials only. Hale, himself an organic chemist, convinced Dow that he should enter the *organic* field. With Hale as Director of Research, and later with the able assistance of Edgar Britton (recently deceased), whom he had trained at the University of Michigan, the work of the Dow Company expanded into the great success we know it to be today. Hale's second contribution, or perhaps the first in chronological order, was to remind Dow that they could not have good research without a good library. He was sent to Europe to assemble all the material necessary to found an adequate scientific library.

Hale never remarried and spent most of his time at his home in Midland. After his daughter Ruth married and moved to Washington, Hale furnished a room for himself in the Cosmos Club so he could occasionally be near his family and his grandchildren.

But in following Hale, I have wandered away from my life in Ann Arbor. I was there from 1905 to 1913 except for the one year spent abroad which will be treated in the next two chapters. I carried on some research and published a few papers. I supervised laboratory work in General Chemistry and had charge of the laboratory of Physical Chemistry under Professor Bigelow. I studied the propagation of gas flames and attempted to stop them electrically as the explosion wave passed through an electrostatic field. The results at first looked promising but on refinement of method proved to be spurious and were never published in full, though some reference to their negative character was made in another connection. I did not regret the undertaking however, as I learned much from it. Using a different gas mixture someone else *was later able to retard* explosive waves in an electrostatic field.

One incident that occurred in Ann Arbor I shall mention though the main facts leading up to it will be related in the following chapter. On returning from Paris and Vienna where I had begun work on the chemical effects of ionizing radiation I had no radium available at Michigan to continue my experiments. I therefore turned to the literature and collected all the published data from various sources that had a bearing on the subject. I then developed methods of calculating the quantity of ionization involved in each reaction—a very arduous undertaking—and was surprised and pleased by the agreement between the number of ion pairs and the number of reacting molecules involved in many reactions of very variable character. I concluded that this meant a fundamental relation between the two and evolved a theory for the reaction mechanism. When finished, the paper was quite lengthy but impressed me so favorably that I thought it worthy to be published in a foreign journal and accordingly sent it to the Philosophical Magazine in England. To my chagrin the manuscript was promptly returned with a one-sentence note of rejection. The editor deigned no explanation. I do not remember his name. He has doubtless long ago passed to another world. If in heaven I hope he has repented. If in the other place, he may deserve it.

Being of the opinion—and I still am—that the paper was good and that it pioneered in a future field of importance, I submitted it, where it should have gone originally, to the Journal of Physical Chemistry, under the editorship of Professor Wilder D. Bancroft of Cornell University. It was accepted at once without change and was published in 1912 (J. Phys. Chem., *16*, 554-613).

The theory of ion-molecule reactions is based on the observation that when chemical reaction between molecular species is brought about by application of an ionizing agent a definite relation exists between the number of molecules (M) reacting and the number of ions (N) produced. The simplest case ($M/N=2$) is found in a saturated gaseous hydrocarbon like methane where the charged molecule CH_4^+ reacts with neutral CH_4 to form ethane and eliminate hydrogen ($CH_4^+ + CH_4 = C_2H_6 + H_2^+$). Since CH_4 has no affinity for the free electrons liberated when CH_4^+ is initially formed, the only function of the electrons is to reestablish electrical neutrality by combining with H_2^+ ($H_2^+ + e^- + M = H_2 + M$) (where M is any neutral molecule in a three-body collision), so that the net result is the formation of ethane and hydrogen in equal molecular quantity and the disappearance of two methane molecules. But if one component (e.g., O_2) has affinity for free electrons a negative ion (O_2^-) is formed which also reacts and enhances the ion yield beyond that from the positive CH_4^+. Chain reactions may also result, in which case M/N may become very large before termination of reaction by ion neutralization (e.g., in $H_2 + Cl_2 \rightarrow 2HCl$). (See details in Chapter 8).

I then made efforts to induce some of the philanthropists in the East who had supported various scientific institutes to found a Radium Institute in the United States and supply it with radium which then had to be purchased from the Armet-de Lisle Company in Paris. Of course I would have been willing to act as Director, though I believe I did not mention it. But I did emphasize that I wanted an opportunity to work with radium. These efforts had no success and I was forced to look elsewhere (see Chapter 10).

As a bachelor while living in Ann Arbor I had time during vacations for outdoor recreation, golf and canoeing being my favorites. I persuaded the University librarian, John Koch, a fellow Apostle, perhaps somewhat corpulent, to take up golf. One Sunday morning as we were playing, Koch had an embarrassing accident. As he bent over to place his ball on the tee his tight trousers could not stand the strain and gave way where needed most. This did not interfere with our game. But as we were walking back to town (in the days before automobiles) we met many pious people returning from church. To save Koch from disgrace and the good people from shock I had to walk in lock step close behind him until we got home.

In canoeing I made summer trips of two or three weeks in the streams and lakes of Ontario north of Toronto with companions from the University. In the summer of 1909 we embarked about 120 miles north of Toronto, outfitted at a Canadian Hudson Bay post and made a two weeks trip down a beautiful river to a terminal lake on the railway without ever seeing another party. We were perhaps the only ones that made that trip during the entire summer. I managed to keep us supplied with bass by trolling en route or by getting up early or fishing late after camp supper. The other men did not care for fishing, although they enjoyed eating my catches. Bass were abundant in these remote, seldom fished wilds.

On the canoe trips in Canada I preferred to run the rapids if at all safe, rather than make a portage. But in the wilds one could not risk losing luggage and food supplies. My companions usually urged portaging. On one occasion they insisted on portaging their duffle bags with clothes and food while I assayed the canoe run after having assured myself of its safety. In five minutes or less I had run down to the agreed meeting place and waited hours for them to arrive, plodding slowly in the heat with their heavy burdens. This also avoided a double portage to carry the canoe. Another time we reached the head of swift rapids and high falls just before dark. Scouting disclosed the only camp site just above the falls but beyond the rapids. The problem was to be able to shoot the rapids and stop at the camp site without going on over the falls which would have been disastrous. Owing to approaching darkness and need for haste we all three embarked at once with full luggage. The load weighted the canoe so low that it was difficult to steer and liable to tip and take water. We shipped water several times and just made the landing above the falls with water nearly up to the gunwales in the canoe.

But canoeing was not always in the wilds. A favored trip was to ship a canoe up by rail to a lake, take the train up and return down the Huron River to Ann Arbor. This required a full day. If skillful one could shoot some rapids or small falls on the way. But usually we made portages—always when girls were in the party.

Also, short afternoon trips on the Huron River above Ann Arbor were pleasant. One afternoon I was returning from one with my friend Phil Bursley when we encountered an overturned canoe and two ladies standing in water more than waist deep. In attempting to change seats they had upset. Fortunately the current was not swift and we managed to rescue the ladies and get them into their canoe again. That evening in the reception line for newcomers to the University Faculty it was amusing to find one of the two ladies from the overturned canoe—a recruit for the women's athletic department. In being introduced I could not refrain from saying, "I believe we have met before."

But I must not wander away from Ann Arbor in a canoe. Forty miles west of Detroit, it was in 1905 well known as the seat of the University of Michigan, founded in 1837, which became the first of the midwestern universities to attain rank and distinction equal to that of the larger eastern institutions of learning. The population grew to thirty or forty thousand. Former residents of Detroit began to maintain residence in Ann Arbor, while commuting daily between the two. The University grew to thirty or forty thousand students, especially numerous immediately after the two wars when students returned from military service to resume their studies. Today the University of Michigan maintains its high standing, but many other midwest universities, including its own sister institution at Lansing are its rivals in many respects.

As I look back over my early days as Chemistry Instructor at the University of Michigan, just entering a half century of teaching and research, I long for return of the youthful enthusiasm and inspiration that I felt. My contacts with beginners in freshman chemistry laboratory were pleasant and gave me the opportunity to see the extent of their preparation and feel that I was accomplishing something worthwhile in introducing to them the technique of experimentation. For many of them the course was only a required subject under a discipline that had not too much to do with chemistry, but its training and exact treatment were of benefit to them nevertheless.

I also had constant contact with my fellow chemists and with other faculty members from many different fields of learning. Neither academically nor socially were the penurious young instructors high hatted by the more advanced faculty members. If one possessed high hat, frock coat and cane, all doors were opened to him, especially if he were a member of the élite Apostles Club.

Eight weeks of Summer School still left enough time for vacation and travel. I have already mentioned canoe trips and bass fishing. In my time, trout were to be had in Michigan only by visit to the North Peninsula. The famous Au Sable and other former trout streams in the Southern Peninsula were no longer productive.

Michigan for me was a good beginning. Although I never attained rank higher than Instructor, I had enough time for study and research and earned the privilege of a year's leave which I spent in Europe as described in the next two chapters. By the time of my return I had begun to get my teeth sharpened and to bite into the subject of *radiation chemistry* which became the main theme of most of my later work.

CHAPTER 6

MADAME CURIE'S LABORATORY, PARIS

While an Instructor of Chemistry at the University of Michigan I became convinced that radioactivity would surely become a field of increasing importance and that I should gain some firsthand knowledge of it. I therefore decided to apply to Madame Curie for admittance to her laboratory in Paris. I was fortunate in being accepted and in having a year of earned leave making me free to work there.

I spent the summer of 1910 in France studying the language, as I had done in Germany, in preparation for lectures and laboratory work in the fall. To have an opportunity of speaking French with as many different people as possible I decided to go to one of the smaller resorts on the North Sea coast where I could afford to stay in one of the beach hotels. I chose St. Valery en Caux, a small resort about fifty miles from Dieppe. This choice proved to be fortunate. A school of French for foreigners was located there and I could associate with many students having the same object as mine. Through them I also met many people at the Casino in the evenings where I could hear French.

My day at St. Valery consisted of a light breakfast in bed (*café au lait* and a roll), study for several hours, a dip in the ocean just before noon, lunch at the hotel where I learned to enjoy the cider of Normandy, native to that region. A postprandial nap prepared me for the rest of the afternoon at the beach where I wandered about, talking French with as many different acquaintances as possible, so that I would not inflict my poor French on any one group too long.

This was a pleasant and profitable way of spending the summer in preparation for the Curie Laboratory, which I entered in October. Some of the laboratory rules were rather onerous, not due to Madame Curie nor Professor Debierne but imposed by the University itself. The laboratory closed at 7:00 p.m.—no work evenings nor holidays—and also closed for lunch from 12:00-1:30. One could remain, if he wished, locked in. I often did remain but some of the men did not enter until after lunch and then worked through until seven.

At that time (1910) the Curie Laboratory was not in its present location, but occupied part of an apartment building at 12 rue Cuvier. The laboratory consisted of about a dozen research rooms scattered over the ground floor, including a small shop and library. (The étages above were rented as private apartments with no relation to the laboratory below.) Only workers already having the Ph.D. degree were accepted. Madame Curie interviewed me in the little library and advised me to take a course of laboratory training in radioactivity from her first assistant, Dr. Debierne. This I did and at the same time attended Madame Curie's lectures on radioactivity in the Sorbonne. Her lectures were most interesting in tracing the history of the discovery of radium and polonium by herself and her late husband, Pierre Curie, and their subsequent studies of them. As was the custom for lectures by one of great distinction, her first few lectures were attended in her honor by many other scientists of high, established rank. The number dropped soon to the regular auditors of her course in Radioactivity.

During my stay in her laboratory, Madame Curie was not greatly in evidence. Two things keep her engaged in the apartment she had rented in Paris where she spent the week days before returning to her home and two daughters in a suburb. She was occupied in writing her two volume treatise, "Traité de Radioactivité" (Gauthier-Villar, Paris, 1910), and in making a candidacy for election to a vacancy in the Academy of Sciences.

The latter she had undertaken only upon persuasion of her friends, somewhat against her own will. No woman had ever been elected to the Academy of Science. It was customary and almost obligatory for the candidate to call upon each of the forty members to solicit their votes. This was not only time consuming but personally embarrassing to Madame Curie. But she undertook it in good faith and received much encouragement. It appeared that a safe majority of chemists would support her and the question of admitting women members was put up to the Institute, consisting of the five Academies. The Institute reiterated its opposition to women as members but at the same time agreed that each Academy should be free to make its own choice. The Academy of Science then indicated it would probably give Madame Curie a majority vote nevertheless. She then proceeded with her candidacy and it appeared a foregone conclusion she would be elected. The Paris Sunday papers ran full page accounts of her and of her accomplishments. This continued until about two weeks before the election when sudden opposition developed—because she was foreign born (Polish), because she was a woman, because she was falsely designated as Jewish, and perhaps for other reasons.

At the Laboratory we felt so sure she would be elected that we provided flowers to present with our congratulations. But to our great astonishment and disappointment she was defeated by one vote. A Frenchman of much less distinction was elected instead. Our head mechanic hid the flowers behind his lathe. Madame Curie never consented to make a second candidacy for the Academy of Science but later was elected to the Academy of Medicine for her valiant work in World War I in aiding victims of exposure to radiation.

To begin my work in radioactivity I took a laboratory course in measurements under Professor Debierne and became associated with Professor William Duane in the collection, purification, and measurement of radon from the laboratory solution of radium salt. Professor Duane, on leave from the University of Colorado, had worked one year some time earlier in the Curie Laboratory and had been instrumental in securing for it a donation from the Carnegie foundation, which was recognized by a plaque in the entrance to the Sorbonne. As a consequence, Professor Duane was given a prolonged research appointment under the fund so that he might continue work in the Curie Laboratory. As fellow countrymen we became associated, as already mentioned.

One project in which we cooperated was the preparation of a thin-wall glass container of radon which would transmit alpha particles efficiently. Duane had been using an organic binder to seal a minute cylinder (< 5 mm^3) with a thin mica window that transmitted alpha rays from radon within this container. Due to the transmitted alpha radiation, the odor of ozone was intense in the air immediately in front of the window. (It was this ozone that attracted me first and at once to the study of gas reactions under radiation.) But inevitably the alpha rays attacked the binder and produced internal gas pressure that always blew the window off overnight.

Rutherford and Royds had previously described a research in which they used radon confined in glass capillary tubes of the required thinness to transmit α-rays, but had given no description of their preparation, which is not an ordinary glass-blowing operation. About this time a French student returned from a visit to Rutherford's laboratory and informed us that the thin capillaries had been made by a commercial glass blower by his secret process not imparted to Rutherford. But a student had spied and reported that the work was done inside a glass tube as heat shield and by using enhance pressure for blowing.

With this meagre information Duane and I tried to make thin-walled glass containers. We mistakenly supposed the outer shielding tube had been heated and drawn out at the same time as the inner one. This was not successful. So we then conceived that the outer tube of harder glass was used merely as a *stationary* shield, open at both ends so that the soft glass tube could be heated, and drawn down inside the shield to the necessary thinness, while using just enough pressure to prevent collapse of the thin capillary during drawing at the temperature of the oxygen flame, reduced by the fused quartz shield.

