



# Autobiography of Philip B. King

By Philip B. King,<sup>1</sup>

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Initial product - 1973

The original hard copy of this report consists of 404 pages produced by King on his manual typewriter. Trudy Myrrh, Phil's daughter, kindly lent the original manuscript.

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# AUTOBIOGRAPHY

PHILIP B. KING

VOLUME 1 1903-1945

## PHILIP B. KING -- AUTOBIOGRAPHY

### Early years, through High School (1903 to 1920)

I was born on September 24, 1903, on my grandmother's farm in Chester, not far out of Richmond, Indiana. I don't know why I was born there, as my father was at the time a teacher at Pratt Institute in Brooklyn, and my babyhood was spent in Brooklyn. (I seem to have vague memories of Brooklyn, but they are probably enhanced from photographs in the family album).

My father came from a farming family, a part of a Quaker community which had come out from Pennsylvania to settle in the Richmond area. Like many Middlewestern farm families of the time, they were people of some cultivation, who valued education. Father and his three sisters all attended Earlham College, the Quaker school in Richmond. After graduation, he taught in country school for a time, and it was at that time that he met and married my mother, who similarly came from a farm family, this one in Illinois.

Mother's father was Jasper Burke, a music teacher from Kentucky, who had come to the Illinois community. He died shortly after mother was born, so he is no more than a shadow in the past. Mother's mother shortly after married Jake Lash, a plain farmer, and had two other children, Elsie and Boyd. The Lash family afterwards moved to Kansas, and lived for a time in Lebanon in the western part of the state, later in Sabetha farther east.

(To get ahead of my story, I strangely remember a visit with my mother to Lebanon. I was sitting in my high chair when they brought in the morning paper and spread it out on the kitchen table, saying, "Why! There has been an earthquake in San Francisco!" Checking the dates I was between two and three years old at the time!)).

Father had higher ambitions than being a mere country school teacher, and he took his bride to Chicago where he eventually obtained his Ph.D. in education at the University of Chicago. Possibly his first job after this was at the Pratt Institute in Brooklyn. Shortly after my birth, he obtained a professorship at the University of Michigan in Ann Arbor, and it was there that my brother Robert was born in 1906. In Ann Arbor the family built a home in the "mission" style that had a vogue in the early years of the century; it must have been exceptional, as it was written up with photographs in a magazine called "The Craftsman."

Clearly, father intended to settle down in Ann Arbor, but circumstances in the University which I do not understand forced him to move on, and he obtained another position at the University of Iowa in Iowa City, where we arrived in 1908. I remember something of our train ride there (in a day coach) and a ghastly first night in a railroad hotel across from the station. After that, we stayed in a boarding house across from the campus, while a search was made for a more permanent home.

This was a place on the west side of the Iowa River, across from the town, but within easy walking distance of the University. Nevertheless, this was looked upon as outside the pale of the town. I remember mother saying over the telephone, "This is Mrs. King on the West Side." It was even difficult to get groceries delivered there. Nevertheless, there was a line of homes on the top of the bluff overlooking the river. To north, on another spur of land, were some really substantial places, surrounded by tree-shaded lawns, inhabited by important families such as the Whetstone's and the Byington's. The homes in our area were more modest, and ours, which we rented, was one of the most modest of the lot. Next to it, a little higher on the hill, was a more imposing place with big pillars in front, which belonged to the Superintendent of Grounds of the University. After a few years we purchased this place, and moved there.

Soon it was time to enter school, and I was enrolled in the old First Ward School. Later, I was transferred to a grade school in another part of town, where a complex of school buildings occupied a large square --- a grade school, a grammar school, a high school, and a gymnasium.

Here, my troubles began, and my general "queerness" among the other boys became apparent. I had no aptitude for sports and other boyish activities, and was bookish and introspective. I was branded as a stuckup professor's boy. Many of the boys in school were from "Bohemia town" in the northeast part of the city, that was settled by blue-collar Czech families, who had no respect for intellect or education. As a result, most of my early school days (up to High School, in fact) were made miserable. But the less said about this, the better.

Robert was more outgoing than I was, but both of us were much inclined to reading, especially about foreign travel and exploration in distant places. I remember our spending one summer reading books about the Greenland and Vineland voyages of the Vikings. This habit grew upon us both, and more about this later.

Every summer the family made a long visit to the Indiana home. We would stop off in Chicago between trains, and were taken to the parks, and also to the Field Museum, still housed in the lath and plaster buildings that were a holdover from the Chicago World's Fair of 1893. We went through Chicago in August, 1914, when newsboys were crying extras about the outbreak of the Great War in Europe, which had burst suddenly on us in the sleepy Middle West, casting a sense of doom over the city. What a contrast was Iowa City! On our return home I excitedly asked my schoolmates which side they were on, and received only blank looks.

On one of our trips to Indiana, the family decided on a treat -- to go by river steamer to St. Louis, thence east by train to Indiana. We went by train from Iowa City to Muscatine on the now defunct Burlington, Cedar Rapids, and Northern Railroad, and waited on the river bank for the steamboat from the north. We went aboard and were off. At night we went through the locks at Keokuk, and all the next day went down the river, past big limestone bluffs, to St. Louis. It was a genuine old-time river steamboat, one of the last of its kind, even in that day, and was an unforgettable experience for which I will always be grateful to my family.

Father had many interests, and among them an amateur interest in geology. I remember once in Richmond, when I was between 5 and 10, how father and an Earlham professor went by horse and buggy on an early morning south on the Whitewater River to a shale bank where there were abundant Ordovician (Richmondian) fossils. I remember the pristine summer morning, and climbing about on the shale bank; at one point the professor killed a copperhead with his hammer that I had nearly stepped on. I remember the name of Streptelasma corniculum, a cup coral that was very abundant on the shale bank.

In Iowa City, father made the acquaintance of Samuel Calvin, the austere grand old man of Iowa geology, and we visited him in Old Science Hall, the geology building, and I remember looking with envy at his fossil collections, and wishing that I could find some of the fantastic trilobites there on display.

One day, father took Robert and me to Solon in the northeast part of our county, where there were broader outcrops than usual of the fossiliferous Devonian limestone. We went on the B.C.R. & N. Railroad, changing at a junction on the way. Robert was very little, and talked afterwards about our trip to "Holon".

About the time I reached high school age, a University High School was organized, independent of the city schools, which met in University buildings, and I was transferred to it, and graduated from it in 1920. My troubles with the rough boys at the city schools came to an end, although I remained an odd duck, different from my school-mates.

The Great War was gradually making itself felt, even in bucolic Iowa. There was strong pro-German sentiment in Iowa, because many of the people were of German descent, and German was the standard foreign language course in the high schools. But by the time I began in high school, German disappeared and the former German teachers were teaching Latin. I took Latin for two years, then French in my final two years in high school.

For a few months I had a paper route, and I delivered papers the morning after the Wilson-Hughes presidential election in 1916. Wilson was for "peace at any price." The morning papers said Hughes had won, and it was only later in the day when the returns from the western states came in that Wilson's reelection was decided. The next year, war was declared against Germany, and everyone suddenly became very patriotic, and pro-German sentiment went underground. I remember the excesses of the time, the fervent belief in all the propaganda, the Holy Crusade "to make this world a better place to live in". "If all the little German boys had only learned baseball, this never would have happened," and all sorts of other fatuous bilge.

Father was ardently pro-Ally from the beginning, and with the start of hostilities he decided to take a civilian job with the Y.M.C.A. He was away for a year or two, at Army cantonments in San Antonio and El Paso. Partly, this was a matter of his discontent with affairs at the University, where he felt that he was not properly recognized, although he had written and published a good many books on his subject. Promotions had not come, and we were very poor.

Armistice day came while father was away, and the whistles began to blow early in the morning. The whole town went wild with excitement, and I was asked to get on a truck with a gang of celebrants and blow my horn as we cruised the town.

Our youngest brother, Edward, had been born on June 4, 1915, and was still a tiny tot. He used to say afterwards "Tonny-waddo, peace celebration" ("tonny-waddo", we found out, meant "cans rattled", because many people tied tin cans to their vehicles to make a racket).

By the time I was a senior in high school, the war was over and many men had returned from service to tell of their experiences; some of them reentered our high school, which they had left several years earlier.

During our high school years, Robert and I continued our reading, mostly on foreign travel and exploration. Robert's chief interest was in the Arctic and Antarctic, and he read all the reports of the explorers. My interests ranged more widely. I spent much time ploughing through a prosy 3-volume narrative of Barth, an explorer who crossed and recrossed the Sahara from the north coast to central Africa. I also read everything I could find on Baja California.

I haunted the University library, and asked for so many books at the desk that they finally gave me free run of the stacks. There, among other things, I looked at the shelves of Congressional documents, which were hidden away in the basement. During the period 1840-1860, especially, these contained many reports on exploration of the west. I discovered the Pacific Railroad Reports of the 1850's with their lengthy texts and many maps. I also found Bancroft's many-volume history of the American west. It was only long afterwards that I realized the historical importance of these things, which I had discovered for myself.

In going through the card catalogue of the library, I found many interesting-looking geographical books labeled "Talbot Library," and I finally received the privilege of looking at this. I don't know the history of this collection, or its subsequent fate, but I hope that it is now carefully preserved by the University, under lock and key. At that time it was most disgracefully treated -- on some shelves in a corner of the zoology library in the basement of Science Hall. The collection must have been assembled by some unknown enthusiast for old geographical books. There were books dating back to the 1500's and 1600's in a remarkable state of preservation, and old atlases. There were also many books of later travel and exploration, down to the early part of the 19th Century. Robert and I took out and read the first edition of Sir John Franklin's first expedition to the shores of the Polar Sea in northern Canada; the Canadians told me a few years ago that this was a very valuable item in Canada today.

Father, on his return from service, felt increasingly alienated from the University, and resolved to go into business in Iowa City. The baby chick business was beginning and he decided to start a chick hatchery. This involved leaving our old home on the "west side", and building another home and plant in the east part of the city. He shed his intellectual interests, and reverted to his earlier upbringing as a farmer's boy; we had many empty lots surrounding us in which he set out gardens, strawberry beds, and the like. The chick hatchery occupied several long buildings in the back of our house. For a while, business boomed and he made a great deal of money. Later on, competition increased, and specialization cut into the profits -- not helped by the Great Depression which began 9 or 10 years after the business began. Nevertheless, he remained in the business until his death in 1937.

My four years of high school came to an end, when I graduated from University High School in June, 1920. The last year seemed glamorous and exciting, and I found it difficult to adjust to the wider outside world of the University into which I entered.

### University of Iowa (1920 to 1924)

In September, 1920, I enrolled as a freshman at the State University of Iowa. There were no stringent entrance requirements in those days; one's high school records were sufficient for the registrar. Most of my classmates in high school who went on to the University fitted right in with the larger University world, to which they had been exposed on the same campus. Needless to say, I was not one of these; I was poor, shabby, and friendless, and had few social graces. Going to the University in the same town had both advantages and disadvantages. It saved money, because I did not have to leave home, but when classes were over I went home to a distant part of town, and was not thrown among the other students, so I saw little of the real University life, at least for the first few years.

I took European history, English composition, chemistry, and geology; also, the required courses in public speaking, physical training, R.O.T.C. military training, and Freshman lectures.

The latter was a 1-hour per week session given by the Dean of Men, mainly on the perils of venereal disease. I discovered that I had no aptitude for chemistry, despite an earlier exposure to it by my father. The chemical formulas and equations were bewildering, and I did badly in the laboratory. History and English were taught by two sympathetic and able women. The history lectures were informative, but I did not study very much because I knew most of it already; at the end of the year my teacher told me she would have given me an A if I had ever studied. My English teacher was impressed with my writing ability, and was very encouraging. I had hoped to take Art as a freshmen, but there were irreconcilable conflicts.

As to Geology, I had, as I have said, an interest in geography, and I took geology as the next best thing. The summer before, I had found among father's books a copy of a high-school edition of Salisbury's "Physiography" which I had read with interest, and our first semester text was a college edition of the same book, so the subject was familiar. Our second semester text was W. J. Miller's "Historical Geology". This was not much good, as I realized about 10 years later, when I was forced to use it as our text when I taught beginning geology at the University of Arizona.

The Geology department was in Old Science Hall, on the other side of the street from the main campus -- an imposing old-time three-storey brick building. In the early part of the Century it had stood on the main campus, but when New Science Hall was to be built on the site, it had been skidded over to its present location (I still don't see how they did it). The building housed the Geology department until just a few years ago, when it was moved to a newer building farther north, but Old Science Hall is still in use for other purposes.

The geology faculty were all University of Chicago graduates. In those days, Chicago was the great fountain-head of geology in the Middle West, and practically all the departments all over the region were staffed by Chicago men. This was due to the leadership of T. C. Chamberlin, with the able help of R. D. Salisbury, Stuart Weller, and others. Chamberlin (with Moulton) had put together a "planetesimal hypothesis" for the origin of the earth and the solar system, and this was preached as dogma in all the Middle-western schools. This situation led to a provincial outlook; we heard vaguely about such men in the east as William Morris Davis, Joseph Barrell, Charles Schuchert, and others, but they could just as well have been on another planet. Such European ideas as continental drift were away beyond the pale.

The Iowa faculty at the time consisted of George F. Kay (head of the department, Dean of the College of Liberal Arts, and State Geologist of Iowa), Arthur C. Trowbridge, Abram O. Thomas, Ralph C. Cheney, and Joseph J. Runner. C. K. Wentworth was an instructor, and there were various other instructors who came and went, and were soon forgotten. Kay was a stuffed shirt, chiefly notable now for being the father of Marshall Kay (of whom more later). Trowbridge was a truly dynamic guy, whom we all called "Trow," a competent general geologist and greatly interested in sedimentation. Thomas was the paleontologist, a benign person deeply interested in Devonian invertebrates. Cheney was a paleobotanist, who left soon after for the faculty at Berkeley, where he attained much greater fame. Runner taught mineralogy and petrography (of whom more later).

First semester Geology 1 was physical geology, and was taught by Trowbridge; second semester was historical geology, and was taught mainly by Thomas, partly by Cheney.

During my sophomore year I realized at last my desire to take Art. I had shown much aptitude for drawing and art in high school, but was prevented from taking it during my freshman year at the University because of hopeless conflicts. The art classes met all afternoon nearly every day, which was a big block of time to take from other course work. I took first year art during my sophomore year, and continued with it two more years until graduation. First year art was charcoal drawing from plaster casts, second year was still life in color, third year was design. Fourth year was life (which I did not get to take).

In the art classes I formed the only real attachments that I made at the University. Our group was relatively small so we got to know each other well, boys and girls, and for the most part we remained with it to the end. We had a Christmas party each year, and our big event of the year was the artist's ball in the spring.

I began to practice cartooning, and many of my drawings were published in the University humor magazine "Frivol," and in my senior year I contributed drawings to the annual "Hawkeye."

During my sophomore year I also took Physics (a bore), French, and English literature among others. I did well in English, and the next year our teacher (a man) asked me to read themes for him -- a drudgerous chore, but it brought in a few hundred dollars, a great blessing to my straitened finances.

Deprived of my geography and geology that year, I spent much time in the Geology departmental library looking at books and reports. In the spring when I was there, C. K. Wentworth came through and stopped, saying "I've seen you around a lot, who are you?" After introductions, he said, "Why don't you take my field course at Baraboo this June?" Which I did, for it was a welcome escape from the boredom of a long summer at home. It was my first introduction to field geology.

I went by train to Baraboo, changing in Chicago. Baraboo is a little north of Madison, Wisconsin, and has a set of Precambrian quartzite ridges, surrounded and partly buried by lower Paleozoic sediments, mostly sandstones. The area was a favorite field of study of many Middlewestern Universities because of its accessibility. Our camp was on the shore of Devils Lake south of the town, which fills a valley cut by the pre-glacial Wisconsin River through one of the quartzite ridges. There were about a dozen in our group, mostly Iowa

students, some from other schools, and two girls. I was assigned as tentmate and fellow field worker to an Indian student named Roy, with whom I did not get along; everyone called him "Rajah".

Wentworth was a hard taskmaster. We lived in tents, and he made us wash our own clothes in the lake. Our toilets were two trench privies, in which we rested on two-by-fours. When we moved camp in the middle of the field season about ten miles to the southwest, he turned us loose and told us to make our way to the new location on foot, living off the country. Nobody would take me and Roy in for the night, so we spent a miserable night in a barn. Some of the other partners were more gregarious with the local people and reported that they had had most pleasant experiences.

Wentworth never told us specifically what to look for; he marked out a route for the day on the map, and we were supposed to observe what we could. For my and Roy's first day, he sent us along a road in the Baraboo basin, ending on a high hill. In the evening he asked what we had seen. "Well, Mr. Wentworth," I reported, "all we saw was a lot of sandstone." He looked disgusted. I found out later that we had walked over the entire lower Paleozoic sequence of the area, from the Cambrian into the Lower Ordovician, crossing several thin limestone marker beds in the sandstone, and ending in the St. Peter Sandstone at the top of the hill!

My perceptions got a little better by the end of the field season, but I still was not a real field geologist. I learned more about what I had missed in the area by reading reports after I got home.

Our course was only for four weeks, and after that I went home on the train, and was at home for the rest of the long summer. Father paid me to work around the place, but I nearly went crazy with loneliness. It was that summer, I believe, that I read nearly all of the historical novels by Dumas.

The next fall, I wrote a report on the field course, which was required, and typed it on father's ancient Blickensderfer typewriter. I compiled a lot from publications to flesh out what we had seen. Wentworth gave me a B, which was very generous of him.

Wentworth was an odd duck, who had a stormy career. He never could hold a job long, and always got into trouble and had to move on; he also had a succession of wives. He was very brilliant, and proud of his brilliance. Once he wrote out for us the value of pi

to a hundred places; he also carried in his head a set of logarithmic tables. He was especially interested in sedimentation, and he and Trowbridge worked closely together. His size classification of clastic rocks is still used as a standard. His final years were spent peacefully in Hawaii, where he acquired a Portuguese wife.

In my junior year I took mineralogy and petrography under Runner, with whom I had had little contact hitherto. He was jolly and outgoing, quite a contrast with the rest of the department, and something of a maverick; he shocked them all by talking seriously about continental drift. For some reason, he liked me, and took me under his wing. He gave me an assistantship to sort out the Department's mineral collection, in which there were many beautiful specimens. I was entranced with them, but didn't do very well because of the drudgery involved.

The summer following my junior year I took the Black Hills field course under Joe Runner. This would not begin until the first of July, because he wanted to use the month of June for a consulting job in North Dakota. He took me along as his assistant. I went by train to Minneapolis, then by the Northern Pacific to Bismark, then by a dinky little train to Mott at the end of the railroad, in the southwestern part of the state, where I met Runner. All this was in the Great West, and was quite new to me. I took many pictures from the train with my Brownie box camera, on our ride down the Missouri River valley, and up the Cannonball River valley to Mott. This was High Plains country in the continental Upper Cretaceous (Lance) and lower Tertiary (Fort Union). The wind blew clear and strong over the prairies, and the days were sunny. Mott was a dreary little cluster of houses, and we stayed at a large frame hotel, where they served good hearty meals.

Runner was drawing \$100.00 a day for his work for a local landowner. This was the standard fee for consultants, and was big money in those days. His job was to locate surface structures that might be drilled for oil. This wasn't easy, as the Fort Union Formation around Mott was a very lenticular deposit, in which the beds came and went, with no persistent beds from which structures could be mapped. Anyway, we were furnished with an old Model T Ford, and made some kind of a plane-table map, but I don't think we accomplished very much. (Actually, the area is in the Williston Basin, in which oil pools have been discovered long since, deep down in

in the Paleozoic formations; I don't know whether any have been discovered near Mott).

There was a gap of a week or two between the end of this work and the start of the field course, and Runner proposed that I go to Yellowstone Park on my own before going to the Black Hills. I took a bus north from Mott to the Northern Pacific, then a night train west by Pullman (one of my first trips on a Pullman car). The next morning I watched the Montana scenery go by and the mountains come up ahead; I had the Geological Survey's Northern Pacific guidebook bulletin with me, and followed the geology as we went along. We diverged south from Billings to Gardiner, at the park entrance. I bought a five-day bus tour of the park, ending again at Gardiner.

Night stops were at Mammoth Hot Springs, the Old Faithful Geyser Basin, Yellowstone Canyon, and Yellowstone Lake. I was entranced at the beauty of the country, at the big mountains, and the clear blue of the river waters. We stayed at camps (houses with tent roofs), and the personnel were all college students who were doing this as summer jobs. I was intrigued by the idea of these summer jobs, and wanted to do it myself next summer, but this was not to be. I heard later that the glamour of the job soon wore off. I took many pictures with my Brownie camera, which I still have.

The last day we went north from the lake over Mount Washburn, past Tower Falls, and into Gardiner, where I took the train in the evening. I have never been back to this part of the country again, but treasure it in my memory.

I rode the Burlington Railroad to Hot Springs at the south edge of the Black Hills, then north on another train through the hills to Deadwood in the north part. After a few days in Deadwood I went down to Whitewood, where the field camp was to be. I was made conscious that I was in The West, and Iowa seemed very far away. When I purchased cigarettes in Deadwood, I was given \$9.00 in silver dollars as change for a \$10.00 bill, and protested. The girl at the counter said coldly, "You're in the West, son!" Now, the Black Hills do not seem very far from Iowa, and all these regional customs have disappeared.

The Black Hills are an outlier of the Rocky Mountains, a forested upland rising far out in the Great Plains. The uplift brings up the characteristic Rocky Mountain Paleozoic and Mesozoic formations, with a large Precambrian core. Deadwood and Lead are at the north end of the Precambrian area, and Lead is the site of the great Homestake gold mine, still producing actively today. In the early twenties there were also many small gold mines in the area, still operating or recently abandoned, and many railroads ran through this part of the hills, with marvellous zig-zags and switchbacks, up and down the mountains; I guess they are all gone now.

Our field area at Whitewood, northeast of Deadwood, was in the Paleozoic and Mesozoic at the edge of the hills, and our main job was to map the Whitewood anticline that plunged north out of the hills. My first day in Whitewood, before the others arrived, I walked over to a great arch of Minnekahta Limestone on the crest of the anticline, prominently in view from the town.

Shortly after, the others came in, and we moved from the country hotel to the tents of our field camp in a park at the edge of town. There were about a dozen in the party, all men (there had been women in the parties of the years before, and Runner hated them). The participants were mostly University of Iowa students, but there were also two Yale men -- Charles Clark and June Binney (who arrived later).

Clark was the son of a professor of French at Yale, and was assigned to me as my tentmate and field worker; we got along very well. He did not stay with geology later; his father brought him back into French soon after. Binney was a real Eastern Establishment type, and the most world-weary man I have ever met. He had been at a Stanford field camp in California earlier in the summer. He was slowly dying from something, and was an inveterate smoker of Omar cigarettes. His family were Connecticut fat cats, and endowed a Binney fellowship in geology at Yale after his death.

We set to work on our mapping, getting acquainted with the formations, and marking them on enlarged topographic sheets --- the Minnekahta Limestone, Spearfish Redbeds, Sundance Formation, Lakota Sandstone, Fuson Shale, Dakota Sandstone, and higher Cretaceous formations. We did all our work on foot, and some days it involved long walks.

At the end of our Whitewood mapping, we moved camp to Lead, where Runner took us around to demonstrate his ideas of the structure of the Precambrian, which was the host of the Homestake ore body. We also had a day in the Homestake Mine itself, and one Sunday we took a train southeast to the Nemo district, which Runner had worked out, where there was another set of Precambrian formations -- quartzite, conglomerate, iron formation, limestone, etc. -- all with a highly complex structure. On another occasion, we took a long bus trip across the northern part of the Hills to Devils Tower, a monolith of Tertiary intrusive rock. Rain came up on that trip and the roads were impassable on the way home, so we spent the night in Sundance, an out-of-the-way little western town, where the field party played pool in the evening.

At the end of the field season we went down for a few days to the area of the Harney Peak Granite, and spent the night at the Devils Lake Hotel. It is a region of great granite knobs and monoliths, and Runner demonstrated his ideas of how it was intruded. (One of the monoliths later became Mount Rushmore, where the great figures have been carved, but that was after our day).

The summer was over, and we went back to Iowa on the train, this time on the day coach. I returned starry-eyed from my summer in the Great West, and Iowa seemed very dull by comparison. In my design course in the Art department I used western scenes as a motif, until our teacher warned me that designs "were not supposed to tell a story."

During my senior year I took Advanced General Geology, Trowbridge's banner upper class course. It was a good course, and I am surprised that more geology departments do not give something like it. His lectures repeated the same subjects as in Geology 1, but in more advanced terms, slanted toward the frontiers of the science. We were supposed to do a great deal of assigned reading, which I was never able to keep up with. Trowbridge's ideas were stimulating, but a bit provincial because of the Chicago background. In the historical part in the spring he cast out what seemed to be memorable pearls, and it was only later that I discovered that most of them came from Chamberlin and Salisbury's mammoth Textbook of Geology. Some of them, such as his pronouncements on the Permian, I found out later were manifestly wrong. For laboratory we studied topographic maps

in the fall and geologic maps (from the folios) in the spring -- a rigorous exercise that firmed up my long interest in maps. Few young geologists in other schools ever get such rigorous map training.

Among those who took the course was Marshall Kay, who was now majoring in geology like myself. I had known Marshall slightly for a long time, as we had been through University High School together, but during his first years at the University he had drifted off into chemistry, and had only come into geology later. We were, I think, the stars of the class, and long afterward he confessed that we were very jealous of me. I don't recall that I was jealous myself; after all, he was the Dean's son, who would have all the privileges. I was to see much of Marshall in later years.

My friends in the Art department could not understand my interest in geology, and after all, I have wondered how any one in Iowa City could have been interested in the subject, for it was a most uninspiring locale -- a terrane of dissected Kansan loess, without any distinguishing features of any kind. The only bedrock near the campus was a ledge of Devonian Cedar Valley Limestone at the base of the bluff on the far side of the river, on which some potholes had been cut by some stream during the Pleistocene. Student field trips were taken to the outcrop. (My brother Robert told me that after I had left school, he and another student had gone over one dark night and filled up the potholes with cement). The only other prize outcrop near the campus was the Interurban Cut, also on the west side of the river, which displayed a sequence of Pleistocene strata, mostly loess of various ages; the interurban line has long since disappeared, and the famous cut has been landscaped over.

In spite of its uninspiring surroundings, the Iowa department turned out many geologists, nevertheless, most of whom went into petroleum geology, and many of whom distinguished themselves later. John Emery Adams (later President of the A.A.P.G.) and Harry Ladd (later with the U.S. Geological Survey) were graduate students a few years ahead of me. Alfred Sidney Furcron (later State Geologist of Georgia) was in my class. There were others, both before and after me. Possibly some of them, like me, found geology as a means of getting away from the dull scenery and people of Iowa, into more exciting regions.

In the spring semester, Wentworth gave a field course, where we learned the rudiments of plane-table surveying. The plane-table at the time was a "must" for any aspiring geologist, especially a petroleum geologist, and thousands of square miles in the Midcontinent and elsewhere had been geologically surveyed by plane table. We only got an introduction to the method, but what little I learned helped me much in the oil company work later in the year. During bad weather Wentworth put us to work on arithmetical and mathematical problems relating to geology; my only real exposure to mathematics and made much clearer than the abstruse mathematics I had been exposed to elsewhere.

In the spring, some of us drove to Ames in Harry Ladd's Ford to attend the meeting of the Iowa Academy of Sciences. Here, I gave my first scientific paper, on some speculations about the physiography of southwestern North Dakota. It was a poor little thing, but I believe an abstract was published in the Academy Proceedings; the longer manuscript, mercifully, has long since disappeared.

My memory is curiously blank about my brother Robert during my University years, but he was in the University too. He was three years younger than me, but for reasons I do not now understand, he ended up only a year or so behind me in school. Unexpectedly, he decided to major in geology too. He took the Baraboo course the year after I did (when I was in the Black Hills), and the Black Hills course the next year (when I was in Texas). He was much more adventurous than I was. When the Baraboo course was over at the end of June he sent his suitcase home by express, and struck off on the freight trains for the wheat fields of the Northwest, where he worked as a field hand the rest of the summer.

I also took Comparative Zoology of Invertebrates during my senior year, given by Professor Nutting of the Zoology Department. He was an old-time naturalist and one of the best. Zoology at Iowa at the time was divided into the Department of Zoology under Nutting, and the Department of Animal Biology under Professor -----, where the emphasis was on the newer concepts of zoology, and had huge classes of pre-med students. Needless to say, the two professors and their departments were rivals. The comparative zoology course was considered the best training for geologists, and we studied in the laboratory the anatomy of the different invertebrate animals.

I continued to the end my French, and by my senior year was reasonably proficient in the written language, although I never learned to speak it. What I needed after graduation was a year in France to be really exposed to the language, but this was not to be. It was only years later that I was actually exposed to French-speaking people. Most of my fellow classmates were dolts, who even as seniors were unaware of the fine points of the language or its pronunciation; they called D'Artignan "Dart-Again", and synge (monkey) "singe"!

I had a happy time in the Art Department and had many friends there. I considered Art my "minor" and thought seriously about becoming a professional artist. Wisely, I gave up the idea. The only paying jobs in art were in advertising and similar fields. I was good, but I was too meticulous and pedestrian. Art was then, and still is, very faddish and fickle; the ones who make names in it are slap-dash and daring, and willing to follow the trends.

During my University years I knew a good many girls and was friendly with some, but I never dated anyone, and did not go to the dances or other college affairs. Women were still an unknown quantity.

The year was drawing to a close, and my University days were about over. In May, I was elected to junior membership in Sigma Xi (the scientific fraternity), and glory be! to Phi Beta Kappa. Election to this was entirely on grade averages, and I was rated B plus. I had bad marks in some subjects, but the many hours in Art classes where I always got straight A's brought up the average. My diploma was labeled "With high distinction" (there was one more category, "With highest distinction").

The Illinois politician Frank O. Loudon had endowed a series of prizes in different subjects for Iowa graduates; each was only for \$10.00, so it was a purely empty honor. That year, the Loudon prize in geology was split between me and Marshall Kay (the faculty couldn't decide between us), so we each got \$5.00. Months later, this piddling sum was mailed to me in Texas, and I probably spent it on cigarettes.

The only sour note was that a few weeks before graduating time the registrar's office notified me that I had not fulfilled all the physical education requirements, and if I did not, I couldn't graduate. Back in freshman or sophomore year, the physical education department had everyone perform a set of feats -- chin oneself ten times, etc. --I couldn't do these at the time (and still can't), but each one that tried was checked off. One of the requirements was to dive into the pool and retrieve an object on the bottom. The whole thing was such a farce that I didn't bother to even try this. Now, I had to do it in order to graduate, so I went down and practiced, and succeeded in passing the test.

With graduation facing me, I finally had to look ahead to my future, which was uncertain and cloudy. I took the Geological Survey Civil Service examination in the spring, and passed with a low grade of 73, but no Survey offers were made. Professor Nutting offered me a paid assistantship in Zoology, but my geology professors advised me to turn it down. There was vague talk about finding me a job as field assistant in some geological party in the Rocky Mountains, but nothing came of it. The geology faculty suggested that I get jobs in consulting work in Iowa, but I was too inexperienced for this and too timid; they also suggested that I just strike out on my own for the oil field country to the south and work as a roustabout, but this was another unknown quantity.

Dear Joe Runner took a real interest in helping me, and wrote to friends, including J. V. Howell, an Iowa graduate who was now in the oil business in Oklahoma and Texas. The week before graduation, Howell sent a telegram asking me to report to the office of the Marland Oil Company in Dallas, which I accepted, and so my career as a geologist began. (I actually did not meet Howell until years later, after I had made myself a reputation. I thanked him profusely for his help in getting me started, and he was much surprised. He had no recollection of what he had done). I was not quite twenty-one; my twenty-first birthday would be in September.

