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. Professor 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