It happened that I wanted thin glass *spheres* about 1 mm in diameter, to transmit alpha rays equally in all directions, so I could calculate ionization of oxygen in the production of ozone. Accordingly we drew a thin capillary, inside the quartz shield, to as fine diameter as would transmit air pressure and thus expand a small sphere at the tip of the sealed capillary. I soon obtained thin-walled spheres with wall equivalent in alpha ray absorption to about 1 cm of air. These thin glass spheres proved later to be very effective in studying ozonization of oxygen. Upon exhibiting the first specimen to Dr. Debierne, he admiringly pressed it not too gently between his finger and thumb in testing its strength. Of course, to his embarrassment, it collapsed. But with the process established we were soon able to make others at will.

In the Curie Laboratory I also made some experiments in the hydrogen plus bromine gas system, and on hydrogen bromide, both gaseous and liquid, using both β-γ and α-irradiation. The penetrating (β-γ) radiation caused no detectable change in either HBr or $H_2 + Br_2$ in 37 days irradiation from 200 millicuries of radium chloride. The synthesis of HBr from its elements was effected by radon giving an ion-molecule yield of 0.54, in agreement with Gillerot's later result (Bull. soc. chim.

Belg., *39,* 503 (1930)) at the University of Louvain. At 303°C, where the thermal synthesis of HBr from its elements is very rapid, I found radon produced no detectable enhancement, in contrast with the photochemical effect. Liquid HBr was decomposed to the extent of about 2% by 10 mc of radon and 1 cm^3 volume in 14 days. The energy required to decompose that quantity of liquid HBr is about 3.5% of the total energy of the radioactive radiation. Experiments in aqueous solutions of HBr and of KI, both with penetrating (β-γ) radiation from 200 mgs $RaCl_2$, and with α-radiation from 5 mc of radon per cm^3, disclosed decomposition of both, much greater with α-radiation than with β-γ, and greater in acid than in neutral solution.

The proposed study of ozonization of oxygen required the α-rays of radon and of its decay products (RaA and RaC′), since the absorption of penetrating (β-γ) rays by gas is too slight. Owing to the heavy demand for radon in the Curie Laboratory I could be supplied only about once a month. On learning that the new Institute for Radium Research in Vienna was well supplied with radium, I applied for admission there and was accepted with the assurance that an adequate quantity of radium would be reserved for my exclusive use. Therefore, with many regrets, I left the Curie Laboratory about Easter time in the spring of 1911 and transferred to Vienna.

I cannot express too strongly my indebtedness to the Curie Laboratory for my cordial reception and for the training I got there in about seven months. But in my efforts to learn to measure the intensity of radiation I never mastered the manipulation of the piezoelectrique instrument that had been devised by P. Curie in his studies of piezoelectricity. It requires practice in the gradual lifting of weights of different magnitude suspended from a quartz crystal in a certain axial direction so that the tension shall just counterbalance the discharge due to the piezo-electric effect, thus keeping the indicator at exactly zero during the entire operation of some seconds or a few minutes, depending on the weight and the radiation intensity being measured. To the uninitiated, my description may be as difficult as the operation of the measurement itself. My timing of the lift was never exact enough, running ahead or behind without control. This is a criticism of myself, not of the instrument or its proper operation. But others (including Dr. Elizabeth Rona) also had difficulties in its use. Although the Curies and their assistants, however, used it with excellent accuracy in all of their work. But I later preferred a gold or silver leaf electroscope, calibrated with known sources of radioactivity and have used this in all of my subsequent research. But in spite of my difficulty with the piezoelectrique the ability to collect and handle the gas radon from radium solution, which I studied together with Professor Duane, has been of the greatest benefit to me subsequently and I trust that my work in preparing standard radium salts and solutions has helped other workers in this field. (See Chapter 8 on "Colorado").

CHAPTER 7

The Radium Institute in Vienna

The discovery of large deposits of pitchblende in Czecho-Slovakia, which was then that part of the Austro-Hungarian empire, known as Bohemia, made Vienna an early center of radioactive research. It was in the old pitchblende residues supplied from Vienna that the Curies first discovered both polonium and radium, descended from uranium. An Austrian philanthropist named Kuppelwieser donated a fund to the Vienna Academy of Science to found and support the Institute for Radium Research which, as property of the Academy, was turned over to the Department of Physics of the University of Vienna for administrative operation. Profesor Stefan Meyer became its first Director. It was by him I was received as visiting scientist.

Meyer was supported by an able staff. Victor Hess, later awarded the Nobel Prize for the first discovery of cosmic rays in the higher atmosphere, was Assistant Director. Fritz Paneth, Otto Hönigschmid, and Karl Przibram were also there during the few months I spent in Vienna from Easter to August in 1911. Meyer received me most cordially and immediately gave me a key to the Institute for use at all times, and a letter to all instrument makers and glass-blowers in Vienna so that I could place orders at will. Materials were delivered promptly directly to my desk, which greatly facilitated my work on ozone production.

My work on the production of ozone by the alpha rays of radon and its decay products was taken up at once on my reaching Vienna. Radon, after collection and purification by the method of Duane, was confined precisely in a Lind thin-wall alpha-ray sphere about 1 mm in diameter and with a wall thickness equivalent to about 1 cm of air, leaving the longest alpha rays a path of about 6 cms in oxygen in all directions except where the thicker tip and stem intervened. This source was then placed at the center of a glass sphere a little over 6 cms in radius. A stream of oxygen was ozonized on passing slowly through the large sphere containing the central alpha ray source. The ozone was measured by absorption in potassium iodide solution to liberate iodine, which was titrated with 1/200th normal thiosulfate solution. The resulting ozone showed a maximum yield of two molecules per ion pair which might be represented by: $O_2 + (\alpha) \rightarrow O_2^+ + e^-$; $O_2^+ + e^- \rightarrow O + O$; $O + O_2 + M \rightarrow O_3 + M$, or some other suitable mechanism. The yield was rather variable, increasing with the rate of flow, and highly dependent on the oxygen pressure. The reverse reaction, ozone decomposition, a long-chain reaction as later shown by Bernard Lewis, doubtless lowers ozone synthesis, even at the low concentrations of ozone attained.

Hönigschmid, then unmarried, was my closest associate. He had worked a year as post-Ph.D. graduate under Theodore Richards at Harvard where he studied the methods of exact atomic weight determination of which Richards was the world's master. Hönigschmid was interested in America and Americans. We usually lunched together and in the unusually hot summer months spent much holiday time at the beach on the Danube. The beach had three sections, one for males, one for females, and one for members of both sexes admitted together at the entrance. If girls or men wished to enter the mixed bathing they could wait until some one of the opposite sex arrived with the same desire and form couples or parties, which might dissociate as soon as entrance was gained. Hönigschmid and I sometimes availed ourselves of this opportunity. A large well-supplied restaurant served bathers—in their bathing suits if they wished.

Hönigschmid continued assidiously his work on the purification of radium salts, the atomic weight of radium and the establishment of large standards which were found to agree closely with the Curie official standard. He later married and became Professor in the University of Munich, where he continued his research for many years. After the Second World War when Munich was being occupied, Hönigschmid and his wife both committed suicide rather than give up their apartment to the enemy, which I regret was said to be American. The largest of the old Vienna standards (about 600 mc) was recently opened safely for fear it had developed a dangerous gas pressure.

After World War I, Paneth moved to England where he was well received and enabled to carry on research until his death. He became an authority on the helium content of meteorites and the determination of their ages. Professor Przibram remained in Vienna and continued research there, particularly in the coloring of salts or minerals and their radio-photoluminescence.

Professor Meyer and his family left Vienna during World War I, and retired to Bad Ischl, where his family had its ancestral seat. Both Meyer and his wife came of wealthy families but both were hard struck by the War. One evening in Minneapolis some years ago I received a telephone call from a young man who said he had just returned from his military service in Austria and brought me greetings and a note from Professor Meyer. The young man kindly accepted my invitation to give me an account of his stay in Bad Ischl. His regiment had been quartered there and he was living with other young officers in the home of Meyer, who with his family had moved into his "Hinternhaus" formerly occupied by the family servants. He had seen Meyer often, but more frequently talked with his daughter, Agatha, who spoke English more fluently. Meyer's note was on a small scrap of paper, showing how he and Austria had been impoverished by the war. But the note was cheerful and uncomplaining. While American troops lived in Meyer's house, their senior officers were living in the former home of Johann Strauss across the street. Agatha Meyer kept up correspondence with some of the American officers.

Upon my completion of the work on ozone in August it was nearly time for the meeting of the British Association for the Advancement of Science in England, where I was to meet Dr. G. N. Lewis. I regretfully left Vienna and have never had the good fortune to return nor of seeing again any of the friends I met there except Professor Hess, who came to America during World War I and was employed by the U. S. Radium Corporation. He later returned to Austria, only to encounter trouble and deprivations in World War II. He then returned to New York permanently and became Professor and head of Physics at Fordham University, where he remained.

The Institute for Radium Research in Vienna has continued its activities and publications. Its present Director, Dr. Berta Karlik, succeeded Profesor Meyer upon his retirement. She has been in the United States several times, and has been the guest of Dr. Elizabeth Rona of the Oak Ridge Institute of Nuclear Studies. The publications emanating now from the Institute show it is recovering its position among the centers of radioactive research.

The Institute of Radium Research was at the time of my visit one of the three great centers of research in radioactivity in Europe, the others being the laboratories of Professor Rutherford in England and of Madame Curie in Paris. Under the able direction of Professor Meyer the Institute attracted a small but brilliant group of research workers, but later (during World War I) lost them all but Prizbram. Fortunately they were all able to continue their researches in their adopted countries to which they managed to escape, Hess in the United States, Paneth in England, Hönigschmid in Germany. One American was trapped in Vienna for the duration but was able to obtain permission to carry on his work in the Institute.

The city of Vienna has always been one of the most attractive in Europe, perhaps next to Paris both in beauty and popularity. Its cathedral and other fine buildings, its layout of avenues and boulevards in the inner and outer city, its old walls remaining from feudal times, all add to its physical attractions. But these external beauties were not exceeded by the contributions from the cultural side, its superb composers, exquisite music both classical and popular, its unexcelled symphony orchestra. That Vienna is today more or less free from the domination of Communism is due to the insistence of its people on retaining their time-tested way of life. Of course the Austro-Hungarian Empire is a thing of the past but Austria and Vienna are themselves better off in being free of the shakles binding them to their former associates.

CHAPTER 8

CARNOTITE IN COLORADO

Early in 1913, while I was still at the University of Michigan, my attention was called to a U. S. Civil Service announcement of an "unassembled" examination for chemists with some knowledge of radioactivity to work in the newly established U. S. Bureau of Mines laboratory in Denver on the recovery of radium from Colorado carnotite. The opportunity seemed so attractive that I immediately applied and filled out the examination papers, which consisted only of one's record of education and experience. My hope was to gain access to a supply of radium to continue my researches in radiation chemistry using radon as source of alpha rays.

Fortunately my training abroad was given a high rating so that I ranked a few points above the next applicant. I think R. B. Moore, in charge of the work in Denver, would have preferred the second man, who had worked under him at the University of Missouri, and in whom he had well justified confidence. Moore could have passed over me, since the law allowed appointment of any one of the first three ranking applicants. Instead, he consulted his chief in Washington, Dr. Charles A. Parsons, Head of the Division of Chemistry of the Bureau of Mines. Parsons invited me to Washington. I was especially glad of this opportunity to meet him, already the Secretary of the American Chemical Society and possessing an early vision of building it into the great institution he later made it. Parsons then (1913) had but one secretary for the entire Society, and its activities and records were confined to two rooms (rent free) in the Bureau of Mines. Later, on giving up his position in the Bureau of Mines in order to devote full time to the Society, he rented a suite in an office building near the White House, before expanding to an entire floor in the same building. Compare this modest beginning with its present status representing a membership of about one hundred thousand under Parson's trainee and direct successor, Dr. Alden H. Emery. Parsons received me well and after going carefully into my qualifications decided to give me the appointment, to begin at the close of my academic year at the University of Michigan. This gave me some time to familiarize myself with carnotite and to practice uranium and vanadium analysis.

I arrived in Denver on my 34th birthday in 1913. After a cordial welcome from Moore I was introduced to the staff. I remember that Tracy Mulligan, the Chief Clerk, as he was officially designated, raised his eyebrows and twitted me when he noticed I had arrived with a golf bag. Evidently he thought me an inveterate playboy. We became good friends later. He found I confined my golf to Saturday afternoons and Sundays.

My closest associate in our Denver work was a graduate of the University of New Hampshire (under Parsons), Charles Whittemore, an excellent chemist who some years later worked for one of the radium-producing companies where he must have had too heavy exposure. Afterwards he was employed on other work but succumbed to a malignancy believed to have been induced by his earlier work with radium and mesothorium, which latter is even more hazardous. There was, of course, no certainty that his ailment was caused by radioactivity. None of our other scientists or plant workmen was ever affected by it, as far as we knew.

The principal objective of the Denver laboratory was to investigate carnotite (a potassium uranium vanadate deposited in sandstone) as source of radium, vanadium, and uranium, the three valuable constituents. Genetically, radium should occur in any uranium-bearing mineral in the proportion: 1 part radium to 3,000,000 parts uranium by weight. To verify this we analyzed about 30 specimens of carnotite from various deposits in Colorado and Utah. In all samples from lots of several tons of ore we verified the constancy of the genetic Ra:U ratio and gave it a slightly different value. This constancy was commercially important because carnotite, previously sold abroad for its radium, had been subjected to a penalty of 10%, on the claim that its radium content was that much below the genetic equilibrium with its parent uranium. All radium ore was bought and sold by the uranium content because it could be determined by direct chemical analysis as radium could not be.

In small hand specimens of ore, however, we found *varying* ratios of radium to uranium, higher than equilibrium in some, lower in others. This we attributed to weathering—removal of radium from one part of the ore bed, thus lowering the ratio, and depositing it in another part, so as to raise it. But when large portions of the ore bed were removed, thoroughly mixed, and correctly sampled, the ratio was *normal*, as we found it. The 10% penalty was accordingly discontinued, to the immediate advantage of American producers.

Whittemore was an excellent analyst and I had practiced on the analysis of carnotite at the University of Michigan while waiting for the expiration of the academic year in June when I would report to Denver. We therefore had considerable confidence in our analyses and were surprised when a sample representing several tons of ore showed a radium content some 30% below normal. Karl Kithil, the engineer in charge of the mining and purchase of ore, also became suspicious and examined the sample under the microscope. He was able to pick out several crystals of *uranium phosphate*, a yellow substance resembling carnotite in color, which had evidently been added to raise the uranium content and thus give a spurious radium value. We also confirmed this by direct radium analysis. Upon being confronted, the seller confessed the fraud and was permitted to make up the difference in good ore. I do not know why he was not prosecuted for fraud, but imagine the Bureau wished to obtain more uranium ore from him, now that he knew he must be honest.

Carnotite was not acceptable at that time with less than 2% uranium oxide, because of the higher cost of extracting radium and vanadium from lower grade ore. Vanadium had ready sale for alloying in steel, but had no genetic relationship to uranium, hence no constant ratio. Our ore usually carried about twice the percent of vanadium as of uranium. At that time uranium itself had little value and could be sold only in small lots for coloring glass and silk. Nevertheless we thought it too rare to be discarded and recovered all of ours (about 30 tons) after extracting the radium and vanadium and converting it to the black oxide UO_2. I shall refer to it again.