Texas, Yale, and Arizona (1924 to 1930)

In June, 1924, a few days after graduation, I went by train from Iowa City to Dallas. Father sat with me at the station until train time, and made doleful remarks about my inexperience, how in the great world people would take advantage of me, and how I would be subjected to temptations. Dallas and the Southwest seemed a long ways off in those days; it was another world, far removed from bucolic Iowa.

I arrived in Dallas of a morning, and its tall buildings rose up across the park from the train station. I had been in big cities before, but only to change trains; now I was to live in one for awhile. I checked my bag at the station and went to the office of the Marland Oil Company of Texas, which occupied the floor of an office building. I made myself known to the Geology Department, and met Walter Berger, in charge of the office. He didn't know who I was and I was not expected. J. V. Howell, my sponsor, was in Amarillo, so Berger had to call him there and confirm his offer of a job to me. With that, I was hired as geological assistant at \$100.00 a month. I was told to spend a few days at the Jefferson Hotel while I looked for lodgings, and it was suggested that I stay at the Y.M.C.A., which had a big dormitory in the downtown section.

The Marland Oil Company was the creature of E. W. Marland, who had made a fortune in the Ponca City oil field in Oklahoma, and had founded his company there. Now, his company was engaged in vigorous expansion; the Marland Oil Company of Texas was a subsidiary, and there were others. Among other places, Marland geologists were exploring Baja California in Mexico, and the shores of the Gulf of California. Although many good men were on the staff, all this expansion did not pay off so handsomely, and by the early thirties the company gave up, and was merged with the Continental Oil Company. Marland himself went into politics, and for a time in the thirties was U.S. Senator from Oklahoma.

In the office I was put to work plotting well logs from Mississippi, a new experience, with the plan that I would be sent to the field in about six weeks. Berger, a kind-hearted older man, was in charge. Also in the office was Danvers, a younger man recently from the University of Oklahoma. He was a collegiate football type, and I was amazed to find such a man going into geology; at Iowa,

only the odd-balls and queers studied geology, men such as Danvers would have studied engineering or law. Drew D. Christner, a rather sinister figure, was a sort of senior advisor.

The Y.M.C.A. was a dreary place, with spartan rooms. I took my meals at restaurants, and ate often at Britling's Cafeteria. I remember how a girl went from table to table offering hot soda biscuits. At home, soda biscuits were something which mother made up when we were out of bread. In the Southwest, I found that biscuits were considered a delicacy, and that real bread was nearly unknown. In the evenings, I went to the movies or to vaudeville, of which there were several good corny programs. I saw my first traffic lights in Dallas; they had not yet come to Iowa. I started to walk across a street and a policeman waved me back and told me to wait for the light. The lights were rather primitive; when a light changed a bell would ring during the yellow interval.

While in Dallas, I purchased from the Bureau of Economic Geology at Austin their "Review of the Geology of Texas" and their "Geologic Map of Texas", which I studied avidly. All the formations were laid out nicely, according to age and sequence, but I was intrigued to discover that the formations in one area did not fit the general scheme. In the Marathon region there was a set of peculiar lower Paleozoic formations, and in the adjoining Glass Mountains there were Permian rocks that included great masses of dolomite. The Permian was supposed to be all redbeds; what were these dolomites? (As things turned out, much more of this later).

Late in July, my dreary time in Dallas came to an end, and I was sent to West Texas to join a Marland field party at Rankin. West Texas was an unknown country to the oil companies at the time, but was believed to have great possibilities. Oil had been found prolifically on the Bend arch in north-central Texas, from whence the strata dipped westward under the Llano Estacado. On the opposite side, in New Mexico and in the trans-Pecos mountains the strata dipped east, thus forming a broad intervening basin. Oil shows had been found in the few wells that had been drilled, and on the strength of these a few small oil fields had been discovered near the edges of the basin -- at Artesia in New Mexico, at Westbrook in Mitchell County, and more recently at Big Lake in Reagan County. The oil occurred deep down in the Permian, and all the surface was covered by Cret-

aceous and Tertiary, several unconformities above. But the only thing known to do at the time was to survey the Cretaceous rocks for structure, hoping that these structures would become more important with depth.

I left Dallas by evening train, changing cars at Temple, a little to the south for the Santa Fe train to San Angelo. Next morning when I awoke, I watched the Texas scenery go by, and was in new and different country -- mesa-like forms in the flat-lying Lower Cretaceous limestones and marls, down-at-the-heel little towns with dusty streets, scattered houses, vacant lots, brick store buildings with metal awnings extending out to the edges of the sidewalks. At the stations I saw signs "for whites" and "for colored," but there were few colored people in evidence. San Angelo, where I arrived in the evening, was larger than the rest, with a good new hotel, the Angelus, where I spent the night.

Next morning I was to take the Kansas City, Mexico, and Orient train for Rankin, 100 miles to the west, leaving at 6:45, so I arrived promptly at the station. But this was a staggering railroad, long bankrupt, whose schedules turned out to be very uncertain. We were finally off after 10:00 on a mixed train, mostly freight cars, with a few coaches. The trainmen obviously didn't care much, and the train would unaccountably stop for long periods at some village, or even in the open country.

The Orient had been ambitiously laid out in the early part of the century, to run from Kansas City to the port of Topalobampo on the Gulf of California, and thus open up a new trade route to the Orient. But at that time only parts had been finished -- from Wichita, Kansas, to Alpine in West Texas, and short segments in Mexico out of Chihuahua and out of Topalobampo. Deprived of through connections the different parts languished. (The projected line is now complete. The part in the States was taken over by the Santa Fe which extended the line from Alpine to Presidio on the Rio Grande. The Mexican government has now completed the part in Mexico. Even today, however, there are no through trains from Kansas to the Gulf of California).

Leaving San Angelo, the country became more barren. Farmlands gave way to sterile grasslands and groves of mesquite. I was out in the semi-desert, in which I was to spend much time for the next several years. We came up onto the flat surface of the Edwards Plateau, stretching off for miles in every direction. We went through the little town of Big Lake, and a few miles to the west through the new Big Lake Oil Field, with its two settlements of Texon and Best. Oil rigs projected in every direction; there was a smell of hydrogen sulfide in the air, and crude oil was on the ground. The settlements were raw collections of hastily built frame houses. It was a meager prospect, but a portent of things to come.

From the oil field, the land sloped west to the Pecos Valley drainage, and after awhile we reached Rankin, a dismal collection of run-down houses, with a more substantial courthouse at the end of the street. Not far from the railroad was the Holmes Hotel, a sagging frame structure where the Marland geologists were staying.

Late in the afternoon our party came in. In charge was Melvin J. Collins, a chinless nearly bald person with a high-pitched laugh, who had some real ability but an erratic personality. Working together were Block and Jackson. Block was married, and he and his wife were living in a cottage nearby. He was somewhat older, a nice guy but something of a blow-hard, a University of Kansas graduate who was continually talking about "Doc Moore." Jackson was a rugged University of Oklahoma product, a square shooter who was proud of his fistic ability, which he used on occasion; he was a top-notch surveyor, very proud of his ability to close a long plane-table traverse within a foot or two of elevation. Finally, there was Lafayette Brown Herring ("Pinky"), newly transferred from San Antonio, with whom I was to work. We disliked each other from the start. He was a Kentuckian a few years older than I, with a certain amount of ability, but whose main interest was tail-chasing. He was the first man I ever met who considered it a moral necessity to have his "piece" every week.

With this mixed assortment I was to spend the greater part of the coming year. Afterwards, Block, the nicest one, disappeared completely from the scene and I never saw him again; I heard that he left geology and went into railroading. Jackson likewise disappeared. I saw Collins briefly in San Antonio in the mid-thirties, and heard that he died soon after. I saw Herring unexpectedly in Houston in the late forties, and he told me that he was then a mineral consultant in one of the Houston banks.

The party was surveying the "Hurdle structure" (later called the McCamey structure) west of Rankin. From here to the east the Lower Cretaceous limestones have a long, very gentle regional dip to the east, but west of Rankin they ran out in great mesas toward the lower lying Pecos Valley, and at the edge of the valley they bent down abruptly but somewhat erratically toward the valley, thus creating a closure or arch. Some people even then thought that the bending down was due to solution of salt in the underlying Permian, but this was hotly denied by others, including Collins. (Actually, we now know that it was due to salt solution, but oil fields have been found along the structure, so there was some kind of relation to the structures beneath). Collins was one of the first to recognize the structure, and the Marland Oil Company was one of the first to survey it; it was surveyed many times later by other oil companies.

The next morning Block, Jackson, Herring, and I went west in the company's Model T Ford to the area of survey. The great mesas of the Cretaceous, only 300 feet or so high stretched off endlessly in the distance, separated by broad, level-floored valleys -- a scene of great monotony. The same kind of country, we found later, extended clear to Fort Stockton, 75 miles or more farther west. No more than a few hundred feet of section was exposed in the whole expanse. At the bottom were the Basement Sands of the Cretaceous, followed by a set of limestone ledges, then by slope-making marls, and finally by the limestone caprock at the top of the mesas. (We now know that the lower limestone ledges are Fredericksburg, and the higher beds are lower Washita, but this was very uncertain at the time).

Surveying was done with alidade and plane-table, solely by stadia-rod shots; none of the party knew the fine points of triangulation. Traverses were run in long loops, and were supposed to close out at the ends within a few feet of horizontal and vertical distance. Ledges were walked out by the geologist, who put up the rod, and the assistant at the instrument sighted on the rod and determined the elevation. From these results, contour maps were made of the structure.

Block and Jackson had made a start on the Hurdle structure, but they were to be moved within a few days to another area west of the Pecos, and Herring and I were supposed to finish the survey of this area. What little experience I had gained at Iowa with the plane table helped me much, but it was pretty rudimentary and I never could come up with the precision of which Jackson was so proud. Our survey took us around the big mesa of King Mountain north of the railroad, and we finished the job toward the end of September. The equipment was inadequate. The plane table had a wobble in it and was hard to keep level. We had a Model T Ford which was always breaking down and getting many flat tires. Sometimes, when all the spare tires were used up, we had to drive the long miles back to town on the rim.

When the first weekend came, I was treated to the universal custom of the oil crowd. Everyone left their field headquarters and went into San Angelo for several days of sociability and carousing. It was at such times that Pinky found his inevitable "piece". These were prohibition days, but liquor was not hard to get; I had my first taste of booze on one of the weekends. But I did not care much for these weekends, and often stayed in Rankin.

The oil people were a rough crowd, quite different from anything I had been exposed to in Iowa, not only the drillers and roustabouts but the technical staffs --- land men, scouts, and geologists -- a carousing, drinking, whoring crowd. They made me a butt of their jokes, but other "nice boys" were also picked on. One chap with the Gulf found a rattlesnake in his room, and was given a cooked road-runner to eat. Later, I came to suspect that West Texas was the Siberia of the oil world, where the companies set the misfits they didn't know what to do with elsewhere, so I was seeing the dregs of the business. I was surely one of the misfits myself!

After finishing at Rankin, Pinky and I were sent for a few weeks to Sanderson, and then to Sheffield on the Pecos south of Rankin. We were supposed to run a plane-table traverse along the highway west from Sheffield to Fort Stockton, determining the structure of the Cretaceous ledges visible from the highway. Sheffield was a benighted, out-of-the-way village peopled by raw frontier types. The hotel where we stayed was run by Mr. and Mrs. McSpadden, two rough-hewn characters. One night two traveling whores stopped at the hotel and set up business for the night; men came from miles around and they did a roaring trade.

Block and Jackson had mapped a large block of country south of our traverse, living at an isolated ranch. When all our work was done, we all moved to Fort Stockton for the Christmas season, staying at the Stockton Hotel at the north edge of town. Fort Stockton was a nice town, as things went in West Texas, peopled by ranch families who lived in adobe houses. Many of them had sent their children away to college -- to the University of Texas at Austin, to Southern Methodist University in Dallas, to the College of Industrial Arts in Denton, etc. -- and they were home for the holidays. The Stockton Hotel was a large stone structure, run by Doctor Sibley (a dentist) and his wife, and we had a comfortable time there. Fort Stockton had had a brief oil boom a few years before, when a "miracle well" had flowed oil for a while from a depth of only a few hundred feet. As a reminder of the boom, there was a large empty building near the hotel with a painted inscription, "Dad's Dobe Dormitory -- clean beds 50 cents".

With the children home, there was a round of social activities, including a Christmas dance at which "Joe Teagarden and his orchestra" played. We danced until all hours. Pinky and Jack had many dates with the local girls. Jack was enamored with the Rooney girl, and when she went off to S.M.U. in Dallas, he entrusted her to his friend Danvers. This proved to be a mistake, because the two were married.

After Christmas, it was decided that the Marland Oil Company should open a district office in Midland. Midland was north of Rankin on the Texas and Pacific Railroad, and was a town about the same size as Fort Stockton, and similarly a forlorn collection of adobe houses of the ranch families. It had been through a land boom some years before, when there had been speculation in cotton agricultural land, but the country was too dry for it and the boom had collapsed.

I was assigned to the office to draft maps and plot well logs. Since I would not be on an expense account any longer my pay was raised to \$150.00 per month. Our office was in the Llano Hotel and for a time I lived in a room in the hotel. It was a dreary existence, as my room looked out on a court, so I rented a room with a family nearby. The room was unheated, and at night the continually blowing sand would come through my window and form little drifts across the bed.

(The Midland office proved to be abortive, as all the other companies had opened their offices in San Angelo, so after six months the experiment was abandoned, and the Marland also moved to San Angelo. It was only some years later that Midland became the office town for the oil companies, which it now is).

In the spring, Collins gave me an application for junior membership in the American Association of Petroleum Geologists, which I signed, and he urged me to attend the convention of the Association in Wichita, Kansas, and I went on the train. The A.A.P.G. was at that time only 5 or 6 years old, and the national meetings were smaller than its regional meetings today, most of the membership being from the Midcontinent or the Gulf Coast.

The meeting gave me an opportunity to see the wider world of petroleum geology. I heard papers on the structure, stratigraphy, and oil fields of Oklahoma, Kansas, and elsewhere. Several Survey men were there, including Bill Rubey (who had been working in Kansas), and Marcus Goldman (who presented plans for a color chart). I was thrilled by an exhibit of new Survey maps -- geologic maps of Oklahoma, New Mexico, and Arizona, and a map of the Black Hills (all but the first were by N. H. Darton). In the evening there was a satirical show, modeled after the Pick and Hammer Shows in Washington (many oil geologists were ex-Survey men), with topical skits about people and current developments.

In Midland, I was stymied because I had no automobile of my own to get around in, and I had a chance to buy a second-hand Essex Four touring car for a few hundred dollars. (The Essex was a junior edition of the bigger Hudson and did not survive the twenties; the Hudson itself ceased existence in the late thirties). This was the first vehicle I had ever owned, and wreck though it was, I was very proud of it.

In April, I was reprieved from the office work and sent to the field again (still at \$150 per month), this time to work with Block in the country east of Midland. This was a pleasant association; we got along well, and Mrs. Block was a kind-hearted, sympathetic woman. For awhile we mapped Cretaceous limestone around Sterling City, northwest of San Angelo, and then moved north to Colorado City on the T. and P. Railroad, which was off the Cretaceous escarpment in the Triassic redbeds. We worked in several areas that later became oil fields. I lived in rented rooms in private homes. Colorado City was a particularly attractive town, compared with the dismal West Texas places farther west. The country was moister, and there were more trees and vegetation.

It was in Colorado City in May that I got a letter from Robert. He was to be graduated that year, and was looking for a subject for a Master's thesis, especially one where he could collect fossils. I had already decided to quit the oil company when my year was up and go back to graduate work at Iowa myself. I suggested that we try the Glass Mountains, which I had always wanted to see. In the spring, the big shots of the Marland had made a trip there under the guidance of Dr. J. A. Udden, and the party had assembled at the Midland office -- D. D. Christner, W. A. J. M. Van der Gracht, Melvin Collins, and others. I dearly wished to be taken along, but was not invited. Here was a chance for Robert and me to see the Glass Mountains ourselves, on our own.

The Glass Mountains and Marathon country were wide open for anyone to work there. This was before the days when professors staked out large blocks of country as their personal field of research. The region had been investigated in reconnaissance only ten years before by Udden and his associates, Emil Bose and Charles Laurence Baker. Many people had gone there and looked around, but nobody had any thought of settling down and really working it out. A few years before, the U.S. Geological Survey, in cooperation with the Texas Bureau of Economic Geology, had topographically surveyed the area (the Altuda, Hess Canyon, Monument Spring, and Marathon 15-minute quadrangles), so the region was ready for geologic mapping.

Robert met me in Colorado City, and after farewells to the Blocks, we set off in my Essex, going through Midland and Fort Stockton in familiar country, then south to Marathon. Leaving Fort Stockton, we went through the too-familiar flat-lying Cretaceous mesas, but after awhile the geology changed. We could see a decided dip of the Cretaceous, off the Marathon uplift. Finally, we came to a gap in the hills and could look out over a vast plain broken by little ridges, stretching away westward to mountains in the distance. We were on sacred ground; the gap was Gaptank, and the plain ahead was the Marathon Basin.

Proceeding across the plain we came to the little town of Marathon, and found lodgings at the Chambers Hotel, a rambling frame structure south of the Southern Pacific railroad tracks, where we were given two rooms on the second floor of an outbuilding. The hotel was run by the Chambers family, ex-cowboys, but the real operations were conducted by Aunt Jane, an enormous colored woman, who cooked big hearty meals for the guests.

It was our plan to start at the west end of the Glass Mountains, in the Altuda quadrangle, and work east, mapping, measuring sections, and collecting fossils. For the first few weeks we worked in the plain south of the mountains, in the south part of the quadrangle, but the outcrops of Pennsylvanian there were complex and confusing, and we did not make much out of them. (As things turned out, this was the most difficult part of the whole region, and it has only been in the last few years that I really understood what went on there).

Then we started in on the Permian rocks to the north, where relations were plainer. I climbed the hill labeled 5280 on the topographic map, next west of the big intrusive knob of Iron Mountain. There was a considerable thickness of basal Permian limestone conglomerate --- a coarse-textured limestone crowded with big crinoid columns, and of chert pebbles and cobbles derived from the lower Paleozoic. I had never seen such a rock like this before, and was uncertain whether it was unusual. (Later experience has shown that it is very unusual).

Beyond this line of hills was a belt of lower country, cut on higher Permian beds, leading up to high escarpments formed by the Vidrio dolomite. Most of the low country was cut on what Udden called "shale", but it, too, was a most peculiar rock, thin-bedded like shale, but very hard and siliceous. Interbedded in the shale were thin limestone layers, whose weathered surfaces were replete with silicified fossils -- the true Permian fauna of strange brachiopods, ammonoids, bryozoans, and the like. Robert spent his time collecting. We made late starts in the mornings, but worked long hours and came in very late. Sometimes we came in after regular meal hours, and Aunt Jane left a cold supper for us on the dinner table.

The inhabitants of Marathon were a clannish, unfriendly lot, very suspicious of "foreigners"; a traveling man who was passing through said they were inbred. To add to our troubles, Robert had a taste for low company, and took up with a local guy who had just been released from Leavenworth, where he had served time for some infraction in the army. The only cosmopolitan note was two rubber experts staying at the hotel, who had had experience all over the Far East, and were supervising the processing of guayule at a plant east of the hotel. Guayule is a desert shrub from which rubber could be extracted. That summer it was being gathered up by the Mexicans and brought to the plant. Being a wild crop, the Texas occurrences were exhausted that summer, and the plant did not operate again. (It still grows in Mexico and various attempts were made to use it during World War II).

During August there was a spell of rainy weather, and we spent much time in town. One day, we saw a truck of the Geology Department of the University of Texas, and introduced ourselves. The drivers were geology students who said they were from a field party that were camped at the picnic grounds several miles southwest of Marathon. We had not had any geologists to talk to all summer, so we went down for a visit. Professor Luther Whitney was in charge, and his wife and daughter were along, as well as a group of students -- all cowering out of the rain under canvas. It was the summer field course of the department which had originally been planned for east Texas. They were rained out there, and Whitney decided to take the group to the west, hoping for better weather. We brought along our maps, and explained to Whitney what we were doing.

Our Essex ended the season the worse for wear. Besides the usual mechanical breakdowns and tire trouble, there were several greater disasters. One day we decided to drive to Terlingua in the Big Bend to see the country. On the way, ashes from my pipe blew into the back and ignited the upholstery. It was just a little burn and we tried desperately to put it out, but the upholstery was old and dry, so the fire kept spreading. In the end, we had to rip out all the back upholstery. Another time, we came back from a day of field work in the Glass Mountains to find the back curtain of the top in shreds. A cow standing nearby was still chewing on the pieces. She had evidently found the canvas was salty, and made a meal of it.

By early September, we had mapped the south half of the Altuda quadrangle, and it was time to quit and go back to Iowa. We loaded everything in the Essex and started back on the long trip home. We spent the first night in Del Rio and the next night in New Braunfels beyond San Antonio, where we had been confused and lost. The next morning, we stopped in Austin and visited the Bureau of Economic Geology, where we showed our map to Dr. Sellards, the Assistant Director. He was a kindly man, but an austere New Englander, who said little but "yes" and "hah!".

Beyond this, we made our halting way, through Texarkana and Little Rock, up the west side of the Mississippi, through St. Louis, and north to Iowa City. We could make only a few hundred miles a day, and stayed in country hotels in the little towns. Cross-country travel was not easy in those days. Few roads were paved, and the highways were not well marked. There were no State or Federal highway markers, only routes laid out by promoters, marked by painted insignia on telephone poles. Frequently, we would get into a town and not be able to find our way out, often getting on the wrong road and having to turn back.

Reaching Iowa, the roads were smoother and we sailed along, eating our dinner at Mount Pleasant only 50 miles from home. But Iowa at the time did not believe in paving, merely in grading and smoothing the clay. When we left Mount Pleasant it started to rain, and soon the roads were slick. We had to go in second or even low gear to make progress. About 8 miles from Iowa City, we ran out of gas for this reason. Robert proposed that I stay with the car and that he would walk home for help. Sometime in the early morning he returned with the family Model T, and with this help we limped home.

I entered the University of Iowa again, now as a graduate student, taking invertebrate paleontology under Thomas, and sedimentation. Trowbridge, who would ordinarily give this, was away that year on a foreign job in Mesopotamia, and his place was taken by a newcomer, Alan C. Tester, from the University of Kansas. There may have been other courses, but I can't remember them. I also had a graduate assistantship, helping in the Geology 1 course. At the start of the school year, Dean Kay called all the new assistants together for an indoctrination talk. He didn't say anything significant, except a long harangue about how no one should smoke in the building -- an obsession of his.

I worked hard at the University, but didn't seem to accomplish much -- later, I was to characterize it as "furious lost motion." I was much discouraged, but soon my fortunes were to take a dramatic turn for the better. Toward the end of October, I received an amazing letter from Professor Whitney, offering me a job as instructor at the University of Texas at \$1,800.00 per year. He mentioned as an inducement that Professor Schuchert would be there as Visiting Professor during the coming winter term. I arranged to accept the offer at the beginning of the winter quarter, after Christmas, and was permitted by the geology faculty at Iowa to leave before the end of their first semester. They were much surprised at the offer, because it was unheard of that anyone should get an instructorship when he did not even have a master's degree, and had only made a start in graduate study.

This was peculiar anyway, and later I was to learn the background. During the preceding year one of Professor Whitney's pets, Dorothy Ogden, had completed her master's with a thesis on the Foraminifera of the Walnut Clay, and Whitney had had her appointed as an instructor for the following year. But during the summer she had married a rising young Humble geologist, Ben Carsey, and had resigned her appointment. Whitney was a great do-nothing and procrastinator, so the appointment was unfilled. The rest of the faculty were desperate for more help in Geology 1, and along in the fall gave him an ultimatum. He had no ideas, and casting about all he could think of was my interview with him during the summer, and decided to offer me the job. (I didn't meet Dorothy Ogden Carsey until years later, and surprised her by thanking her for the opportunity she had unwittingly given me).

After Christmas at home, I went south by train to Austin, and arrived in the early morning. I ate breakfast at a little cafe across from the station, and went to the University several miles to the north. The Geology Department was on the third floor of the Old Main Building, but it was not yet 8:00 o'clock, so I had to wait in the hall for more than an hour for anyone to come in. Finally, Professor Whitney showed up, greeted me cordially, and introduced me to the rest of the staff.

The lecturers in Geology 1 were Fred Bullard and Arthur Dean; the only lab instructor of the course was Gordon Damon, with whom I was to work. Besides Whitney, the other senior faculty member was Professor Simonds.

Simonds was a charter member of the Geological Society of America, but since his arrival in Austin years ago he had done nothing significant. He had a booklined office, and taught physiography and regional geology -- out of the books, and with little imagination. Whitney taught paleontology and specialized on the Cretaceous, especially on the fauna of the Glen Rose Formation, on which he had researched for years. He was a charming, genial man, and a great talker, but a do-nothing and his crowning research on the Glen Rose was never completed. Bullard and Dean were younger and more active. Bullard was to achieve great fame as a popular lecturer, and retired a few years ago with honors. Dean was a good lecturer, too, but was flip and glib; even then he was peculiar. He was taking organ lessons, and he had a great fondness for boy students. Years later, he was fired from the faculty because of his homosexuality. Damon was a little, tired-looking older man, already gray, who was a nice, kind-hearted person, with whom I got along well. The Geology 1 laboratory sections met every afternoon. The classes were very large, and were divided into men's and women's sections. Bullard told me that they were going to give me the girls, and I brashly said, "Oh boy!"

I looked around for a place to stay, and found a room with Mrs. Bennett, who had a nice home in a residential area north of the campus. She was a widow and had two daughters who were away at school, and she gave me breakfast every morning. At noon and in the evening I ate at the faculty club, which was in an old rambling house west of the campus.

The University was a fine old institution, a real beacon of culture in an intellectual desert. But it was a struggling institution, set on a huge campus, but with inadequate funds and buildings. A few years later, oil royalties began to flow in and the University endowment increased greatly. The University owned huge blocks of land in the useless arid part of West Texas, but it turned out that many of the oil fields of the West Texas Permian Basin were on University land -- the first of these was the Big Lake oil field. Most of the University activities were in the Old Main Building, a huge three-storied stone structure on the crest of the hill in the center of the campus. On warm spring evenings the windows at the ends of the long corridors were left open, and bats sailed through, scooping up the insects swirling around the lights. (The Old Main has been gone these many years; with the oil money a huge new building surmounted by a tower has taken its place).

The supposed "godlessness" of the University was hotly resisted by the Texas religious element. All down Guadalupe Street west of the campus there were churches and institutions -- Baptist, Methodist, Presbyterian, Lutheran, and Catholic -- all designed, I believe, to counteract the already corrupted minds of "our girls and boys", and bring them back to the true faith -- in the Bible.

The Bureau of Economic Geology had its offices in the basement of B Hall, an ancient structure down the hill from Old Main, whose upper floors were a men's upper-class dormitory. The Department and the Bureau didn't get along, and there was rivalry between their senior men. J. A. Udden had been Director for many years, but was now old and withdrawn; he would stalk through the outer offices trailing cigar smoke and disappear into his own sanctum. Sellards, the Assistant Director, really ran things. At the time I arrived the only other geologist there was John T. Lonsdale, recently arrived from the University of Oklahoma, an aggressive chap who had graduated from the University of Iowa 10 years or so before I did. With this in common, we became great friends. The other geologist was Walter Scott Adkins, who at the time was in Europe on a Guggenheim grant. He was a specialist in Cretaceous paleontology, so Whitney hated him, and was constantly belittling his ability. It was base treachery to Whitney, my benefactor, but I was more attached to the Bureau than to the Department, and spent much of my spare time there.

After my discouraging previous year, Austin seemed a most delightful place, and I blossomed out. (Even in recent years when I returned there it has seemed more like coming home than any place where I have lived). Austin was a town of about 75,000, just big enough to be interesting, without being too big. It was more of a Southern city than any other place in Texas, with fine old homes and a lot of grace and charm. I bought a Model T Ford to get around in. The University swarmed with pretty girls. This was a short skirt period, the effect marred by the fact that their thighs were concealed by long bloomers. There were occasional road shows at the local theater -- traveling editions of plays like "The Green Hat," musicals like "Rose Marie," and revues like "George White's Scandals" and "Earl Carroll's Vanities." In between road shows, there was vaudeville.

Our Geology 1 class took many field trips around Austin, and I learned the formations and fossils of the classic Cretaceous (especially Lower Cretaceous) section, made famous by Robert T. Hill in the Austin Folio. With John Lonsdale and a few students, I also made weekend trips into the Llano area of Precambrian and lower Paleozoic rocks northwest of Austin.

Very shortly after my arrival I met Professor Schuchert, here for the winter term as Visiting Professor, who was lecturing to the students in the evening in a sing-song voice, mostly a rehash of what was in his textbook of Historical Geology. To my surprise, he was very interested to meet me. It seemed that he had decided that the Glass Mountains had the key section of Permian for North America, and when he was Visiting Professor at the University of Arizona some years before he had stopped off at Marathon and spent a few days there. The chief reason he had accepted the visiting post at Austin was to find some young geologist whom he could sponsor to work out the section. But Robert and I had already made a start there, so we were it.

He had me down to dinner several evenings at the Stephen F. Austin Hotel where he stayed, and we had some long talks. The upshot of it was that he wanted Robert and me to continue our work there, and that we should both go to Yale for our doctorates, and write our dissertations on the Glass Mountains. Robert, he took sight unseen, planned to buy his collections, and pay him for future collections, all to go to Peabody Museum at Yale. This was a dazzling prospect.

Up to then, I had had no further plan than to get a Master's degree at Iowa; I had never conceived of the idea of getting a doctorate at one of the famous schools in the East. So -- Robert was to go to Yale the following fall, and I would follow next year, when I finished my work at the University of Texas. Financing of further work in the Glass Mountains would come from a subsidy from Professor Schuchert for the fossil collections, and from expenses that had been arranged by Schuchert from the Bureau of Economic Geology.

Despite Schuchert's fame in geology, he was looked on askance by some of the University people outside the Department. Why, he didn't even have a University degree! (He was self-taught, like many geologists of an earlier generation). One lady said, "He came down here and told our girls and boys that white men were descended from nigras! It's dreadful that the University should pay a man to come here and corrupt our children!"

That spring, the meeting of the American Association of Petroleum Geologists was to be in Dallas, and I went up with the rest of the faculty and some students in Departmental vehicles. I gave a paper on the work so far done, on "Geologic structure of a portion of the Glass Mountains, west Texas," and it was published that fall in the Association Bulletin. Even today, I think it was a well-thought out little paper; in it, I contrasted the structure of the Permian rocks with those of the unconformably overlying Cretaceous -- a subject of much interest at the time in view of the extensive surface mapping of Cretaceous structures by the oil companies in West Texas.

Dr. Sellards invited me over for an evening at their home, and I met Mrs. Sellards and their two daughters, Helen and Daphne. Mrs. Sellards was a kind-hearted, out-going woman, and something of a social arbiter. She loved to have visiting celebrities at their home, and I met many of them there on later evenings. Helen and Daphne were supposed to sit in the circle after dinner, silently while their elders talked.

The Lonsdale's also had me to dinner and I met Mrs. Lonsdale, also a delightful woman, and I saw much of them during my time in Austin. Mrs. Lonsdale taught me how to behave in company; I was a pretty uncouth savage, with few social graces. Her younger sister, Ellen Van Arnem was there and she encouraged me to date her, although

she had already arranged a match for her with some Episcopalian divine. Ellen was very pretty, but very dull company.

