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 dia-

monds 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