After confirming the value of Colorado carnotite as a source of radium, the Bureau of Mines found two men interested in radium as a therapeutic agent. Dr. Howard Kelly of the Johns Hopkins University Medical School wanted it for his cancer clinic on Eutaw Place, Baltimore, and Dr. James Douglas, President of the Phelps-Dodge copper mining company wished to make a gift to the Memorial Hospital in New York City, which he had established in honor of a daughter who had died of cancer. These two men organized a corporation known as the Radium Institute and arranged with the Bureau of Mines to produce radium for them under a contract by which they would meet all the expenses of mining and production except the salaries of the regular employees of the Bureau. The initial amount of the contract ($150,000) would today seem, and be, quite inadequate for such an undertaking. Nevertheless, a small plant for grinding and treating the ore was constructed in south Denver and put under direct supervision of Richard B. Moore reporting to Charles Parsons, in Washington. The Bureau of Mines was then under the direction of Joseph A. Holmes, who upon his death in 1915 was succeeded by Van H. Manning, who remained in charge during the remaining period of the contract.

This put the job of devising and operating a suitable process squarely up to the Denver staff. Sir William Ramsay had suggested a chemical process to Moore, by which carnotite should be treated by an acid that would dissolve radium, rather than by the much cheaper sulfuric acid that would precipitate insoluble radium sulfate, difficult to recover and treat to obtain radium. This meant either hydrochloric or nitric acid as extractant. We soon found that nitric acid was somewhat better. Unfortunate as this was on account of its higher cost, its better extracting power would justify the use, if we could recover the nitrate so as to use it again after regenerating the acid. This we learned to do, so that only 10% of nitric acid was lost in each cycle.

The first operation of extraction was carried out under my supervision using hot 30% nitric acid, in large earthenware pots, in the top of the Denver plant without any hoods or ventilation. Vast clouds of red fumes of nitric oxide poured out around us, but we had to continue without allowing the equipment to cool and thus lower the extraction. It was a miracle we were not all killed. Nitric fumes are very treacherous. They are not unpleasant, even sweetish at the time of inhaling them, but are busy nitrating the tissues of the throat and lungs nevertheless. I remember waking up in the middle of the night with an uneasy shortness of breath and some pain in my lungs. But this persisted only a few hours and next day I was all right. Needless to say we installed hoods, with strong draft over each pot, before beginning again and teaching the workmen to take over the operation.

And so it went on. As soon as we learned in the laboratory downtown how to do the next operation, we went out to the plant to install it and teach the men there to carry it on. But the plant extraction got ahead of us. We had accumulated acid extract and radium-barium sulfates from five carloads of ore before we knew the next step, the enrichment of the radium by separation from barium. Radium is chemically similar to barium to such an extent that their salts act as if they were isotopic—practically inseparable. This is good in the extraction, for the barium originally in the ore (or added) brings the radium out with it as nitrate. Then the problem is to separate them. It is like sending a dog in for the rabbit and then having to rescue the rabbit.

We wished to avoid fractionation by crystallization of the chlorides. Each step is long and excessively many are required. Also, we always added some barium in the extraction to ensure enough dogs for the rabbits. We sought an initial ratio of 1 million barium to one radium. We thought fractional *precipitation* as sulfate might be a quicker method of enrichment, though our avoidance of sulfuric acid as extractant should have taught us better. To a large amount of our radium-barium nitrate solution we added enough sulfuric acid to precipitate one-tenth of the barium in hopes of some separation. But our analysis quickly showed that the precipitate had exactly the same ratio of radium to barium as in the parent solution. We wasted no more time in this effort but quickly adopted fractional crystallization, which Madame Curie had devised and used. First at the plant by successive fractionations we brought the ratio down to 1000 barium to 1 radium, then at the laboratory to about 1% radium. Each day's collection was then sealed for a few days for radium measurement by γ-ray comparison against a radium standard. At the end of each month, all the daily collections were opened, thrown together, and in two or three days stepped up by an experienced crystallizer to any desired enrichment, usually about 50% radium. The month's radium was then divided into two equal halves, one for Baltimore, one for New York, but first to the Bureau of Standards for measurement, according to the contract.

Naturally we had stepped up some radium chloride to 100% and prepared our own unofficial standards, which were much higher in radium content and, we thought, superior to the U. S. Bureau of Standards secondary smaller standard. Our standards were necessarily made from ore bought by the Bureau of Mines outside the contract with the National Radium Institute. Also under the contract, our deliveries of *vanadium* had to be assayed by A. R. Ledoux of New York, who made excellent analyses.

The opening of a tube of the month's collection of high percent radium chloride, to divide between Baltimore and New York was ordinarily a simple operation. (Today the close contact and high exposure to radiation without shielding would not be tolerated). But on one occasion it was attended by unfortunate results. I was opening a glass tube containing 400 mgs of radium element as high grade chloride salt. Ordinarily the salt ($RaCl_2 \cdot 2H_2O$) is thoroughly dehydrated to $RaCl_2$ by heating for some time at 250°C to remove water which under the α-radiation would be decomposed to hydrogen and oxygen thus producing high gas pressure in the small volume of the tube. But on this occasion the man responsible for the dehydration had made a bad blunder. (I do not name him). Some one had used his electric furnace at a temperature of only 105°C by substituting a heating coil with higher resistance than that of the usual one, and had left it so without changing back to the original coil. Not noticing his low thermometer reading the operator placed his salt in the furnace and exposed it the usual length of time for dehydration. But the water of crystallization, still there, proceeded to decompose under the α-irradiation, gradually giving a high gas pressure within the small tube containing the radium chloride. The glass tube withstood the pressure until the moment when I scratched it with a file for opening. There was a violent rupture. Most of the radium salt was ejected and scattered across the room sprinkling the highly active salt on an oily motor and radioactive dust over the entire room. Despite all efforts of recovery, only about 90% of the salt was recaptured by washing the walls, floor, ceiling and all other objects in the room for over two weeks. The rest (44 mgs) must have been lost as fine dust. I evidently inhaled some, as I was not masked and protected only by gloves and a glass shield. My urine showed radioactivity for a few days but I suffered no permanent consequences.

Although I was continuously exposed to excessive radiation in all my earlier work, I suffered only one exposure that had evident consequences. In 1919 I carelessly held a drawer containing several hundred milligrams of high grade radium salt, with no protection except the glass wall of the tubes and the thin wooden floor of the drawer. Of course, I received no alpha radiation, but the intense beta and gamma rays reached the palm of my hand and produced a "burn." One feels nothing at the time, but three days later the palm became reddish and began to be sensitive to pressure. The red spots increased in size and number up to ten days when they united into one water blister about the size of a half dollar and half an inch high. There was only slight pain, which could be relieved by draining the liquid with a sterile needle once or twice a day. I went on with my work as usual. After about a month the burn ceased to suppurate and was covered by a hard scab. In time this came off leaving only a whitish scar which is still visible. I have had no after effects although such radiation burns are said to end sometimes in cancer formation. This could still occur, but after forty years I do not fear it.

Professor Robley D. Evans of the Department of Physics of the Massachusetts Institute of Technology was very desirous of knowing how much radium might have been permanently deposited in my system, replacing to some extent the calcium in the bony structures. For some reason, not clear to me now, I had not submitted to a measurement, although I suspected I carried some excess which, owing to its long life of 1620 years, would be quite permanent. Upon one occasion when Mrs. Lind and I, upon a visit to Professor Evans in his laboratory, were engaged in conversation with him, I became aware of a counter clicking away behind my back and it was evident that he was taking advantage of the occasion to get my measurement. I submitted willingly and was glad to learn what I carried about in my system. [He permanently carried about ¼ of a microgram of radium in his body.]

The great precautions now recommended by authorities of health physics to assure that no radioactive substances are introduced into the bodies of human beings are in sharp contrast with ideas that prevailed in the early period of atomic energy. Belief in the virtue of radioactivity was so strongly entertained that its introduction by any means into the system was recommended. I recall that I once charged a liter of water with about 200 millicuries of radon and gave it to my wife to drink for possible benefit to her health. Fortunately the solubility of the inert gas radon is not great, so that it quickly disappears from solution. This together with the short life of the immediate decay products Ra A, B and C probably saved Mrs. Lind from any deleterious consequences which probably would have resulted had it been an equivalent amount of the longer lived and less volatile parent element, radium. Whether she obtained any benefit was not evident since her health was already excellent.

I forgot to say that before we had made any deliveries, the original $150,000 approached exhaustion. But the Institute was so sure of our ultimate success that it raised the total to $500,000. This proved to be good judgment. When the work was concluded, we had produced 8½ grams of radium element in the form of $RaCl_2$, worth more than $1,000,000, at the then prevailing price of $120,000 per gram. And the income from sale of vanadium reduced the cost to $40,000 per gram. The principal commercial producer of radium objected that this estimate of cost was too low because of the subsidy of Bureau of Mines salaries for the scientists employed in the work. But even after allowing for that, our costs were lower because of his long freight haul of ore from Paradox Valley to Pittsburg, and the less efficient process used there. Unfortunately, the same producer had let all of his uranium, a much greater amount than ours, go down the drain. Today it would have high value.

We had a contract for the sale of our by-products, both *vanadium* and *uranium*, to an Eastern steel company, based on the current market price. There was no difficulty selling vanadium. It brought a return which helped to lower the net cost of our radium. But the contracting company would never order any delivery of uranium. Nevertheless we continued to recover and convert it to high grade UO_2, the black oxide. To keep a uranium price established, we sold small lots of uranium produced from ore not subject to the contract. Finally, when our work was finished, the Radium Institute brought suit in Federal court in Delaware to force the steel company to take our uranium at market price according to the contract. The company resisted on the ground that there was no market for uranium, hence no market price, and no obligation to take any uranium. We produced bills of sale for the small lots we had disposed of, but the case dragged on in court and might have been lost but for a very ludicrous incident. A lawyer for the steel company produced a file of letters which he said were from companies saying they had no use for uranium and would not buy any. But the file he threw on the table was a fatal exhibit. Our lawyer picked it up and after glancing at the top letter asked permission to read it to the court. It was from a larger steel company stating it was greatly interested in uranium as a possible component of alloy steel and would like to purchase quite a quantity for experimental purposes. The entire courtroom burst into uproarious laughter. Our opponents' lawyers asked the court for an intermission and retired with red faces. After a short time they returned with two alternatives for compromise, either to buy at a low price, or to pay $35,000 damages and let us keep the uranium. We accepted the second offer in the belief that uranium would some day become valuable, since it was rare and possessed the highest then known atomic weight. How we would have been set up as prophets could we have looked forward to 1940, to Uranium-235, atomic power, and, alas, the atomic bomb.

As a consequence, our 30 tons of uranium oxide remained in storage in Colorado until the dissolution of the Radium Institute. Then it became the property of Archibald Douglas, the son-in-law and heir of Dr. James Douglas, one of the two founders of the Institute. The entire lot was shipped East to Mr. Douglas and I wonder if it may still be hiding in some old barn, forgotten—awaiting the day when it may be discovered and brought forth to participate in the modern uses for atomic fission and nuclear power. Incidentally, it may be mentioned that the present high price of uranium extends its acceptability from 2% ore down to 0.1%. This widens the ore fields greatly, and the high demand brings a monthly production exceeding our Colorado accumulation extending over four years.

The total produced in Denver for the Institute was 8½ grams of radium element. Under the contract, more than two grams became the property of the Bureau of Mines. This division disappointed the Institute and it applied to the Bureau to remit a greater proportion. The Bureau found it had no authority to make *donation,* but agreed to a *loan,* divided equally between the New York and Baltimore clinics, leaving ½ gram to the Bureau for scientific research. This decision had both happy and unhappy consequences. The two clinics had to keep the Bureau's loan separate from their own radium. This meant that in the collection of radon from the radium solution for therapeutic use, two separate systems must be operated daily for one or two hours each. This kept the operator exposed twice as long to radiation, which may have contributed to the untimely death of two operators in Baltimore before sufficient shielding was provided.

After many years of operation, the Baltimore clinic was closed in 1952 and the building in Eutaw Place was condemned as contaminated by radioactivity. The populace became so alarmed that many would not pass near the building and, according to the papers, the city is considering whether to decontaminate the building or to tear it down. In Cincinnati also, rather extreme measures were taken with a building that became contaminated by an accident with a moderate quantity of radium used in making luminous paints. The New York Memorial Hospital clinic has, so far as I am aware, been free of unfortunate incidents and its radium is still used therapeutically.

All of the *early* American companies that produced radium have now ceased to operate. The supply from the Belgium Congo ore enabled a price to be set in Belgium just below the American cost of production. Production later began in Canada and continued for many years giving a large supply. The use of radium has greatly diminished, owing to competition of x-rays of high energy, and of γ-radiation from Cobalt-60, which is artificially prepared and is now available in large quantity at low cost. Consequently both the price and production of radium have sunk to a low level. And the prejudice against its use, promulgated by the authority of Health Physics, has almost put it out of use, both in therapy and scientific research.

But let me revert to the happy consequence of radium production by the Radium Institute already referred to. It gave to the Bureau of Mines a half gram—quite sufficient for scientific uses. While I was still in Golden at the Colorado School of Mines station of the United States Bureau of Mines, the Bureau entrusted me with the entire half gram of radium. This was the goal that had taken me first to Denver four years earlier and had kept me busy during this entire time in the radium work, all the long way from laboratory methods through installation and supervision of plant operations. This same radium has remained with me through the years—in Golden, Colorado; Reno, Nevada; Washington, D. C.; Minneapolis, Minnesota; and finally Oak Ridge, Tennessee. It has been constantly used in studies of radiation chemistry and has provided research material for some two dozen collaborators and graduate students.

Even during the production work we got in a little incidental research. Frank Whittemore and I made an authoritative determination of the radium: uranium ratio in carnotite, which we published in the Journal of the American Chemical Society in 1914 (*36,* 2066). I published three papers on radium analysis, the first describes an improved type of electroscope, the second, the emanation (radon) method, and the third, other methods, all in the Journal of Industrial and Engineering Chemistry (7, 406, 1024 (1915); *12,* 469 (1920)).

In Golden, as soon as freed from the routine of radium production, I put my ½ gram of radium to research use. About half was devoted to radium standards of different contents, 150, 60, 10, and 1, mgs of radium element (as $RaCl_2$). These were used in measuring radon by γ-ray comparison. About 250 mgs of radium in the form of $RaCl_2$ were put into solution in 5% hydrochloric acid for the collection of radon.

The first chemical reaction studied in radiation chemistry, using radon collected from the Bureau of Mines radium just mentioned, was the combination of the gases hydrogen and oxygen to form water vapor. While hydrogen and oxygen combine readily at higher temperature and even explosively if their pressure and temperature are high enough, they do not react at all at ordinary temperature unless activated by some agent. It was therefore thrilling to find them uniting steadily at room temperature when mixed with radon as a source of powerful alpha rays, the doubly positively charged nuclei of helium produced by the spontaneous disintegration of radon and of its decay products RaA and RaC'.