When the winter term was over, Schuchert went home, and the visiting post was given to Arthur Keith of the U.S. Geological Survey, who gave lectures on the regional geology of North America, on which he was working for his G.S.A. Presidential address the following winter. He had spent years in geologic mapping in the Appalachians, and I found his analysis of maps and structures most stimulating. He had started his work as a young man, but by then he was old, dignified, and stiff-necked; later, I realized that many of his ideas were pretty old-fashioned. Nevertheless, he took a liking to me, and gave me some good advice on geologic problems.

It was time for another field season in the Glass Mountains (1926). Robert came down by train and drove the Model T to Alpine, where we were to start work on the unfinished part of the Altuda Quadrangle. I stayed in Austin to finish up the University work for the term, then went on later to Alpine by train.

Alpine was a more lively and more endearing place than Marathon, and was the seat of Sul Ross State Teacher's College, so many students were about for summer study. We had been over to Alpine several times during the preceding summer and had been attracted by the place. We stayed at the Garnett Hotel, a big frame boarding house on a hill south of town, run by two widows, Mary Garnett and her sister Pearl Pulliam. Mrs. Garnett had two daughters, Grace and "Babe". There was an amusing group of guests -- "Tiny" Bankhead, a fat school teacher taking summer work at Sul Ross, a young printer, a local grocer, a crabbed old optometrist, and a barber who was a Bible student, who was horrified at our godless ways; he once said, looking at us, "I and my people have always been good American citizens!"

We took "Tiny" along on some of our longer trips, especially one to Carlsbad Cavern which was just being developed by the Park Service. We had much tire trouble, and when we came out of the Cavern late in the afternoon two of our tires were flat. We pumped up one and put the spare on the other, and so limped into Carlsbad for repairs. A kind-hearted lady let us sleep that night in her back yard. Next day, we decided to go back by way of El Capitan, the bold cliff at the south end of the Guadalupe Mountains that was plainly in view from the Caverns. This was the locale of the "Guadalupean fauna", made famous

by G. H. Girty in Professional Paper 58, whose fossils were like those of the Permian of the Glass Mountains. The roads were incredibly bad, no more than a winding rocky track, but we arrived at Guadalupe Pass, where we could look down over a great sequence of sandstone and limestone strata. We descended over an incredibly steep and winding road, which it would have been impossible for us to have ascended in our Model T. On the way down, we met two boys in another Model T, who had been working all morning trying to come up the grade.

On another trip, Robert and I went to El Paso, where I had arranged to meet <sup>Alma Buuck</sup> an old girl-friend of Iowa days, who was teaching school at Clifton, Arizona, and with whom I had been corresponding sentimentally. She was staying at the Paso del Norte Hotel, where we also registered, but the rooms seemed very expensive, and we went back to Alpine without much money. The three of us went to Juarez, and I went shopping with her in El Paso. It seemed like a pleasant visit, but at the end she gave me a lecture on my manners, ending with the observation that my finger-nails were dirty. I knew this was the end, and the following winter she wrote that she was getting married. On this trip we looked at some geology; we climbed one of the lower spurs of the Sierra Diablo north of Van Horn and found Permian fossils like those in the Glass Mountains in what had been mapped as Pennsylvanian Hueco Limestone; we also visited the gypsum quarry at the north end of Malone Mountain.

Our work on the Altuda Quadrangle progressed well. The country was dry, whereas the preceding summer it had been very wet so that many roads were impassable. The geology was less interesting than the south part, all poorly fossiliferous upper dolomites of the Permian and the overlying Cretaceous. On Bissett Mountain, however, we found a unit of redbeds and conglomerate between the dolomites and the Cretaceous that had not been described before, and named it the Bissett Formation.

After finishing this quadrangle, we regretfully left Alpine and moved back to the Chambers Hotel in Marathon for the rest of the summer, to work the eastern part of the Glass Mountains. We worked along the south-facing escarpment of the Glass Mountains in the Hess Canyon Quadrangle, past Wolfcamp to Gaptank at the eastern end. We had not hitherto visited the famous Wolfcamp, but the country was open, and we went along the Fort Stockton road until opposite the hills, then across country to them. This was not to last long; that same summer

the area was bought up by Mr. Taylor for a sheep ranch, and he was busily engaged in fencing it. We found the shale with Uddenites in the lower part of the section, where many little ammonoids weathered out free, which Bose had proclaimed was the base of the Permian.

At the end of the season we drove back to Austin, and we both took the train back to Iowa City, I for a short visit, Robert to go on to Yale, for his first year.

That fall, I found that I could no longer get my comfortable quarters with Mrs. Bennett again, so had to go elsewhere. I found a room with Mrs. Penn, who lived in a huge old run-down house farther west in town. She was a widow who had a huge family -- two daughters and <sup>four</sup>~~three~~ sons; the two younger boys lived in the place, and one of them, Billy, became a geologist and went to Stanford. She was an aggressive, tempermental old biddy, who was sometimes hard to take. The housekeeping was done by a slovenly "nigra" girl (and how she hated "nigras"!)

One bright feature of the place was that the Lonsdale's had moved into a cottage on the corner of her big property, so I visited them often (and maybe wore out my welcome at times).

That fall, W. S. Adkins returned to Austin from Europe, where he had been studying ammonoid zonation of the Cretaceous under Spath and other "greats". I met him at one of Mrs. Sellards' inevitable evening parties, where he was very grumpy with the ladies, and later visited him at the Bureau. On my first visit he handed me an ammonoid and asked me what I made of it; said he couldn't make it out. I didn't know, of course, but I found him quite fascinating, and my disloyalty to Professor Whitney increased.

After his return from Europe, he had spent the summer in the Fort Stockton Quadrangle, making a start on the stratigraphy of the West Texas Lower Cretaceous, on which there had been the wildest conjectures hitherto. He was able to show that the greater part of the section was of Washita age, although it contained interbedded rudistid limestone layers which other people had called Edwards (Fredericksburg).

That fall term the Visiting Professor was William Morris Davis, the great geomorphologist ("physical geographer" as he would call it), who had been visiting various western schools after a long career at Harvard. I sat in on all of his lectures (as did Adkins) and took detailed notes that I was only able to work up years later. From him, I learned the deductive method of analysing geologic problems. Already at Iowa I had found that mathematical methods were the coming thing in geology, but I had no aptitude for them (and still don't). With Davis's inspiration, I decided that I could continue a while longer with non-mathematical methods in geology.

Except for Adkins and me, Davis's lectures at Austin fell on deaf ears; the Texas students were too provincial to be prepared for him. One day he gave a long geomorphic description of an area (clearly southern England), and asked a student, "Where is it?" The student said, "Sounds like the country down round Houston!" Davis was naturally frustrated and disgusted.

More to the point, Adkins and I learned about the new results and concepts of desert geomorphology that directly related to the field work that we were doing in West Texas. Most people had hitherto believed that the great alluvium-covered plains of the desert were deeply filled with detritus, but it now began to be realized that large parts of them were cut on bedrock, and that the alluvial cover was thin or wanting. This was particularly clear in the Marathon Basin (as well as the Fort Stockton area). During the fall, I worked up the geomorphology of the Glass Mountains and Marathon Basin for a Master's thesis at Iowa.

With Davis, the University had about shot its wad on available celebrities, and the two succeeding Visiting Professors were an anti-climax. During the winter term J. W. Beede was there, an old fuddy-duddy who had spent years on the Permian without accomplishing much. During the spring term, G. D. Harris, Whitney's old professor at Cornell, was visiting professor. He had done much work on the Tertiary and its fossils of the Coastal Plain, but he gave incomprehensible lectures.

During the preceding spring I had been much attracted by a beautiful girl in my laboratory class named Lucia James. That fall I saw her in the corridor of Old Main, and on an impulse invited her to a revue that was soon to be in Austin. To my surprise and delight she accepted! During the winter we went to many shows together, and in the spring to swim at the pool at Barton Springs. I wasn't in love, but I admired her deeply. But she always had a reserve that I could not penetrate. At the end of the year, when I was leaving Austin for good, I said goodbye very sentimentally and begged her to write me, but she never did, so that was that.

At Christmas, I went north by train to Iowa City, and Robert came back from New Haven. When I was leaving Austin, Mrs. Penn picked a bouquet of roses from her yard for me, and I still had them little wilted when I got home in sub-zero weather a few days later.

I brought along my manuscript on the geomorphology of the Glass Mountains and submitted it to the Geology Department for a Master's thesis. The Department gave me some written and oral examinations, and I was given my M.S. Degree in absentia in February. Trowbridge was astonished at the idea of rock-cut plains that I propounded (following Davis, Kirk Bryan, and others) and said it was quite new and unsettling to him.

After Christmas, some of the geology students at Iowa were driving in a Model T to Madison, Wisconsin, to attend the annual meeting of the Geological Society of America, and Robert and I went along. The weather was sub-zero, the roads were icy, and it was very wintry in Madison. The meeting was being held at the University, and we stayed at student dormitories that had been vacated for the holidays.

The G.S.A. meetings in those days were small, but all the "greats" were there -- Eliot Blackwelder and Bailey Willis from Stanford, A. C. Lawson from Berkeley, W. H. Hobbs from Michigan, A. C. Lane from Tufts, Charles Schuchert from Yale, E. O. Ulrich and others from Washington -- and many others. It was great to see them in action, and to rub elbows with them in the dormitory washrooms in the morning. At the meetings, they always sat in the front row and discussed each paper, sometimes acrimoniously between themselves. The papers were a minor event, the real show was the discussion that followed. For us, the important event of the meeting was a conference

between Professor Schuchert and Carl Dunbar of Yale, and Dr. Sellards, in which an agreement on our work in the Glass Mountains was hammered out in more permanent form, between Peabody Museum and the Bureau of Economic Geology.

Other people were getting interested in the Permian outcrop areas, and the Marland Oil Company had a research team in the field, with W. Grant Blanchard as field geologist and I. A. Keyte of Colorado College as paleontologist. They had been in the Glass Mountains, as well as large areas farther north in Texas and New Mexico. In the spring, the West Texas Geological Society held a field trip in the Glass Mountains with Blanchard and me as leaders, and I went out by train to meet them at Alpine, in the gap between the winter and spring quarters. Blanchard was an able rough-and-ready guy but rather ignorant of the fine points of geology. He and Keyte had developed some ideas different from ours, especially on the Pennsylvanian-Permian boundary at Wolfcamp, which they put above the Uddenites zone (it is still controversial), so the discussions were somewhat acrimonious, though inconclusive. I went back to Austin in a University vehicle with Dr. Sellards and some other people.

During the winter, Professor Schuchert had told me that Udden and Bose had talked about how the upper dolomites in the Glass Mountains might be reefs, and asked me to look into the matter. I was unconvinced, and for confirmation looked up the subject in Grabau's "Principles of Stratigraphy." There, to my surprise, I saw illustrations of the Triassic reefs in the Tyrol which had structures like some of the strange structures we had seen but not understood in the upper dolomites of the Glass Mountains. So perhaps they were great reefs after all! I was to find that the same idea occurred at about the same time to other geologists for the Capitan Limestone of the Guadalupe Mountains, and in the next few years the reef idea for the Permian rocks of West Texas was in full flower.

During the spring I decided to get into print, and was surprised how easy it was. I worked up our discovery of the Bissett Formation at the top of the Permian section in a paper for the American Journal of Science, and I submitted an interpretation of some erosional features near Austin to the Journal of Geology as "Corrosion and corrasion on Barton Creek, Austin, Texas."

The spring was over, and it was time to start the new field season in the Glass Mountains. But first, in late May or early June, the West Texas Geological Society had a field trip in the Delaware and Guadalupe Mountains. We drove west with Gordon Damon in his Buick four to Pecos, where the group assembled. We went across the Apache Mountains, along the Gypsum Plain east of the Delaware Mountains, and ended at McKittrick Canyon in the Guadalupe Mountains. Many of the leaders were ignorant and uncouth; one would get on an outcrop and say, "Boys, this is your Hueco Lime!" At McKittrick Canyon, Floyd C. Dodson expounded his idea that the Capitan Limestone intergraded with the Castile Gypsum to the southeast (a conclusion later found to be untenable).

After McKittrick Canyon, we went to Van Horn and spent the night at the old ratty hotel there. We were joined by W. S. Adkins who took us to see the Jurassic on Malone Mountain and the Cretaceous section on Cerro de Muleros, on the Mexican border west of El Paso.

We returned to Austin, where I packed up, said my unlamented goodbyes to Mrs. Penn, and drove west again to Fort Stockton, where our field work was to begin. We stayed again at the Stockton Hotel of pleasant memory. Many other geologists were staying there, for the region was very active that summer. Dr. and Mrs. Sibley were still the same kind hosts as before.

Our first effort was to survey Sierra Madera, the little knob of Permian carbonate hills projecting through the Cretaceous south of Fort Stockton. We found the rocks greatly confused, smashed, and jointed, and hence hard to understand, but were able to find two marker beds that extended discontinuously around the hills that proved a steep domal structure. The origin of such a complex uplift was an enigma. (Increasing evidence in later years shows that Sierra Madera is an "astrobleme" formed by extra-terrestrial impact, but recent detailed surveys show that our mapping of the rocks is nearly correct).

From Sierra Madera we worked west along the north side of the Glass Mountains, in the north part of the Hess Canyon Quadrangle and beyond. Dr. Sibley at the hotel told us that he owned a block of land here, and at his invitation we spent a week at an empty house on the property. The Sibley boy, a teen-ager at the time, went with us. We hauled out our camp equipment and supplies, and since there was no water there, we took with us a barrel of water that had once

held coca-cola extract. The first few days were idyllic, and we covered much ground. Later in the week, however, our presence became known to the local population. Flies, bugs, packrats, and other varmints moved in on us. The coca-cola extract in the barrel gradually polluted the water, so that it became nearly undrinkable. By the end of the week, we were glad to come in.

We then moved back to Marathon, where we found there was a fine new hotel, a two-story brick structure north of the railroad on the highway. It seemed that old Mr. Gage of San Antonio, who owned a large part of the Marathon Basin was tired of the cruddy accommodations in Marathon when he came out to visit, and decided to build a comfortable hotel of his own. It was being run by Mr. and Mrs. Pacetti, an Italian couple, who kept it spic and span, and served excellent meals.

Shortly after our arrival, a party showed up under the guidance of Paul Ruedemann, an oil geologist, who was showing the country to Rudolf Ruedemann, his father, who had attained fame for his long years for the New York State Museum in Albany. Also in the party were another geologist and his wife, Mr. and Mrs. Small. They were going to Boquillas, on the Rio Grande in the Big Bend, and invited us to go along. We went down to Boquillas amidst thunderstorms, and camped on the river bank. Ruedemann was a fine old genial gentleman, a great talker, with endless stories about life in Albany and the many geologists and personalities there. We made a trip to the village of Boquillas on the other side of the river. One was supposed to go to the river bank and halloo, and a man would come across on a horse to ferry us one at a time across the river. Mrs. Small was quite a character, who hopped on the horse, skirts and all. In the village we visited a cantina and drank tequila. Mrs. Small climbed on the bar and shouted "Viva Mexico." to the cheers of the natives.

Later that summer, we got a call from Mrs. Darton in Alpine, who said Mr. Darton was anxious to meet us. They were staying at the Garnett Hotel, of all places, and he was at work assembling the new Geologic Map of Texas. So we drove over and Mrs. Darton took us to her husband, who was at work on his maps on a card table in a second floor bedroom. I had heard about Darton for a long time, beginning in the Black Hills which he had mapped in a series of geologic folios. We had heard mixed reports about his work, as being hasty and superficial, but about his ability to cover enormous tracts of country in

in an amazingly short time. We found Darton compiling the Texas map one 30-minute quadrangle at a time, and he was not only marking the geology, but was drawing the topography first! Our visit was most pleasant, and our friendship ripened through the years.

Another pair of visitors was Frank Clark of the Mid-Kansas Oil Company, and his field geologist Leo Horton. Leo was a chunky, jolly guy whom we liked at once. We found he was a tail-chaser (like Pinky, of hated memory), but a much more amusing one. He was a precisionist with the plane-table, and he helped us to map the northeast part of the Marathon Basin within our project area, where there was no topography. One long weekend we went with him into El Paso where he rounded up some girls. We had a good time, part of it in Juarez, dining and drinking, with overtones of sex.

Toward the end of the summer, Carl Dunbar came down from Yale to inspect our dissertation work, and Leo, being an old Yale man, drove us around. The visit was rather irritating. Dunbar was continually picking flaws in our work, and finding contrary evidence. One time, out in the Pennsylvanian, he claimed he found a large Permian fusulinid, and it took much work by Robert to convince him it was just a fragment of a bryozoan. Maybe he was practicing the Socratic method, but it resulted in acrimonious arguments.

After the Glass Mountains had been looked at, Dunbar said he wanted to see the Carlsbad Caverns before he went home, so Leo drove us there. While we were going through the cavern, Leo screwed a woman, which was something of an achievement. He took up with an attractive, somewhat faded lady, who was going through the caverns by herself; they dropped back behind the party, and caught up later. (Leo's specialty was respectable ladies such as this one, who were a little older and whom love had passed by; they were very grateful).

After going through the Caverns we spent the night with Mrs. Glover at Pine Spring Camp near Guadalupe Pass, who was now taking in visitors for the night. Next morning, Leo drove us north along the west side of the Guadalupe Mountains to Dolf Williams's Ranch at Bone Spring Canyon -- a famous locality of which we had heard much, but had never seen before. After that, we went west across the Diablo Plateau by ranch tracks to Sierra Blanca (there were no good roads in the plateau in those days), where we put Dunbar on the train for home.

We used the summer of 1927 to finish our geologic mapping and geologic study of the Glass Mountains, and by the end of the season it was complete. During the last week in the field we came to realize that the lower Paleozoic rocks in the hills south of the west end of the Glass Mountains were overthrust on the adjacent Pennsylvanian on a nearly flat fault plane, and many miscellaneous facts in the puzzling area west of Marathon (where we had started our work in the summer of 1925) fell into place, and showed that movement on the great thrust plane amounted to nearly 9 miles. We named the feature the Dugout Creek overthrust.

At the end of field work, we drove the Model T to El Paso, and stored it for the winter in Longwell's Garage, then took the Southern Pacific north to Iowa City for a family visit, before going on to Yale.

I went east alone by train to New Haven (Robert was to come along a little later). As a grownup, I had never been east of Chicago before, so the country, the towns, and the people were new and strange to me. Arriving in New Haven, I went to Kirtland Hall, the old building that housed the Geology Department, then walked up elm-shaded Hillhouse Avenue, past venerable homes, to Peabody Museum, a newer pseudo-gothic structure that was the seat of the paleontologists and stratigraphers. I met Sally Donahue, the cute switchboard girl and librarian, and Valerie Jourdan, Dunbar's fossil preparator, and got my mail.

After Robert arrived, we looked for a place to stay (the previous year he had stayed at a Yale graduate dormitory which was pretty awful), and found rooms in a private home in the southwest part of New Haven. After Christmas, we moved to the Orange Inn, a rooming and boarding house not far from the Museum. I was disillusioned by the layout at Yale. Instead of being on a big campus, like Iowa and Texas, the buildings were stuck around higgledy-piggledy in the downtown section, with other quite different buildings in between. (I was to find the same thing at Harvard; only Princeton lived up to my ideal).

The Yale Geology faculty at the time consisted of A. B. Ford in mineralogy, Adolf Knopf in petrography, Alan Bateman in economic geology, Chester R. Longwell in structural geology, Richard Foster Flint in geomorphology and glacial geology, and Carl O. Dunbar in paleontology. Roland Brown was an instructor; Agar was also on the staff, but left at the end of the year. Richard Swann Lull, the Director of the Museum, gave lectures in organic evolution which drew an enormous enrollment. Hangers on, of whom we saw little, were Elsworth Huntington and R. W. Wieland. Schuchert was emeritus, and had a little office next to Dunbar's in the Museum. Longwell and Dunbar were relatively young and had only been there a few years, and we felt closest to them. Flint was the "baby" of the department, and not much older than I was, fresh from the University of Chicago.

During my first year, I took Stratigraphy under Dunbar, Structural Geology under Longwell, and Optical Mineralogy and Petrography under Ford and Knopf. I felt I had learned enough geomorphology from Davis's lectures the autumn before, so I did not take Flint's course. I was not interested in Economic Geology (and we had begun to form a dim view about Bateman, anyway). In the evening, I took beginning German, to prepare for my doctor's examination.

I had been given a graduate assistantship at a salary of \$700 per year, plus free tuition, which required that I had to supervise a lab and grade papers in Geology 1. Robert had a fellowship that paid about the same amount, but with no obligation to do any work but his own.

The graduate students began to troop in. Robert and I had desks at the Museum, as well as Stuart A. Northrop, G. Arthur Cooper, Geoffry Crickmay, Leeds (another man from British Columbia), Lamborn from Ohio, and others. At Kirtland Hall there were Freleigh Fitz Osborne, Aaron C. Waters, Don Sulchow, Bob LeClerk, Sam Lasky, Wickwire, and others. Harry Hess and Dave Gallagher were undergraduate majors. Many of these I was to see in later life, and I formed my firmest college friendships during my two short years at Yale. We were particularly close to Northrop and Cooper, with very contrasting personalities.

We were a thing apart from the Yale undergraduates, who were a breed of college men quite different from anything I had seen before -- in the mass a body of young aristocrats unlike any students in the Middlewest or Southwest in clothing and manners.

In the fall, we went to all the Yale football games in the Yale Bowl when the team played in New Haven. That year, the two big games, with Harvard and Princeton, were played on the other two campuses, and we went by train to see them in Cambridge and Princeton. This gave us an opportunity in the mornings to walk around the other two campuses. At about the same time, we also visited the campus of Columbia University in New York, and looked up Marshall Kay, who was a graduate student there. To our surprise, we found Marshall smoking a cigarette, which would have given his father, Dean Kay, a fit of apoplexy!

We got up late, and worked late in the evenings at the Museum, walking home after dark. Compared with our adventurous summers in Texas, our social life was at a minimum, and our life was spartan, to compress all we could in our short time there. I had a few dates, mostly with the Museum girls, but the romances ended inconclusively, leaving only a soon-forgotten pang. Robert was more gregarious. Somehow, he had made contacts with the blue-collar Irish in West Haven, and had many dates with a cute girl who was the daughter of a policeman. Her mother spoke brooklynesse, and called "soil" "serl."

During the winter I decided we should work up a preliminary statement of our results on the Glass Mountains, before final publication, to give our results promptly to the local geologists. With Robert's help, I wrote a manuscript and drafted a map which were sent to Dr. Sellards, who promised to publish it promptly in the Bulletin of the Bureau of Economic Geology.

After our return from our Christmas holiday in Iowa City in December, we attended the meeting of the Geological Society of America which was held at Western Reserve University in Cleveland. After that, we went down to Washington for a day, where we had never been before, and looked around. However, it was the New Year's holiday and all the offices were closed. We wandered around in the cold, and among other things climbed the Washington Monument.

In the late spring, a group of us came up for oral examinations for the doctor's degree. As the time grew near, tension increased. Like the rest, I reviewed all the geologic textbooks and reports. The evening before the examination, my friends took me to Savin Rock, the New Haven amusement park, and made me ride two or three times on the roller-coaster, which turned out to be very good therapy. My examination, and that of Aaron Waters, was scheduled for the following evening. My time was 7:00 P.M., and that of Aaron's (poor fellow!) was 9:30 P.M. It was quite an inquisition; the applicant was alone in the room with the whole geology faculty sitting around a long table, who asked questions in turn. Professor Ford helped the applicant to feel at ease by clowning the show, and reading a newspaper part of the time. To my surprise, I was very much at ease, and had ready answers to most of the questions asked. At the close of the session there was a short discussion among the faculty, and one of them came out to tell me that I had passed, and "with honors". Aaron Waters, the next victim, was waiting in an adjoining room, nervous and white as a sheet. He then went in, and passed too, but without "honors"; he was a victim of the bad hours, because his ability was as great or greater than mine, and he ordinarily spoke with greater assurance than the rest of us.

During the coming field season (summer of 1928) it was our plan to make a general reconnaissance of the rest of the Permian of trans-Pecos Texas, in order to put our work in the Glass Mountains into broader perspective. Robert went out several weeks early; I followed by train when my courses were over at Yale.

Robert took the Model T Ford out of storage in El Paso, and started work in the Hueco Mountains, the next low range east of El Paso, where Richardson in the El Paso Folio had mapped the Hueco Limestone, supposedly a thick mass of indivisible Pennsylvanian carbonates; a later reconnaissance by J. W. Beede for the Bureau of Economic Geology had shown that the Hueco actually consisted of units of late Mississippian, Pennsylvanian, and early Permian ages.

Robert found a place to stay in a rooming house and a place to eat in a boarding house, in a residential area not far from downtown El Paso. To get to the Hueco Mountains involved a 20-mile drive each way, through El Paso, then on a winding track across the sandhills of the Hueco Bolson, to Hueco Tanks on the west side of the mountains. (The highway across the Bolson was only put in a few years later).

In the rooming house lived Alice and Virginia Hamilton, two beautiful and saucy, but rather aloof girls, as well as several other girls and boys who had office jobs in El Paso. Robert spent many evenings in Juarez, the Mexican town across the Rio Grande from El Paso, which was reached by a streetcar from El Paso. It was a gay place at the time, with many bars, restaurants, and curio shops catering to Americans, as well as less reputable establishments. There was no Prohibition in Mexico and liquor flowed freely; one could legally buy a drink there. Due to some bureaucratic foolishness, the bridges were closed at 10:00 P.M., so all the whoopee had to be done early in the evening. There was an out, however, that summer. A Mexican entrepreneur had opened a bar, called the "Hole-in-the-Wall" on Cordova Island, a cutoff meander of Mexican territory on the American side of the river, which was open all night. I fell in with Robert's life-pattern when I arrived there.

The Hueco Mountains are a desolate group of hills, rising no more than a thousand feet above the Hueco Bolson to the west, partly drifted over by sands blown eastward from the Bolson. They were very unattractive scenically, despite their stratigraphic interest.

In the Hueco Mountains we followed the outcrops on the west side of the range south from the Texas-New Mexico border to their southern end, measuring many sections of the Mississippian, Pennsylvanian, and Permian strata. We found a prominent angular unconformity at the base of the Permian (to which the name Hueco Limestone was later restricted), and to our surprise found that the Permian beveled all the older formations southward, and lay on the Upper Ordovician at the south end. The Permian contained fusulinids and other fossils like those in the Wolfcamp Formation of the Glass Mountains, so we correlated the unconformity at the base of the Permian with the one we had worked out in the Glass Mountains.

During our stay in El Paso we heard from Mr. and Mrs. Darton, who were stopping off in a hotel in El Paso. Darton proposed to us that we go with him to look at the upper formations on the west side of the Franklin Mountains north of El Paso, so we went one day, driving in from a road from the Rio Grande Valley west of the range.

Darton walked very slowly, and we were running around like two terrier dogs, looking at outcrops on the way. He seemed old and feeble, and we stopped occasionally to offer help. But then we realized that this was his method of saving strength. It was a very hot day, and by noon, when we reached our objective, we were pooped and had drunk all our canteen water. Darton gave us cans of fruit juice that he had brought along for lunch, and in the evening when we came down he bought us each an enormous milk shake at one of the little towns in the Rio Grande Valley. Our little trip was quite productive, for we found that the Hueco Limestone in the Franklin Mountains had the same Mississippian, Pennsylvanian, and Permian ages as in the Hueco Mountains, as well as a shale at the base, lying on the Silurian Fusselman Dolomite, that was of Devonian age.

Our work in the Hueco Mountains was about over, and it was time to move on, but first there was a West Texas Geological Society excursion that was to assemble in Van Horn, so we went down by train to join it. The party assembled at Toolen's Camp, a rather primitive motel. To our delight, Leo Horton showed up with his Buick, and we rode with him. I don't remember much about the trip. We must have looked at places around Van Horn, then Charles Laurence Baker showed us the Jurassic in Malone Mountain, and we went into the Finlay Mountains. We went across the Diablo Plateau, a long line of vehicles eating each other's dust, ending in El Paso. The last day of the excursion we were supposed to go to the mountains on the Mexican side that loomed up back of Juarez, but we were by then so full of beer and conviviality that the day was inconclusive geologically. In the evening we took Adkins to the "Hole-in-the-Wall", and he was much impressed.

In early July we left El Paso and moved down to Van Horn, to look at the Permian rocks of the Sierra Diablo. We inquired for weekly rates at Toolen's Camp, and while they were probably moderate, they were too steep for our budget; besides, the camp was uninviting. We decided to try elsewhere, and inquired of the Post-mistress. She told us that Mr. and Mrs. Duncan rented rooms. They proved to be a raw-boned, kind-hearted couple, who had a rather dreary-looking house in town, so we stayed there while we were in Van Horn. (And I would continue to do so for nearly ten years thereafter).

The Sierra Diablo was much more interesting scenically than the Hueco Mountains -- a Basin-and-Range uplifted block, faulted off on the east toward the Salt Basin in escarpments 3,000 feet high. As in the Hueco Mountains, all the rocks in the Sierra Diablo were mapped by Richardson in the Van Horn Folio as Hueco Limestone of Pennsylvanian age. Our brief climb on one of the foothills the year before had shown, on the contrary, that much of the rocks were Permian. (Late in our stay we did find one little outcrop of Pennsylvanian limestone at the base of the scarp at Marble Canyon, unconformable below the Permian, as in the Hueco Mountains). We climbed the east-facing escarpment in many places, measuring sections. The escarpment had a three-storied profile -- limestone ledges below, a middle slope formed of black shaly limestone, and an upper set of cliffs that we named the Victorio Peak Limestone. Wolfcamp fossils occurred in shaly beds at the base, and just above the lower ledges were silicified fossils like those in the Permian of the Glass Mountains. North of Marble Canyon, however, this stratigraphy went to pieces, and the whole turned into a great mass of featureless limestone and dolomite, which we interpreted as a great reef.

Many other interesting-looking mountain groups were visible from Van Horn -- spiny Precambrian ridges in the Carrizo Mountains to the west, bold cliffs of Lower Ordovician in Beach Mountain to the north, and various mountain masses of Permian limestone. We explored some of these -- enough to give a hint of the variety of interesting geology in the region (that I was to work out in later years).

Our Model T had begun to show the wear of several years of hard field service, and we had much car trouble. Flat tires were almost routine (Robert would patch inner tubes at lunch time, for relaxation). We regularly broke springs (the most vulnerable part of a Model T); one time on a long camping trip to Sierra Prieta and around the north end of the Sierra Diablo, we broke both the front and back springs, bringing the car down flat on the frame, and had to limp into town. On another evening coming home, the rear axle sheared off and a wheel rolled off into the desert. We got a man to tow us into Allamore, then had to walk the 10 miles into Van Horn at night because no one would give us a lift. On still another day, the brackets that held

the motor broke; the engine would still run, but the hose to the radiator was broken, so that we lost all our water.

We ate at the Owl Cafe in Van Horn -- "Where tourist meet and greet" said its sign. The food was very bad, and toward the end of our stay I developed stomach trouble and was quite ill.

After a month in Van Horn, we got word that Longwell was coming down to inspect our dissertation work, and we drove into El Paso to meet him. We then went to Marathon and the Glass Mountains. Ray Baker (an old Yale man) and some other Texaco geologists came out to go around with us. Unlike Dunbar, Longwell was very sympathetic, and we were pleased when we heard him explaining and defending our interpretations to the Texaco men. As with Dunbar, Longwell wanted to see Carlsbad Caverns, so we went there from Marathon. It was a rainy spell, and afterwards we went back to Van Horn in the mud and rain, with the storm clouds low on the mountainsides. We got there late at night and were glad to roll in with the Duñcan's.