The reaction proceeded as was to be expected, two molecules of H_2 combined with one of oxygen to form two molecules of water. As soon as the concentration of water vapor reached the vapor pressure of liquid water, droplets began to appear on the wall of the glass vessel. Spherical vessels were used in order to simplify the calculation of the length of paths of the alpha rays within the sphere, which controls the amount of reaction they produce. Since radon is a gas, its alpha rays can originate at any point within the sphere or from its wall and travel in a straight line in any direction until again reaching the wall. Since the number emitted per second is very large, about 111 billion per second per curie, the law of averages applies perfectly. I therefore only needed to know the average length of all straight lines in all directions from all points within a sphere until they reach the circumference. I could not find anyone had ever needed to solve this geometrical problem or to use the relation, though possibly some mathematician, perhaps in ancient times, had worked it out for his own amusement and left it buried in a long forgotten parchment. The mathematics for this solution was solved with the aid of Professor Lunt of the University of Chicago and gave a very simple result. *The average of all straight lines from all points within a sphere to the wall is exactly three-fourths of its radius, and from all points on the inner surface in all directions to the wall is one-half of the radius.*

Estimations for volumes other than spherical indicate that the average of all straight line paths from all points on the interior surface in all directions until again reaching the wall is one-half of the radius of a sphere of equal volume; while that from all points within the volume is three-fourths of the equivalent sphere radius. This similarity to the sphere is restricted to volumes which have no re-entrant angles. A restriction in the calculation of effective path of ionization, common also to the sphere, is that no path shall exceed that of the diameter of the equivalent sphere. The relation seems to apply also to cylinders in which the ratio of length to diameter does not exceed about ten to one.

The hydrogen-oxygen reaction was also used to study the action and ionization by recoil atoms projected in the opposite direction from the emission of alpha rays from radon or from one of its decay products Radium (A) and Radium (C′). This is an example of how a physical problem may be solved by chemical means [J. Am. Chem. Soc., *41*, 551 (1919)].

The reaction of hydrogen and oxygen under α-irradiation to form water vapor deserves some detailed consideration as having principles common to all radiochemical gas reactions. Both the hydrogen and oxygen molecules are separately activated by absorption of energy from the highly energetic alpha rays as they pass in straight lines through the electronic structure of the molecules. About half of the absorbed α-energy produces ionization of the molecule struck, i.e., removal of one or more of its electrons, usually one, sometimes two, rarely more. If more than one electron is removed the doubly charged molecular ion will at ordinary pressure soon encounter a neutral molecule of either kind and quickly (probably on first collision) extract an electron from it, thus giving two singly charged ions. Ionization is then one form of activation, but the energy expended by the α-particle in each collision is about twice that necessary to ionize. Evidently then some other active forms are also produced. They may be neutral radicals or atoms. There is much question about their contribution to the reaction yield. In my early study of this reaction (J. Am. Chem. Soc., *41*, 531 (1919)) I found that the effect of excess of either hydrogen or oxygen could be predicted on the basis that the change produced in the ionization of the gas mixture would change the reaction velocity correspondingly. According to Bragg the molecular ionization of oxygen is 1.09 and of hydrogen 0.24, compared with air (1.00). Consequently an initial excess of oxygen increases the relative action velocity, which continues to rise as the mixture is enriched in oxygen by the chemical reaction. Exactly the opposite is the case with excess hydrogen.

The ion yield, the number (about 4) of water molecules formed per ion pair by radon, in an electrolytic mixture of hydrogen and oxygen can be interpreted by the assumption of clustering of neutral-molecules about the positively or negatively charged ions through electrostatic attraction. Since negative ions of hydrogen are not formed, the electrons are free to attach themselves to and activate oxygen which has high affinity for electrons. The cluster theory lacks a good theoretical basis, but experimentally reaches its highest degree of probability through the common value (4) found in the oxidations of H_2, CO and CH_4, very different types of molecules, with quite different stopping powers (absorption) and specific molecular ionizations. The products are four molecules of water per ion in hydrogen and oxygen mixtures; four molecules of carbon dioxide in oxygen-carbon monoxide mixtures; two molecules of carbon dioxide and four molecules of water vapor per ion in mixtures of methane and oxygen. In none of these do the ion yields just given depend on the mixtures being stoichiometric. But, of course, the total ionization per length of α-particle path is increased in a mixture with excess of component having the high stopping power (absorption), for example, oxygen in its mixture with hydrogen. This increase becomes greater as the reaction proceeds to remove hydrogen. In a mixture with initial excess of hydrogen the stopping power would be lower and increasingly lowered as the reaction removes oxygen, thus increasing the H_2:O_2 ratio. In carbon monoxide and oxygen the stopping powers differ by only 15% and in methane and oxygen by still less, but in the latter case two gaseous products are produced, and interchange of activity by ion exchange according to their individual ionization potentials complicates the theory of prediction. Nevertheless the ion yield for the consumption of methane and oxygen is surprisingly constant over 90% of the reaction, which is difficult to interpret.

But I must not leave Colorado and carnotite without mentioning two happy events that had little to do with carnotite except that it brought me to Colorado. In 1915 I courted and married Marie Holladay of Omaha, Nebraska. She has accompanied me in all my wanderings since then as mentioned in the previous and succeeding paragraphs. Also in Colorado our son Thomas was born, who was with us in Reno, Washington, and Minneapolis, until he himself married and moved away to Oklahoma and last to Freeport, Texas, where he is a chemist with the Dow Chemical Company.

CHAPTER 9

The Bureau of Mines in Reno

Soon after the research on the synthesis of water by the action of radon was finished, our station was moved in December 1920 from Golden, Colorado to Reno, Nevada. The reason for this move arose from a disagreement between the United States Bureau of Mines and the Colorado School of Mines. The Chairman of the Board of the latter felt the school could no longer let us use the building we occupied, and wanted us to take quarters in its Experiment Station, a large building used for ore dressing and experiments in metal recovery. The Experiment Station was not adequately heated and had no offices or laboratories suitable for our use. The Bureau declined these quarters and, when the School of Mines remained adamant, decided to move the station to the Nevada School of Mines at Reno. This decision was made in Washington by our able Director, Van H. Manning.

The University of Nevada put at our disposal temporary quarters until we could construct a small building on its campus. It was during this construction, while our radium was idle, that we experimented with diamond coloring by alpha rays as described in the following paragraphs.

While we were still in Denver, a local company of wholesale jewelers had brought the question of diamond coloring by radium to our attention, and loaned some large cut diamonds which we placed in contact with the glass tubes containing our radium standards—when not in other use. After two months' exposure not the slightest change in color had taken place and we became skeptical of the entire claim of color production by radium. The company then brought us a long paper by the distinguished British scientist, Sir William Crookes (Proc. Roy. Soc., *74*, 47 (1904)), in which he reported coloring a diamond "bluish" by prolonged exposure to a smaller amount of radium than we had used. The only difference being that his diamond had been in direct contact with his radium salt, whereas ours had been separated by the glass wall of our radium container.

This difference in method gave me the idea that it must be the non-penetrating alpha rays responsible for the coloring. We would not open any of our standards for this kind of experimentation to use direct contact of radium salt with diamonds. But in moving from Golden to Reno we had to dismantle our radon collecting system, evaporate the radium chloride solution to dryness, and dehydrate the radium salt before sealing it in a glass tube for storage until we should be ready to use it in Reno. This seemed an ideal time to put diamonds into direct contact with radium chloride. The company loaned us two stones of a few carats each. We had just enough salt to cover one of them; the other could but rest on the radiating surface. The rays colored the glass walls of the container to deep blue so rapidly that we could not see if the diamonds inside were being colored. Thus they remained sealed in the tube for two and a half months until we were ready to open and put the radium salt back into use in Reno.

We were very agreeably surprised to find that the diamond that had been completely covered had acquired a brilliant *grass-green* hue, while the other was colored green only on the side that had been in contact with the radium salt. When the company in Denver learned this result it became intensely interested and sent a younger brother of the firm to Reno with four cut stones of 4 to 9 carats.

We did not wish to expose our radium salt to the inevitable small losses that would be involved in using it for experimental coloring, but decided to use the gas radon, which has no lasting value beyond a month, but initially would emit three-fourths as many α-rays as the parent radium, and would have the much greater advantage of losing none of the alpha rays by absorption in the salt. In fact, when one uses the solid salt, he is getting alpha rays only from the small fraction of radium within 1/1000 of an inch from each crystal surface, the rest and much larger part being lost by self-absorption within the intervening radium salt crystals. On using gaseous radon we found the same degree of coloring could be produced in two or three days as in seventy-five days by the salt.

Believing that the Bureau of Mines safe did not provide sufficient protection for such valuable diamonds, Marcus Zimmelschied, the Denver company representative, insisted each day on transporting them to one of the Reno banks for overnight safety storage. But this developed trouble. Since we could not put any cushioning material within the tube without intercepting α-radiation, the diamonds were left bare in the glass. The movements during transportation caused rubbing of hard diamond crystals against the softer glass inner wall which occasionally cracked the tube, thus contaminating our surroundings with a high degree of radioactivity—not feared at that time, but today highly condemned as a health menace. The transportation was therefore abandoned, more on account of the loss of radon and of time, and the diamonds then remained secure in our safe during the remaining experiments.

The company made no secret of the fact that its interest was to sell green colored diamonds at the high price they would command on account of the rarity of natural green diamonds. And we made it no secret that our interest was solely in the scientific information we would get, and in the possibility we could benefit the American diamond industry. Our work was well known to and approved by the Bureau in Washington, which believed our work might help the small diamond production in the United States. How wrong our psychology proved to be is shown shortly. But first let me describe an experimental stumbling block that we soon encountered.

Coloring diamonds by using gaseous radon instead of radium salt was most effective. We obtained no color except green. All the diamonds submitted by the Denver company had an original yellow tint which very much lowered their value as gems. These undesirable colors disappeared or rather were masked by the green color produced by radium so that the artificial coloring had a double advantage. But a serious disadvantage soon appeared. The diamonds colored directly by radon often developed so-called "carbon spots," black spots about the size of a small pin-head, which grew larger on continued irradiation. This disturbed the owners very much, as carbon spots detract greatly from the value of a diamond. The spots occur occasionally in natural diamonds and cannot be removed by any known process. A jeweler in mounting such a stone will try to conceal the black spot by the mounting. The spots in our artificially colored diamonds were found to lie well below the crystal surface—much beyond the penetration of the alpha rays that produce the green color. A stone was sent to a diamond cutter in New York who reported that at least one-third of its weight would be lost in attempting to cut out the spots. The owners then suggested that we try to remove them by chemical treatment. After trying various hot acids in vain we gave up this line of attack. The persistency of the owners then paid off. They tried heating to red glow in a blow torch, which had to be done with great care to avoid cracking the stone. Their efforts were at first unsuccessful, but on repeated trials of succesive heating and cooling, they described the spots as rising gradually to the surface and there vanishing. But the heat also removed the green color without touching the original unwanted yellow tint. It was most difficult to explain the behavior of the artificially produced carbon spots. Naturally occurring ones cannot be removed by heating, and carbon or graphite is not volatile at the temperatures used. In fact, the problem of the spots and of their removal has remained unsolved, though modern ideas of the behavior of atoms within a solid might suggest a theory. This left it all where it started, but a final difficulty showed up which ended the entire effort, as I shall now relate.

One day in Reno I received a telegram from the Secretary of the Jewelers Protective Association in New York. Having heard we had learned to color diamonds artificially he asked for information. I naively wondered if they wanted to color some! As I was planning to go East soon, I replied that I would call on him. I found him very agreeable and helpful. I told him of our experiments. He said there would be no market for diamonds artificially colored—that people would not knowingly buy them, even though they had been colored by an agent as rare and expensive as radium. This proved to be right. I have never known of any purchase except one by a gem collector in Los Angeles who wanted an artificially colored diamond as a curiosity in his collection. The Secretary of the Jewelers Association further informed me that a well known jewelry firm in New York had just acquired four large green colored diamonds thinking them natural, but which he suspected had been colored by radium. He arranged to have me inspect them on the following day. One of three young brothers, sons of one of the members of the firm, appeared with their four recent purchases. They had been acquired at various places, from San Francisco to Omaha and Buffalo. They thought they were getting great bargains, as true green diamonds are extremely rare and their prices fabulous. At first sight I thought I recognized them as the first four we had colored in Reno. After inspecting them, I said I could not tell by merely looking, whether they had been radium colored, but if so, they would still be strongly radioactive. I advised taking them over to Dr. Victor Hess, physicist for the U. S. Radium Corporation in New Jersey, who could examine them with an electroscope. They were found to be strongly radioactive, confirming the opinion that they had been colored by radium.

The Secretary informed me that he had sent cablegrams to all of the 2500 members of his Association in all parts of the world, warning them against radium colored green diamonds. This ended their sale. A member of the Denver firm had just left for Europe with some to sell. But in the days before air travel he was outstripped by the telegraphic warning from New York and met only refusal.

Altogether we had colored some twenty to thirty diamonds on loan from Denver. They were claimed to represent all the different diamond fields of the world, but we had no assurance of this. All were originally yellowish and took only a green color. The Denver Company (long ago dissolved) had protected itself in its sales by giving only the usual certificate that the diamonds had been lawfully imported through the U. S. Customs.

We did no more coloring and I do not know what eventually became of the ones we had colored. They could easily be restored to their original yellow by heating to red glow, which was also effective, as we had found, in toning down any green color that had become too dark by over-exposure. The shade of green could be reduced to any desired tint by gradual heating. The radioactivity, however, would remain, which today would prohibit their being worn, because the recommended tolerance of radiation has been greatly lowered since that time.

After my experience with the Jewelers Protective Association, I decided to visit the well known mineralogist, Dr. George Kunz of Tiffany and Co., for whom the mineral kunzite had been named. I thus learned more about the psychology of gem sellers. I sent in my card, but instead of inviting me into his office, he received me very brusquely over the counter, almost as if he suspected me of being a diamond thief. He stormed about our work in coloring, which he said could be very detrimental to diamond sales. Tiffany, he claimed, had the world's finest collection of colored diamonds, all colors, green being one of the rarest and most beautiful. If the public suspected genuine diamonds to have been artificially colored, their sales would be diminished and their value greatly depreciated. No one, he said, would knowingly buy an artificially colored stone even though it had been colored by radium. He said he had helped Dr. Holmes form the Bureau of Mines by dissociating it from the Mineralogical Survey and that if we planned to continue diamond coloring he would go to Washington and have it stopped at once. I explained to him that our diamond coloring had been only incidental to scientific experimentation with about thirty other kinds of minerals to see the effect of radium irradiation. I told him I knew little about colored diamonds and asked if I might see the famous Tiffany collection. He refused flatly and would show me nothing. I am sure he feared I could reproduce any color, once having seen it. I withdrew, feeling no little indignation that he as a scientist should show no courtesy toward nor understanding of a fellow scientist, though one much younger. Incidentally, I may say that some years later my wife and I met him and his wife at a Canadian summer resort and found them charming people. How upset he must have been by the threat to the value of colored diamonds. And how naive I was in regard to the value attaching to an innocent piece of carbon, no greater than that of a similar chunk of coal except for its rarity and the ephemeral taste of the public for baubles. And today this value rests largely on the fetish of impecunious young couples for diamond engagement rings.

The work of our laboratory in Reno, both scientific and applied, was undisturbed by the coloring episode. D. C. Bardwell and I published two papers in the Journal of the Franklin Institute (*196*, 375; 521 (1923)) covering our work on the coloring and thermosphorescence of some thirty different minerals and one on the diamond coloring, both of which were copied with our permission in the American Mineralogist (*8*, 171; 201 (1923)). We also continued work on the radiation chemistry of gases, while the other staff members under my direction worked on the metallurgy of the rare metals, like vanadium, uranium, tungsten, etc.

While I was in Reno in 1921, Madame Curie came to the United States with her two daughters, Irene and Eve. She was presented with a gram of radium by President Harding, donated by the women of America in recognition of her outstanding contributions to science. After the ceremony of presentation in Washington, she made a trip to the West which included a few days stay at the Grand Canyon. I was glad to seize this opportunity of seeing her again and felt fortunate in obtaining an appointment. I had heard she was very tired and exhausted. But when I arrived at Grand Canyon, on the way to the hotel I was surprised and delighted to see her out horseback riding with her two daughters. I concluded that she only wanted to get away from the world and have a rest, while enjoying the beauty of the Grand Canyon.

In our conversation that evening she disclosed that her daughters would like to make the trip down to the floor of the Canyon on the Colorado River, and not feeling up to it herself, she would like me to accompany them. Not having been on a horse for years I could imagine what the trip would do to me. There were two choices, a round trip of one day or a two-day trip including camping out overnight on the Colorado. I proposed a compromise. I would go on the one-day trip but felt two days would be too much. But the girls insisted on the two days, and I sensed they did not want a chaperone in any case. So I was excused, and bidding them farewell and bon voyage I returned to Reno. Madame Curie I was never to see again. Irene I visited in 1935 at her laboratory in Paris in the Institute Curie, and Eve I saw at her public lecture in St. Paul, while I was at the University of Minnesota.

But I must not close the Reno chapter without a word about the town itself with a population of about 10,000, the metropolis of Nevada, the state with the smallest population in the Union. About 90% of its area is completely mountainous and arid. Reno itself is an oasis located on the Truckee River, which arises from beautiful Lake Tahoe, flows north for some miles until it turns east down the slope of the Sierras and reaches Reno at an elevation of about 4500 feet. Then it flows on east until it turns north for several miles and empties into dead-end Pyramid Lake, so named from a rocky island in its center shaped like a pyramid. While we were in Nevada a prolonged dry spell caused Lake Pyramid to continue to be lowered by evaporation until people began to wonder if it would go back to desert. But when finally it disclosed a long submerged bench mark placed many years earlier by the U. S. Geological Survey, they ceased to worry. Since then some water has been diverted from the Truckee River for irrigation, again lowering the level of Pyramid Lake, but it is, I believe, still there, up and down with wet or dry years. The lake then abounded with a species of land-locked salmon varying in weight from a few to forty or fifty pounds. The Indians on a reservation at the eastern end of the lake depended much on fish, which they ate or dried for sale. Bardwell and I had some luck on Pyramid trolling from a row-boat with large artificial lures. We preferred, however, the Truckee River which provided good trout fishing along much of its length. When the season in Nevada closed at the end of September one could move a few hundred yards up the Truckee into California and fish on through October. California law then allowed fifteen trout and required all be kept regardless of length. Whereas Nevada required any under seven inches to be returned to the stream.

This difference of law in the two states got Bardwell and me into the following difficulty. We had fished in a small stream up in the Sierras. Believing we were in California, we had taken several trout below the size limit of Nevada. On coming down from the mountain we met two men from Reno whom we knew. They expressed a desire to see our trout, which we readily showed. One of them smiled at the number of small ones. We explained that our Bureau was having a picnic next day for the wives and children and we needed all the fish we could get for the picnic. It turned out that one of the men was a secret Nevada game warden. He could have taken an under-size sample and placed a charge against us. The Nevada size limit applied to possession as well as catching within the state, since there would be no means of determining where they were taken. However, he placed no charge against us, but did report it to his chief, the state attorney. Even he, I think, would have overlooked the matter, but when he found I had previously been fined for a violation he took the matter to court and, of course, had witnesses, though no sample. We decided we needed a lawyer and consulted the late Hon. Pat McCarran, who later became nationally known as Senator from Nevada. He thought we had a defense, but when he looked at the jury panel of hard-boiled old Reno professionals, he advised us to plead guilty and avoid the expense of a suit.

He made no charge for this advice but the judge was not so lenient. Although Bardwell pled guilty for the whole catch, the judge, on account of my former violation, doubled the fine from $50 to $100. I now think I should have paid the whole, but then we agreed to divide it equally between us.

In the earlier violation I alone was involved. Soon after moving to Reno I drove one afternoon up the Truckee several miles to a dam across the river. I was wholly unaware that Nevada had an unusual law that prohibited approaching within 100 yards of a dam either up or down stream. A small sign posted below the dam announced this prohibition. But as I approached from upstream, the notice was not visible to me, and I boldly walked out on the dam itself to fish. My luck was good, but after I had caught several trout I wondered why people on the highway would bother to stop and wave to me to come ashore. Thinking they just wanted to see my catch I waved back and went on fishing. Finally two couples drove up—probably on their way to a picnic—stopped, and authoritatively ordered me ashore. Of course one of them was a warden, who charged me and took all of my trout—probably for their supper. My protestations were of no avail—that I was new in Nevada, ignorant of the unusual law about the dam(n) fishing and had not seen the poorly placed notice. The warden could have taken me in at once but he probably wanted to continue their excursion and eat my fish. The result was that in court I pleaded guilty, with extenuating circumstances, and was fined $50. The high fines for illegal fishing in Nevada were said to be due to the fact that everyone fished for trout, and they were most desirous to protect them. Even within the city limits of Reno one of our employees caught over 500 trout in a single season by fishing short periods morning or evening outside his working hours.

As to dams on the Truckee, it was said that the Indians objected to them as obstructing the migration of fish in the spawning season. Although the dams were provided with fish ladders, the Indians would destroy the dams. Hence approach was prohibited.

The population of the city of Reno was divided into two or three classes. The transient divorce colony lived mostly in the two principal hotels for the then required six months residence. A group of divorce lawyers practiced, with fees varying according to their Eastern affiliations, and to the wealth of their individual clients. There was little association between the stable Reno citizens and the divorce colony, on account of lack of mutual interests and the brief duration of residence which was later reduced to six weeks, in competition with Las Vegas, Florida, or other divorce centers. Also, there was an ever changing group of Californians who came to gamble in the Reno casinos. Las Vegas had in the early twenties not begun to compete as now in gambling, divorcing and high living. This has come about through the large growth of Los Angeles closer to Las Vegas.

Then there was the academic group located at the University of Nevada. It had little or no contact with the divorce colony and rather scorned its existence and objectives. Most of the associations of our Bureau of Mines members were with the University people.

Finally there were the genuine Renoites with the western traditions and outlook. A leader among them was the late George Wingfield, the millionaire banker. He had gambled successfully in Tonopah and in the Klondike, had established the largest Nevada bank in Reno, went flat broke in the depression of the early thirties, but later came again into prosperity before his recent death. In financial circles he attained standing, but in other ways he did not free himself from his reputation of earlier days. He always was armed and constantly guarded. He was armed even when playing golf. He was said to be the owner of the Reno Stockade, where, within a high-walled fence, women shamelessly followed the world's oldest profession, though most of them dwelt there only during business hours. Correspondingly they were not allowed to practice openly in the city. I once took a visitor through the stockade to let him see the pitiable sight of degradation. I believe it has since been destroyed. But Reno still flourishes with gambling and divorcing, though having strong competition in both by Las Vegas. The old Golden Hotel that had housed so many would-be divorcees was completely destroyed by fire early in 1963.

CHAPTER 10

WASHINGTON, D. C.

Several administrative changes in the Bureau of Mines brought me to Washington in the summer of 1923. While the Bureau station was still in Colorado, Charles L. Parsons had resigned his post as Chief Chemist in order to devote full time to the secretaryship of the American Chemical Society. He had been succeeded by Richard B. Moore while we were still in Golden, and I had succeeded Moore as head of the Colorado station and retained the headship when the station was moved to Reno in late 1920. In the summer of 1923 Moore resigned in order to accept a post with the Dorr Company in Connecticut, and I moved to Washington to succeed him as Chief Chemist. I was permitted to retain the services of Bardwell, and we took the radium to Washington where we continued to use it for researches in radiation chemistry.

One of my most prized experiences in Washington was the direction of the Ph.D. thesis of one of our chemists, Frank Porter. He had taken the required course work for his degree at George Washington University and was permitted to carry out his experimental thesis work under my direction in the Bureau of Mines. We chose the subject of the combination of hydrogen and chlorine under α-irradiation by radon. The influence of light on this reaction had been many times studied, ever since photochemistry began to be a science under Robert Bunsen. By a very ingenious combination of the Bunsen photometer for the reaction with light, and an actinometer for the reaction rate under alpha rays, Porter was able to compare the quantum yield of the former with the ion yield of the latter source. The rate of both reactions increased in equal proportion as concentration of inhibitors in the mixture of hydrogen and chlorine were gradually removed by the electrolytic evolution of hydrogen and chlorine. This progressively enhanced the rate of reaction by several hundred fold until it would reach explosive velocity. But always the ratio of the two rates, photochemical and radiochemical, remained constant. This means that each source, light or alpha rays, produces the same chain reaction in which the atomic (Nernst) chain becomes longer as inhibiting impurities are removed from the hydrogen and chlorine by continued electrolytic production of the two gases from concentrated hydrochloric acid. This definite ratio between the ion yield and the photon yield of HCl produced by alpha rays or by light quanta was, according to Porter (J. Am. Chem. Soc., *48*, 2603 (1926)), about four to one. But much later at the University of Minnesota, Robert Livingston (J. Am. Chem. Soc., *52*, 593 (1930)) found a value of one to one. It should perhaps be tried once more to settle the difference, as it seems important to know whether the photochemical and radiochemical reaction chains have different or the same lengths, or whether a different number of each is initiated.

In order to circulate his hydrogen and chlorine gas mixture, Porter had needed a pump that would expose no metal parts to the corrosive action of chlorine. This meant an all-glass pump (Ind. Eng. Chem., *18*, 1086 (1926)), but how were the moving parts to be actuated? I conceived the idea that if one enclosed a soft iron core within a sealed glass tube, which would just fit the gas-conducting channel without much friction, this would constitute a plunger that could be moved back and forth by opening and closing the circuit to a surrounding electro-magnet. This would move the plunger by successively making and breaking the magnetic attraction to its iron core. The valves consisted of light glass spheres which rose and fell with the gas pulses, and fit a glass socket closely enough to prevent excessive leakage at each stroke. Bardwell helped Porter with the construction which proved very effective. Modifications were later made. Vertical types excell horizontal ones on account of the friction of the latter, since lubrication is not permissible. Livingston at Minnesota (J. Phys. Chem., *33*, 955 (1929)) later developed a type that utilized both strokes and thus doubled the capacity.

One of my duties in the Bureau of Mines was to supervise the work of the Helium Laboratory which was established to devise a suitable process for the separation of helium from the natural gases of Texas and other states. A Board of four engineers, consisting of M. H. Roberts, R. C. Tolman, W. L. Debaufre, and Edgar Buckingham, had been employed to review unsuccessful attempts and to devise a process which would give a good recovery of the two percent of helium, and return the remaining hydrocarbons to the pipe lines. A plant was constructed at Fort Worth, which city used the gas after helium stripping. When this field became exhausted, improved plants were constructed in the practically unlimited field around Amarillo. Over the years the world demand for helium has greatly increased. More plants have been installed, the cost of helium has been miraculously reduced, and yet much helium is still wasted in burning gases without separating it. This is a problem still unsolved. Helium has become indispensable for many purposes. The United States has the only known commercial resources (except possibly Russia). Helium should not be wasted. Yet it is difficult and expensive to store. Its use does not equal the quantity contained in natural gas currently consumed. But the owners of such gas could, in this country, hardly be prohibited from disposing of it. This question of conservation versus waste of helium remains a problem for our Congress.

The preservation of helium containing gas by storing in abandoned salt caves from which the salt has been removed is currently being considered. The caves are quite dry and fairly impervious to deep penetration of helium. The cost of such storage must be balanced against the value of helium and what its cost is likely to become in the future. Such factors as increased use, exhaustion of present resources, discovery of new ones, must all be considered. But it would appear deplorable to lose it in the atmosphere when nature has kindly given us 2% helium in natural combustible gas that compensates for its cost of separation. If once lost, although indestructible, it cannot be regained.

After I had been about a year and a half in the Bureau of Mines in Washington, Dr. F. G. Cottrell, Director of the Fixed Nitrogen Laboratory under the Department of Agriculture, informed me that he needed an assistant and offered me the position of Assistant Director. I had known Dr. Cottrell when he had for a short time been Director of the Bureau of Mines. Knowing we could work together well, I accepted, under the condition that I would have time for research and could bring both Bardwell and the radium with me. Cottrell agreed to both and I resigned with much regret after nearly ten years in the Bureau of Mines where I had always been most happy in my work and with my associates.

In the Fixed Nitrogen Laboratory Bardwell and I worked on the radiation chemistry of saturated and unsaturated (with J. H. Perry) hydrocarbons, the oxides of carbon, their reactions with oxygen and with hydrogen, and the remarkable influence, which we discovered, of the ions of inert gases when mixed with reactants. The ions of a chemically inert gas in a mixture with reactants may have an influence in several different ways depending on whether the ionization potential of the inert is greater or less than that of the reactant. If greater it will contribute to the rate or ion yield by favorable ion exchange. If less it will diminish the rate by unfavorable exchange. When there are two reactants the change of rate is complicated by circumstances—whether the I.P. of the inert is above, below or between that of both reactants. It may be quite difficult or impossible to evolve a mechanism fitting the experimental data. This year and a half together at the Fixed Nitrogen Laboratory saw the culmination of my joint work with Bardwell, and I was most fortunate to have had such an able and skillful associate.

While I was with the Bureau of Mines in Washington we had a visit from M. H. Roberts of New York, a member of our committee on the recovery of helium from natural gas. In entertaining him, my wife and I started to drive him out to dinner using my old car. Although closed cars had come into fashion, mine was still an open type two-seater with side curtains, and far from new. Our guest surveyed it with undisguised misgivings and entered it with some hesitation. His uneasiness grew as he listened to the rattles, until finally he could bear it no longer, and insisted on a taxi for the rest of the journey. So I was obliged to park, hail a passing taxi, and take the guest in better style to our destination only a few blocks distant. I took this occasion to point out the reliability of my car and its advantage over the "glass cages," which I claimed would, by their many reflections, interfere with vision. But the experience soon convinced me that our car was badly out of date and that I should get a new enclosed one, which I did.

Also in Washington, I had two distinguished visitors from Germany, Professor Fritz Haber and Dr. Franz Fischer. Dr. Haber, inventor of his well known process for ammonia synthesis and one-time Director of the Kaiser Wilhelm Institute, came to the United States while investigating the possibility of recovering gold from sea water. I do not refer to sunken wrecks but to natural occurrence. He had envisioned paying off the huge German war debt of World War I through a secret process, but had to abandon the idea upon finding the average gold content of sea water much lower than he had believed. He appealed to me and I helped him to obtain data on American waters from our Coastal Survey authorities, which ended his project.