After this, we went back to El Paso, where Longwell looked up a couple of chaps who had discovered a remarkably preserved ground sloth that had been purchased by Peabody Museum the year before. Longwell wanted to see the site, which was in a lava cave in Afton Crater, in the plains of southern New Mexico northwest of El Paso. Longwell, Robert, and I went out with the men to the crater. The others descended by a rope into the cave. I was still too ill to want to try it; they thought I was "chicken". For our last evening, Leo Horton had given us admission cards to the El Paso Club, on the top floor of one of the downtown office buildings, where the three of us had drinks and dinner.

Robert and I decided to spend a few more weeks in El Paso. He wanted to look at the Permian outcrops in the outlying areas in the Diablo Plateau, such as the Cornudas Mountains, which he would do on long camping trips. I wanted to stay in and recover from my illness, and also study German for the doctorate. I had tried the German test in the spring, given by Professor Knopf, but he told me to study some more and try again. This time, we got a room at the Outlaw's only a few blocks from where we had been before. Dr. Outlaw was city health officer, and I consulted him about my troubles; he perscribed mainly laxatives and a bland diet, and I was gradually on the mend. Their daughter, Alice Outlaw, was at home; she was a beautiful girl who had been on the stage in a revue in New York.

For the final part of the summer we went back to Alpine and the Garnett Hotel. Again, I stayed in and studied German, while Robert explored the Permian of the Shafter district to the southwest.

After that, Robert went east by train. I, for some reason I can't now recall, drove the Model T to Austin, where I sold it to a used-car man for \$100.00. Considering its dilapidated state, I felt I had done pretty well. I, too, then went east to New Haven.

During my second year at Yale there were many old faces among the graduate students, and many new ones. Stuart Northrop had finished the spring before and had accepted a job at the University of New Mexico in Albuquerque (where he remained until his retirement a few years ago). The newcomers included Dave Andrews and Louis Bumgardner from the University of Missouri, Charlie Hunt from Colgate University (a friend of Cooper's), M. N. Bramlette, Frank Parker from U.C.L.A., and J. Brookes Knight from St. Louis. Knight was an older man from a wealthy St. Louis family, a Princeton graduate who had retired after a successful business career, and had taken up paleontology for something to do. He had a large family and they rented a big house at Killam's Point, on the shore east of New Haven. (Brookes and I were to collaborate through later years, so more of him later).

This time, Robert and I rented rooms with the Wilhelm family, who lived in a residential area quite a ways northwest of the campus. Frank Parker decided to get a room at the same place. He was a rich boy from Los Angeles, who upon arrival in New Haven had bought a Wills St. Clair, a very posh vehicle of the time. He was fat and sleepy-looking, but it turned out that he had a very good brain.

A few weeks after our arrival, Robert unaccountably became ill and had a high fever, so was taken to the University Hospital, where his case was diagnosed as pneumonia. He was very sick and his life was despaired of; the miracle drugs would have knocked it out quickly, but they were some years in the future. Mother was sent for, but by the time she had arrived the crisis was over, and he was on the mend.

The Presidential elections were that fall, Herbert Hoover against Al Smith. Hoover ran on a rock-ribbed Republican platform; Al Smith was a maverick who was for repeal of Prohibition, and a Catholic. Feelings ran high. Mrs. Wilhelm said that the Pope had granted absolution to all the priests and nuns so that they could vote for their man. We sent our absentee ballots to Iowa City. On election night

word came out that Al Smith had carried New Haven, so the locals staged a victory parade downtown. Next day, when all the returns were in, it turned out that Smith was overwhelmingly defeated, even in Connecticut.

I tried a few courses that fall, but they were too much, so I dropped them and concentrated on writing my dissertation on "The geology of the Glass Mountains." My effort paid off, and I still marvel today at how well presented it was. I did, however, audit Lull's lectures on Organic Evolution.

After our visit home at Christmas, Robert and I attended the meeting of the Geological Society of America in New York. I gave a short paper on the Dugout Creek overthrust, which we had discovered two summers before.

On the Friday evening of the convention, Robert and I attended the Society banquet at the Hotel Astor. Before the occasion, we walked about the corridors looking for someone to eat with, and spied Josephine Wells, who had been a graduate student in geology at Yale, and a college girl friend. They were happy to eat with us, and with this as a nucleus we snagged others who came by and made up a full table. After the banquet, we took the two girls to a late movie at the Paramount Theater on Times Square.

At the banquet we all signed each others programs, and I have mine still. The names are: Philip B. King, Everett J. Lees, Charlotte E. Webster, S. Raeburn Kirk, Frances W. Tatge, G. Arthur Cooper, Catherine R. Chase, J. Brookes Knight, Josephine Phelps Wells, and Robert E. King.

Lees was a graduate student at Yale from British Columbia. Kirk was also a Canadian who had been at Yale before us, and was then at the University of Manitoba. Cooper and Knight were our friends at Yale. I am blank about two of the girls, Charlotte Webster and Frances Tatge, but feel I ought to know who they are. Jo Wells's friend was Catherine Chase, and they had both been to one of the eastern women's colleges.

The occasion was historic, sort of. Jo Wells and Gus Cooper had both been around Yale for a year or two without noticing each other, but on that evening Love Bloomed, and they were married next year. Her girl friend was an unknown, but a few years ago when I told about the incident to a group in Menlo Park and mentioned "Jo Wells and another

girl" Catherine Campbell (Ian Campbell's wife) to my surprise said, "That was me!" I had met Catherine when the Campbells had had us to dinner in Pasadena in 1955, but she had never revealed that we had met before.

There was a flu epidemic that winter, and shortly after our return to New Haven Robert and I both came down sick, and were put in the University Infirmary, where I spent a week or so flat on my back, forbidden to work or do any reading.

In the spring, many of us took the Geological Survey Civil Service examination for Junior Geologist at \$1,800.00 per year, which was held in the Post Office Building downtown. The examinations required long written answers, quite different from the short multiple-choice questions of later years, and must have required much work to appraise and grade. All the younger men on the Survey at the time were ex-Yale men, and the questions were on subjects that were being taught at Yale, so were quite familiar to us. Most of our group therefore passed successfully, including Frank Parker, who took it more or less on a dare.

I also applied for Assistant Geologist at the higher salary of \$2,600.00 per year, by means of an "unassembled examination", which required merely a statement of record, recommendations, and the applicant's publications, and I qualified for this also.

I finished my dissertation in May, and hired Sally Donahue to type it for me, then submitted it to the faculty. The last formality was to "defend the dissertation" which consisted of giving a talk on the results before the geology faculty and anyone else interested.

The same evening Gus Cooper presented his dissertation. He had been working along on the Middle Devonian of New York State, without

anyone paying much attention, or giving him any encouragement, Every - one thought that the Devonian of New York had all been worked out before, and that there was nothing new there. But Cooper's results were revolutionary! He showed that the units previously mapped were not time-stratigraphic but were facies, so that continental beds in the Catskill Mountains to the east were equivalent to marine shales on Lake Erie.

Robert and I, and other candidates, were taken into full membership in Sigma Xi (I had been elected to Associate Membership at Iowa), at a banquet given for the candidates.

About the time I was finishing up my dissertation, I got a message from Mr. Darton, asking me to work for several months for the Geological Survey, making a reconnaissance of the pre-Permian rocks of the Marathon Basin to be used on the Geologic Map of Texas. This involved getting my Ph.D. in absentia, as commencement at Yale was not until mid-June, but I arranged for this and went down to Washington, and to the Survey offices in the Interior Building on F Street (now the G.S.A. Building).

Mr. Darton introduced me around, and I was taken on the rolls as a geological assistant (I received a permanent appointment as Assistant Geologist when I was in the field at Marathon, and was sworn in by the local Post Master). I met George Rogers Mansfield, my Branch Chief, and was taken to the office of Mr. Mendenhall, the Chief Geologist, where I talked grandly about our West Texas work on the Permian. I met George H. Girty, the Survey paleontologist on the later Paleozoic, and arranged to see him in the field while I was at Marathon. I was taken to the lady clerks, who showed me how to make out Government expense vouchers. Darton took me to the Cosmos Club for lunch at noon. Washington seemed like a welcome change from New Haven and New York, where we had become accustomed to Italian, Jewish, and other foreign faces. All the people in Washington seemed to be white Anglo-Saxons like me -- and colored people who were the messengers and waiters.

In the evening I walked down to the Union Station and took the Southern train for New Orleans, then the Southern Pacific to Austin, where I visited briefly and took out of storage a Survey vehicle to use in the field work -- a Model A pickup, that I was destined to use off and on for the next six years.

So I was back again at Marathon, and again at the Gage Hotel, with the Pacetti's still as managers, but I was alone and under different auspices. Business was dull at the hotel and not many people were stopping there. A geologist with the Amerada was there for a while, and was good company in the evenings. I set to on the field work with a will, and walked many miles each day, with only an apple in my pocket for lunch.

An important problem to be solved was the sequence in the lower Paleozoic, below the Caballos Novaculite and Maravillas Chert. Charles Laurence Baker, in his early reconnaissance, had called much of this the "Marathon Series", and had listed scattered observations on rocks and fossils at different places, but giving no clear idea of any kind of sequence. He seemed to think that the rocks were hopelessly confused, and this idea was carried on by his successors. J. W. Beede liked to proclaim, "When you stand on a hill in the Marathon Basin it looks like everything has been stirred up with a spoon," and this fatuous remark was treated with respect by his admirers.

But Robert and I had obtained a glimmering of a solution. In 1927, on one of our short trips south of Marathon, we had looked at outcrops about 6 miles southwest of Marathon where the rocks below the Maravillas had an evident sequence, and Robert had collected graptolites from several levels. This sequence proved to be the key for untangling the lower Paleozoic.

Here was a sequence of well-marked strata of contrasting lithologies, comprising a set of formations hitherto unknown. Next below the Maravillas was a shale, below that a ridge-making cherty limestone, then thin-bedded flaggy limestone, and finally sandstone at the base. Most of Baker's fossils had come from equivalents of the upper shale. The cherty limestone was something new and unknown; Baker had seen it in one or two places, and had assumed that it was Dimple Limestone. The flaggy limestone contained Lower Ordovician graptolites, and Baker had collected such graptolites at only one place, from what turned out later to be in a fault slice; the wide extent of the Lower Ordovician had been completely unknown.

Using this sequence as a key, I set about mapping the lower Paleozoic. I found the same formations all over two great anticlinoria in a series of anticlines and synclines. The difficulty with the lower Paleozoic that had confused others turned out to be that

no one had realized that the structures were on a much smaller scale than usual; anticlines and synclines were orderly but were very close together. During a later year I gave formation names to these units (Woods Hollow, Fort Pena, Alsate, Marathon, and Dagger Flat), and they have become standard for the lower Paleozoic of the Marathon Basin.

The Maravillas Chert and Caballos Novaculite seemed to offer fewer problems in mapping. They stood in prominent ridges all over the central part of the Marathon Basin, and I liked to say that they practically worked out their structure by themselves. Nevertheless, there were some problems. The prominent Dagger Flat anticlinorium southeast of Marathon was surrounded by a wall of the chert and novaculite. At its northeastern plunging end the chert and novaculite went around from the northwestern to the southeastern side in a series of loops, seemingly orderly at first. But when I climbed the novaculite ridge at the end I was disconcerted to find another novaculite ridge out in the plain beyond it, and when I climbed this one I saw still another novaculite ridge beyond it. It became apparent that over the anticlinorium the chert and novaculite had been repeated, shingle-fashioned, in a series of thrust slices what had subsequently been folded into the anticlinorium. The same curious structure was found in the region farther southwest, and at the southwestern end of the anticlinorium.

The younger formations of the Marathon Basin, the Tesnus, Dimple, and Haymond, offered fewer problems of sequence, and I mapped them southeastward up to the bounding escarpments of Cretaceous. I also went over the outcrops along the west side of the basin, along the foot of the Del Norte and Santiago Mountains.

About the middle of my field work, Dr. Girty arrived with Professor I. A. Keyte, who had been in the area with the Marland Oil Company, and the three of us went over the controversial outcrops along the base of the Glass Mountains escarpment that had been variously called upper Pennsylvanian and basal Permian. After Keyte had left, Girty and I spent a few days looking for fossils in the older Pennsylvanian, especially in the Dimple Limestone east of Marathon.

By the end of June, after 5 weeks in the field, I had been over most of the Marathon Basin in reconnaissance, but there was still a little more to do to finish up, so my employment was extended another

week, into July. Darton had come through late in June and looked over my work. He urged me to go to the extreme southeastern corner of the basin, in the Dove Mountain quadrangle; I had not hitherto attempted to do so, because it was so remote and inaccessible. So with some trepidation I undertook the trip on the Fourth of July. The area was reached by a rough winding road south from the Southern Pacific Railroad, over the Cretaceous mesas. The area was a lowland extension of the Marathon Basin, nearly surrounded by high Cretaceous escarpments. The Jones Ranch house in the center I found deserted; its people had probably all gone into Sanderson for the day by another road, for the Fourth of July celebration. In fact, I was very much alone, and saw no one all day. I walked around and saw all the outcrops, then drove back the way I came, arriving in Marathon in the late afternoon.

My future had given me much concern, and there was no certain indication of what I would do after I finished up at Yale. Mendenhall had told me that with the end of my field job my work for the Survey would end for the time being, as all the Survey slots were filled up. He urged me to find another job for a year, and try again the following June (1930). The Yale faculty had received letters about an opening at the California Institute of Technology, but apparently I was <sup>not</sup> considered seriously for it (the place was taken next year by Ian Campbell). During the spring I had gotten out the catalogues of all the western schools and had sent letters to them, but with no response.

Then, in June when I was in the field I got a reply from Professor Guild of the University of Arizona in Tucson. Yes, they had an opening for an instructor to teach beginning geology, but for one year only; they were expecting to appoint an eminent professor from the east the following year. Would I please send recommendations and name my salary? This sounded ideal, as it was in an interesting area and would take care of my blank year; besides, Professor Schuchert had been a visiting professor at Arizona and few years before, and Darton was well known there for his work on the Arizona map, so my recommendations were taken care of. I said I would come at my Assistant Geologist's pay of \$2,600 per year. Everything went smoothly, and my appointment was confirmed before I left Marathon.

Early in July, I took the Survey pickup back to Austin for storage, then returned for the rest of the summer in New Haven.

I spent most of the summer drafting the geologic map of the Glass Mountains for publication, and Robert was there finishing up his work on the West Texas Permian brachiopods. Stuart Northrop was also at the Museum, after his first year in New Mexico, completing for publication his dissertation work on the Port Daniel area, Quebec. Cooper was away on a long collecting tour of the West. He had never been west of Lake Erie before, and he sent back a series of postcards to the girls at the Museum, each with the silly and uninformative message, "Best regards, Coop." Brookes Knight was also around, but otherwise the Museum was very quiet.

Stuart had had a great year in New Mexico, and was enamored with the region. He had fallen in love with a young French instructress at the University (but he had been through a whole series of love affairs before which did not last, and this one didn't either). He had hitherto been scornful of the west and had had an unhappy summer in Nebraska, but Albuquerque and northern New Mexico were glamorous, different, and more cosmopolitan.

Robert had decided that he did not want to be a paleontologist all his life, and wished to cast his lot with oil geology. We went down to New York a few times and were interviewed by hard-headed, unsympathetic executives, but he finally obtained a position with Texaco (Ray Baker's company) for foreign work in Colombia.

In early September the family came east to see him off, and we all went to the pier in Brooklyn from which the Grace Line ship sailed. Presently the ship was a little speck in the distance, and our five years of collaboration were at an end. I was to continue my career alone, and it was an empty feeling.

It was time for me to leave New Haven also, and I planned to drive west as far as Albuquerque with Stuart Northrop. He had an Essex Four, a better model than the one I had bought in Midland, which he had driven east for the summer from Albuquerque. His New Mexico license plates had created a sensation in New Haven; the Connecticut natives had no idea there was a state by that name, and thought it was a foreign country.

I went by bus to Stuart's home town in Danbury in northwestern Connecticut, and we started off from there. We made our way across the country, going as far as we could each night, but made slow progress. We crossed the Appalachians in Pennsylvania over the old Lincoln High-

way (U.S. 30), and somehow across the Middle West. In Kansas, we stopped at the village of Belpre to call on his enamorata, who was spending the summer with her mother. She was a beautiful, soulful girl, but it was evident that the romance was over for her. We spent the last night at the La Fonda Hotel in Santa Fe, and drove down to Albuquerque next morning with the sun bright on the mountains.

I had a day's sight-seeing around Albuquerque with Stuart, during which he took me to the overlook at the top of the Sandia Mountains, then I took the Santa Fe train south. In those days there were Pullman connections at Deming with the westbound Southern Pacific train, which I took to Tucson. We went west over desert country like that with which I had become familiar, but before long the country became more picturesque. I saw my first tall cactus, the saguaro, and thought it was notable, but soon there were whole forests of them! Coming into Tucson in the evening there was outlined against the sunset a fantastic array of sharp peaks of the Tucson Mountains.

Next morning I went to the University and met Professor Guild, and also George Montague Butler, Dean of the School of Mines of which the Geology Department was a part, as well as others of the faculty. Butler put me up for a few days at the Old Pueblo Club, until I could find a place to live. The faculty men drove me around to look at different rooms. Rents seemed quite high; landlords expected an influx of winter visitors and charged accordingly. I finally took a room with two bachelor girls in the physical education department, who had a comfortable, thick-walled adobe house opposite the far end of the campus, with an extra room at the back which they rented out.

I set about buying a car. I had my heart set on a Model A Ford, which had just come out the year before, but I had to shake off the Chevrolet people, who gave me a hard sell. I bought a "sport coupe" (cloth top with a rear curtain that came down, and a rumble seat), in "rose beige" color. It was a sturdy little vehicle, very sprightly for its time, which I held onto until 1938.

All the geology courses were in the College of Mines except mine. Geology 1 was a course in the College of Liberal Arts. The Mines faculty didn't care much about the course, and it was out of the control of the College of Liberal Arts, so I could run it as I pleased. This was a welcome change from my university work hitherto, at Texas and Yale, where I was always under some sort of supervision, by someone higher up. I designed my own lectures, the laboratory work, and the

field trips, and put a lot of enthusiasm into it. Enrollment in the class amounted to nearly 100, and it met in a lecture hall in the Liberal Arts Building.

The University of Arizona was a small school in those days, with an enrollment of only about a thousand, so everyone knew each other. It was set in a big square campus with the buildings in the center, surrounded by olive groves and cactus gardens. Registration was cosmopolitan. Besides the local Arizonans many students had come from the East, and there were also many refugees from California. They had come away from the enormous schools there (Berkeley and U.S.C. at the time), where nobody knew anyone else, and they could not make headway socially. (Now all is changed; the University of Arizona is big too, and there are so many new buildings that little of its aspect in 1929 is visible today). The University had a good deal of money and was aggressively hiring staff. B.S. Butler had been hired from the Geological Survey to teach economic geology, and I learned that they were negotiating with Kirk Bryan at Harvard to fill my temporary job. (Bryan eventually decided not to come. Within a few years the depression was on and the State and the University were nearly out of money, but that was after my time).

Of the geology faculty, besides Guild and Butler, I remember A. J. Leonard who taught general geology, and A. A. Stoyanow who taught stratigraphy and paleontology. Stoy had had a career in geology in Imperial Russia before he fled the country during the Revolution. The two graduate assistants in the department were Edwin C. Eckel and William Thomas, who were working for master's degrees. Thomas, a rather sad fellow, was my lab assistant. I became quite fond of Eckel, who had aspirations of joining the Survey, and of his girl-friend Le Charles ("Chuck") Goodwin, who was majoring in archeology. Sometimes in the evening they sat in his ancient vehicle in the University parking lot, and when I was lonesome I would join them.

I ate all my meals at the University dining hall, where all the students ate, and where there was a special table for faculty. It was a congenial group. Sometimes I got up too late for breakfast at the dining hall (it closed at 9:00 A.M.), and had to get some kind of breakfast at a drug store across from the campus.

Early in the fall we heard about the great stock-market crash on Wall Street in New York City, but it was a far-off event, whose far-reaching effects were not apparent at first, and particularly in distant Tucson.

Early in the fall B.S. Butler gave a trip for his economic geology class to the great Copper Queen Mine at Bisbee, owned by Phelps-Dodge, and I went along. We had an interesting day underground in the mine. In the evening, everyone went to the border town of Naco, 8 miles to the south, and caroused in the bars. My year had been very abstemious, and the opportunity to drink was most welcome. I rather overdid it, and foolishly mixed drinks as well, so I got back to the hotel in Bisbee quite plastered. Next morning I couldn't eat breakfast, or climb Mount Martin, where the party was going to look at the Paleozoic section. I gradually revived coming home to Tucson in the afternoon.

I thought I was disgraced at the University, but the event was ignored. Instead, a day or so later one of the sorority girls came by and asked me to chaperon their up-coming dance, so my reputation as a good spirit was made. Thereafter, through the winter, I was invited to chaperon many sorority and fraternity parties. Ordinarily, my dance program was made up for me, with all the attractive girls there. At last I was able to enjoy the college social life of which I had been deprived through my own university years.

Along in the fall, also, the archeology students invited me on a trip to the Empire Mountains southeast of Tucson, where elephant bones had been discovered in a deposit of terrestrial gravel. We went to an old mine, appropriately called the Total Wreck Mine, then walked several miles down an arroyo to the site. In the evening I rode back to town with the young instructor in archeology, Clara Lee Fraps. She had a Model A convertible; the top was down and the desert moon was full. It seemed as though we were kindred spirits, and I suddenly decided I was in love. Put it down partly to my stringent years at Yale, when I was unable to have any interesting girl-friends.

Anyway, I saw much of Clara Lee in coming months, and my love added color to me stay in Tucson, and also put something of a blight on it. She came from a Tucson family and had four brothers. She also had a circle of interesting friends. She turned out to be a girl who

lived for her emotions; her affection for me cooled after awhile, and she was off chasing new romantic bubbles. So, for me, it all ended rather sadly.

One of her friends, who was also on the field trip, was Winnie Belle Cochran, a red-head who, it turned out, grew up on a ranch in the Marathon country, where her father was one of the oldtime cattlemen. We became friends also. Another friend in the group was Dorothy Gay, who arrived from Berkeley at the beginning of the second semester, and who took courses in both archeology and geology.

A French teacher, a Swiss, was interested in climbing the mountains around Tucson, and I went on some of his trips. A memorable trip was a climb up Mount Lemmon, on the crest of the Santa Catalina Mountains, by trail from Sabino Canyon on the south side -- a 5,000 foot climb. We had arranged to spend the night in a cabin in the pines at the top, and came down next day by the same trail. It turned out that the Swiss and his companions were interested mainly in hiking to the objective, whereas I liked to stop occasionally, look at the rocks and view the scenery. Clara Lee met us at the bottom and drove me home, tired and weary.

Sometime in the fall, B. S. Butler came to me. He had been commissioned by the Geological Survey to examine some dam sites for the Bureau of Reclamation, and he asked me to do one of them as a temporary Survey employee. The site was on the Gila River above Safford, and a local rancher took me in on horseback, with supplies for two night's stay alone. I slept on a sandbar beside the river, and fixed my own meals. The site was at a narrows in the river in massive andesite. I climbed around, measuring joints and other structures. I found that the canyon had a two-storied profile -- an open valley above where the rocks were much weathered, and a narrow gorge in fresh rock below -- all of which had an effect on the engineering possibilities of the site. At the end of the two days, the horses returned and I came out. I don't know whether a dam was ever built there, but the trip was a good adventure.

At Christmas time, the family decided to come out to visit mother's half-sister Elsie Harter and her family in Los Angeles, and I went with them. There had been blizzards in the Middle West and the trains were very late, but finally they arrived. Mother, father, and Edward stayed with me for a few days in Tucson. The two girls had

gone away for Christmas, so we had the house to ourselves. They had a chance to see something of the country, and we called one afternoon on a lady from Iowa City who had a job with the University, who lived in a house in the foothills of the Santa Catalina Mountains. Edward was fascinated by the saguaros that grew outside the house, and while we were talking went outside to feel them.

We then went on by train to Los Angeles -- my first trip to California, and all new and strange. In the morning we were in the desert of the Salton depression, with incredibly barren mountain ridges off to the northeast. Then the train made the long, slow climb to San Geronio Pass, with the great San Jacinto and San Bernardino Mountains on each side, with patches of snow on their tops. Then the train descended rapidly into the orange groves and towns of the Los Angeles Basin. There was no smog in those days, and the air was crystal-clear and sunshiny.

Earl Harter and Mr. Ramsey, a partner, had a big hardware store on Hollywood Boulevard, and the Harter family had a house on a side street near its eastern end, not far from the Egyptian Theater. They had two sons, Leland and Richard, who had grown up in the Hollywood atmosphere and were a wild pair. I went out with them in the evenings racing around by car in the streets. Sometimes they were chased by the police, and drove this way and that up side streets without lights to shake them off. There were other relatives about, including a nice girl cousin of whom I became fond, who lived in Burbank; the boys said Burbank was a hick town out in the country.

Hollywood Boulevard in those days was a real glamor street, lined with sophisticated shops, and with many of the movie "greats" strolling about. (It has now gone into a sad decline). Los Angeles as a whole was truly a never-never land, quite different from anything farther east -- all fake Spanish architecture, palm trees, and the snow-capped mountains rising above in the clear air. (When I came back years later it was just another unglamorous big city, draped in smog). At Christmas we sat around the Harter house and drank egg-nog, and Earl gave father a hard sell about the greatness of Los Angeles and its bright future. The stock-market crash? That was just something that happened far away in the east; it wouldn't affect the prosperity of Southern California. (How wrong he was!)

*Later in*

~~Sometime during~~ the winter I drove over to Albuquerque at Stuart Northrop's invitation. I remember the lonely mesas followed by the highway on the west side of the Rio Grande, the deep barrancas which interrupted them into which the road must descend, and the dusting of snow on the mountains. Stuart had a contract with the Fred Harvey people to assist in training their girl couriers who would conduct tour groups to the sights of northern New Mexico, and one of their training trips was coming up, to which I was invited. It was a good opportunity to see the most romantic part of the state. We went mainly to Indian pueblos, where we had the guidance of an anthropologist from the University -- Taos, Zia, etc. -- and into the Jemez Mountains to Jemez Hot Springs on the edge of the great caldera. We spent a night at Laguna, on the railroad west of Albuquerque, and in the evening the group gathered in the hotel lobby and played blackjack. After awhile I began to come out ahead, so gambled recklessly to reduce my gains. The more reckless I became the more I won -- the only big winning streak at gambling in my life. At the end of the evening my pocket was full of \$25.00 or so of silver. Next day was the high point of the trip, a visit to Acoma on its lonely mesa south of the railroad, where I used some of my ill-gotten gains to buy Indian pottery. Back in Albuquerque we visited Stuart's fiance Ivah and her mother. Ivah proved to be a sprightly and vivacious girl; Stuart had found his true love at last, and their marriage has endured until today.

The second semester William Morris Davis arrived as Visiting Professor at Arizona (he had been doing this for several years from his base in Pasadena). His wife Lucy was with him, a most attractive lady. He remembered me from his Texas visit, and greeted me as an old friend. I saw a good deal of him that semester, and heard about his research enterprises, but I did not attend his lectures. He loved to assemble the faculty for picnics at some spot of geomorphic interest, where he played the great man and showman, scrambling about over the outcrops in an old straw hat, very active despite his more than 80 years.

In the spring semester, I ran a field course. There were 8 or 10 geology students, all men, a nice bunch and we got on well together. We were to survey a small area in the hills around Colossal Cave, southwest of the Rincon Mountains, where there was a succession of lower Paleozoic -- Cambrian to Mississippian sitting on Shaw Granite -- broken into a confusing array of small fault blocks. I became interested in the area, and spent many weekends there, extending the geology. I did not have time to finish the area, and the lack of good topographic maps precluded successful mapping, but I did determine that the Paleozoic and its granite basement were thrust eastward over the gneiss of the Rincon Mountains. (It has only been recently that the area has been adequately mapped geologically.)

The spring was drawing to a close, and it had been a most happy and eventful year (except for my emotional disaster with Clara Lee). It now developed that Kirk Bryan would not come to Arizona after all, and the faculty urged me to stay on at Arizona. But I was looking forward to a Survey appointment, and this was confirmed by a telegram from Mendenhall, so the die was cast for my Survey career ahead. The plan was that I was to survey in detail the Marathon Basin, which I had covered in reconnaissance the previous summer.

In early June of 1930 I said my fond farewells to Tucson and to all my good friends there, and set off in my car for west Texas and Marathon.

U.S. Geological Survey -- first period (1930-1940)

Arriving again in West Texas, the first thing to do was to attend a field conference of the West Texas Geological Society in the Marathon Basin. The group especially wanted me to demonstrate the Ordovician sequence which I had worked out the summer before. Charles Laurence Baker was along, and tried to take over the show. He clearly was not reconciled to the simple sequence that I had worked out, and talked grandly about structural complexities, such as great overthrusts. He had found veins of "comb calcite" which he thought were fault planes. In my opinion, most of his "overthrusts" were bedding slips between and within the formations.

After the field conference, I drove my Model A to Austin, to get the Survey pickup, and put my own car in storage. There had been some heavy rains, and the low-water bridge over the Devils River was under water, so I had to wait for hours for it to go down, and had to spend the night in Del Rio. At Sanderson, I had taken as a passenger a young ranchman, and on reaching Del Rio he invited me to go with him and his friends for an evening at Villa Acuna, across the river.

After seeing my friends at the Bureau of Economic Geology, I returned to Marathon in the Survey pickup and stayed again at the Gage Hotel. Unfortunately, this had gone into a decline, and was not making much money. The Pacetti's were ready to give up and left for parts unknown in a week or so. The lease was taken by the local Lochausen family. They tried to keep the dining room open for awhile, then closed it, so that the only place to eat in town was a rather indifferent restaurant down the street. The town now had an electric power plant (at the hotel this replaced a Delco system that had provided lights before); the plant was located a few blocks west of the hotel, and its diesel motors ran noisily all night and kept me awake.

With this unfavorable turn of events, I decided to change my mode of operation -- to camp out in the Basin and make the Garnett Hotel in Alpine my base, going in there for the night every third or fourth day. I purchased a portable cot to sleep on, and a Coleman stove for cooking, as well as other camp gear.

It became my custom to have hot coffee in the morning and a can of hot soup at night, making the rest of my meals on cold food. There was a problem about shelter at night. The night skies were generally clear, but on one of the first nights out I could see lightning and hear thunder off in the distance, and presently it rained on me. Early in the summer when Darton came through he loaned me a lightweight tent which I used for awhile, but it ripped apart in a wind storm and was no longer usable. One or two fortunate evenings I was near old ranch houses, and was able to set up my cot under shelter on the porch. Finally, I decided to sleep in the back of the pickup. This was equipped with bows and a canvas cover. I put the mattress in the back under the canvas, and by letting down the tail gate had room for my head. On clear nights, I could throw back the canvas from my face; when the weather threatened I could pull it back over my head and be very snug.

I had a kerosene lantern which I lit in the evening, and sat on the running board and read for awhile until bedtime. I had subscribed to the Book of the Month Club, and had one of their books along on every trip. One of these was an edition of Sherlock Holmes which I read clear through for the first time. Even the "Hound of the Baskervilles" was not too terrifying, even far out alone in the dark wilderness. Sometimes a pack of coyotes would close in and I could hear them yelp and see their eyes, just out of range of the light.

I made it a point not to make a production out of camp life; if I had done so I would use most of my time on camp chores. When the sun came up I would have a simple breakfast and strike off for field work, leaving the pickup and the camp gear where it was for the day; it was never bothered. Returning to Alpine every few days, I would get a bath and catch up on my food, ~~then~~ spend a day plotting up my maps and organizing my notes and fossil collections, then back to the field again. I followed this routine through the summer and fall, into November.