Dr. Fischer was in the midst of his protracted researches leading to the later Fischer-Tropsch process. He was interested in the α-ray work of Bardwell and myself on the reduction of carbon dioxide by carbon or by hydrogen. Through discussion we became convinced that our ionic results had little or no relation to his thermal reactions. He seemed relieved. Possibly he had feared patent conflicts, but it may be that he had only scientific interest in our non-thermal reactions promoted by radiation. I never saw either Haber or Fischer again after their visits to Washington.

While in Washington I was awarded the Nichols Medal by the New York Section of the American Chemical Society, and delivered the presentation address in the auditorium of the Chemists Club in New York on the subject, "Ionization of Gases as a Type of Chemical Activation." I reviewed the reactions studied up to that time. I recall that my uncle, Warner Colville, honored me and punished himself by attending, and told me later he had not understood a single word—which I can well understand, as his interests were in art and theatre, not in science.

I cannot leave Washington without telling something of the city itself in the early twenties. The end of World War I had brought its population down to nearly normal, around a quarter of a million, though many of the temporary wooden "quonset huts" were still left in the area between the Station and the Capitol and in some other parts of the city. The Senate and House office buildings had not yet appeared, and Constitution Avenue had not been improved by the many stately buildings that now make it worthy of our Capital. The Union Station and the Library were then quite new and handsome buildings. No change had taken place at the other end of Pennsylvania Avenue around the White House and Jackson Square. Pennsylvania Avenue, connecting them with the Capitol, was rather a jumble of old unsightly buildings. The new Cathedral was in the earliest stages of construction. The Lincoln and Jefferson Memorials did not come until later and the Pentegon was of course a World War II project. The American University had almost succumbed in World War I, and some of its buildings were rented to the Government. One of these was used by the Fixed Nitrogen Laboratory in which Bardwell and I carried on in 1925-26.

Rents in Washington were then reasonably low. My wife and I first had an apartment just off Connecticut Avenue a few doors from the handsome Connecticut Avenue bridge above Rock Creek which we overlooked. It was only a ten minute walk to my office in the then new Interior Department Building.

Later, when I transferred to the Fixed Nitrogen Laboratory we moved to a new apartment just off Massachusetts Avenue which gave me a walk of one mile along Tunlaw Road to my new place of work. There were no houses then on Tunlaw Road and it was like walking on a country lane with practically no traffic. The name Tunlaw, the reverse of walnut, had been fancifully assigned by someone when its entire length was lined with black-walnut trees. Regrettably, these had all been sacrificed in World War I to make rifle butts, so that I never walked under their shade. That part of Washington had not yet been invaded by residence. Within but few minutes from our apartment one could pick blackberries and black raspberries in summer and persimmons and nuts in the fall or early winter. At Christmas time, covered with frost, persimmons reached the height of delicious edibility with no trace left of their earlier astringency.

It was in the first half of 1926 that I began to be tempted to leave Washington. Two offers came almost simultaneously to be head of chemistry in two of the most prominent midwestern state universities—Michigan and Minnesota. I was almost overwhelmed by having to decide between them. Either post would be adequately rewarding and there were many things to be considered. At Michigan I would be returning to familiar surroundings and many old friends. Most of my former associates in chemistry were still there. This might have disadvantages as well as attractions. I wondered if it might be embarrassing to some to have me return to a position over them where I had not so many years before been a mere Instructor. I also wondered if it would not be more interesting to enter a new field rather than to return to surroundings which

I already knew. Minnesota, like Illinois, had one arrangement which I thought advantageous. Chemical engineering was wholly within the chemistry department and controlled by one faculty. While at Michigan chemical engineering was independent of chemistry and under the engineering faculty.

I visited both places and was cordially received. At Ann Arbor the Apostles Club was still active and I enjoyed dining there though I greatly missed many of my old associates. The chemistry building, constructed while I was formerly there, already was becoming too small, and I realized that on account of its being a completely closed rectangle it would be difficult to add to it. At Minnesota the chemistry building was quite new and so large that some of the rooms on the top story were used as drawing rooms for other engineering students. No director of chemistry had been appointed since the retirement of Professor Frankforter two years before. Professor Bussey of the Mathematics Department had served as acting head. It was evident that a director was needed, and one that could help heal the wounds that had been caused by the failure of anyone within the department to be made director.

Upon my visit to President Coffman of Minnesota he told me he had thought it wiser to bring in a new head. In my interview he saw it would not be easy for me to decide and sensed that I needed a new incentive. This he accomplished in two ways. He pointed out the advantages to be director of a School of Chemistry with an independent faculty (including chemical engineering), rather than to be head of a chemistry department under a faculty of Arts and Science. He also showed his executive ability by raising my offer by $1000—which was no mean percentage raise, as university salaries were then not what they have now become.

I promised President Coffman I would give him my decision soon after consulting my wife. But I believe I was already convinced I should accept Minnesota. This was a relief as at times I had threatened to settle it by flipping a coin. But when my decision was announced some of my friends twitted me that I had decided with Minnesota's superior trout streams clearly in mind. I protested that I had never tried them, as I was never in the state of Minnesota until I went to be interviewed by the University. But I had heard they were good and hoped to find them so, as I later did.

CHAPTER 11

THE UNIVERSITY OF MINNESOTA

My attraction to this University in 1926 was due to a number of reasons. I regarded it as having a strong and unique organization. Its charter under the state constitution had given it unusual authority of administration without appeal to the Legislature. Its Board of Regents was rendered effective by its small number of nine Minnesota citizens appointed by the Governor under a precedent of careful, non-political selection. The Chairman of the Board is elected by vote of its own members.

The successive presidents of the University, chosen by the Board of Regents, were without exception men of high ability. The first, William Watts Folwell had studied at Hobart College and later in Europe but his academic career was interrupted by the Civil War in which he became an army engineer, advancing from lieutenant to colonel. In 1869 through the influence of friends in Minnesota the Regents were persuaded to appoint him as first president of the University of Minnesota. He had a high conception of what a university should be—"not just an overgrown college." He proceeded to organize it on a basis of high and rigid scholarship. He had many discouragements in finances and students' scholarships, or lack of them. He got great support from Governor John S. Pillsbury, an able financier, so friendly to the University that he was known as its "Father." The Morrill act also helped financially to save the University. But difficulties mounted until in 1882 Folwell thought it best to resign the presidency, but continue as professor of history. He spent the last years of his life writing a monumental four-volume history of Minnesota.

The second president, Cyrus Northrop, was brought from a Yale professorship of English to replace Folwell. He had just the qualifications needed to succeed as peacemaker and bring quiet to the University over which he presided happily for the next twenty-five years. During his administration the student body grew from a few hundred to about six thousand and the professional schools which Folwell had envisioned were brought into being. The benign pacification by Northrop extended to both faculty and students as Minnesota expanded to become a real university.

In its third president Minnesota was again most fortunate. Governor John Lind influenced George E. Vincent to come to Minnesota from the University of Chicago where he was professor of sociology and dean of the faculties of art, science and literature. His dynamic personality at once pervaded and electrified the entire University. This made it easy for him to reorganize and modernize those departments, which under the excessive conservatism of Northrup had gotten somewhat out of date. He reformed the Law School against great professional opposition and brought William Reynolds Vance from Yale to carry out this tough task as head and give the University of Minnesota a law school worthy of the State. Vincent also took a great step in establishing a relation between the University School of Medicine and the Mayo Clinic at Rochester, which through the years has proved of outstanding benefit to both. Vincent not only exerted his talents in stimulating the academic activities but also, with the help of Mrs. Vincent rejuvenated the social life of both students and faculty. He was in constant demand as speaker on all kinds of occasions and universally performed with brilliance. Another of Vincent's highly regarded procedures was his changes in the School of Medicine, not so drastic as that in Law, but equally effective. Dean F. F. Wesbrook accepted a call to the University of British Columbia as president and was replaced by Dr. C. M. Jackson as Dean of Medicine who for many years advanced its professional and scientific status. Vincent brought Guy Stanton Ford from Illinois to be first dean of the new Graduate School. One of Vincent's last acts as president after extricating Education from the Arts College was to persuade Lotus Coffman to come from Illinois to found a School of Education. It was with great regret that the University learned in 1917 of Vincent's approaching resignation. His work was done. He had given Minnesota a great university and a new stimulus for further progress. His imprint on its structure has been a lasting one.

Marion L. Burton became the fourth president of the University of Minnesota. Although a Minnesotan he did not linger long in his native state, but within a year accepted a call to be president of the University of Michigan. His term in Minnesota coincided with our participation in World War I. The War raised some question of divided national loyalty of a few faculty members and one of them was investigated and dismissed. Years later he was exonerated and recompensed for his unjust dismissal. Loss of both students and money, due to the War, extended beyond Burton's short tenure.

Lotus Coffman was in 1920 advanced from Dean of the School of Education to be fifth president of the University. This proved to be an excellent choice. By carrying on the plans of Vincent for expansion he justified and strengthened the "big university" idea by his persistence in appointing only those with outstanding qualifications to major positions in the many schools now comprising the University. Guidance through this critical period of growth by a man with Coffman's ability, perseverance and strength of character was of untold value. When need was, he battled valiantly with the state legislature, with the university regents, faculty or students. And sometimes he faced opposition from several local professions and from the general public. But he usually won and later events proved he had been right. Of all the nine presidents I think Coffman contributed most to the strength and prestige of the modern University of Minnesota. It is very fitting that the Coffman Memorial Union stands as a monument to such a grand man. He was the only one of Minnesota's presidents to die in office.

Following Coffman the University had two successive short-term presidents, both limited by retirement age. Ford was immediately called upon Coffman's sudden death (September 22, 1938) in his stead. He was later made president which he held for a little over three years until he reached retirement age. He was succeeded by Walter C. Coffey, Dean of the School of Agriculture who had come to Minnesota, as had also Coffman and Ford, from the University of Illinois. Coffey's presidency was terminated after only three years. But despite the brevity of both these terms, many important changes took place and the university continued to grow, as it always has, except when disrupted by World Wars I and II. The Graduate School, which under Ford as dean had swelled from 175 to 3300 students, continued to grow under Dean Blegen to around 6000. The University Press was created and immediately became a success, mainly in publishing books by the faculty members. Coffey's term was marked by steady progress. He continued his interest in the School of Agriculture that was made about the best in the country by such men as Clyde Bailey, Elvin Stakman, Herbert Hayes, Henry Schmitz and many others too numerous to mention.

James L. Morrill, eighth president, came from a brief term as president of the University of Wyoming after prolonged administrative experience at Ohio State University as dean, vice-president and acting president. With the able assistance of two men who shall be soon mentioned, he carried on during his term of sixteen years at Minnesota the progressive policies initiated by his predecessors. His suave demeanor generated no antipathies in the Legislature and many fine additions were made to the university, notably the Coffman Memorial Union, the twelve story Mayo Memorial, housing all the medical arts, the Duluth University branch, the Rosemount Research Center, buildings for Journalism and for the School of Business on the main campus.

The most recent president of the University, O. Meredith Wilson, came to Minnesota from the presidency of the University of Washington just when expansion in Minneapolis to the other side of the Mississippi River had begun. This ambitious program stimulated Wilson to support the move and to make large requests of funds. But instead of granting increases the Legislature made drastic cuts which not only embarrassed the administration but caused actual retrenchment in many directions. These periodic and sometimes spasmodic bursts of penuriousness on the part of the Legislature have cropped up occasionally from the very beginning of the University. But always the public reaction has been in favor of education and has practically forced legislative appropriations. It may confidently be expected that this will again happen and come to the rescue of the present administrative programs.

Coffman and all successive presidents had the invaluable aid of two men with unusual talents, Malcolm Willey and William Middlebrook. They came from the East where they had been fellow students at Dartmouth. It was Middlebrook's duty as Comptroller and later Business Vice President to assist the President with internal finances and to appear before the Legislature in the biennial appeals for appropriations. Owing to his many years of experience Middlebrook had acquired good methods of approach, a mixture of tact and insistence. In a state not so abounding in funds as some of the other midwestern states, this appeal was most critical and both the President and Vice-President heaved sighs of relief when it was over—and sometimes only sighs. The growth of the University constantly called for more funds, but during the Depression years severe cuts had to be taken. On the whole the Minnesota Legislature has dealt fairly with the University and at times generously.

Dean Malcolm Willey's assistance was more on the academic side. He had a brilliant mind, wrote well and spoke well, and was constantly called on for various kinds of chores, some of them challenging, others just dull and routine. But always he was willing and efficient. His aid will be greatly missed. He has recently retired after a long term of outstanding service.

Through the years one other man has rendered valiant service to all recent presidents in the central administration of the University. In his several and combined capacities as treasurer, comptroller, business manager and finally vice-president Lawrence Lunden exercised good judgment and nice discernment in carrying out his duties. He succeeded to the office of vice-president upon the retirement of Middlebrook in the summer of 1961.

But I am not writing a history of the University. For that one must see James Gray's recent "The University of Minnesota, 1851-1951." (University Press, 1951, 609 pages). Leaving that excellent text I must get on with chemistry and chemical engineering at Minnesota.

Chemical Engineering was one of the six divisions of the Department of Chemistry and was housed in the new School of Chemistry building when I became director in 1926. Professor Charles A. Mann, the Head of Chemical Engineering, possessed a very dynamic personality and had developed chemical engineering to the extent that the number of its graduates at the Bachelor stage exceeded those in chemistry by several fold. In graduate work, however, the other divisions of chemistry held their own in research and advanced degrees, notably organic chemistry under Professor William Hunter assisted by Dr. Lee Smith, who later became head upon Hunter's death and carried on for many years with the aid of Drs. Walter Lauer and Fred Koelsch. My own research was in physical chemistry, of which Professor Frank MacDougal was head, with a staff of Robert Livingston, Lloyd H. Reyerson and Bryce Crawford, who later became Dean of the Graduate School. In 1927 Professor Izaak Kolthoff was brought to Minnesota from the University of Utrecht to be head of Analytical Chemistry. My choice in selecting him became later more than justified by his outstanding contributions in research and his well known texts and treatises that extended to fields quite outside analytical chemistry, which he has also developed to a high plane of excellence. His graduates have attained high positions in their fields, Ernest B. Sandell at Minnesota, James J. Lingane at Harvard, Herbert A. Laitinen at Illinois. Regrettably, Professor Kolthoff reached the age of retirement in June, 1962.

The division of General Chemistry was presided over by Professor Cannon Sneed with an able staff. Besides the teaching of beginning chemistry for various departments of the University, research in inorganic chemistry was carried on by Professors Sneed, Maynard, Barber, and Heisig who distinguished himself especially in radiation chemistry. L. H. Reyerson has recently retired from the field of physical chemistry in which he has become so well known, but continues to publish recent work. Professor Sneed, now in retirement, is bringing out successive volumes of his Encyclopedia of Chemistry.

Beside my administrative duties in the School of Chemistry I found time to direct the work of assistants and graduate students in the radiation chemistry of gases. The loan of the Bureau of Mines radium had been extended to Minnesota and was used as a source of radon and as radium standards, principally in charge of Professor Robert Livingston.