The last days of camping out were late in November, when I slept for the night at the foot of Horse Mountain, the highest novaculite peak in the Marathon Basin. Next morning I was up early and was at the summit when the sun was coming up, with the whole basin spread out like a map in the morning sunshine. I took many

photographs of the view. That evening, I slept again at my camp at the foot of the mountain. During the night I could hear a storm beating on the canvas, and when I threw back the cover in the morning I discovered it was snowing and sleeting. Unfortunately, there was not room under the canvas to dress, so I had to leave my snug bed and dress in the cold outside. Thoroughly chilled, I gave up and went into Marathon, where I drank hot coffee to warm up, realizing that my days of camping in the Marathon Basin were over.

My first objective in the field work was to map in detail the Monument Spring and Marathon Quadrangles which covered the central part of the basin. I had at first hoped to extend this detailed mapping into adjacent areas, but the two quadrangles took the whole field season, so my reconnaissance of the adjoining areas would have to do. (Later, the Santiago Peak and Hood Spring Quadrangles to the south were mapped by others).

I had double enlargements made of the Monument Spring and Marathon Quadrangles on which to plot my field work and sometimes these did well enough. But in the complex Ordovician areas I had to work on a larger scale. Sometimes, I laid off a grid on the map, then made a larger grid on a sheet of paper and plotted the geology on that. In the northern part of the Dagger Flat anticlinorium this was not enough; the relief was very low and the contours were not sufficient for location, but the structure was very complex. Here, I devised a system of pacing traverses. I ran parallel north-south traverses about 500 feet apart, and paced each line, marking the geology as I went. At the start of each traverse I took a compass bearing on a distant landmark and walked in a straight line toward it. The results came out very well, and closely matched the air photographs that only became available 7 or 8 years later.

Counting paces became automatic, and I would wake up sometimes from my sleep at night counting paces. I did the work during the hot days of August, and the walking and the perspiration gave me such a bad case of hemorrhoids that I had to stay several days in Alpine to recuperate.

At intervals, I would have to suspend the mapping to measure stratigraphic sections and collect fossils. For the most part, fossils did not appear on the surface of the rocks, but had to be revealed by splitting the layers, especially in the graptolite-bearing beds.

I accumulated large quantities of fossil material, which I laid out on an upper corridor of the Garnett Hotel. I had hoped that my collections would form the basis for a definitive treatise on the Ordovician of the Marathon Basin, but alas they were only studied in a cursory manner by the Survey paleontologists later. I am told that the collections are still in the National Museum.

I had little amusement in my lonely life, but I went frequently to the little movie theater in Alpine. That year there was a succession of musicals, which were a good escape from boredom. I remember especially Paul Whiteman's "King of Jazz", with a chorus dancing to music on top of an enormous piano. Maybe these would have been corny in some other setting, but in the dull life in the field and in Alpine they were a godsend.

At intervals, there were a succession of visitors. During the first part of the work, while I was still in Marathon, Charles Laurence Baker would frequently come out from Houston. He was supposed to have a job there with the Rio Bravo Oil Company, and I don't know how he got away. But he was very possessive of the Marathon Region, where he had done the first work, and there was an unspoken implication that he thought I was a young upstart and interloper. He looked down on my working hours and was always waiting for me in the hotel lobby when I came down after seven.

At any rate, his visits were quite helpful. We went to many of the Ordovician sections and collected fossils together, which served as a tie between his early observations and collecting and my own. In the field, where we both had our feet on the ground, his ideas and suggestions were most helpful. Then he would go back for a week or so to Houston, and the great defect of his scientific character (that plagued him all his life) would begin to surface. He would start speculating and spinning theories, and these got wilder as the days passed, and he would write me long letters in his crabbed handwriting. I had to learn to ignore them and follow my own counsel.

In the fall, various Survey people came by. The first was Edwin Kirk, the paleontologist responsible for the lower Paleozoic in the western United States. We went over the Ordovician sequences together and he cleared up many problems and misconceptions. I had formed an idea from Ulrich's early identifications of the Maravillas Chert as

being composed of a Trenton part and a Richmond part, and I had conceived the idea that the two were separated by an unconformity that cut well down into the Trenton part in places. We went over the evidence and he took it apart piece by piece, eventually finding supposed Trenton and Richmond fossils in the same rock slab. This taught me a good lesson in not trusting too fully in my speculations, no matter how convincing my observations might seem to be.

We could find no justification for Baker's Upper Cambrian and "Ozarkian" zones in the sequence, based on Ulrich's identifications, which had been a mystery to me. Kirk confirmed my own conclusion that the rocks at the localities cited were simply the shale next below the Maravillas Chert (which I later named the Woods Hollow Shale). However, the shales at these localities did contain large round limestone "concretions" from which we broke some fossils. Later, in the following winter when Kirk and I looked at the collections in the Museum, he found that they contained the missing faunas. The "concretions" were actually exotic boulders, from older formations, embedded in the Middle Ordovician shale!

Kirk was a great talker, and he kept up a continuous stream of conversation during his stay with me -- about stratigraphy and paleontologists, about politics in Washington, then about his many hobbies of collecting antiques and rare prints. The silence was deafening when I finally put him on the train.

My next visitor was David White, who wanted to look at the Carboniferous plants of the Marathon area. He was a benign, genial gentleman, a former Chief Geologist, interested in rocks and sediments as much as fossils. He was shocked that I did not carry a hand lens, and was continually showing me things that he could see under the lens. A number of plant localities were known in the Tesnus and Haymond Formations, and I planned to take him to these. But our progress from the car to the localities was painfully slow, as he looked at everything on the way, so it was often late in the day before we finally reached our objective.

We did make some collections, nevertheless, but it was clear that it took an expert to obtain diagnostic plant material. White scorned the bark and logs which were plentiful at some of the localities, and collected and saved little pieces which he said were flowers and fruiting bodies.

On one trip, to a locality in the Haymond Formation southeast of Gaptank, we crossed a bed of conglomerate that Robert and I had mapped in 1927. As there were many conglomerate beds in the Gaptank Formation and in the higher beds in the Permian, this one in the Haymond had not especially impressed us. White, however, thought it very unusual, pointing out that it contained boulders of Devonian novaculite and of Ordovician cherty limestone several feet across. He urged me to do some more work on it -- anticipating more sensational developments in the Haymond that were to come in a month or so.

The third Survey visitor was George R. Mansfield, my Section Chief (= Branch Chief), who came early in November. I showed him the various features of the area -- the stratigraphic section, the structure, the great overthrust -- and he was very sympathetic. He proposed that, when the Marathon work was done I should join Levi Noble in California, and assist him on his Death Valley project. This was an intriguing possibility and I was eager to try it, but as things turned out later, nothing came of it. (The job was given in a few years to Tom Thayer, with disastrous results, and it was only years later that I was to see much of Levi in California). Mansfield also told me things about Survey procedure that I had not realized -- how I had accumulated annual leave that I could take when I chose. I resolved to go back to Tucson for a visit when the field work was over.

The last week or so of field work there were some things left to be done on the Carboniferous in the eastern part of the Marathon Quadrangle. I had made the acquaintance of two Gulf geologists, A. G. Nance and John Bean, who were plane-tabling the Cretaceous east of the Marathon Basin, and were staying in the hotel at Marathon. They agreed to spend a day with me plane-tabling and measuring a good section of the Tesmus east from the novaculite ridges to a ridge of Dimple Limestone.

We finished our section about noon, and decided to spend the rest of the day looking at a piece of ground in the Haymond Formation at the east edge of the quadrangle which I had not yet visited. This involved the highest beds in the trough of the syncline. As I walked down the valley in the trough I realized that there was something new here. Scattered about were pebbles and cobbles of foreign rocks -- novaculite, chert, and sandstone that were only in place thousands of feet lower down in the section. There were even rounded cobbles of

crystalline rocks quite foreign to the region. As we proceeded along the syncline the fragments became more numerous, and finally, just before the bedrock was covered by terrace gravels at the eastern end, there was a huge block of Dimple Limestone more than a hundred feet across! I had discovered a "boulder bed"!

The Caney (later called Johns Valley) boulders in the Ouachita Mountains had been known for nearly 50 years, and were a source of great controversy. I had rather hoped that something of the sort would turn up at Marathon too. The summer before, Sydney Powers of the Amerada, a roving sage of Southwestern geology, had stopped off at Marathon and at the hotel that evening he was explaining the many problems of the "Caney" boulders. I remarked, "Sydney, we will find them at Marathon too before it is all over." And he said, "My God! I hope not!" Now we had the boulders too, but at a somewhat higher level than those in the Ouachita Mountains.

I was much excited, and when Charles Laurence Baker came through a few days later, I took him to see my new discovery -- which may or may not have been a mistake. He said, "This is a glacial deposit, and let's firm up the evidence before anyone else gets to it." I didn't think it was a glacial deposit, and was very lukewarm about this.

With this high note, my field season was over for the year, with the Monument Spring and Marathon Quadrangles all mapped in detail. Before going east, I took my vacation trip to Tucson, going out on the Southern Pacific train.

It was good to get back to civilized life and to see all my friends again. For some reason, I had left all my books at the University in June, and I now boxed these up to ship east. I saw Clara Lee, and one morning we drove to Sabino Canyon to see the Arizona desert again. I kissed her under the desert mistletoe (an idyllic moment) and begged her to marry me, which she wouldn't. So this was over, but the memory dragged on for a year or more.

Working out of Tucson for the Survey was Bernard Moore, recently from Cal Tech, whom Mansfield had assigned to go over the Tucson Quadrangle that had been worked up years before by C. F. Tolman for a Tucson Folio. The folio had never been published, but the maps and manuscript were in the files in Tucson. I took Moore and B. S. Butler to see the Colossal Cave Hills in the south part of the quadrangle where I had worked the year before. (Moore worked for several years on the quadrangle, but made a mess of things and his results, like Tolman's were never published).

My last evening in Tucson I paid a visit to Clara Lee's brother, Mason Fraps, a biochemist who had set up his workshop in the old Desert Botanical Laboratory on the hill west of Tucson. The two of us and a friend sat around and drank and played records, finally going off on a wild drive across the desert. It was a drunken night, and they finally put me on the train in the morning, much shaken.

Back in Alpine, I put my things in the pickup and went to Austin, where I spent a few days and talked about my work to the graduate students in Sellards' class at the Bureau. Baker had taken Sellards out with him to see the boulder bed, and together they had reconnoissanced northeastward from my mapping, where I had thought there was only surface gravel along the foot of Housetop Mountain, and had found more outcrops of the bed with even stranger exotics, including great slabs of a fossiliferous Pennsylvanian limestone like nothing exposed in the Marathon Region.

I got my own car out of storage at last, and started off alone for Washington. Winter was coming on and it was very cold. Toward the end of the trip I spent the night at Bristol, on the Tennessee-Virginia line, and when I awoke next morning it was snowing hard. I waited a long time for the weather to clear, and finally struck off with chains on the wheels, making Lexington for the night. Next day, I at last drove into Washington, parked outside the old Interior Building, and reported in to the Survey.

I spent the first few nights at the Roger Smith (Powhatan) Hotel, up the street from the Survey offices on Pennsylvania Avenue, while I tried to decide where to live. Living in Washington was a far different matter from going there for a day or so to sight-see, and a depressing contrast to the free-and-easy life in Texas and Arizona. There were miles and miles of middle-class row houses, mostly inhabited by government employees. There were few garages, so everyone left their cars on the street at night -- solid ranks of parked vehicles. Ancient street cars of several systems went about.

The young Survey people were mostly newly married by now, and lived in apartment houses near the Survey, where they rented places when they were in the office for the winter with "army-navy clauses" which gave them the privilege of vacating when the field season opened in the summer. Then, they stored their furniture, went to the

field for the summer, and found a new place to live when they returned in the fall. This kind of life didn't appeal to me much and was out of my capabilities; I was unmarried, and I had no furniture.

Mr. Mansfield told me that Mrs. Mansfield was renting rooms at their house, and I finally settled on a hall bedroom there (small but cozy) on the third floor. It was a three-storey row house at the end of a street facing Rock Creek Park and its trees, and within walking distance from the end of the Mount Pleasant carline. Nearby, was a turnin into a vacant area where I could leave my car off the street. I lived there for two years.

The Survey offices were in the middle wing of the old Interior Building (now G.S.A. Building). The printing plant was in the basement. The Library was on the first floor, and the chemists and engravers were at the far end of the corridor. The second floor was Conservation Branch (= Division); the third floor was Water Resources Branch; the fourth floor Geologic Branch; the fifth floor was administrative and Alaskan Branch; the sixth floor Topographic Branch. George Otis Smith was Director and W. C. Mendenhall Chief Geologist, but about the time I arrived Smith accepted a post with the Federal Power Commission, Mendenhall became Director, and T. W. Stanton Chief Geologist.

All the geologists were on one corridor, old and young (the group was small in those days). There were many old-timers about, some of whom had been with the Survey since the eighties, shortly after it was founded. There were my friends N. H. Darton and Arthur Keith, also M. R. Campbell, F. C. Schrader, Francois Matthes, Arthur Collier, L. M. Prindle, A. C. Spencer, G. B. Richardson, F. C. Calkins, E. F. Burchard, W. C. Alden, and others. Somewhat younger men were Wendell Woodring, Clyde S. Ross, H. G. Ferguson, and D. F. Hewett. H. D. Miser was Chief of the Fuels Section, G. F. Loughlin Chief of the Metals Section, L. W. Stephenson Chief of the Coastal Plain Section, and G. R. Mansfield Chief of the Areal and Nonmetallic Section. G. W. Stose was Map Editor. The Alaskan Branch was very tiny. Philip S. Smith was Chief; geologists were Mertie, Moffit, and Capps; Sargent and Fitzgerald were topographers.

A group of younger men had come on in the mid-twenties, most of them Yale men, and included W. C. Bradley, W. W. Rubey, James Gilluly, Carle Dane, T. B. Nolan, A. A. Baker (brother of Ray Baker of Texaco), Bill Johnston, and F. G. Wells.

Money had been found to hire a new crop of my own generation, with many of whom I had been at school at Yale: C. B. Hunt, T. A. Hendricks, W. G. Pierce, Dave Andrews, Frank Parker, and M. N. Knöchtel. Most of these were newly married. Two young paleontologists had been hired at the same time: Josiah Bridge to work with E. O. Ulrich, and J. S. Williams to work with G. H. Girty. I could mention many other people, both old and new, but the list would be tedious.

We were placed two in a room, each person taking half. I had a succession of roommates -- Lou Currier, A. L. Koschman, Walter Lang. In the late thirties I got one of the smaller rooms at the end of the corridor, and at last had an office to myself. I set about working up the results of my field work for a report on the Marathon Region, which took me (with interruptions from other things) ~~for~~ the next three years.

It was a lonely time, and I felt very bereft of the activities and friends I had had in Tucson. The older people did all they could to make me feel at home. The Darton's entertained me at their apartment on 16th Street, where I saw much of them and their daughter and son-in-law, Nan and Bill Kerlin. The Mansfield's invited me down to share their Christmas; their two daughters and three sons were there, but I felt very much out of it. At New Year's, Mary and Foster Hewett invited me to their party at their place on Woodley Road, where the guests included Bill Johnston and his fiance Madeline, and Francis Wells. Miss Julia Gardner was a friend of all young people and newcomers, and invited many of us to her dinners at the Arts Club.

I began to try to make some kind of social life of my own. My aunt Jessie King was a Professor of Physiology at Goucher College in Baltimore and shared an apartment there with my Aunt Bertha. I went over there for many weekends, and through Jessie met some of the Goucher girls, with whom I had some inconclusive dates. The Darton's had a young visitor from Iowa, a niece of Bill Kerlin, and they encouraged me to date her, but she was dull company. A nice girl in the accounts section of the Geologic Branch, Alva Schenken, was very friendly, and frequently invited me to dinner at her boarding house; I took her to shows (she later married Carle Dane). Sunday evenings I frequently went to supper at Gus and Josephine Cooper's. Gus was down from Yale like me, and was now with the National Museum. But all in all, life was dull and lonely.

Between Christmas and New Years, just as I was getting settled, the Geological Society of America held its meeting in Toronto, and I went. The Washington group was large enough so that we had our own Pullman car. Meetings were held at Toronto University. Sellards had brought with him lantern slides of pictures of the Haymond boulder-beds, and with his help I was worked into the program for an impromptu report on the discovery. I saw much of the Darton's at the meeting, and Mrs. Darton was very kind and affectionate. On the way back, the train paused on the bridge over the Niagara River and we had a fine view of the famous falls.

In February I went back to Iowa for a brief family visit. I don't remember much about it, except that on the way back I saw hundreds of empty freight cars in the yards in Pittsburgh, and was told that this was due to business stagnation resulting from the depression.

My Glass Mountains report was published as a Bulletin by the Bureau of Economic Geology in February or March of 1931 -- very prompt publication. I had read proof in Alpine the preceding Autumn.

I went over frequently to the National Museum to confer with Edwin Kirk about the Ordovician at Marathon, and to go over the collections with him. By spring, the section had begun to shape up, and I proceeded to name the formation units. A meeting of the Geologic Names Committee was called, where I presented the names, which were approved after some discussion.

In the spring of 1931 the American Association of Petroleum Geologists met in San Antonio. I attended and presented a paper on the newly worked out lower Paleozoic section at Marathon.

On the way there, I stopped in Austin for a few days. One of the hangers-on at the Bureau was a beautiful red-headed girl named Elizabeth Hindman (she called herself "Boog"), and we were attracted to each other at once. She said her brother-in-law was city editor of the New York Herald-Tribune, which seemed incredible; later, I realized that he was the famed Stanley Walker, about whom there were many legends. Boog was attending the A.A.P.G. meeting, and I took her to the evening dance. One day we drove to one of the parks in San Antonio, and we kissed passionately -- an idyllic moment. Our romance flared, and when I went through Austin on my way north a week or so later I visited her at the Scottish Rite Dormitory for another passionate evening. But I never saw her again.

After the meeting, a select group went out to Marathon to see the region under my guidance. Most of them went out by Pullman, but I drove out with Hugh Miser and Clarence Ross in a cramped Survey vehicle. The group included Mendenhall, our Director, E. B. Branson of the University of Missouri, Charles Dake of the Missouri School of Mines, Jo Bridge of the Survey, and Mr. and Mrs. Stille from Germany. One day we killed a rattlesnake, and the Stille's talked for a long time afterwards about the "riddlesnakes." Stille was particularly interested in my scheme of deformations and orogenies in the region, and I realized later that he was trying to fit the postulated events in his mind into his scheme of world-wide orogenies. I showed the group the Haymond boulder-beds, and my Ordovician section. I had seldom been in the Marathon Region in the early spring before, and I had never seen the country more green and beautiful; little flowers were in blossom on every hillside.

After the main party had left, I spent a few days longer at Marathon with Miser, Ross, Bridge, and Dake, looking especially at the Ordovician. Dake had spent the previous year on a sabbatical working for the Sinclair Oil Company on the Ordovician of Texas, most of the time in the Llano area, but partly at Marathon. I feared that he would be a dangerous rival, but he was very sympathetic to my ideas, and helped to explain them to the others. Jo Bridge's enthusiasm in the field were contagious; he took movies of my beating my way across the country in a Survey vehicle.

After the conference, Miser, Ross, and I returned to Austin, then reassembled again at McAlester, Oklahoma. The following summer there was going to be a big field conference of the Kansas Geological Society in the Ouachita Mountains, and Miser wanted to go over the ground. Various other people joined us in different places, notably C. W. Honess, who had mapped the southeastern part of the Ouachita Mountains in Oklahoma for the Oklahoma Survey; he showed us the complex core of the Broken Bow uplift. Miser also wanted to show Ross and me some of the "Caney" (= Johns Valley) boulder beds, and one day we took a Frisco train north, got off at a way point and walked the tracks to Bengal, where we took another train south. Bengal was a very isolated place out in the woods, and when we came in the natives looked as though we were men from Mars.

In Arkansas, we saw something of the Ordovician core rocks of the Benton uplift, the section of Arkansas Novaculite at Caddo Gap, and the Carboniferous sandstones and shales of the Athens Plateau to the south. We visited the diamond-bearing peridotite at Murphreesboro and the Magnet Cove intrusive complex southeast of Hot Springs. Our trip ended at Hot Springs, whence I returned to Washington by train. I have made trips to different parts of the Ouachita Mountains since then, but this is the most comprehensive review I have had.

The summer field season of 1931 was approaching, and I began to think about what to do next. I told Mansfield that I was ready to go to California to start in Death Valley with Levi Noble, but for some reason he was evasive. In the field the autumn before David White had proposed that I should make a regional investigation of the Permian of the Southwest, which I conceived would be a study of key sections and their correlation, and this project was set up for me, with a plan to make a start on it during the coming summer.

The next problem was to find a field assistant. I wanted someone who was not only qualified, but who would be good company, as I was very low in my mind. I didn't warm up to various young geologists that I interviewed. Along in May, Dr. Stanton came to me with a problem. It seemed that Brookes Knight, my older friend at Yale, had applied for a Civil Service job, and had qualified, but had received no offers. He was close to 45 years of age, which was the cutoff age for appointments, so he used political influence through his St. Louis connections to obtain a Secretarial appointment -- a matter viewed askance by the Survey. Stanton said they would have to hire him, and could I use him? I was delighted, and jumped at the chance.

In early June I went down to Texas and drove the Model A pickup to Marathon to do some additional cleanup work, and Brookes joined me there in a week or so. I looked at various odds and ends, but my main objective was to map in detail the extension of the Haymond boulderbeds past Housetop Mountain that Baker and Sellards had discovered the previous December. With the aid of a plane-table and alidade I made the survey, and marked every large boulder along the outcrop. When Brookes joined me, I put him to work collecting fossils from the big slabs of Pennsylvanian limestone.

With this accomplished, we set off for new fields of endeavor. I planned first to look some more at the Permian in the Sierra Diablo, so we drove to Van Horn. We found that Van Horn had been much revitalized from the run-down village I had known two years before. A new version of the main highway to the west had been cut directly through the town, and new buildings had been put up along the main street, including a fine new El Capitan Hotel. The next morning I looked up the Duncan's and found that they had done much to improve their run-down house. We took two rooms there on the first floor, with a porch in the back. We ate our meals at the El Capitan Hotel, which had an excellent restaurant.

I decided to look first at the buttes that rose above the Texas and Pacific Railroad at Eagle Flat, about 15 miles west of Van Horn. Brookes collected from the Permian rocks of the buttes, while I roamed the surrounding countryside. I was in top form, and walked long distances over the foothill country south of the Sierra Diablo, mapping as I went. I was interested, not only in the Permian, but quite as much in the overlying Cretaceous and in the underlying Precambrian. I found that the Permian lay to the south on schistose metarhyolite, which had a south-dipping contact with folded limestone on the north, and that north of this was a great mass of coarse conglomerate that graded up into red sandstone. The Precambrian greatly intrigued me and I was eager to spend more time on it, but was only able to do so about 8 years later.

Then we moved our operations closer in toward Van Horn, and on north along the east side of the Sierra Diablo. We covered a lot of ground and looked at many interesting things, but it was clear that we could not hope to exhaust the possibilities of this intriguing region in the time available. (Brookes and I were to spend two more long field seasons there).

I walked and climbed long distances, making geologic maps; Brookes measured sections of the strata and made large fossil collections. We climbed the Ordovician cliffs at the south end of Beach Mountain; we went over the northeastern part of the Baylor Mountains; and the complexly faulted rocks on the point of the Sierra Diablo north of the Figure Two Ranch, as well as the escarpments of the Sierra Diablo itself. We discovered that the Upper Ordovician Montoya Limestone was overlain in places by Silurian Fusselman Dolomite and Devonian chert, which were hitherto unknown in the region.

We made one long trip to the top of the Sierra Diablo, but were caught in a great cloudburst, and had difficulty getting out and getting home. We hired horses at Figure Two Ranch, and took a long ride far up Apache Canyon. The horses were skittish and we dared not leave them unattended; there was another cloudburst, and we returned to the ranch tired and stiff; I resolved not to try this method again. Brookes discovered a remarkable "molluscan ledge" in the Permian far up on the escarpment northwest of the Figure Two Ranch, and we went back next day and collected, each carrying down more than 50 pounds of fossils on our backs.

Toward the end of our stay in Van Horn, Robert joined us as a guest. He had had an adventurous two years in South America, and while his work was well received by Texaco, they would not renew his contract when his time was up. The depression was setting in earnest, and geologists in the oil business were losing their jobs right and left. (It was to be another three years before he was employed again, but he had accumulated a large sum of savings).

I wanted to spend the last part of the summer in the Hueco Mountains, so we left Van Horn and moved into El Paso, Robert with us. We took rooms again with the Outlaw family. There was now a good highway across the Hueco Bolson, across the Hueco Mountains and into Carlsbad, New Mexico. Brookes set to work, not only on the Permian (Hueco) limestone, but on the Pennsylvanian (Magdalena) limestone of Powwow Canyon, measuring a detailed section and collecting large quantities of fossils. As before, I scouted around, mapping as I went.

Soon we fell into the routine of spending our evenings in Juarez. I discovered to my dismay that Brookes was a compulsive gambler. We went to the casino, I merely to look around, but Brookes had to play the tables, losing a lot the first time, hence determined to try again and recoup his losses. We also went to many of the cabarets, including a place called the "Big Kid's Palace" where there was a floor show with pretty girls, with some of whom we became acquainted. With Robert's taste for low life we also got involved with less reputable places, whorehouses and the like. Robert took up with a girl who called herself Justa, who was hanging around Juarez for reasons not apparent. She was an unstable character, who said she had taken courses at U.C. in Berkeley, but was now living in low company. She attached herself to us. On one or two days she went out in the field with Brookes and

me. At the end of the month I gave her enough money to get back to California, and was glad to see the last of her.

On the higher side, we got acquainted with some of the geologists and mining engineers in El Paso, and at the College of Mines. One of them was a dynamic chap named Barry, who later became for a time president of the College of Mines.

We made a long trip west to the Chiricahua Mountains of southeastern Arizona, where we met Stoyanow briefly and he showed us his Paradise Formation, a Mississippian unit like the Mississippian of the Hueco Mountains. We spent the night in a tourist cabin at Portal on the eastern side of the mountains, where it was good to hear a running stream outside the cabin at night, after our months in the parched desert. On the way, we stopped for lunch at a wayside place, and Brookes obeyed his gambler's instinct and put money in the slot machine; he won the jackpot and paid for our lunches, with money in his pocket besides.

The season was over. We stored the pickup, packed our fossils to ship east, and I drew my money from the checking account at the El Paso bank. Well that I did, for the bank folded the following winter, and next time I was back the building was a movie theater.

Robert had gone on west to California, and I followed him. We spent a few days with the Harter's in Hollywood and I saw Grandmother Lash. She was very old and feeble, and it was the last time. Robert and I went over to Cal Tech in Pasadena, but found the place nearly empty. Finally, upstairs in the Geology Building we found a chap bending over a microscope. He turned out to be Ken Lohman, who said he was on the Survey too, and was moving soon to Washington. I also wanted to call on Ralph Reed, chief geologist for the Texaco in California, whose office was downtown. Ken took us in his Model T, on a wild ride through Los Angeles traffic. I had an interesting talk with Reed, whom I had met several times before.

Then, we decided to go up to San Francisco, where I had arranged to see my friend Dorothy Gay. I had rather hoped to strike up a romance, but while she was always a good friend, this didn't work out. We went up on the Southern Pacific Daylight, and had our first look at coastal California. Dorothy met us at the station, and found rooms for us at the Sutter Hotel in downtown San Francisco.

Next day, we decided to go down to Stanford, and Dorothy went with us. We went down on the train. I wish I could remember more about the trip, because I would see much of the same country 25 years later, but it is mostly blank. Nor do I know how we got from Palo Alto station to the University -- was there a trolley car? Anyway, it being the end of the summer, Stanford was nearly deserted. We went to the empty geology building and were about to give up when Bailey Willis came down the stairs. Robert, bold as brass, went up and said, "Dr. Willis, I guess you don't remember me!" He didn't, of course, but was very cordial. He took us to lunch at The Cellar, and afterwards showed us the drawings he was making for his projected book on "Living Africa."

On another day, we took the ferry across the Bay (there were no bridges in those days) and looked around the Berkeley campus, then called on Dorothy and her parents on Tulare Street. The fog was heavy, and I was quite cold on the ferry in my summer clothes. One evening Dorothy and a girl-friend went with us to the theater; I believe the play was "The Greeks have a word for it." We ate our meals in restaurants, but all we could find had French or Italian cuisine, with five-course dinners. Finally, in desperation, we ate sandwiches in drug stores.

On our last day in San Francisco, Robert wanted to look up F. M. Anderson, the venerable Mesozoic paleontologist at the California Academy of Sciences. He had done some work in Colombia which Robert wished to discuss. We went out to the Academy in Golden Gate Park on a street car, and found him to be a very genial and talkative gentleman. When he found I was with the U.S. Geological Survey, Colombia was forgotten. He was carrying on a war with the Survey paleontologists about the Mesozoic on the west side of the Sacramento Valley. The Survey people had decided the section was all Cretaceous, whereas Anderson had divided its lower part into a Jurassic Knoxville unit, and a Lower Cretaceous Paskenta unit. He pulled out drawer after drawer of ammonoids and other fossils, saying triumphantly, "Show this to Dr. Reeside!" Time was getting on; we had a 7:00 P.M. train to catch, so we finally broke away and ran for it.

Dorothy Gay was returning to the University of Arizona, and we had arranged to be on the same train. She was on a Pullman car from Oakland that was connected with our train in San Jose. Unfortunately, the train was crowded with boisterous conventioners, who were going to some kind of meeting in Los Angeles. We were glad to see the last of them when we got there next morning. Dorothy and I had breakfast in the railroad station, then took the Southern Pacific train east. All day we talked about this and that, as the train made its slow way across the desert. When we reached Tucson in the evening we said our goodbyes and I kissed her. I went on to El Paso, picked up my other ticket, and continued the tedious train trip into Washington.

Returning to Washington was like our return to the dull life of New Haven, after our eventful summers in the west. To add to the misery, Washington was undergoing a late season heat wave that continued into October. The cold and fog of San Francisco of the week before seemed far away, an impossible dream.

I continued work on the Marathon report, but interrupted it many times that year on other things. The Bureau of Economic Geology was preparing a book on "The geology of Texas". Sellards was writing the Paleozoic, Adkins the Mesozoic, and Plummer the Cenozoic. Sellards sent me proof of the Paleozoic part to look at, but his treatment of the Permian seemed so inadequate to me that I determined to write my own summary. This task took me a good part of the coming year, and was published by the Geological Society of America in 1934. In it, I was able to use some of the observations Brookes and I had made the summer before. At some point, we had a meeting of the Committee on Geologic Names on the terminology to be used. G. B. Richardson was very obstructive. He had made the El Paso and Van Horn folios years before, and couldn't believe the new things we had been finding there. As the discussion progressed, however, I realized that Girty was on my side. One result of the meeting was the decision to restrict "Hueco Limestone" to the Permian part, which contained Girty's "Hueco fauna".

A Survey visitor that fall was W. A. J. M. van Waterchoot van der Gracht, the famous Dutch geologist whom I had seen at a distance during my year with the oil company in Texas. He was now at work on his great synthesis on "Permo-Carboniferous orogeny in the south-central United States." He had seen my Glass Mountains Bulletin and was much impressed with it, and was very friendly. He was applying European concepts to

our Southwestern geology -- concepts on the Alps and on Continental Drift, and they did much to sharpen my own thinking.

The International Geological Congress was scheduled to meet in Washington in the summer of 1933, and much of the Survey activity was being diverted to preparing for it, under the direction of Harry Ferguson and Marcus Goldman, the secretaries. Stose and his helpers were busy on a new Geologic Map of the United States, to be passed out at the Congress, and Fergie was busy rounding up contributors to the Congress guidebooks. It was decided that part of <sup>the</sup> transcontinental excursion would be in West Texas, so Darton and I prepared a guidebook for it.