Among the assistants who worked with me at Minnesota under various grants should be mentioned Dr. George Glockler, later at the University of Iowa and now at Duke University, who observed the effects and products of electrical discharge in a bank of ozonizer tubes in several gaseous systems, notably, butane from which he collected the liquid hydrocarbons condensed by the discharge and studied their forty fractions obtained by distillation. The largest fraction consisted of a mixture of octanes and octylenes produced by doubling the butane molecules by ionization according to the original mechanism of Lind. Dr. Hubert Alyea synthesized phosgene by the action of light and by alpha rays in the reaction mixture ($CO + Cl_2$) and determined the ion yields to be quite high but not nearly so high as in the $H_2 + Cl_2$ reaction. Dr. Robert Livingston measured the photopolymerization of acetylene, and the radiochemical reaction of hydrogen with chlorine (long chain) and with bromine (non-chain). Dr. George Schultze compared the rates of condensation in electrical discharge of the lower members of the paraffin, olefin and acetylene series. Dr. B. E. Cohn observed the color and luminescence produced in zinc borate glass; and Dr. J. C. Jungers of the University of Louvain with Dr. Shiflett and myself determined the ion yield of the radio-polymerization of di-deutero acetylene to be the same as that of ordinary acetylene.

Of the students who took the Ph.D. degree under my direction were Bernard Marks, who worked in electrical discharge in hydrocarbon gases; E. F. Ogg (deceased), who studied the temperature coefficient of the radiochemical synthesis of hydrogen bromide from its elements; E. C. Truesdale, who studied the reactions in the gaseous system hydrogen: sulphur: hydrogen sulphide under the influence of radon; Charles Rosenblum, who studied the kinetics of the $CO + O_2$ reaction induced by radon; C. H. Shiflett, who studied the temperature coefficient of water synthesis from its elements under α-irradiation, the polymerization of C_2D_2 by radon, the combination of deuterium and oxygen induced by radon, the exchange reaction between sodium and ethyl iodides using radioactive indicators, and the oxidation of cuprene by α-rays; C. S. Copeland, who produced neutrons by bombardment of light elements; F. C. Lanning, collaborator in my only venture since Paris into the radiochemistry of aqueous salt solutions, studied by means of dissolved radon the reactions of aqueous solutions of hydrogen iodide, hydrogen bromide, iodine and potassium permanganate. The primary reaction is the decomposition of water (yield about 1) and the secondary reactions are between the transient decomposition products (H_2, H_2O_2, H, OH) and the given solutes, hence both reduction by hydrogen, and oxidation by hydrogen peroxide. Miss Keren Gilmore, later Mrs. K. G. Brattain (deceased) studied the radiolysis of hydrogen iodide.

Midway is my long tenure at the University of Minnesota President Coffman decided to combine the College of Engineering, the School of Chemistry, and the School of Mines and its Experiment Station into one administrative unit with a single representative in his Advisory Committee of Deans. I was made dean of the new Institute of Technology. The Massachusetts Institute of Technology unofficially raised some objection to the use of the term *institute of technology* for a unit within a university and claimed not only priority in this terminology but exclusive right to its use. President Coffman, however, was not deterred in its adoption. The three faculties; (1) Engineering and Architecture, (2) Chemistry and Chemical Engineering, (3) Mining and Metallurgy, maintained their independence and held separate faculty meetings, as in the past at each of which I presided. On account of the approaching retirement of Dean Appleby, the School of Mines was not brought into the Institute until the end of his tenure, two years later.

The Mines Experiment Station had always enjoyed a large degree of independence under its very able director, C. W. Davis, who devoted most of his time to the painstaking development of a process for the treatment of the almost inexhaustible tonnage of low grade iron ore in northern Minnesota. This long-range project became more important as the high grade ores approached exhaustion in many mines. The low grade ores require extensive up-grading to prepare them for iron production in the blast furnace. Mr. Davis' timing was so opportune that his process, after some twenty years of experimental development, reached maturity just when it could be economically utilized. Davis also reached retirement age at the University about this time and immediately took control of the large installation on Lake Superior to apply his process in the beneficiation of the low grade ores by one of the commercial companies. Thus the iron ore indsutry was saved to Minnesota and to the United States for a long time to come. A town named Davis has been created with all modern facilities to house the workers in the enormous plants supervised by Davis on the north shore of Lake Superior. All of this is described in the Engineering and Mining Journal of December, 1956, to which the reader is referred.

How much credit for this development, beyond wise administration, is due to Dean Appleby I do not know. But I am sure I deserve none except in leaving Davis entirely free in the undertaking that I think turned out to be a model of the proper relation between university and essential industry by which the University benefits through a continuing financial support and the state from a large perpetuated industrial undertaking.

The Institute of Technology had occasional faculty meetings attended by those with rank above Assistant Professor. The meetings dealt with matters of common interest to all three units and was careful to avoid infringement on the prerogatives of the three individual faculties.

Dean Appleby, after many years of wise and able service, died soon after his retirement. Dr. Mann unfortunately did not live to achieve two of his most cherished ambitions—a separate building for chemical engineering and a faculty separate from that of chemistry. Both of these deaths came soon after my retirement in 1947, and also after two distinguished professors of chemical engineering had left the University, Drs. George Montillon now at the Gaseous Diffusion Plant at Oak Ridge and Ralph Montonna (deceased).

When we entered World War II many of our students wished deferment from service so as to finish their studies. The petition of each student had to be submitted to his particular Draft Board and had to bear my recommendation—following an interview. Thus to contact every student in the Institute as soon as he reached draft age took most of my time for the duration of the war. Although it interrupted my research I felt repaid in helping the students as well as the armed services.

While I was at the University of Minnesota I was elected President of the American Electrochemical Society in 1927, President of the American Chemical Society in 1940, and received the Nichols Medal of the New York Section of the American Chemical Society in 1926. I was elected to the National Academy of Sciences in 1930, to the American Philosophical Society in 1943, and to the Minnesota Academy of Science in 1940. I was elected member of the International Radium Standards Commission in 1928; the other members were Madame Curie, A. Debierne, A. S. Eve, H. Geiger, Otto Hahn, Stefan Meyer, Ernest Rutherford and E. Schweidler. I was given the honorary Doctor of Science degree at the University of Colorado in 1927, at Washington and Lee University in 1939, at the University of Michigan in 1940.

In 1938 during my tenure at the University of Minnesota I had been appointed one of the delegates of the American Chemical Society to the meeting of the International Union of Pure and Applied Chemistry which was to meet in Rome in June. I secured a leave of absence from the University so that I might attend. Mrs. Lind accompanied me on the Italian liner Count de Savoia (destroyed in World War II). Drs. Charles Parsons, James Norris, Thomas Midgley and many other friends made a pleasant party of American chemists on the boat trip. Mrs. Lind and I landed at Naples with our car when the fine harbor port was just being completed. After visiting Naples briefly, including the beautiful trip around the Amalfi Drive, we made a leisurely journey to Rome where we put up at the Hotel Roma during the week of the convention.

Of course the beauty of Rome entranced us but we did not find the use of a car very convenient in a city designed for horse or foot traffic centuries before the advent of automobiles. Our garage near the hotel was so crowded with closely packed cars that nearly all had to be moved or removed every time any one of them was extricated. So we used our car very little in the city after a mild encounter with a too hurried French bus arriving from Paris. We attended a gay party at the Hotel Excelsior, but owing to the bounteous festivities my ready cash ran out. The hotel declined to extend me credit since we were not staying there. Fortunately my friend Dr. Colin Fink (distinguished Professor of Electrochemistry at Columbia University) came to the rescue until I could draw on my travel funds next day.

I attempted to visit Professor Fermi in his laboratory at the University of Rome but did not succeed in finding him. Professor Hahn, I believe, had the same experience. It appeared later that he had left Rome in anticipation of the reprisals of the Mussolini regime soon to come. So he had disappeared and doubtless was making plans to come to the United States where he later made his home in Chicago until his untimely death in 1954. Besides his many other brilliant contributions Fermi is perhaps best remembered as presiding over and actually constructing piece by piece the first atomic reactor in the world to attain automatic operation. This success, due to his sagacity and patience, at once put uranium reactor on the map the world over. Fermi's untimely death was a great loss to science and it is most fitting that his name should be perpetuated in naming the 100th chemical element, FERMIUM.

Dr. Max Bodenstein, my former thesis director at the University of Leipzig, who had since changed to Hannover, and finally to Berlin, was in attendance as one of the German delegates to the International Union. With his customary hospitality he gave a fine dinner at an elegant restaurant. The number of his guests had to be restricted on account of the limited funds which a traveler could take out of Germany at that time. Present besides Mrs. Lind and myself were Professor Otto Hahn who later discovered the fission of U^{235}, Dr. Herbert Freundlich, author of the adsorption isotherm equation, and A. Mittasch of I. G. Farben Industry. But there was no limit to the bounty of the board and refreshments.

Dr. Hahn was already engaged in his experiments with uranium but had not yet reached the explanation with which he was soon to astound the world and set in motion the events that led to nuclear energy and atomic warfare. The latter was deplored by him later in his book "New Atoms" (p. 34) (Elsevier Publishing Company, 1950). He says: "The energy of nuclear physical reactions has been given into men's hands. Shall it be used for the assistance of free scientific thought, for social improvement and the betterment of the living conditions of mankind? Or will it be misused to destroy what mankind has built up in thousands of years?" The answer (he said) must be given without hestitation . . . but now twelve years later has not yet been given.

On leaving Rome we made Professor Bodenstein our guest in driving up to Florence with some visits to cathedrals and places of historic interest on the way. Arrival at our hotel was attended by an amusing incident. While I was looking after the car Mrs. Lind and Professor Bodenstein were shown into the hotel. On rejoining them I found the hotel manager insisting on lodging them together. I succeeded however in convincing him that I was the husband and that Professor Bodenstein should be segregated—much to his amusement at my expense.

We left Professor Bodenstein in Florence and journeyed to Paris where I visited the new Curie Laboratory and renewed acquaintance of Professor Irene Curie. She took me to one of the afternoon conferences of Professor Perrin. I also visited the laboratory of her husband, Professor Frederic Joliot, already engaged in the experiments that *later* made them both famous and won them a Nobel Pribe in Chemistry in 1935 for the discovery of artificially induced radioactivity.

While in Paris I had occasion to draw on my funds with the American Express Company. Later in England I began to believe I had been overpaid in Paris by the amount of one hundred dollars. But I was unable to confirm this until I returned home and could make a final balance of my expense accounts. On learning that my supposition seemed to be correct, I wrote to the Express Company in Paris, giving the date and inquiring if they had found a shortage—and in what amount. They confirmed a shortage of one hundred dollars on that particular date. I offered restitution on condition that no employee should be discharged but would be retained on receipt of my reimbursement. I received assurance and sent a draft for $100, but with the misgiving that the poor clerk was probably walking the streets already.

Mrs. Lind and I made the usual sight-seeing tours of England, Scotland and Wales, but since they involved no chemistry nor unusual occurrences, they are not recounted here.

In London we were joined by Professor and Mrs. George Burr from the University of Minnesota where he was Professor of Biochemistry. They had left their car in New York and our car was turned over to them and Mrs. Lind for a tour of several weeks on the continent, while I returned by boat to New York and picked up the Burr car for my use until they should return to Minneapolis.

In 1935 cars were not so abundant as now. There was no difficulty in parking on Fifth Avenue or anywhere else in New York City. After making a few purchases I had an uneventful drive back to Minneapolis where I returned to duty in the School of Chemistry of the University.

Looking back over my twenty-two years at the University of Minnesota I am most gratified by the contacts and friends I made there, not only in the School of Chemistry but in all other departments of the University and among chemists and other scientists that I met at scientific meetings.

The first year after my retirement at Minnesota in 1947 I remained in Minneapolis. The University kindly gave me quarters for my office and laboratory work. Dr. Marcel Vanpee, a research fellow from the University of Louvain in Belgium, worked with me in utilizing radon to study some reactions induced by alpha rays. I found him not only an agreeable associate but one utterly fearless of radiation. And if one could judge from his attitude, the opinion in Belgium did not support the extremely low tolerances that are being urged by Health Physics authorities in the United States and adopted by the international committees, to which further reference will be made later.

The state of Minnesota which supports its University is rich in natural resources. Its wide area extending between 43°34′ and 49°23′ north latitude and 89°34′ to 97°12′ west longitude make it an ideal location for agriculture, stock raising, mining, manufacture and industrial arts of all kinds. Its ten thousand lakes, distributed broadly over the state, give it an unexcelled water supply. Its long boundary on Lake Superior from Duluth and Superior (Wisconsin) northeast to the Canadian border provides water transportation for its rich and inexhaustible deposits of iron ore which move down, practically by gravity, from the centers about Hibbing to the highly equipped ports at Duluth and Two Harbors and thence by water to the blast furnaces. Its large grain supplies also move by water through Duluth to the eastern mills now in the vicinity of Buffalo. Modern conditions make it more economical to ship wheat to Buffalo for milling than to make flour on the Mississippi at Minneapolis as was earlier done. This left the great mills on the Mississippi in Minneapolis almost idle.

Unfortunately Minnesota is totally devoid of one most essential natural resource, fuel, either solid, liquid or gaseous. For this reason iron ore cannot be treated in Minnesota but must move down the lakes to meet fuel at the blast furnaces near Chicago and Cleveland. This lack of Minnesota fuel also curtails many other industrial processes. If atomic energy can be made to supply power cheaply enough, it may meet one of Minnesota's greatest industrial needs. The first attempt in this direction is now being made in the Elk River (Minnesota) atomic power reactor. A region without natural fuel is a made-to-order place to try out the potentialities of nuclear power, since the cost of fuel makes it possible to provide high funds for experimenting and later for operating. The answer to this economic question should be found in the near future.

CHAPTER 12

OAK RIDGE, TENNESSEE

In the spring of 1948 I attended a meeting in Oak Ridge and had my first glimpse of the activities that had been kept so successfully under cover since their beginning in 1943. Since I had no clearance my visit was quite limited in scope, but I saw enough to attract me strongly. I therefore applied to the Atomic Energy Commission (A.E.C.) for clearance, which took the usual course of a few months before issue. In July, I was duly cleared and soon after moving to Oak Ridge was appointed consultant to the Union Carbide Company that had the contract under A.E.C. to operate the three plants and laboratories. Due to my being over the age limit of sixty-five years, I could not be appointed as a regular employee but as a consultant, which had both advantages and disadvantages. My contract, which had to be renewed each year, gave me no paid vacations nor annuity insurance. Compensation was for each actual day of work only. Otherwise my contract was liberal in that the number of working days in the year was not limited. I could take vacation any time, travel officially at laboratory expense, and otherwise have all the privileges and duties of regular employment.

My wife and I moved to Oak Ridge and took the only quarters then available, one of the meagerly furnished, so-called Efficiency Apartments near the Guest House (now Alexander Hotel). We remained there for eighteen months and then moved to the Garden Apartments on the day they opened, January 1, 1950, and have lived there comfortably ever since.