I was interested in putting together regional tectonics, and had made a tectonic sketch map of part of the Southwest. One day, when Fergie came in to see me about the West Texas guidebook, he saw the tectonic sketch map and said, "I'm looking for someone who will write up the structural geology of the United States for a guidebook. Why don't you try it?" He said he had thought of various eminent geologists, such as Reginald Daly, but had had no takers. This was a dazzling prospect for someone as insignificant as myself, so I set to work on it in the spring of 1932. I went over all the publications, and interviewed the geologists on our fourth floor who were experts on different parts of the country. I had some general ideas on how to proceed, mostly based on my course work with Carl Dunbar and Chester Longwell at Yale, but I ran into conflicts with various people. George Stose and Anna Jonas did not like my ideas on the Appalachians, and Mendenhall rejected my first try on the California Coast Ranges, which I had based on the syntheses of Bruce Clark. Mendenhall said Clark was dishonest and wrong. It took much work, with the help of Wendell Woodring, to work out a new version. Woodring referred me to some of the classic reports on California geology that were published by the University of California, and I was glad to become acquainted with them.

In December I went west for a family visit in Iowa City, and Robert was there. After leaving me in Los Angeles in September, he had obtained a little money from Professor Schuchert to collect Paleozoic fossils from an area in Sonora, Mexico, that had been reported years ago by Dumble. He had gone to the place, in southern Sonora, called La Casita, but had found little. On his return he discovered

that there were two La Casitas on the map, so he had been to the wrong place; the other was in central Sonora, not far out of Hermosillo. He decided to go back again. He had found that living in Mexico was cheap and pleasant, and a good place to live on his accumulated savings.

The Geological Society of America meeting was to be in Tulsa, Oklahoma, in the week between Christmas and New Years, so Robert and I went down to that, he to go on from there back to Mexico. I went by train; he arranged for a ride with a school-marm who was driving down from Iowa to Oklahoma. In Kansas City, who should get on the train but Joe Runner and a graduate student, both looking very sharp in derby hats? Joe was in top form and enlivened the trip. Looking at the timetable, he discovered we would go through a place named Owasso. He said, "Why, we used to have a cat named Owasso! I'll have to look for this place." We got there along in the afternoon, and he stuck his head out of the window and shouted "Oh-Wasso!" Everyone in the car thought he was drunk, but he was just being exuberant.

It was a good meeting, with many interesting papers. I gave a paper on the Haymond boulder-beds<sup>^</sup> and another on Permian reefs. One of the Oklahoma geologists felt this was a fitting occasion to bring Southwestern geologists into Fellowship in the Society, and included me in the group. I thus joined the Society at the age of 28, years before most of my contemporaries.

After the meeting there was a field trip to the Arbuckle Mountains and the western Ouachita Mountains. I saw the Arbuckle Mountain geology for the first and only time. Tomlinson had arranged lunch in Ardmore, served by some local church ladies, and we saw the Ardmore Basin geology with him. We went on to Antler that night, and the next day drove into Johns Valley by a very difficult road to see the type area of the famous boulders. Next day we went north to see the Potato Hills and its "window". It was a very rainy spell and the creeks were rising. It was very hard to get the cars across the creek and into the hills, and still harder to get back. I rode with Miser back to Tulsa in the pouring rain, and took the train back to Washington.

The great Depression was settling down over the country, and there were hard times everywhere. Washington swarmed with beggars, who would stop one on the street and ask for handouts. Government employees were relatively fortunate, as we were on fixed incomes. But there was a demand that we suffer too, and the Hoover administration proposed an  $8\frac{1}{2}$  percent paycut, which was passed by Congress, so my already small pay was still less. Survey appropriations were also low, and there was no money for field work in the summer of 1932 ahead. This was not an immediate worry, as there was much for me to do to catch up, and a summer in the office wouldn't hurt.

My social life continued its halting way through the fall, winter, and spring. Sometime during the winter, Julia Gardner told me that Professor Knopf's daughter Agnes was in town and proposed that I meet her. She had the two of us to dinner at the Arts Club. Agnes proved to be a beautiful but rather willowy and cadaverous girl. She had been in school at Bryn Mawr and had done very well, but had become bored and had come down to Washington to live with relatives, <sup>her mother</sup> and had obtained a secretarial job with some arts foundation. Agnes and her sister and brother were children of Knopf's first wife; he was a widower who later married the geologist Eleanora Bliss.

I invited Agnes to the movies and afterwards suggested that we have a drug store drink. She said, "Come on home and I'll fix something better." So we went home and had gin highballs. Her relatives were a bridge-playing couple who lived in a tree-shaded area in the western part of the city. Agnes was fond of cigarettes, gin, and bridge. She was a girl of strong and rather perverse ideas, and she had a pose of being very superstitious.

We went many places. We went to see the stage show at the colored Howard Theater on U Street, and saw Louis Armstrong perform on his trombone. I took her to a night club in Baltimore and to a play by Mae West in Washington. She was impressed that it was closed next day by the authorities (it was rather racy, but what created public indignation was that one of Mae's lovers in the play was a negro). Sometimes Agnes became very passionate, and we would sit at home and "neck" over our gin.

One Sunday in February, for want of entertainment, I proposed to Maxwell and Alice Knèchtel that I call on them. Once there, Alice announced that she thought it was a good idea to have someone else too, and had invited a young friend of hers, Helen Carter. Helen arrived late, and proved to be a vivacious talker. She had come by taxi, so I proposed taking her home. On the way there, we pulled into a side street and "necked". This was a surprise on our first meeting, but it was a while before I followed up our acquaintance.

The office group, Alva and her secretarial friends, had many activities, into which I joined. In the fall, I went with her and some friends to the Navy-Notre Dame football game in Baltimore. In the spring, there were swimming parties at Widewater, a wide place in the old Chesapeake and Potomac Canal. In June, we all went to Youghogheny Lodge in West Virginia.

Youghogheny Lodge had been built the year before by Frank Reeves, an ex-Survey geologist, and a friend of his. He had left the Survey to do geological work in Australia, and had hoped to get his Survey job back when he returned, but there was no place for him. For something to do, he had built the lodge, high up in the Alleghenies of West Virginia, planning to run it as a tourist resort, and many Survey people had gone up there on vacations. We had a fine time, and spent the evening getting plastered on gin.

I began to see a good deal of Helen Carter, and we went many places together. I went to some of her parties and met her aunt and uncle with whom she was living. Along in mid-summer we decided to go together to Youghogheny Lodge for three or four days. She knew the Reeves's, especially Frank's wife Lottie. On our return we found that Helen's aunt Gertrude was incensed because we had gone alone, but we were well chaperoned while we were there.

Toward the end of the summer, after a date, Helen told me that she was giving me up, and would not change her mind after my tearful protests. Very blue and low in my mind, I had a date with Agnes, and she wormed out of me why I was acting so strangely. She was naturally incensed that I was carrying on another romance and seeing her at the same time. So that was the end of that, too, and all my painfully built up social life was collapsing around me like a house of cards!

About this time, it transpired that Ray Becker and Chester Longwell planned to go over the route which the New England excursion of the International Congress would take the following year, and I decided to get away and go too. Ray Becker had been hired by Ferguson to go around the country and make arrangements for accommodations for Congress excursionists. He was to make contracts with hotels and eating places so that everything would be taken care of when the guests arrived at a particular place at a particular time.

We assembled in Albany, New York, and drove north through the Adirondacks, then by ferry across Lake Champlain to Burlington, Vermont, where we looked at the complex Paleozoic stratigraphy and structure of the Champlain lowland. Professor Schuchert was spending the summer there, and showed us some of the interesting things he had discovered. We then went south through the Vermont marble belt and the Taconic Range, where Prindle had worked out an amazingly complex structure, and on to Amherst, Massachusetts, in the Triassic. It was good to get out of the Washington heat and into the far northern country, where it was crisp and cool, and autumn was already in the air. We went down to New Haven, and I spent a few days with Brookes Knight and his family at Killam's Point, before going back to Washington.

Shortly after my return from New England in September, I had a phone call from Helen. She had changed her mind and was taking me back! We had lunch at the Carlton Hotel, and a happy reunion. We had some more dates and I proposed marriage, and to my surprise she accepted! I had in mind a long engagement, citing the uncertainties of the times. She argued that it would always be that way, and urged that we should get married as soon as possible; the date was set for the coming October 30.

We set about looking for a place to live. Helen said that living in Georgetown would be best; it was a little different from the rest of Washington, and more relaxed. We found a place at the Mammoth Oaks Apartments, on 32nd Street just off of Q Street, which faced a large open square on the opposite side of the street, and we rented a one-room apartment.

There were many things to do. I had to make my peace with Harry and Gertrude Finney, the uncle and aunt with whom she was living, and they welcomed me into the family. I met her many other relatives who were living in the Washington area -- the Whittlesey's, the Finney's, and the Denham's. It was strange and wonderful to have such a large family of relatives! (Most of them, alas!, are now dead and gone.) I bought a diamond engagement ring and wedding ring for several hundred dollars. I met the people with whom she was working at the H. L. Rust Company, and her many friends.

During October, we went several times to a place in the woods that Helen had found, on Difficult Run near the Potomac River on the Virginia side below Great Falls. It could be reached by a track off of the main road, and was known to only a few people. One day when we were walking in the woods, we heard someone coming through the brush. When he finally broke through, who should it be but Francois Matthes of the Survey? He told us that he had invited some of the young Survey people to a nature hike in the area the following weekend, and invited us. On the hike, Helen decided to announce our engagement, and we received congratulations and best wishes from everybody. Carle Dane said, "I certainly like your fiancee," and I embarrassedly said, "I do too," to which he said, "You ought to, you're going to marry her!"

The wedding was held at Helen's grandmother's home on Webster Street, with Z. Barney Phillips, the Episcopal chaplain of the Senate officiating. I invited Mr. and Mrs. Darton and Gus and Josephine Cooper; Helen had a few of her relatives. Helen was 24 years old, and I was 29.

We planned to have our honeymoon at Virginia Beach, and drove there after the ceremony, arriving at the Cavalier Hotel late in the evening. The season at the beach was about over and the hotel was nearly empty, so we had it pretty much to ourselves. We spent two days there, and then drove back to our new home at Mammoth Oaks. I was surprised at the great quantity of wedding presents, especially in these hard times -- china, silver, and many other things -- from Helen's and my relatives, and from many of my Survey friends. (Some of the gifts, especially the china, have long since disappeared, but some we still have). The gifts set us up nicely in our new home.

A new world opened out for me, in bright contrast to my previous dreary existence in Washington, and I relished my life with Helen and with the many friends and relatives, and the social activities. We loved each other very much. Helen had a job as bookkeeper at the H. L. Rust Company, a real estate firm in the city, and we decided that she should keep it for the time being, and save what we could of her little earnings. We ate our dinners at a little restaurant in the apartment building, for Helen had only a rudimentary idea of cooking, so far.

Around Christmas time we went to a party at one of Helen's wealthy young friends, the Langhenheim's, where much gin was consumed. Next day, we went to dinner in Baltimore with my aunts Jessie and Bertha. Between Christmas and New Years the Geological Society of America met in Cambridge, Massachusetts, and we went together by train. I gave a paper ~~on~~ my West Texas Permian work, which was well received. During the meeting Sayles conducted an impro<sup>m</sup>ptu field trip to the Squantum Tillite for Walter Bucher and John Rich, and we went along -- one of Helen's first introductions to field geology. After the meeting we went down to New Haven for a day or so, and stayed with the Carl Dunbar's; they gave an evening party for some of the new graduate students, including Norman Newell.

In the election the previous autumn, Herbert Hoover ran against Franklin Roosevelt, an unknown quantity, and Roosevelt won. The country was eager for a change, for the Depression, instead of easing off, was getting worse. Inauguration of the new President and his administration, following the custom of the time, was not until the following March. During the long interregnum, banks were closing right and left, and there was great uneasiness. We stayed away from the city during the inauguration, and went to Maxwell and Alice Knechtel's place to hear it over the radio. Roosevelt gave a vigorous, impressive speech, beginning with "We have nothing to fear but fear itself." The band played "Happy days are here again". -- but we wondered whether the "happy days" were for us, or just the Democratic politicians.

Roosevelt's first act was to close all the banks for a prolonged "holiday". No one had any money except the cash in their pockets. The banks were rigorously inspected, and only those that were solvent were allowed to reopen. Many of those in Washington did not, but fortunately the Union Trust Company, where we had our accounts, did reopen. During

the first few months, the Roosevelt administration did many things, some at cross-purposes, but the general feeling was that any action was better than the stagnation we had been going through. One of Roosevelt's first acts was to decree a 15 percent pay cut for government employees, which made things even harder for us on the Survey.

One act of the new administration was to order a cut in personnel of the Survey. As a result, many of the older geologists, including Darton and David White were ordered to retire, and some of the younger ones, including Francis Wells and Dave Andrews, were slated for dismissal. One of the men to be dispensed with was Daniels in the Survey printing department. He also happened to be mayor of Falls Church in Virginia (this was before the Hatch Act), and he sent his protests up through the Democratic hierarchy. Irregularities were found in the procedure used; it seemed that performance ratings were doctored in the Director's office. The whole thing was called off. A scapegoat was made of Shelse, the Chief Clerk (much hated), and he was demoted. In the end no one was hurt; even Shelse went on to a high position in the Public Works Administration that was being organized in the Interior Department.

During May, Robert showed up from Mexico, and spent a week or so at a room near us. He had spent more than a year in Mexico, mostly in Sonora, but during the last month at Las Delicias, Coahuila, where he had worked the Permian section and collected fossils with the aid of a small sum that Schuchert had sent him. He was now virtually without funds, as he had used all the savings that he had put away during his two years in Colombia. During the winter, the G.S.A. had received a magnificent bequest from the estate of R. A. F. Penrose, and was preparing to use some of it for research grants. At the suggestion of Foster Hewett and with my help, Robert had applied for and obtained from the Society a grant to continue and expand his Mexican geological work.

Robert was good company, and Helen was entranced with this adventurous brother. He spent some time visiting around the Survey, and also went to the Capitol and heard some of the proceedings in the Senate.

Helen's two weeks vacation was coming up in early June, so the three of us drove west for a week with the family in Iowa City. I believe this was the first time that Helen met father, mother, and Edward. After our week there, Robert went back to Mexico, and Helen and I started for home.

On a Sunday afternoon, at the east edge of Illinois, the car suddenly ceased to run and we were stuck. I tried all the simple remedies to no avail, and it was doubtful that most repair garages would be open that day. A young chap coming by told us that his brother had a little shop off the highway ahead, and could work on the car, so we got the car there somehow, and he did. He spent the afternoon on it. A fly-wheel on the Model A, one of its most vulnerable parts as it was made of some kind of plastic, had given up the ghost. By the late afternoon he had replaced it and we were ready to start again. But places to stay did not appeal to us, and I decided to drive all night. We went through Indiana and Ohio in darkness. Helen slept through most of it, and next morning when the sun was coming up and the birds were starting to sing, I drove off on a side road and kissed her, an idyllic moment. Late that afternoon we arrived at Youghogheny Lodge in West Virginia, and I collapsed for a well-earned rest. We took home with us a gallon of apple brandy made in Garrett County, Maryland's westernmost, and a hive of moonshiners.

Our last week of vacation was spent at Uncle Charlie's cabin on Chesapeake Bay. We were virtually alone and spent our time walking on the beach, and listening to the waves at night from the sleeping porch above the water.

The time was approaching for the International Geological Congress in August. The Survey had hoped for a subsidy from Congress to help finance the meeting, but this was not forthcoming. The G. S. A., with its new source of funds, came to the rescue. It was arranged to hold the meetings in the United States Chamber of Commerce Building on Lafayette Square. Foreign delegates began to arrive, and the entire Survey staff was busy in arranging for them, and for the meeting. I attended the opening session, but afterwards was too busy preparing for my part in the Transcontinental Excursion to hear the rest, and I left early to go to El Paso to get ready for it. Helen and I did go to a garden party at the rose garden of Whitman Cross in Chevy Chase.

I was given a generous amount of money to carry out my part of the excursion, and went west by the Santa Fe to Albuquerque and down to El Paso. It was good to see the west again after an absence of two years; even the flat plains of western Kansas looked good. In El Paso I looked up Barry and talked over the El Paso part of the excursion, and visited the Greyhound bus people who were to provide transportation. Edgar Kraus, with the Atlantic Oil Company in Carlsbad, had volunteered to help, so we drove over the whole route of the trip, trying to visualize the possibilities of reaching the off-the-road localities by bus.

We went first to the Guadalupe Mountains, which Edgar knew better than I did, and firmed up our arrangements at White's City, where we were to spend the night after our trip to Carlsbad Caverns. We also visited Wallace Pratt and his wife at their lodge in McKittrick Canyon; the Pratt's were going to be hosts at a breakfast at the lodge after our visit to the Caverns. I was surprised and delighted to learn that the Guadalupe Peak Quadrangle was in the process of topographic survey by the Topographic Branch. I had put in an urgent request for such a survey a year or two before, but had been told in Washington that most topographic funds were allocated for other purposes, and that the possibilities for it were remote. This opened up the possibility for a geologic study of the mountains, and I resolved to get started on such a job as soon as circumstances would permit.

Edgar and I also went over the ground of the excursion in the Marathon country, and then went to Van Horn to await the arrival of the excursionists.

The Pullman car containing the excursionists was put off on a siding at Van Horn at night, and in the morning they emerged sleepy-eyed -- a strange spectacle in Van Horn with their foreign faces and foreign field clothes. They caused something of a sensation among the natives. Sad to relate, I kept no record of the attendees, and wished many times in later years that I had done so. There were two Russians, a Japanese, a Portuguese, a Britisher, three Frenchmen (including Paul Fallot), and a German (Born). Americans included Hodge from Oregon, Jbhanssen the petrographer from Chicago, Ver Wiebe from Wichita, and two girls from New England colleges (I have wished many times since that I had their names, and have made inquiries of various New England

professional ladies, but to no avail). Several local geologists from Midland joined the trip, including E. Russell Lloyd. Times were hard, and the local firms were glad for our business; the passenger agent for the Texas and Pacific Railway came along, just to make sure that everything worked out all right for us.

The Greyhound bus was waiting for us as per schedule, and Edgar and I were appalled by its size and unwieldiness, and wondered whether it would get us into the off-the-road localities we had picked. It did, though, for the driver was very skillful. He told us that he had driven a pipe-line truck in Wyoming, and the company evidently picked him because of his previous experience.

After breakfast, we set off. We went east along the main highway, past Lower Cretaceous mesas, then south through the volcanics of the Davis Mountains to Alpine, where we had lunch at the Holland Hotel. Then we headed for the Marathon Region. We looked at localities that afternoon, returned to Alpine for the night, saw more localities next day, and after dinner in Alpine that evening drove back to Van Horn. I took them to one of the northern outcrops of the Haymond boulder-bed, just south of Highway 90, to the Ordovician and Devonian at the Picnic Grounds and Alsate Creek southwest of Marathon, and to a broad expanse of Word limestone northeast of the Hess Ranch, where innumerable Permian fossils were silicified on the surface. Other places are mentioned in the guidebook, but I don't remember them.

Johanssen, the petrographer, had been put on our trip because there was no room for him on the other half of the excursion, which went into southwestern New Mexico. He felt out of things, because our trip emphasized stratigraphy. After dinner in Alpine, he begged me to stop at Paisano Pass, from which Osann, years before, had named paisanite, an alkaline aplite dike rock. We did so, but with inconclusive results.

After a night in the Pullman at Van Horn, the excursionists went north along the east-facing escarpment of the Sierra Diablo to the Guadalupe Mountains, stopping to see the geology from distant views along the way. We spent the afternoon going through Carlsbad Caverns, which impressed everyone; there were cries of "Magnifique!" We spent the night at White's City below the caverns. To my surprise, the Russians and the Japanese decided to share a cabin; their two countries were then at sword's points in Manchuria. Next morning we were ready to start, but there was no bus driver; he was worn out and had overslept, poor fellow. I had to rouse him.

Returning the way we had come, we reached the gate at Pratt's McKittrick Canyon property, where Pratt had provided a fleet of private automobiles to take us to their lodge in the canyon. We had a pleasant breakfast on tables in the open under the oaks, bothered somewhat by the many flies. I had some interesting talks with Russell Lloyd about geology and field work in the Guadalupe Mountains, and he told me about Frijole Ranch, where he had stayed when he was doing his field work, and which he recommended.

We then went into El Paso across the Diablo Plateau, and spent the afternoon looking at the east face of the Franklin Mountains in McKillegon Canyon. We used a city bus on this trip, which had greater difficulty in making the grades than the Greyhound bus. We looked at an outcrop near the road where the Bliss Sandstone at the base of the Paleozoic had a clearly unconformable contact with the Precambrian red granite. Hodge, the Oregon man, was always a skeptic, and hunted around until he found a place where he thought that the granite intruded the sandstone.

In the evening, there was a dinner in Juarez for the two halves of the Transcontinental Excursion, ours and the New Mexico contingent. The Barry's were there, and Fergie and Mrs. Ferguson. It was a pleasant and jovial evening. At some point, the wilder of the three Frenchmen came to me and asked, "There are other places?" He was evidently bored. I knew that there was a whorehouse directly across the street, and many "other places" farther on, but I gave him no help. They told me that later during the transcontinental trip he showed up one morning with a black eye, gotten in some other scrape.

My part in the excursion was over, and I was glad to say my good-byes. I spent a grateful night in the Hilton Hotel, but was too keyed up to sleep well. Since I had been able to get out to West Texas at Congress expense, Mansfield arranged for a small Survey sum to allow me to do some field work out of Van Horn before I went home. Sam Lasky had been using the faithful pickup out of Socorro, and he brought it down for me to use.

I drove down to Van Horn and stayed for a week with the Duncan's. I especially wished to see some more of the Precambrian, which had greatly intrigued me two summers before. The outcrops in the eastern part of the Precambrian area, near the old Millican Ranch, were bold and plain, and I thought I could work out something quickly. The out-

crops were there, sure enough, and the beds could be mapped, but their structure was quite baffling, and detailed study would have to be put off until another day. I did, however, find some outlying knobs of metarhyolite resting on the limestone, which seemed clearly to be klippen of the overthrust mass of metarhyolite farther south. I also climbed Tumbledown Mountain on the west side of Beach Mountain, a synclinal mass of the Precambrian limestone, and found to my amazement that the Bliss Sandstone at the base of the Paleozoic had overlapped directly onto the limestone, whereas to the south and north a thick body of the red Van Horn Sandstone underlay the Bliss. <sup>stepped</sup>

With these somewhat inconclusive results, my time in the field was up, and I drove the pickup back to Socorro, New Mexico, to turn it back to Sam Lasky. Sam was working on the mining camps of southwestern New Mexico for the Survey, with headquarters at the College of Mines in Socorro. I spent a day with him there, as he was eager to talk to another Survey geologist, and I met some of the staff of the college. In the evening, I visited Sam and his wife Jackie at their home, and next day took a bus to Albuquerque, and from there a Santa Fe train to the east, and to home.

Helen met me at the station. It was the first time we had been separated since our marriage, and she had been very lonely. We had a happy reunion in our apartment.

Back at the desk, I began to think about field work in the Guadalupe Mountains on the new topographic map, but the prospect of getting funds for it from the Survey were remote. Survey funds were at a very low ebb, and there was even a grim prospect that, on top of our pay cuts, we would also have to take extended periods without pay, just to keep going. I conceived the idea of asking the G. S. A. for a research grant to do the work, which seemed quite legal. Other geologists on the Survey thought of this too, notably Jim Gilluly to work the Ajo district in Arizona. I thought that, even without pay, Helen and I could live in the field just on the G. S. A. expense money. During the fall, many of our research applications were granted, including mine.

In December, most of us were ordered by the Survey authorities to go for two weeks without pay. The Section Chiefs and the Chief Geologist had already taken their payless two weeks during the summer and fall. This was a bit dirty, because by December they were pretty

sure that more money would be coming in, from an allotment from the Public Works Administration, but so it was.

The Public Works money was allocated in January, and plans were made to put all of us on Public Works projects. These were not enticing, as all of them were on strictly short-term economic investigations, and there was widespread discouragement, even though our pay was now assured.

Mansfield had worked out a far-reaching investigation on the clay deposits of the southern states, especially on "bleaching clays" (fuller's earth and bentonite) in the Coastal Plain deposits, and I was assigned to head up a party in the Texas Coastal Plain.

Things worked out better than expected. (In fact, most of the people on the Public Works projects, after the first jolt, said that they had had a good time). The clay project would not start until March, so I could go first to the Guadalupe Mountains, and make a start on the project, then take it up again when the clay work was finished during the summer.

We therefore decided to leave as soon as we could for Texas. Helen had never been west before, so was much excited, and bought many articles of clothing for life in the field. We gave up our apartment at Mammoth Oaks, put our belongings into storage, and Helen resigned her job at the Rust Company. We packed everything into the Model A and started off.

We spent the first night in Johnson City, Tennessee, the next in Jackson, Tennessee, another night somewhere in Arkansas, and arrived in Austin. Robert had come up after his long field season on G. S. A. money, and was staying there for awhile, before going back to do more field work. We visited around, and went to a party at the Adkins' where there was much good food and drink. Then, with Robert, we set off for West Texas.

Once out in the dry country beyond Del Rio, we gave Helen a taste of the new country, and lit a little greasewood fire. Coming into the Marathon country, I showed Robert the Haymond boulder-bed, and we stopped at the Picnic Grounds southwest of Marathon, where Helen was moved to write a poem. In the hotel at Alpine that evening, Robert got a phone call from B. Coleman Renick of the Magnolia Oil Company. Would he come back for an interview? A prospect of a job at last! Renick had

met him at one of the Adkins parties in Austin, and was impressed with him -- especially as he had kept on doing geology even when out of work. Robert decided to change all his plans and go back, so Helen and I were left to go our way alone.

Next day we set off again. This was the "dustbowl" period, and the sky was murky from dust blown in from the Great Plains. Going north from Van Horn, I couldn't show Helen much of the country until, from only a short distance, the great cliffs of El Capitan and Guadalupe Peak loomed up out of the murk. We ascended Guadalupe Pass, and turned in at Frijole, a little east of Pine Spring, which was to be our home off and on for nearly a year.

I had thought we would be expected, but we weren't. I had written the proprietors, the Smith family, from Austin the week before, telling them we were coming and asking for accommodations, but the letter had not been received. Frijole Post Office was at the end of a star-route mail line and received mail only three days a week; my letter arrived several days after we did. I supposed that this would be an inconvenience, but the Smith family were very self-sufficient. Mrs. Smith canned large quantities of vegetables and meats that they had raised on their own place; when unexpected guests arrived all she had to do was open some more cans. The guest cabins were empty and ready for occupancy.

Mr. Smith was a large, genial, oldish Scotchman. We talked about our plans with him in their living room. While we were talking, the pet deer of the place came and looked in through the window; Helen was entranced.

Frijole was about a mile east of the Glover's Pine Spring Camp (mentioned previously) and about half a mile off the main road, set in a grove of oaks, and with a large spring bubbling up in the front yard, which irrigated a garden. Both Frijole and Pine Spring are at an altitude of about 5,000 feet, and stand on a plateau that stretches south along the crest of the Delaware Mountains. This plateau is about 2,000 feet higher than the floor of the Salt Basin to the west, but on its north side the great limestone Reef Escarpment rises 3,000 feet higher to the crest of the Guadalupe Mountains at 8,000 feet. Springs issue from the foot of the escarpment, of which Pine Spring and the spring at Frijole are two.

The Smith family owned the section of land on which the spring was located, and grazed sheep in the surrounding area. As I have said, they were very self-sufficient, and by hard work raised nearly everything they consumed. Their only cash income (aside from renting their cabins to visitors) was a government stipend for running the Post Office. The ranch house and the cabins were lit by acetylene lights; water was piped from the spring to a storage tank, lifted by a hydraulic ram.

Helen and I settled down in our cabin and had our meals with the family. I began to make plans for field work. At first, there was plenty to do just walking out from the ranch, observing the sandstone and limestone along the foot of the Reef Escarpment, and climbing the escarpment itself back of the ranch. The first morning, Helen walked east with me along one of the arroyos cut in the sandstone. We took each other's pictures, and then I went about my walking and observing, while Helen set off to explore the country about the ranch on her own.

After a time, field work was farther away. Helen and I drove my car into El Paso, where I stored it for the year at Longwell's Garage, then we took the bus to Socorro to retrieve the Survey pickup from Sam Lasky, to use in the field work in the Guadalupe Mountains and later at San Antonio.

During our nearly two months at Frijole, I explored along the Reef Escarpment from McKittrick Canyon southwest to Guadalupe Peak and El Capitan. Much had been written during the preceding five years about the relation of the Capitan Limestone to the Delaware Mountain Sandstone, and it had been well established that the Capitan was a great barrier reef that graded abruptly southeastward into the upper part of the Delaware Mountain Sandstone. With the new topographic base, it was my objective to work out precisely how this gradation took place, and its geometry. I climbed the escarpment in many places, and walked far up Pine Spring Canyon into the heart of the reef mass. One day, I climbed Guadalupe Peak at the southwestern end, the highest point in Texas (8,752 feet). It was a beautifully clear day and I could see for miles across the country, but in the afternoon a great wall of dust came in from the northeast, from the dust-bowl country, and I came home in the murk.

Helen drank up her new and strange environment, and wrote ecstatic letters home, some of which we still have. One evening, when I came home at dusk, Mrs. Smith told me that Helen was missing. I went in search of her, and found her stumbling back toward the ranch in the darkness. She had gone to one of the canyons coming out of the Reef Escarpment, and had spent the afternoon communing with nature under the live-oaks. With all the shade, she had not realized that the sun was setting, and was surprised when darkness came on.

Our time was up at Frijole, for the time being, so we loaded our belongings into the pickup and set out for San Antonio for the start of the new project. I had a distantly related uncle and his wife in San Antonio who were running a tourist court in the northwest part of the city, Angelus Courts. Robert's new job had brought him to the Magnolia office in San Antonio, and was staying there, and we decided to do so also, for the length of the project. It was a spacious place, with large apartments equipped with kitchens, open on all sides to landscaped lawns.

I was to go around for a few days with Lloyd W. Stephenson, chief of the Coastal Plain Section, to be indoctrinated in the local geology, chiefly in the Taylor Marl and Navarro Formation of the Upper Cretaceous. I had not seen much of these formations during my time in Austin, where I chiefly saw formations lower down in the Cretaceous. The results were discouraging. The Taylor and Navarro are soft formations, worn down to a plain, which is heavily blanketed by the Pleistocene Uvalde Gravels, made up of chert washed in from the older Cretaceous rocks behind the Balcones Escarpment. Outcrops of the bed-rock occurred only in occasional stream banks or roadcuts, and to my eye all looked alike. Stephenson would dig around in a bank until he found a fossil, or a zone of phosphate nodules which he said indicated an unconformity.

Our first objective was to explore some old bleaching clay pits about 8 miles west of San Antonio, where the clay occurred near the top of the Taylor, just under the base of the Navarro. It was planned that the extent of this clay would be explored by means of hand auger holes put down by a crew of workmen, thus contributing to the alleviation of unemployment.

With the help of Sellards of the Bureau of Economic Geology, I set about recruiting a staff. The first applicant was Henry Coleman Fountain, a self-taught geologist and fossil collector, who had been a telegrapher for the Southern Pacific until he was let off. I hired him as second in command, and then took on two younger men. Contrary to usual Survey policy, political recommendations were required. One of the men was the son of a clay operator in San Antonio (he said he had been out of work for a long time, but seemed prosperous enough); the other chap had had a little training in geology.