My work in Oak Ridge centered first in the area of the Gaseous Diffusion Plant (K-25) which separates U^{235}, the fissionable isotope of uranium, by gaseous diffusion of its hexafluoride. Owing to the low content (less than 7 parts per thousand) of U^{235} in the commonly occurring U^{238}, and the slight difference in their atomic masses (about 1.3%), their separation is a most formidable undertaking. One must admire and marvel at the foresight and courage of those who backed up their calculations by the construction of an enormous plant to carry out, by infinite repetition of a single operation, the stupendous fractionation of two isotopic gases—and all of this construction and expense without any advance assurance of its success. Other processes of separation were considered but only one other, the electromagnetic process at Y-12 was operated on a full scale. Although it confirmed the theoretcial prediction of A. O. Nier (University of Minnesota), by producing enough U^{235} to supply one of the two nuclear weapons used in World War II, its cost was prohibitive and the process, with its many expensive separators, was abandoned except for one or two units reserved for scientific work.

At the time of its construction the successful Gaseous Diffusion Plant in Oak Ridge (K-25) was the largest plant in the world under one roof carrying out only one operation. It has been enlarged by several other larger units since that time. Other diffusion plants have been built at Paducah, Kentucky, and Portsmouth, Ohio. These three plants have continuously produced most of the free world's supply of U^{235}.

In my study of the diffusion process I realized that the fluorides being separated were constantly subjected to bombardment which might disrupt the UF_6 and cause some inefficiency due to precipitation of metallic uranium—or an oxide if any air were accidentally present in small amount. Such solids, actually found in small quantity, represent a slight inefficiency of the process, since the solids will not diffuse through the membrane and would subsequently have to be collected and reconverted to UF_6 to avoid loss of valuable uranium. A previous calculation by one of the staff members had indicated that the radiolytic precipitation of U^{235} by alpha irradiation would be negligible in quantity. I found, however, that this calculation was based on the intensity of radiation effective at initiation of the separation process, before the proportion of U^{235} had been enhanced by passage through the cascade. But the life of U^{235} is much shorter than that of U^{238}, which means that its rate of emission of alpha rays is correspondingly faster. Therefore at the enriched end of the cascade when U^{235} is approaching 100%, and also some U^{233} is arising with a still higher rate of change, the rate of decay and of ionization would be much greater than that previously calculated at the entrance to the cascade. My calculation taking these factors into account indicated that there would be an appreciable precipitation of uranium in the enriched fraction of the upper units of the cascade. This was tested in the laboratory by using radon as a higher source of alpha rays than enriched U^{235}. The result was precipitation. The experiments showed that, after elimination of solid oxides by the exclusion of air, further efforts to reduce the small quantity of solids would be futile, because there is no way to prevent the disruptive action of the alpha rays. The solid precipitates, however, are carefully collected and reconverted to UF_6.

Somewhat later, when changes in the organization at the Oak Ridge National Laboratory (ORNL) caused temporary transfer of Dr. Ellison Taylor, the Director of its Chemistry Division, to other work, I was asked to assume his duties. This brought me into closer contact with all the members of the Chemistry Division, and I was glad to become acquainted with its various problems and researches. I also had contacts with some of the physicists in fields of common interest. Most of my time was now spent at ORNL, but I still retained an office at K-25 where I received all my scientific mail and journals and did a good deal of my reading there, as well as continuing some consultation with the K-25 staff. My continued relation to K-25 also gave me the much appreciated opportunity of occasional visits with Mr. C. E. Center, then in charge for Union Carbide of all four areas, K-25, X-10, Y-12, and Paducah. I should report having seen him (June 13, 1962) after he returned to his office at K-25 following an illness and found him quite well restored.

Besides my earlier administrative duties at the Oak Ridge National Laboratory, I had time to take part in continuing some research in the radiation chemistry of gases. Dr. Philip Rudolph modified the system for radon collection by making the recombination of the undesired large amount of hydrogen and oxygen automatic and continuous. Radon thus purified and collected was used as activating agent in joint study with Rudolph of several reactions. The relative ionization potentials of acetylene (11.41 ev) and of benzene (9.25 ev) permits the prediction that, in a mixture of the two gases, benzene should, by charge transfer, inhibit the polymerization of acetylene, and thus diminish its rate of reaction. Lind and Rudolph proved this to be so—to the extent of about 32% retardation [J. Chem. Phys., *26*, 1768 (1957)]. This retardation in accord with theory is in the opposite direction from the accelerations found by Lind and Bardwell [J. Am. Chem. Soc., *48*, 1575 (1926)] when ionization potentials of inerts are above those of the reactants. Other radiochemical reactions studied with Rudolph were: the polymerization and dissociation of carbon monoxide, the polymerization of acetylene and of mixtures of acetylene with the several noble gases which could, by charge transfer, either accelerate or diminish yields according to the direction of charge transfer to or from reactants as dependent on the relative ionization potentials. Rudolph and Melton supplemented the researches by following these reactions and identifying their products in the *mass spectrometer,* which has the advantage of disclosing and measuring the primary steps, but is usually unable to follow all the intermediate stages leading to final products found by ordinary methods of analysis. A new type of instrumentation is needed to fill this gap.

[In 1949 Lind was elected to the Tennessee Academy of Science. The American Chemical Society awarded him its highest honor, the Priestly Medal, in 1952. In 1963, the University of Notre Dame bestowed on him his fourth honorary Doctor of Science degree.]

Oak Ridge itself is a very unusual community. The site selected early in World War II by a committee in the Army Corps of Engineers had to meet several conflicting requirements; remoteness from large population centers, yet adequate railway facilities to transport promptly all the materials needed for the quick construction of plants and residences; some distance from the seaboard and yet not too far from Washington and the East; sparse rural population without much agriculture which could be displaced without too much disruption or expense; but a location near centers of population that could supply workmen, supervisors, secretaries, etc., without their having to live in Oak Ridge itself. An area of about 90 square miles carved out of two East Tennessee counties, Anderson and Roane, was selected and acquired by the Atomic Energy Commission and administered by the so-called Roane-Anderson Company.

The administration of Oak Ridge (camouflaged as the Manhattan Engineering District) was first under the U. S. Army, with Major General Leslie R. Groves in control. The town of Oak Ridge was laid out by a firm of consulting architects which did an excellent job. Two main roads were constructed, generally East-West in direction—East Outer Drive and West Outer Drive on top of the ridge, at an elevation of 200-300 feet above the "Pike," which was the old highway between the two county seats, Clinton and Kingston. Connecting streets between these two thoroughfares were named for various states of the Union in alphabetical order, east to west. Off the streets, on both sides, lanes or circles were given names in alphabetical succession from the Pike up to Outer Drive with the initial letter the same as the state designation.

The plot was most skillfully and artistically laid out so as to take advantage of all the natural unspoiled beauties of the suroundinrgs. Frequently a home would look out on a valley or ridge with nothing else in sight, as if it were quite alone in a forest, and yet but a few yards below or above, hidden by the trees, another house would be enjoying isolation. The houses themselves were rapidly built and not meant to be permanent. Yet as the life of Oak Ridge has been lengthened, they have held out well and lent themselves to improvement and repair under private ownership that came later.

In the early days the entire area was protected by seven guard-houses, one on each of the entering highways. The three plants were rather widely separated from each other and from the town. The Oak Ridge

Gaseous Diffusion Plant (K-25) was located about ten miles due west near the west boundary of the area formed by a branch of the Clinch River. The Oak Ridge National Laboratory (ORNL) was located about ten miles southwest of the town and seven miles southeast of K-25. The third laboratory (Y-12) was located only one or two miles from Oak Ridge off the route to ORNL. Guard-houses were placed about halfway on each of the highways for additional protection, and each plant had a guard-house at each of the several gates of entrance. One had to show his badge in passing into or out of all guarded areas. Guards were on duty day and night.

In 1949 the outer seven gates to the area were thrown open. This was an event of such importance in the history of Oak Ridge that it called for some celebration. Senator Barkley of Kentucky cut the ribbon at the Elza gate on the Pike and Adolphe Menjou and movie actresses assisted in freeing the city from its shackles. The Oak Ridge scientists were not greatly interested and were skeptical of the appropriateness of such ceremonies for a city of the serious character of Oak Ridge. A few years later the midway gates were opened without éclat, and guarding was confined to the immediate entrances of the three plants. In 1958 large areas within ORNL were opened for anyone with general access. Entrance into wilder portions of the area, somewhat reduced from the original 89 square miles, is still prohibited on account of the expense of patrolling.

In 1956 the A.E.C. began selling the residences to private individuals on very inexpensive terms—first choice to the occupant—then to any other resident of the city—and finally to anyone without preference.

Later in 1956 the sale of individual lots for private residence was initiated and some quite modern and elegant residences were constructed both in the eastern and northwestern sections of the city. Also larger sections were sold at auction for housing projects. Many houses and apartments were built for rent or sale and in 1955 a large modern shopping center called "Downtown" was built south of the Pike.

For the past several years the population of Oak Ridge has remained fairly constant, a little below 30,000. Many of the plant workers continue to come from Knoxville and more distant locations. Anyone can now live in Oak Ridge, and private ownership of residences became popular when the Atomic Energy Commission began selling them.

In 1960 the Atomic Energy Commission turned the city over to a city council elected by the residents. Gradually the different facilities have been taken over by the city. The Council, of course, had and still has many problems, not the least of which is taxation. The fact that such a large proportion of the citizens are employed by a Government agency, which itself does not pay taxes, has complicated the question and tended to throw a larger share on the employees of the laboratories of the Atomic Energy Commission. However, the Union Carbide Corporation, as principal contractor, makes generous contributions to charity and to the community. The problem of taxation is likely to become more difficult as the Atomic Energy Commission diminishes its contributions annually over a ten year period with no agreement beyond that term, and will be subject to continuing negotiations. [The A.E.C.'s contributions have been extended and are still in force (1970)].

Besides the facilities provided for study in the different plant areas, another unique system supported by the Atomic Energy Commission is the Oak Ridge Institute of Nuclear Studies (ORINS). [The name was changed to Oak Ridge Associated Universities in 1966]. It provides courses in nuclear science, intended not only for nearby residents, but especially for students from other states and countries. These courses of different length have brought many students from all over the world and from every state of the United States to study special topics, or the more general fields of atomic energy. ORINS also maintains a public Museum of Atomic Energy which attracts many visitors. Its exhibits, besides those of physical and chemical interest, include radiobiology. The Institute also operates traveling exhibits which tour the country with stops of several days in cities and towns where their displays are welcomed and viewed by many visitors. Dr. W. G. Pollard, the Director, and his assistant, Dr. R. T. Overman [now a private consultant], have been most active and successful in building the facilities of ORINS, and in extending them in many directions, particularly to the southern and western states.

As yet, Oak Ridge has no large commercial organizations. Some efforts are being made to stimulate such development. If and when it comes, the general complexion of the city may be expected to undergo alternation. In the meantime, the City Council has enough problems with new and extended residence districts, streets, water, lighting, etc. to keep it busy, as well as the general question of racial segregation and location of business and residence. It is an advantage to have a clean slate, but at the same time, inexperience and lack of precedents may be disadvantageous.

Finally the question of racial integration is faced in Oak Ridge, as elsewhere in Tennessee, in the South, and in fact in the country at large despite the derision which some sections pour out on the others. Oak Ridge was fortunate in having the school question settled by the A.E.C. itself. Integration in the high school has been easy and without problems. Also integration in the lower grades, where it might be expected to be more difficult, has presented no problem. But this may be deceptive, since the lower grades are adequately taught in the school of Gamble Valley [this school was closed in 1967], itself so isolated from Oak Ridge that there is little practical reason for mixing in grade schools or in housing.

One may think then that Oak Ridge schools have set a shining example of integration. How much credit the citizens can claim (or shun) is tempered by the fact that the decision was not their own but one of the A.E.C. administration backed up by a Supreme Court decision. And if Oak Ridge were inclined to vaunt itself, one should look at its inaction in other directions. There is not a moving picture in the city that accepts integration and few restaurants tolerate it. Each operation is free to make this decision. Some of them would not object to integration but fear it would curtail rather than augment their patronage. One outdoor movie made the effort to integrate, but gave up after two weeks—questionably blamed on rowdiness of colored patrons from Knoxville. The entire picture of integration in Oak Ridge is so unsatisfactory that one must ask why in a community of such high percentage of intelligence, with people of broad views from all parts of the country, no stronger effort is being made. One reason, I believe, is that many Oak Ridgers are so engrossed with their own work and intrigued by its disclosures, that they give little thought to integration or other public questions, and that our Council is so occupied with more immediate problems it does not feel able to take on another.

Will Oak Ridge then wake up, seize its unusual opportunity to do something before it is too late? As Oak Ridge grows, the proportion of superior people may diminish. Even integration in the schools may be challenged, as the Atomic Energy Commission gradually reduces its support. This is the picture as I see it, and I am moved to exhort Oak Ridge to bestir itself and take action worthy of a community of intelligent people. But Oak Ridge is surrounded by a state in which prejudice against the Negro runs high in a population of about 80% white. And color has lost ground in the present twentieth century. When I was a boy in McMinnville the so-called "Opera House" had a second gallery for negroes where they saw and heard the occasional plays and minstrel shows that came to town. Today, as in Oak Ridge and in most towns and cities in Tennessee there is not even a picture show where the Negro is admitted [in 1970, only one business in Oak Ridge remained segregated]. And the relations between white and black are less cordial than a generation ago. The Negroes have retired into a shroud of dignity. The young no longer wish to be addressed by their given names, nor the old to be called "Uncle" or "Auntie." They have lost their smiling spontaneous greetings, insist on formal introduction and have replaced their old-time joviality with reserved behavior.

Scientific and technical progress, and the contributions of Oak Ridge scientists to it, seem now to give more reason for satisfaction than ever before. More new and important projects are being invested here by the Atomic Energy Commission. More scientists and engineers are being constantly employed. The all-important subject of experimental power reactors is being centered here. The fact, well realized, that they will not be created by short term research, lends permanence and future to our scientific structure.

It would be unfair to leave Oak Ridge without picturing it as a place to live. Its housing and original street arrangements have already been described. Unfortunately, the system was not followed in areas later added, and it is a problem to find one's way about in some of the newer districts.

The surroundings of Oak Ridge offer many attractions that are easily reached, the "Smokies," the Cumberlands, Knoxville, the various dams and lakes of the Tennessee Valley Authority where fishing abounds the year around—and where else can one try for rainbow trout at all seasons!

The public schools of Oak Ridge, formerly supported by the A.E.C., were developed with fine teachers, courses and buildings, now taken over by the city administration, and by an able School Board. The new High School with a large auditorium, gymnasium, lunch room and more than sixty classrooms is a model for any city, but already needs expansion.

Finally the recreation facilities of Oak Ridge deserve special attention. An unusually large, centrally located, swimming pool, owned by the city is leased for operation to a private concession that charges small fees. An eighteen-hole golf course, a swimming pool [and tennis and squash courts] provide the members of the country club with good facilities, and a large dining hall takes care of special parties and dancing. An active bridge club with the largest number of "Master" players for a town of this size in the United States has regular sessions and an annual tournament.

In closing, I do not fail to see that much in the foregoing chapters has little to do with chemistry but it attempts to picture the conditions under which chemists live and work. I hope that this background, personal and impersonal, will not be too harshly judged, and will add something to the interest, if any, of my subject.