Having assembled the crew, we set to work around the clay pit. We found that with the hand auger we could put down a hole to a depth of about 30 feet. Below that, lowering and raising the pipe from the hole became too laborious. Besides, the hand auger could not penetrate the Uvalde Gravel that covered the surface in most areas. The samples of clay that were sent to Washington proved to be rather low in bleaching power, probably the reason why the pits had been abandoned. Moreover, we found that the clay bed did not extend more than a mile in any direction.

At the suggestion of the clay operator's son, we decided to shift to a drop-auger, and had one built in a blacksmith shop. From a tripod and a pulley, a rope suspended a heavy iron cup, which was dropped in the hole to bring up samples. Fountain and the boys put down a few holes with the drop-auger to depths as great as 80 feet, something of a record for hand drilling.

While Stephenson was still with us, he received a letter from H. B. Parks of the Texas Apicultural Experiment Station outside of San Antonio. He couldn't look up Parks himself, and suggested that I do so. I found that the experiment station was a branch of Texas A. & M. College, and was engaged in research in bee culture, which was a big thing for producing honey in this part of the Coastal Plain. Mr. Parks proved to be an older man, badly crippled with arthritis, who was an enthusiast in all sorts of local lore and geology. I found that he was eager to get around the country but his family wouldn't take him, so we formed a compact to go around together.

With the unpromising results west of San Antonio, I decided to let the work crew continue their operations and to range more widely myself. The crew used the pickup, and I was given another Survey Model A, a coupe, for my part of the work. I had heard about other bleaching clay workings farther southeast in the Coastal Plain in the Tertiary formations, and with Parks for company I made trips to these. He proved to be a useful companion who knew lots of the local lore and history, which helped to bring to life the otherwise dull Coastal Plain. He knew many bee-keepers, and when we asked for information from natives and in country stores, he would talk about bees first, then lead the way tactfully to what they knew about clay deposits.

There was not enough for my crew to do around San Antonio on bleaching clays, so I decided to put them to work exploring for ceramic clays. The San Antonio Chamber of Commerce a few years before had hired a clay expert to look into the ceramic clay possibilities of the area, and his rather glowing report had been issued as a pamphlet. The Bureau of Industrial Chemistry, of the University, a sister of the Bureau of Economic Geology, was also interested. I enlisted their help, and an agreement was worked out by which they would study our samples. With this, I set Fountain and his men to drilling various tracts around San Antonio for ceramic clays.

Dull though the Survey work might be, San Antonio was a gay place, and Helen and I had a good time there outside of working hours. There were many good movies to go to (we saw a Tarzan movie and "Three little pigs", among others). Somehow we met some young people with whom we went places. Thanks to Roosevelt's prompting, the hated Prohibition Amendment had been repealed. Texas had not yet passed any new liquor laws, but San Antonio was wide open anyway, with bars and drinking places right on the main streets. The proprietors all made sure that they had Federal licenses. The days were fearfully hot and sticky, especially in the clay pits where the light was blinding, and the country swarmed with chiggers, so that all of us were covered with bites. But in the evenings, a cool breeze sprang up from the southeast, which made the end of the day pleasant. I thought that the breeze came from the Gulf of Mexico, until on some of my trips nearer to the Gulf I found that there was no such breeze in the evening.

About the middle of our stay in San Antonio we had a pleasant weekend break, and went to Monterrey, across the border in Mexico. Robert had been sent by his company to look at some property in the mountains back of Tampico, and we arranged to meet him in Monterrey on his return. We went down by train. After crossing the Rio Grande the appearance of the country changed. The eastern ranges of the Sierra Madre are rather close just south of the border, so we saw the Mexican mountains around us, contrasting with the featureless country around San Antonio. And the buildings and the people were different, too. We reached Monterrey in the evening amidst great hubub and confusion in the station, from which we were rescued by the timely appearance of Robert, who took us to the grand and old-fashioned hotel. In the evening we went with him to a succession of night spots, some with tables set out in the open under the trees. In the morning we toured the markets, and Helen bought many things to bring home. We returned to San Antonio much refreshed by our foreign travel.

I received a letter from Drury Phillips, an engineer with Texaco, who was operating a bleaching clay pit for his company near Huntsville, not far from Houston, and at his invitation went down to see it. He proved to be the son of W. B. Phillips, who had been the director of the Texas Mineral Survey, the precursor of the Bureau of Economic Geology, and a very knowledgeable guy. We looked at the Texaco pit, and one nearby operated by the Continental Oil Company, both in the Miocene Catahoula Formation. Their products were being shipped to refineries near Houston, for purifying petroleum oil. I sent samples of the clays to Washington for testing, and they reported that their bleaching powers were rather low. Low bleaching power was compensated by the small expense of shipping the bulky product to the refineries, which were not far away.

With Parks as a guide, I ranged over the country southeast of San Antonio, and saw much of the non-marine Wilcox Formation which contained ceramic clay, and of the overlying barren Carrizo Sand. We went to many clay workings, mostly now inactive, including the pits of the San Antonio Sewer Pipe Company (Saspamco). In one of their pits the producing clays were overlain by the thin upper marine unit of the Wilcox. Parks also took me to the old pits just out of San Antonio where clay had been dug to make the bricks for the Spanish missions in San Antonio.

I was much impressed with Fountain's ability and enthusiasm, and I thought his knack of collecting fossils would be a great help in the Guadalupe work. I had collected a few fossils when I was there in January and February, but not as many as the area seemed to promise from previous reports. Fossils were not as evident as they had been in the Glass Mountains, where they were silicified on the surface, or weathered free. To get fossils in the Guadalupe Mountains would require much breaking of rock, and this I was loath to do, because it would interrupt my field work. I proposed to Fountain that he go back with me to the Guadalupe when the clay project was over, and he accepted eagerly. I couldn't pay him much; all I could spare from my G. S. A. funds was \$50.00 per month. I put in a request to the G. S. A. for a supplemental grant to pay him \$100.00 a month, and this was approved along in the fall. It was a 100 percent raise, but still a pittance.

A report on the clay project was supposed to be written immediately after the close of the work, but rather than remaining in San Antonio to do this, I received permission to go back to Frijole and do it there during August.

Before leaving San Antonio, though, there were still some things <sup>to be done</sup> ~~to do~~ in July. The Survey had received an inquiry from Senator Morris Shepard about clays in Morris County, northeast Texas, and they took this as an order, and I was told to go there. With Helen, I made the long trip. Ed Eckel, with his wife Chuck, was on another Public Works project in this area, investigating brown iron ores, so we arranged to visit them, and Ed said he would go around with me. Morris is a rather small county, and the Wilcox Formation extends across its northern half, so this seemed the logical part to look at. Ed and I drove around over this half of the county, looking at road cuts, with rather inconclusive results.

Another chore to be done was to turn the Survey's Model A sedan over to another field party under Wallace Lee, which was studying the Pennsylvanian in north-central Texas. I was asked to take the sedan to them in Mineral Wells, but I protested because this would be far out of my way. As a compromise, it was agreed that one of their men would meet me in Fort Worth. We went from northeast Texas to Fort Worth, and found our man at a hotel -- Ivan Fenn, a rather oafish fellow who was a University of Iowa graduate. After giving him the car, Helen and I took the night Pullman back to San Antonio.

Late in July, Helen and I left San Antonio and drove back to Frijole in the Survey pickup. Fountain and his wife Eudel had already arrived, and had found living quarters with the Glover's at Pine Spring Camp. He had begun his search for fossils, and had already collected shells from the limestone ridge back of the camp. I settled down to write the clay report, in the cabin or under the trees, in the pleasant mountain air, contrasting with the hot mid-summer in San Antonio. I finished the report by the end of August and having sent it off was ready for field work. While doing the report work I gave Fountain general ideas as to where to collect.

When my field work started again in September, one of the things I wanted to do was to investigate the western escarpment of the mountains, which I had not looked at during our previous visit, where the structure of the mountains was laid bare in a great face 3,000 feet or more high. Many fascinating things could be seen there in the strata beneath the Capitan Limestone. The Bone Spring Limestone rose northward, and changed from thin-bedded black limestone to gray limestone north of Bone Canyon. The sandstones of the Delaware Mountain Formation overlapped its rising surface. The coarse sandstone ledges of the lower part of the Delaware Mountain each abutted against the rising surface of the Bone Spring and disappeared one by one, until nothing was left. At the same time, the middle part of the Delaware Mountain changed northward into limestone nearly as massive as the Capitan, and formed an older reef mass, set a few miles behind the Capitan mass, which rose into cliffs to the north which supplanted the Capitan cliffs -- a feature only dimly perceived hitherto, when it was commonly assumed that all the limestone cliffs at the top of the mountains were Capitan.

I set about mapping all these complex relations in detail. I measured many stratigraphic sections up the face of the escarpment, and I walked out all the lower sandstone beds northward to their point of overlap against the rising surface of the Bone Spring. Fountain went with me every day, and made large collections of fossils.

Another area that I had not visited was the high mountain crest, which formed a deeply dissected forested plateau at an altitude of nearly 8,000 feet, cut in the Capitan Limestone and the backreef Carlsbad Limestone. Most of the upland was owned by the Grisham-Hunter Corporation, who had built a cabin in the heart of the area. I re-

ceived permission to use the cabin as a base, and I hired Willis, the son of the Smith family, to bring in supplies by pack horse. Fountain and I walked in together by trail, and stayed in the cabin for two extended periods. The second trip in, Helen went with us. I ranged over the whole area on foot, and would come back to the cabin in the evening footsore and weary. Fountain collected fossils and made many fine collections from the limestones. One evening I came in and sank down on the bed too tired to talk. Fountain insisted on saying something, but to no avail. Finally he came over and handed me his prize specimen -- a large, beautifully preserved gastropod, with its color markings still intact! I was very sorry for my rudeness.

Sometime in September, we decided on an expedition to Guadalupe Peak -- Fountain and I, Helen, and Mrs. Fountain. I had found the best way to the top the preceding January. One started in a valley west of Pine Spring Camp, climbed the limestone slope to a plateau at 8,000 feet; then across a col and another steep climb to the top. Mrs. Fountain was not an outdoor girl and gave out when we reached the plateau. Helen went on with Fountain and me to the top, where I photographed them. We did not have long to admire the view. Unlike the calm day of my climb in January, this one was threatening, and thunder clouds were gathering over the peak, a focal point. The air became vibrant with electricity and we left there fast. Lightning always terrifies me, and I must confess a streak of cowardice that day. Helen's gait was painfully slow, and Mrs. Fountain's when we picked her up was worse. We decided to go down Guadalupe Canyon, west of our ascent, a steeper but more direct route. When the party was part way down I went ahead and walked to where we had left the car, brought it around and met them on the highway. It was a long and memorable day.

When my pay checks arrived twice a month, Helen and I would get up early in the morning and drive into El Paso for the day. After cashing my check at a bank we would set about spending the money, and sometimes there was little left when we started for home. Usually, we would go over to Harry Mitchell's Cafe in Juarez for lunch, and on cold days have hot tom-and-jerrys first. During September I would bring back a 100-pound block of ice for the Smith's, well wrapped in blankets, and for the next few days we all celebrated with ice tea and ice cream. This was before the time of gas refrigerators (they came along a few years later), so isolated ranches like Frijole were without ice or refrigeration.

I had arranged for Dr. Girty to visit us for a week, to see his old stamping grounds and the new things we had been discovering, and he came out during October. Fountain took him to the places he had been collecting, and the two collected fossils together. Girty was much impressed with Fountain's ability, and wanted very much to add him to the Survey staff, but there was no way to do so at the time.

While Girty was still with us, the West Texas Geological Society decided to hold a field excursion in the area, and several dozen of them descended upon us, headquartering at Pine Spring Camp. I showed the group what we had been finding. On the second day we went around to Bone Canyon and the Williams Ranch. Helen went with us that day, and we formed friendships with some of the Midland people. Photographs show that several other women were on the trip, but I don't remember who they were. As we were leaving, we could see two tiny figures gesticulating at the top of the outer cliff above Bone Spring; they were Ronald DeFord and Russell Lloyd, arguing about the relation of the beds.

Another visit from outsiders was with an archeological group from Carlsbad, who came out to excavate a cave in the sandstone low down on the escarpment south of El Capitan. The guano in the cave contained various mammalian bones, some of species not now living in the area. The visitors included Bill Burnet, a tinsmith in Carlsbad and an amateur archeologist; he was something of a character, as we found out later.

In November, I had a personal disaster. I was measuring a section on the steep slope above Bone Spring and kicked sideways at a bush that was in the way. My kneecap slipped loose and my knee went out of joint. Next thing I knew, I was rolling down the slope and only caught myself by clinging to the bushes. Somehow, I made my way down to Bone Spring, and then with the aid of a sotol stalk for a shaft, down Bone Canyon to the car at the mouth. Fortunately, Fountain was waiting for me to come in and drove me back to Frijole. By the time we got there my knee had swelled up enormously, and I could hardly move.

I stayed indoors for several days and Helen put hot packs on my knee. I thought my days of field work were over, but amazingly in three or four days I was on the mend and could get around fairly well. We went into El Paso to see a doctor but he couldn't recommend much; he suggested that I wear an elastic "ace" bandage to hold the knee in place.

This seemed like a good time for a Thanksgiving vacation, so we went on west to the Chiricahua Mountains, Arizona, where Dorothy Gay and her husband Bill Conrad were running a dude ranch not far from the motel where Brookes and I had stayed in 1930. We spent a few days with the Conrad's and their guests (mostly unpleasant rich people) and had a relaxing time. One day, while Dorothy was in town, Helen, Conrad, and I went to a little bar that had been opened in the canyon and had a drinking session. They put on an act; he was an opera diva who was trying to make a comeback and Helen was "her" impresario. Dorothy was disgusted at our condition when she came back in the evening.

Returning to Frijole, I found that my knee was strong enough to resume field work, and Fountain and I set to work on the northwest part of the area, along the escarpment from Bone Canyon to the New Mexico line. It was difficult country to get into, and it took a long time to drive there from Frijole and Pine Spring. Fountain and I took to getting up in the dark at 5:00 in the morning, and seeing the sun come up when we were well along on the west side of the mountains. When we got as far as we could drive to the escarpment, it was still a walk of an hour or more across the sand hills and up the steep alluvial fans to the foot of the mountains where our day's work was to begin.

We had one such long day on the day before Christmas -- an early start and coming down off the mountain at dusk. We then had to walk down the slope to the fence at which we had left the car, but by now in the darkness we couldn't find it; every bush looked like our lost automobile. I knew that the moon would rise about 10:30, but we didn't want to wait that long. After an hour's search, Fountain halloed from the car, and we were able to start for home. Meanwhile, back at the ranch, Helen was in despair -- especially since the Smith's had invited her in to their Christmas party around the tree. Her despair only vanished when I finally came in, late in the evening.

Throughout the work, I kept careful field notes. I jotted them down on pieces of paper during the day, and every evening transferred them by typewriter to the field notebooks, illustrated by sketches and diagrams. Fountain and I also formed the habit of going over each of our fossil collections and making sight identifications, by comparing the shells with pictures in the publications. The paleontologists didn't always agree with our identifications later, but it gave us a good idea of what we had been obtaining.

I had made an arrangement with Dr. Girty to send the fusulinids we collected to Carl Dunbar and the ammonoids to Art Miller for their study and identification. Girty was not interested in either group, but the experts could do a great deal with them that would clarify the stratigraphy. We therefore carefully saved out the fusulinids and ammonoids to send to them. When the Survey authorities found out about this arrangement later they were greatly incensed, and said that my informal arrangement with Girty was illegal. It paid off, though. Dunbar's identifications appeared in the Dunbar and Skinner report published in 1937, and Miller's in the Miller and Furnish report published in 1940 -- years before the other fossil identifications became available.

Our work was about over. Early in January we had a visit from Sherry Thompson, Renick's colleague with the Magnolia. He tried hard to lure me into working for the Magnolia, but I had too many challenges ahead on the Survey and was not interested. He also offered Fountain employment, which he was happy to accept, so I had the satisfaction of seeing him go on to a paying job when he had finished with me. (He stayed with oil company work for the rest of his life, not doing what he was most talented to do and I last heard from him by telephone in the late forties. I learned that he and his wife had separated and that he had become an alcoholic; that he was killed in a drunken condition in a highway accident).

After Fountain left, I worked through January (1935), mapping the southeast part of the area in the Delaware Mountains, tracing out the hard beds. By then we, too, were ready to quit. The winter was cold, and all we had to heat our cabin was an oil stove, which smoked sometimes. It was especially hard on Helen, who had great difficulty in keeping warm, and in taking a bath.

At the insistence of our Carlsbad friends, we spent several of our days in town there, instead of in El Paso, one time staying with Edgar and Gladys Kraus, another at the La Caverna Hotel. On one of our visits we made a trip to Last Chance Canyon in the northern Guadalupe Mountains with Bill Burnet and a local geologist, George Kroenlein. I guess Burnet was going to show us some archeological caves. It soon transpired that he was an alcoholic; his wife wouldn't let him drink at home, so these trips to the country were an opportunity for a toot. He carried a half-pint bottle of whiskey from which he took a drink now and then, and invited the rest of us to share. I thought that when the half-pint had been exhausted this would be over, but then I discovered that he had brought along a whole box of half-pints! By afternoon he was incapacitated, and it was difficult to bring him out and take him home. We returned to Carlsbad late. Gladys Kraus was expecting us to dinner and was sore; she said she had brought in another couple to eat her dinner and there was none for us. We could make no explanation without ratting on their friend, so said nothing.

Toward the end of our stay in Frijole I received a letter from Mansfield informing me that I had been raised in grade from Assistant to Associate Geologist, at a salary of \$3200.00 per year -- a \$600 jump in pay (our pay cuts imposed by Roosevelt had been in the meantime repealed). This seemed like unexpected riches indeed!

We took our own car out of storage in El Paso and brought it back to Frijole. The Survey told us that the faithful pickup had outlived its usefulness, and should be auctioned off and sold. I announced the auction to the few local people who lived in the area, and they assembled on the appointed day. The first bids were disappointing -- only \$10.00 or so -- and I refused to accept them. Finally the bids became more lively and the Smith family finally bought the car for about \$90.00.

We loaded our belongings in our car for the long trip east. The morning we left it had snowed (the first we had had that winter), and the plateau country to the south of us was white. We went first to Midland for a few days, where some nice young friends whom we had met on the field trip invited us to stay with them. Then we went to Coleman and spent a night with Monroe and Margaret Cheney. Then to San Antonio for a few days and to Austin for a few days more.

Then we headed north for Iowa City, me driving and Helen passing the time crocheting. One night, after a long day, I spied a hotel on the edge of the Lake of the Ozarks in Missouri, where we had a pleasant night. We got to Iowa City in the cold, and were glad to settle down for a few days with the family. Continuing east, we went through Illinois and Indiana. Night had come when we reached Cincinnati and I was almost too tired to look for a place to stay. After going through the city, I spied the Palms Hotel, an apartment complex in the residential area, and we were delighted to be hospitably received and to get a comfortable room for the night.

We had a leisurely breakfast next morning and were on our way again, our next objective being Youghiogheny Lodge in West Virginia. We had learned that Frank Reeves had leased the place to some other people, so I had written them that we were coming. Our day was a long one, and it started to snow. Finally we arrived, long after our hosts had given us up. We found them popping corn around an enormous fire in the fireplace. They proved to be Mr. and Mrs. Zamboni, a charming pair of young neer-do-wells -- she a beautiful girl, he a scholarly guy who was trying to make a go in the rare book business (a chancy profession!). It was a wonderful change from our long days on the road, and we rested there for a few days before going on.

As usual, it was disillusioning to get back to the hard realities of Washington. Gertrude and Harry had had a fire in their apartment, so were living temporarily in the Wardman Park Hotel on upper Connecticut Avenue. They let us use their ~~apartment~~ <sup>room there</sup> for a few days, then we moved to a rooming house in a residential area, until we decided where we were going to live.

We found that all the young people on the Survey had given up their apartment routine and were buying houses. Charlie Hunt had led the way, and had bought a new home in a subdivision south of Alexandria, and was inducing some of the other young Survey couples to settle in the same area. We started looking at real estate too. One time we were shown a fine old house in Alexandria which was selling for only \$5,000.00 or so, and almost argued ourselves into buying it. But it was in terrible repair, and on cold reflection we realized it would cost at least twice that much to make it liveable. We then decided that only a new house would do.

We looked at several places. Then Harry told us that his friend Charlie Jesse, an officer in the Arlington County hierarchy, had a subdivision in the western part of the county called Golf Club Manor, in which there were some new houses for sale, so we went out on a Saturday to look at them. All one side of the subdivision, on 37th Street, had just been built up by Mr. Kurt Karlstromer, and all the places were still vacant and open for sale. The prices were \$9,850, which was a little steep for us. We went down the street and looked at all of them. One place had a high-ceilinged, wood-paneled "studio" living room, and adjoining it a dining room papered with hunting scenes with men in red coats on horseback. We went back to Jesse's house and he fixed us drinks. Under the benign influence of the alcohol we decided we had found what we wanted, and the purchase agreement was worked out on the spot, for 4617 37th Street North.

The rooming house where we were staying seemed unfriendly and desolate by comparison, so we decided to move in at once. We had our scanty belongings moved from storage to our new home. I had a touch of the grippe and it was good to lie in the new home, smell the new house, and hear the appliances going on and off.

We put our savings (Helen's and mine) into a down payment, and worked on the rest of the financial arrangements. The costs were a little beyond our means, especially since we discovered there were various hidden charges, such as a so-called "survey fee" which didn't survey the land or anything else. Somehow we worked it all out, but we felt poorer than before my magnificent raise at the beginning of the year. We had to carry a first and a second mortgage. Federal Housing loans with a single payment were just being started and I went and talked to the Federal Housing people. The man said I was very foolish to buy a house at such a price on my small pay (not realizing that my pay would increase in time, or that values would increase; ten years later we sold the house for \$15,000.00, and it is worth many times that now). Finally in the fall, we got a Federal Housing loan with the help of Kurt Karlstromer, and our monthly payment was a little under \$70.00 per month.

Back at my desk at the office, I set about organizing the results of my field work in the Guadalupe Mountains and of putting them in shape for a report, a task which would take much of my time for the next three years. I had accumulated a great mass of data on the stratigraphy, tectonics, and geomorphology which would require much sieving and generalizing, and many maps and diagrams had to be prepared. I spent more than a year on the Permian rocks and stratigraphy, the chief objective of the work. The other subjects came later.

In 1934, while we were still in Frijole, Chester Longwell wrote me that he had accepted chairmanship of the Committee on Tectonics of the National Research Council, for the purpose of making a Tectonic Map of the United States. He invited me to take part, and to work up the southwestern region, in Texas and adjacent states. I was much excited by the prospect, and during my office time in 1935 I began to assemble data for it. Many data were available, but a good deal was in oil company files, and no one had tried to put it all together in one map. (This project was to take an increasing amount of my time, until the map was finally printed in 1944).

In December we had a Christmas visit in our new home with mother and with Robert and his new wife Clara. Robert had been assigned to Midland for a while and had met Clara there. Shortly thereafter, his company decided to send him to Colombia again, and Clara had followed him there to be married. Work in Colombia wasn't so appealing this time. First he worked in the Magdalena Valley where he had several bad attacks of malaria. Then he was sent to the Barco Concession on the fringes of the Maracaibo Basin. There was no good place to keep a wife where he was sent, and Clara had to stay in Bogata or other towns. Finally, he was fed up with it all, and had gone to Houston and obtained another job, this time with Shell Oil Company, and for work in the States.

In the fall of 1933 before we left Washington for the west, we met Phil and Nancy Porter at a party; Helen had known them somewhat in high school. We found them a delightful couple, and we corresponded during our year in the west; Phil, especially, brightened our lives with amusing letters. We looked them up on our return and found that they were living in an old house in Alexandria. They had a coterie of gay friends and spent most of their evenings drinking gin. To our surprise, we found that Phil was an accomplished jazz piano player,

and delighted us by playing and singing. We also saw a good deal of Frank and Eleanor Smith; Helen had known Eleanor in high school. They were in newspaper work in Washington, he with the Times-Herald, she with the Daily News. Their only defect was that they were very self-centered, and wanted to talk entirely about themselves.

During the year, we decided it was time to have a child; we had avoided this hitherto. In December, Helen was delighted to learn from her doctor that she was pregnant, and we began to prepare for the birth in the coming September.

We decided that I should go ahead and do some field work that summer. My aunt Bertha would come over from Baltimore to look after Helen during the final and most critical time of her pregnancy.

I approached Brookes Knight about another season of field work in the Sierra Diablo region of west Texas, and he was glad to consent. His employment had been somewhat sporadic. During one year he had occupied a vacant post at Occidental College in Los Angeles, and during the next he had been in Europe on a G.S. A. grant, examining and describing the types of genera of Paleozoic gastropods in various museums. He was now at Princeton, his old school, where money had been found to give him a research post. He was eager to see some more of west Texas, and I decided we had made enough of a start during the summer of 1931 so that in another field season we could round out the Sierra Diablo area in a finished report.

I took out a Survey pickup (a Ford V-8 this time) that had been in storage in Washington, and drove it west. On the way, I stopped for a day or two in Tulsa, and gave a talk on the results of the Guadalupe work; I also exhibited for criticism what I had started to compile of the Texas region for the new Tectonic Map.

Back in Van Horn, I got our old rooms with our friends the Duncan's, and Brookes joined me shortly. He had decided to bring his family out -- Mrs. Knight (Madge) and his two daughters (Judy and Martha), and to put them up at Frijole with the Smith's. Shortly after our arrival, Mr. Mansfield came by to inspect the work, accompanied by Mrs. Mansfield. I showed Mansfield the sights of the Sierra Diablo, and we went on to look at the Guadalupe Mountains. Going to Bone Canyon and the Williams Ranch proved to be quite an undertaking. There had been some big rains and washouts since I had been there the year before, and the road was almost impassable. Uncle Dolf Williams

of the ranch didn't care, as he only went in and out on horseback, and he preferred the isolation. In the evening at Frijole, Mansfield entranced everyone, especially the children, by playing on a guitar and singing old favorites such as "There is a tavern in the town" and "The wild man of Borneo has just come to town."

Madge and the girls settled down to enjoy western life at Frijole. The girls collected bags of resurrection plants which they took back to Princeton and tortured -- drying them out, then putting them in water to watch them unfold and turn green. One day Martha reported that she had tried to collect a "desert crustacean" for daddy, really a scorpion; the ranch people asked, "Didn't you see it waving its tail at you?" She had, but its significance was not understood.

We continued our field work, covering more country, measuring more sections, and collecting more fossils. An interesting discovery that summer was a large area of Ordovician carbonates in the plateau country west of the Sierra Diablo rim, where before only Permian carbonates were supposed to be exposed. During the field season in the Guadalupe Mountains I had formed the habit of measuring joints, and found that they had a significant systematic pattern. During the new field work in the Sierra Diablo I started to extend the joint measurements there, which required climbing every hill. I climbed a hill on the north side of Cox Mountain and was amazed to see that the carbonates contained gastropods quite unlike those in the Permian, but looking like those in the Ordovician; we extended our observations and proved that the rocks in the area were indeed Ordovician!

About the middle of August Helen wrote that her time was getting short, and begging me to come home and be with her. So I cut the field season short and went back to Washington (and Arlington) by train. Helen was ashamed that she was so big; everyone told her it was going to be twins (which it wasn't). I received permission to work at home and be with her when the baby arrived.

Weeks passed by and nothing happened. We were not worried, but the arrival of the baby seemed like a never-never event. On September 18 we went out to dinner at a place just beyond the Country Club. On returning home Helen began to feel queer and asked me to call the obstetrician. While I was on the phone she called out for me to tell him that it was a false alarm, that she had just lost her dinner. He said, "Bring her in, fast!" We gathered all her belongings, shut

all the windows, and drove to Georgetown University Hospital, an old structure in downtown Washington. Helen was taken at once to the delivery room. Gertrude and Harry had come down, and they persuaded me to go home with them and get some sleep, rather than spend the night sitting in the visitor's room in the hospital. In the morning they called the hospital and were told that Helen had delivered a girl. We had talked so long about how we were going to have a boy that for a few minutes I thought there had been a mistake. Helen had decided to name the child after either Harry or Gertrude, so the baby's name became Gertrude Burke King.

I went to the hospital and found Helen weak but happy. Her first question to me was, "Is the baby perfect?" Her great fear had been that it would be a defective child. As a matter of fact, arriving late, the baby was already well-formed, unlike many newly born children. I went around and visited Eleanor Smith, who was very sympathetic. That evening Frank and Eleanor took me to the Gayety Theater, where we saw Anne Corio, the reigning queen of burlesque. After the months of tension, it was good relaxation.

Helen and the baby stayed for nearly a week in the hospital; Gertrude and Harry paid for her to have a private room. One day, at last, our neighbor Mrs. Johnson went with me to the hospital and we brought them home in her larger car. We had already purchased a crib and other baby equipment.

Then began a new and rather difficult life. Helen was still weak and well tied down with the baby. I would phone in a grocery order to Brooke and Harry, who would deliver. Helen hired a negro girl to do the housework and cook (in fact, we had a succession of them; the first one was named Carter too, and was illiterate). Our finances were more difficult than ever. I had no idea how we would pay for the baby until an insurance man told me that I had been letting the dividends accumulate on my Mutual of New York policy, and that several hundred dollars were there which was mine to draw out and use if I chose. For an obstetrician we had chosen Dr. Kane, an eminent Washington practitioner; his charges were high, but worth it. We learned that we must now use a pediatrician or children's doctor, and we went to Dr. McLendon, whom we continued to use until we left Washington years later.

The baby grew and was very healthy, although Helen worried greatly about her. In February (1937) mother came east for a visit and to see the baby. While she was there, we discovered that some of the baby's teeth were coming in. Mother arranged for a photographer to come out and take pictures of her.

During 1937 I stayed in and did not go to the field. I used the time to work on the Guadalupe report, partly at the office, and partly for extended periods at home, for which I took annual leave.

In November, word came that father had died. He had been to the bank and must have had bad news, and keeled over and died of a heart attack. He was only about 65, but felt weighed down by adversity and had aged greatly. He had the most gloomy outlook on life and was never very cheerful. None of the King's were, and were always sighing and groaning; it took great effort on my part to shake off the family incubus. The death was a great blow, and I had always thought of my parents as something fixed and stable; now I was alone in the world.

I went out by train at once. Helen stayed at home with the baby, but aunt Bertha joined me for the trip. On the train I made my last communion with my father and had many sad thoughts; we had not been very close for the last ten years, which I now regretted. At home there was quite a family reunion. Edward had come from Michigan where he had a job with Dow Chemical Company, and Robert and Clara had driven up from Texas. I am ashamed to say that the reunion became rather gay, but it helped cheer up mother. At the funeral, the preacher pronounced the usual Protestant homilies which were pretty unsatisfying, and the body was taken to Cedar Rapids to be cremated. One result of the unexpected trip was the discovery that I had used up all my annual leave, so I had to go to Iowa City on leave without pay.

The Geological Society of America met in Washington in December, and was a bright contrast to the events in November. Meetings were held in the Willard Hotel in downtown Washington. Many old friends were there and I met some new ones, including two eager young Texans, Claude Albritton and J. Fred Smith. I gave a paper on my structural results in the Guadalupe Mountains. There was also an organizational meeting of the newly formed Committee on Tectonics, which was held at a luncheon at the Cosmos Club, and we discussed plans and procedures; Walter Bucher was a leading spirit. The Pick and Hammer show was held one of the evenings of the meeting at a high school auditorium on upper Georgia Avenue. One of the acts was laid in the South Sea Islands,

replete with a bevy of beauties with bare waists, bras, and grass skirts -- all played by men. I was one of the "beauties" and looked horribly repulsive.

In April (1938) I attended the meeting of the American Association of Petroleum Geologists in New Orleans, where I gave a paper on the generalizations I was making on the west Texas Permian. This involved a long train trip each way, and going down we were delayed for hours by floods in Alabama and arrived very late in New Orleans. It was not a very happy meeting, as I had very little money, and I had a miserable hotel room. I had some drinks in the bar with my old field companion Henry Fountain, and one evening I went with some other people to dinner at Antoine's, the famous restaurant. This was all the amusement my straitened funds would allow.

We began to feel restless about our lives, and I proposed that we spend another year in the Southwest. That summer, I would spend another field season in the Sierra Diablo, and after that we would rent a house in Carlsbad for the winter, so I could work on the Permian stratigraphy where I was in touch with the local people. Helen would spend the summer in Iowa City with my mother, and join me in Carlsbad when my field work was finished. These plans were carried out after a fashion, with various mishaps that I will recount.

During the spring I finished the text of the Guadalupe report and transmitted it. It still lacked the paleontological part, which Girty had promised to prepare, and which would have to be inserted later.

We set about renting the Arlington house for the coming year, with the plan to have a real estate agent handle it for us, collect the rent and make the mortgage payments. We asked \$70.00 per month, which would just cover the mortgage. Renting proved to be less easy than we had expected. Many people looked at the place, but there were no takers. We finally rented to Mr. and Mrs. McDearmid; he worked at the local airport.

I had become unhappy with our car; the Model A was nearly 10 years old, and far out of date. We had no funds, but decided to go ahead anyway with a purchase, and borrow the money from my insurance policy. We bought a 1938 Ford convertible with white sidewalls -- the most expensive Ford on the market, costing somewhat more than \$900.00. It was delivered only a week or so before we were ready to leave. It was an elegant little car, and we were to use it for more than 10 years.

Helen and I were not especially happy with the way we left our affairs in Arlington, but such it had to be. We loaded our possessions into the new car and started west. We spent the first night in Warrenton, Virginia, and the second night in Chillicothe, Ohio. Gertrude was only about two years old and was not a good traveler. She was very restless from confinement in the car, and in the strange surroundings of the hotels at night she shrieked at the top of her lungs. At Chillicothe we could not get her to bed, and finally let her run around the room until she fell asleep on the rug.

Next day, all went well until we reached Indianapolis, where we began to realize that the traffic was unusually heavy. It dawned on us that this was Memorial Day, the day of the famous Indi-500 auto races. Beyond Indianapolis the traffic got worse rather than better, and I found with dismay that our highway went right past the stadium. I appealed to a policeman, who directed us to a side road. We were out of the traffic, but had lost hours of time. I decided to do as we had done in 1933. and drive all night. It was a long grind, but we finally got to Iowa City and to mother's house early in the morning for a welcome rest.

I spent a few days with mother, Helen, and the baby in Iowa City, but felt obliged to push on, as I was to meet people in Texas. I wish I had stayed longer, as this was to be the last I would see of mother. She died the following spring, full of care at managing the family affairs alone.

I headed south for Austin; the new car ran smoothly and pleasantly, and I spent the night at several places along the way. In Austin, I put the car in storage and took out a Survey V-8 pickup.

At the G. S. A. meeting in Washington the winter before Fred Plummer spoke grandiosly about the things he could show me of the Pennsylvanian and lower Permian of central Texas, and I decided to accept his proposal. The Chief Geologist, G. F. Loughlin, was suspicious of my activities, and detailed Jimmy Williams, the Carboniferous paleontologist, to go with me. When we assembled in Austin, we found that Plummer's promises were so much hot air, for he had no real plans. We went around with him for several days west and northwest of the Llano area, with very inconclusive results. We were relieved when he finally gave up and went home.

It happened, however, that Jo Bridge was spending the summer on the Cambrian and Ordovician of the Llano area, so we went down and spent a few days with him, working out from the hotel in Llano. Jo was good fun in the field, and we enjoyed our visit.

Jimmy and I then drove to west Texas, where I showed him the Wolfcamp section in the Glass Mountains, the section near Marble Canyon in the Sierra Diablo, and some of the sights of the Guadalupe Mountains. We ended up in El Paso, from where he was going on to join Jim Gilluly's party in southeastern Arizona. He had just heard that Gilluly was leaving the Survey to go to U. C. L. A. in the fall at the fabulous salary of more than \$10,000.00 a year, and was sick with envy.

I had decided that our short field season of the summer of 1936 was not sufficient to complete the Sierra Diablo work, and that another season was needed. I approached Brookes Knight to come with me again. He was happy to do so, but said he was a sick man; he had been plagued all his life with stomach trouble and ulcers, and now he was worse. (In spite of all this he reached a ripe old age of more than 70 in the early sixties, and finally died of a coronary). He planned to bring his son Jimmy with him, to help in fossil collecting.

After leaving Williams, I went down to Van Horn and got our old rooms with the Duncan's. Brookes and Jimmy joined me in a few days, and we were comfortably settled.

It was a long and eventful summer, and we had many visitors. Earl Ingerson spent a few days with me, looking at the metarhyolite along the big Precambrian thrust west of Van Horn, and we made and recorded innumerable measurements of its structural features. Sam Lasky went with me to see the mines and prospects in the Precambrian rocks in the south part of the area, and we climbed down the ladders into the Hazel and Blackshaft Mines. Art Miller and Bill Furnish arrived at almost the same time, to see the locales of Permian ammonoids they were studying, and Brookes took them to the Guadalupe Mountains. Harold Vokes of the American Museum showed up with a young companion, desiring to collect Permian fossils. We took him down to the Glass Mountains, and John Skinner showed all of us the Permian rocks of the Shafter district.

Sometime during August Jo Bridge and Will Hass came out from central Texas. I had arranged with Jo to bring out my own car that had been stored in Austin at the beginning of the summer; it was good to see it again, as I had not been sure how I would reclaim it. About this time there was a rainy spell, so they spent a good deal of time with us talking on our sleeping porch. Jo was full of stories about gossip of the Survey and Museum paleontologists, especially about Ulrich; I wish I had kept a record of it, because these reminiscences are now irretreivably lost. When the rains finally abated, we all went out to look at the lower Paleozoic sections, and Jo and Will gave us much help and many new ideas.

After I got my car, I drove up to Carlsbad to look for a place for us to rent during our coming stay during the winter. I found a furnished duplex on North Canal Street and made a down payment on it, to be occupied in September.

Despite all these interruptions, we got much done on our field work. We bought camping equipment, and spent much time sleeping out in remote parts of the region. We camped in the high northern part of the mountains, as well as in Apache Canyon, where we finally worked out to our satisfaction the complex stratigraphy. We also camped at various places in the Sierra Prieta, the igneous mass northwest of the Sierra Diablo, where Brookes made large collections of the Washita (Cretaceous) fossils, including several large "cartwheel" ammonoids that heavily loaded down our pickup. By the end of August, our coverage of the Sierra Diablo area was complete, except for the Precambrian at the south end, which I planned to come back and do later.

Helen, meanwhile, had had a dull summer in Iowa City, but at least it gave mother a chance to see something of Gertrude. One time, Helen broke away from the boredom and spent a weekend at a resort in northwest Missouri (Excelsior Springs?). When she heard I had rented a place in Carlsbad, she decided to come down with Gertrude by train and occupy it in advance of my coming. Unfortunately, she caught a chill on the train and arrived in Carlsbad very sick. When I knew she had arrived I went up to see her, and was shocked at her appearance. She had lost much weight and was in bed in an untidy house, with Gertrude running around without attention. I did what I could to help, but regretfully had to go back to Van Horn for another week or so of field work.

When the field work was finally over, I went up to Carlsbad and settled down for the winter with the family in our temporary home. The situation in Europe had become increasingly ominous. Hitler's star and that of the Nazi party were in the ascendant in Germany. They had occupied the Rhineland and had taken over Austria with little show of opposition from the Western Powers. Now, in September, 1938, they were moving on Czechoslovakia. Chamberlin, the British premier, went to Munich and worked out an agreement which in effect surrendered everything that Hitler wanted. The world breathed easier again, but not for long.

The Conservation Division of the Survey had an office in downtown Carlsbad and I worked there for a while on a drafting table plotting up the geologic map of the Sierra Diablo. But the atmosphere was not inviting, so I decided to work at home, using the front room of our apartment for an office. I started summarizing and typing up the Sierra Diablo field notes in good form. (I continued this work for several years, when time could be spared from other things, as circumstances were against writing a final report; it would not be until 20 years later that I could finally settle down to report writing).

My main objective of the winter, though, was a synthesis of all the Permian of the West Texas Permian Basin, for which I planned a series of maps showing successive stages of development of the Permian. I had thought that somebody with more subsurface knowledge had already tried it, but found that they hadn't, so I set about to do it myself from available publications, and from what I could learn from the local geologists. I was to continue on this into the spring, and during the winter I made several trips to Midland. Robert and Clara were there, and had bought a nice little house on the edge of the business section; he was now working in the Shell office. I stayed with them during my visits. I had many talks with John Skinner of the Humble Oil Company and with various others, in quest of information.

In October there was a regional meeting of the American Association of Petroleum Geologists in El Paso, sponsored by the West Texas Geological Society. The focus of the meeting was on the Permian, of course, and I gave a paper on some of my results -- "The relation of sedimentation to tectonics". After the meeting, there was to be a field trip from El Paso to Carlsbad. I found that no serious plans

had been made for it, so I volunteered to lead the caravan, with impromptu stops at points of interest. By good luck, most of the stops were at roadside places, where all of us could purchase beer.

In November, I left Helen and Gertrude and went back to Van Horn to carry out my cherished ambition to really work out the Precambrian. I stayed at the Duncan's again. The days were sunny, but cold, and the wind howled all day, so field work was rather rigorous. I concentrated especially on the eastern part of the area, where the outcrops were good because of deep dissection, and where there was a good base on the Van Horn topographic map. I made a series of closely spaced traverses and found out where all the different kinds of rock were, but the structure was complex, so many questions had to be left unanswered. I finished up the field work by Thanksgiving time and was happy to go back to Carlsbad and rejoin the family.

In Carlsbad, we already knew Edgar and Gladys Kraus, and through them met many other couples. Helen found a woman who could come from time to time and look after Gertrude when we wanted to get away, so Helen had a little more freedom. We made one trip to Midland in December, to attend the annual tom-and-jerry party given by the local people. One time we had a picnic at Sitting Bull Falls in Last Chance Canyon, and another time drove across the plateau to El Paso Gap on the west side of the mountains. Sometimes we would drive with Gertrude to places around Carlsbad, such as a little park along the Pecos River. One day we were given a tour of the potash mine east of Carlsbad; there was no mining tradition here, so women could go down into the mine.

At Christmas, we set up our tree in the living room and decorated it (in the absence of ornaments) with paper cutouts which we made. On Christmas Day, we drove back to Frijole and had a reunion with the Smith's. They were much the same, and all their family was there. Gertrude played with the other children. Across the street from the apartment was a big supermarket of octagonal shape, with a rotating top that had advertising on it. It had a little lunch room where we ate sometimes.

In March, our life was rudely interrupted. One evening I was told to go to the phone at the service station down the street (we had no phone of our own). Robert called from Midland to say that he had received word that mother had died in Iowa City, and they were driving north next day. Helen decided to break up the Carlsbad life and go to Iowa City to help with the estate. We had to pack her things hastily; I would return later and close the house for good. We bundled Gertrude into the car and set off late at night for Midland, arriving in time to get a little sleep.

We left my car at Robert's place and set off with them. They were able to drive in shifts, so we had long days on the road. One night we stopped at McAlester, Oklahoma, where I remembered a good hotel. To our dismay, we found that the town was full up because of an American Legion convention, so had to accept miserable accommodations instead.

In Iowa City we all stayed at the family home, strangely empty now with both my parents gone. It seemed that mother had been found dead in the hall, with letters she was going out to mail; like father, she had keeled over with a heart attack. We attended the funeral, and took the body to Cedar Rapids to be cremated, like father's. We all attended a session with Mr. Otto, a local lawyer who would be executor of the estate. After a few days I went south again with Robert and Clara, leaving Helen and Gertrude in Iowa City. Edward had come down from Michigan, and would stay on for a while, helping her to empty the family home.

Our affairs in Arlington had not gone well. Before we left for the north our agent had reported that the McDearmid's were behind in the rent (which was little enough). Finally, they announced that they were leaving. Our agent found a Mrs. Taylor who would rent the place. Helen enlisted the help of her old friend, Jim Murphy, a Washington lawyer. On my return to Carlsbad, I had still more ominous news from the east. The mortgage payments were in arrears, and the McLachlen Banking Corporation that held the mortgage was very threatening. I arranged a loan to cover the deficit, and so rested easy for a while. Mrs. Taylor occupied the house until our return in the summer. She was a reliable tenant, but very exacting.

I set about finishing up things in Carlsbad. I cleaned our possessions out of the house, and I made wooden boxes to ship my fossil collections back to Washington.

The American Association of Petroleum Geologists were meeting in Oklahoma City, and I rode up there with Edgar Kraus and Walter Lang. Before the meeting there was a field trip to the Permian red-beds of the Anadarko Basin. On our return we drove all the way to Carlsbad in a day -- a long 700-mile trip.

I left Carlsbad for a week or two of field work before going east. First, I did some more work in the eastern part of the Guadalupe area, determining the relations of the uppermost Delaware Mountain beds to the overlying Castile Anhydrite. Then I went to Van Horn and stayed with the Duncan's again, to clean up various odds and ends of the Sierra Diablo work. Among other things, I collected specimens from the periphery of the Marble Canyon intrusion, and made a reconnaissance of the older Precambrian of the Carrizo Mountains west of Van Horn. I found that these metamorphic rocks had a more orderly arrangement than I had anticipated, and were capable of separation into distinctive map units. (Pete Flawn was to map them in more detail 10 years later).

I took the Survey pickup back to Carlsbad and stored it; it was not a very good vehicle and required constant repairs, so I left it without regret. I then drove my own car to Midland and stayed a week or two with Robert and Clara. While I was there, word came that Hitler had occupied the rest of Czechoslovakia, contrary to the Munich agreement, so we all knew that great trouble impended abroad.

I spent my time in Midland in a final roundup of Permian affairs, and had some long talks with Ronald DeFord and E. Russell Lloyd, during which we worked out the terminology of the main structural features of the Permian Basin (Eastern Shelf Area, Northwestern Shelf Area, Diablo Platform, etc.), and the major subdivisions of the Permian (which later became the Wolfcampian, Leonardian, Guadalupian, and Ochoan Series). I much liked DeFord and Mrs. DeFord, and spent several pleasant evenings with them.

I was invited to talk on the west Texas Permian before the Panhandle Geological Society in Amarillo, so drove there when I left Midland, straight north across the Llano Estacado, making excellent time. From there I went to Tulsa and spent a few days conferring with people about the Tectonic Map project.

Then, I headed for Iowa City. I found that Helen and Gertrude were staying at a rooming house, which in my day had been the Kappa Kappa Gamma Sorority. The room was pleasant, and we continued to live there during the nearly a month we were in Iowa City. We ate our meals at restaurants downtown. While there, I did what I could on the family estate, and had several sessions with Mr. Otto; this helped, as father's affairs were somewhat tangled. Much of my time was spent with Art Miller and Bill Furnish on the Permian, whose ammonoids they were working up into a monograph.

In the evening, Art and Bertha Miller took us sometimes to the Amana Colony about 12 miles west of Iowa City, where we ate lavish German dinners. We also had dinner with Miss Patzig, my old art teacher of student days. We also spent a day on our own at Lake McBride, a reservoir that had been built since my day in the north-eastern part of the county.

Finally, we packed up to go east, and I left Iowa City, never to return. On the road, we spent one of our nights at Richmond, Indiana, and several days at Youghiogheny Lodge in West Virginia, which it was good to see again. Then, we went on to Washington.

It was good to see our home in Arlington again, but it took a bit of doing to get Mrs. Taylor to vacate all her belongings; she and her husband bought a house farther up the street. We had arranged for Helen's aunt, Katherine Copley (Gertrude Finney's sister) to live with us, which made for a congenial family. She was very fond of our little Gertrude, and her presence made it possible for Helen to get out more with me. The only drawback was that Katherine had a clerical job with the Navy, and wanted me to drive her to work every day -- all right most of the time, but sometimes not.

At the office I continued on my manuscript on the Permian, which I completed toward the end of the following winter. Girty had died while we were in the west, but fortunately he had left a lengthy manuscript report on the fossils we had collected in the Guadalupe Mountains, which required only a little editing to insert in the Guadalupe report.

Various major problems existed in regard to the Permian terminology, which had a great bearing on my work. Was the Permian a system, or a series of the Carboniferous (as the Survey had traditionally classed it since the 1880's)? Where was the base of the Permian; at what level was its boundary with the Pennsylvanian? What were the

majot subdivisions of the Permian, and by what names should they be called?

These problems were a subject of much discussion by the Geological Survey personnel, leading figures being Jimmy Williams, Lloyd Henbest, and Charlie Read. Hugh Miser, as chairman of the Geologic Names Committee, took a great personal interest. Williams was an arch-conservative and traditionalist, Henbest was a radical. The problems culminated in 1940 in a series of meetings of an expanded Geologic Names Committee (more of this later).

On one matter, we were forestalled. The Midland group went ahead and made a formal published proposal for the names and definitions of the four Permian series (Wolfcampian, Leonardian, Guadalupian, and Ochoan). I had taken a leading part in working up these subdivisions but could receive no credit, because Survey action had not yet been taken.

Things had gone from bad to worse abroad. Hitler, after absorbing Czechoslovakia, was now menacing Poland. Britain and France were at last roused to action, and offered guarantees to Poland. They attempted to enlist Russian support, but without success. Instead, in August an alliance was announced between Germany and Russia. At the end of the month German troops invaded Poland. Britain and France declared war, and World War II had begun. During the fall of 1939, however, nothing much happened (except for the total annihilation of Poland), and newspapers started calling it a "phony war". The big events were to come in 1940.

In October, I received a long wished-for grade raise to Geologist with a salary of \$3,800.00 per year, which helped greatly in our straitened finances.

Before our year in the west, and after our return, we saw much of Phil and Nancy Porter, for whom we developed a fascination, for they were in a gay and different world. Toward the end of the winter we learned that Phil was very ill. He had no fever, but complained of "pleurisy" in his back. He was taken to a hospital, where it turned out that, fever or no, he had a bad case of pneumonia. I and his other friends were asked to give blood for transfusions. In March, we were dismayed to learn that he had died. He had developed an increasingly gloomy view of life, and Helen was convinced that he had a "death-wish". We attended the funeral. He left Nancy a widow with two small children

to support from her pay as a clerk in the Federal Reserve. When she had recovered, she bought a house in the northern part of Alexandria with her sister "Fras," and we frequently went to see her there, and keep an eye on her.

Assembling the basic data for the Tectonic Map of the United States by the different committee members was now virtually completed. Because of my interest in the project, Longwell honored me by making me Vice-Chairman of the Committee on Tectonics. Also, for my work on the map, the American Association of Petroleum Geologists appointed me a member of their Research Committee. In the spring of 1940, Longwell and I attended the Chicago meeting of the American Association of Petroleum Geologists, on business of the tectonic map. The map was far from completed, however, as all the different parts had to be put together, reconciled, and edited; this would have to be delayed for a year.

In the spring of 1940 the great war began in earnest. Without warning, Hitler's troops invaded Norway, and the country was conquered, despite British support. Then, late in May, the Germans invaded the Low Countries and moved into France, which capitulated in mid-summer.

Partly as an effect of events abroad, an American Scientific Congress was called, to meet in Washington in June. I was asked to present a paper on the tectonics of Mexico. During the spring, in preparation for it, I began compiling data on Mexico. I found that the existing national geologic maps of the country were very poor, although there was obviously much better published data. I started putting these data together, and was able to cover more than half of northern Mexico. The rest, I filled in from general accounts and from inspired guesses. (The compiled geologic map was published much later).

The meetings of the Congress were held in June. Robert had been invited to give a paper on the Paleozoic of western Mexico, so he and Clara drove up to attend, bringing with them their adopted daughter Janet, who was still a baby. We had a pleasant visit with them in the Arlington home. Besides the scientific meetings, there were several general and social events. We all assembled at Constitution Hall to hear Roosevelt speak, and heard him denounce the German invasions. On another evening, Helen and I went to Constitution Hall to hear a concert conducted by Toscanini. There was also a garden party at the Berle mansion, at which Helen and I saw Einstein, and talked to George

Gaylord Simpson and his wife, who seemed lonely.

An era was coming to an end in the summer of 1940. The Survey authorities decided to put the Survey on a "war" footing. We were asked to drop the work we were doing and start investigations of strategic minerals. One set of investigations was to be for manganese in the Appalachians, and I was asked to do a project there. This meant leaving the Permian work in the Southwest in which I was so deeply involved, but I was not too dismayed. On reflection, I decided that after 15 years in the Southwest I had about outlived my usefulness. Other projects could be started, to be sure, such as extending my work northward into the northern Guadalupe Mountains of New Mexico, but they did not seem very enticing. It looked as though it would be much more interesting to go into other fields of geology, and I had always wanted to see something of the Appalachians.

U. S. Geological Survey; second period (1940-1946)

Manganese deposits occur in many parts of the Appalachians, mostly in residuum weathered from the bedrock. One group of deposits occurs in residuum of the early Paleozoic carbonate rocks, next above the basal Cambrian quartzites of the Blue Ridge. One area of these was assigned to me, near Elkton in west-central Virginia. These residual deposits contain large quantities of brown iron ore, which was mined extensively in the latter part of the 19th Century, before the great iron deposits of the Lake Superior region began to be exploited. Some of the residual deposits were also mined for manganese during this period, but mining for manganese did not become prominent until World War I, at which time the U. S. Geological Survey made an extensive study of them. With the outbreak of World War II, the Appalachian deposits were looked upon as an important potential resource, and great hopes were held out for them, for the most part not realized. The deposits proved to be small and pockety, and none extensive enough to warrant any long-term mining program. The work put in by my own and other field parties thus proved to be largely futile in the end, but we were at least able to forestall any hare-brained schemes for extensive mining. Besides, the work was interesting, and the geological problems were exciting.

In the work done during World War I, Foster Hewett and his colleagues developed the idea that the manganese deposits next to the Blue Ridge were mostly concentrated in synclines, the type example being the Crimora Mine near Waynesboro, Virginia, which was supposed to have yielded a fabulous tonnage of manganese ore during the early part of the century, and which occurred in a syncline. In planning the new work, we field men were supposed to map structure first of all, and look for manganese deposits second. As things turned out, the situation didn't work out that way, and synclines were not a guide to ore. However, this gave us the privilege of ranging well over the country to determine the structure of the bedrock.

I was assigned, as I said, to work the Elkton area in Virginia, and my old friend Max Knechtel was assigned another area about 40 miles southwest along the Blue Ridge near Waynesboro. We were supposed to be supervised by George Stose who had taken part in the work during World War I, and was called back from retirement for the purpose.

During August, Stose took us on an extended tour of the area, to get us acquainted with the general geology, and with the specific problem of the manganese deposits. We saw much interesting geology, especially in the Blue Ridge, but the ground at the foot of the Blue Ridge where the deposits occurred was new to us, and discouraging. All the slope west of the ridge was covered by a blanket of quartzite gravel washed out from the mountains. Beneath it, instead of bedrock, were great masses of residual clay, formed by deep and prolonged weathering; it was in them that the manganese deposits were concentrated.

I had not thought especially about having any help in this work, but along in the summer Mr. Mansfield asked if I would consider having John Rodgers as a field assistant. Rodgers had been with the Survey for several years, as an assistant to field parties in the west, but had been something of a problem child and stormy petrel, so Mansfield had begun to wonder what to do with him. He had been Yale's gift to the Survey. In the mid-thirties glowing accounts had come down from New Haven to the Survey, especially from Mrs. Knopf, who said he was a boy genius, so he was taken on. The results were unhappy, because Rodgers proved to have a lot of rough corners. He had had a rather unsuccessful season with Dave Andrews' party in Wyoming, and he had spent a summer with Bill Rubey in western Wyoming.

I said I would consider the matter. John had accumulated quite a dossier in the Geologic Division files, with remarks by various of his supervisors, and I read these carefully. Despite much unfavorable comment, I sensed something, and thought having him with me might be interesting, and worth a chance, so I accepted him.

At the end of August we rented our house for the autumn. This proved to be much easier now than two years ago, as many people were coming to Washington for technical jobs connected with the war effort -- economics professors and other high-class people. We had no trouble renting; I cannot now recall who the tenants were that fall, but we had no complaint. Katherine had left us, and took an apartment in Washington.

We then set out for Elkton. Elkton was a nice little town at the edge of the Shenandoah Valley, very quiet and bucolic. It had once had a tanning industry, but this was now moribund, and ruins of its buildings were scattered around the edges of the town. The place to stay was the Spotswood Tavern, a tourist home run by Mrs. Florey -- a fine old square ante-bellum mansion set in a large tree-shaded lawn. Mrs. Florey rented her rooms to transients and tourists, and had a large staff of colored people in the kitchen who prepared excellent meals. We had a large corner room on the second floor, with a table in one corner where I could work in the evenings.

I started field work by going along the road cuts, examining the different rocks and describing their features in detail. I then began mapping the quartzite ledges on Hanse Mountain south of Elkton, along whose north foot manganese mines had once operated. This was my first acquaintance with Appalachian structure, which was a little different from what I had expected. Sometimes the dips were steep, at others gentle, and the whole formed a regular system of folds.

After we had been there for about three weeks John Rodgers showed up one evening while all of us were eating dinner. He was certainly uncouth! He had on old battered clothes & teeth, made no better after a long bus ride, and a great shock of uncut hair. I introduced him to Mrs. Florey who took one look at him and gave him a room in the basement. He redeemed himself with her a few days later when he sat down at her piano in the parlor and played long passages of Chopin, which made her swoon.

We began to work together in the field, and I found him good company. In the evenings after dinner, he came up to our room to visit, and Helen talked to him mostly, as I was busy at my table (mostly continuing the work on the Sierra Diablo notes). Helen found him good company and quite fascinating, as he had a huge fund of knowledge on many subjects outside of geology. We made a congenial trio.

Gertrude was by now four years old and was developing a personality of her own. She enjoyed the life at Elkton, where there were many children about; there had been few other children at the time in our subdivision at home. She made friends with two girls on the opposite side of the street from Spotswood Tavern, whom she called "Shenefa" and "Rinsel May". "Shenefa" was clearly Geneva, but it is still a mystery as to the real name of "Rinsel May". One afternoon she was

gone for a long time, and we found out that the girls had taken her to "Gone with the Wind" which was playing at the local movie theater. This was pretty heavy fare for a little girl, and she came back much shaken up.

The war seemed far away in the peaceful Shenandoah Valley, and we were not oppressed like we would have been by the constant flow of news in Washington. The Florey's took the Baltimore Sun instead of the Washington papers <sup>and</sup> which we looked at occasionally, learning about the Battle of Britain that was being fought in the air. The invasion of Britain by the Germans, which all of us had dreaded, never happened.

With John, we made a trip down to Waynesboro to visit with Knechtel. His area was much like our own, with quartzites in the Blue Ridge bordered by residuum and gravel-covered plains. But the interpretations he was making seemed quite mysterious. He had conceived the idea that the quartzites were overthrust on the rocks of the valley, and he had formed many other strange ideas that did not fit what we had been finding in our own area. (Later, it became clear that this was all a delusion, and that he had badly flubbed the general geology in his area).

During the field season a large party of visitors arrived. Foster Hewett and George Stose were showing the Bureau of Mines men around. With them, we looked at many of the old manganese workings, now slumped and overgrown. The Bureau men were looking for places where they could prospect for manganese, and one area seemed to intrigue them -- the Watson tract at the edge of the quartzite ridges north of Elkton, where a shallow syncline plunged out of the mountains beneath the valley. It was decided to prospect the Watson tract by means of drill holes and adits. After our field season was over, Rodgers was detailed to stay on and work with Warren Wagner, Knechtel's field assistant, making a plane-table survey of the area, to prepare for the exploration work by the Bureau.

Washington was less than 150 miles from Elkton, and now and then during the autumn I would get up at 5:00 A.M., drink some coffee and orange juice from thermos bottles, and go into town for the day. I could park my car and be at the office about opening time, and spend the day at the office before driving home in the evening. One reason for going in was to attend the meetings of the Names Committee on the Permian.

The meetings were large and well attended. Much expert testimony was presented, and there were long discussions. Miser had arranged for a battery of girls to keep a record of everything, and this record is probably still in the files of the Names Committee, unread by later generations. During private discussions with Miser, it began to transpire that he was on "my" side and the side of the other innovators, although he outwardly kept up his necessary impartiality as Committee chairman. At the last session a vote was taken and recommendations were sent up to the Chief Geologist for his decision. (He didn't follow our recommendations, as we learned to our disgust a year later).

The field season ended in November, and we retruned to the Arlington home for the winter. We had made a good start working out the geology of the Elkton area, but there was still much to be studied and explored, especially in the northern part, and John and I were eager to continue the work next spring.

During the winter I put the final touches on my manuscript on the Permian, and made it ready to send to the American Association of Petroleum Geologists for publication. Stose was busy assembling a new Geologic Map of North America, and was much interested in my compilation of the geology of northern Mexico, so I spent time that winter fixing the results up for him to use. The Tectonic Map project had to lie fallow, although there was great need for someone to edit the results. This would have to be postponed until later. Walter Bucher was very impatient over this, and was throwing his weight around as current Chairman of the Division of Geology and Geography of the National Research Council, and it took much effort to restrain him.

It was planned that I would return to Elkton for more field work at the end of the winter. John Rodgers would rejoin me later. He had been sent to serve as field assistant to Gil Espenshade, who was working on another manganese project in the Lynchburg area on the opposite side of the Blue Ridge. We rented the house to Mr. and Mrs. Tostlebe, a nice couple from Wooster, Ohio, where he was an economics professor. In order to rent to them, we had to vacate the house early, so we stayed several weeks at a rooming house in Chevy Chase before setting out.

We set out for Elkton in March. It was still late winter and the Blue Ridge was covered with snow, giving it a more alpine appearance than usual. I started field work north of Elkton, in the interior of a large bulge in the quartzite ridges which I called the Shenandoah Salient. This was far from the manganese-bearing area along the outer edges of the salient, but the geology of the lower rocks was interesting, and was needed to round out the area.

I became acquainted with the lower formations of the Chilhowee Group, with the Catoctin Greenstone on which they lay, and with the underlying earlier Precambrian Injection Complex. I found that at the bottom of the Catoctin was a thin sedimentary layer (later called the Swift Run Formation), which was unconformable on the Injection Complex, thus proving that it was older. This relation had been debated in the past, and the earlier interpretation had been that the complex intruded the Catoctin. I also found another unconformity at the top of the Catoctin, by which, toward the interior of the salient, it and the Swift Run were truncated by the basal layers of the Chilhowee, until none of the underlying rocks were left. (Some subsequent geologists still refuse to believe this truncation).

The Bureau of Mines project of prospecting the Watson tract had gotten under way during the winter, when a series of core holes had been drilled. While still in Arlington I had agreed to go out with a Bureau of Mines man to see the work. It was snowing that morning, but the man was determined to go anyway, so we went out in the cold and the snow. The core holes had drilled much clay, and in some of them had encountered nodules of iron and manganese oxides. In one of them, to my delight, they had drilled through the clay into unweathered Shady Dolomite. We had, up to then, been unable to find any surface outcrops of the Shady, so it was good to know that it really existed.

By the time we arrived in Elkton the Bureau project had been put in charge of Mitchell Kline, a nice Jew with a blonde Mormon wife. It had been decided to run a long incline from the surface through the clay to strike the most promising aggregate of ore nodules that had been encountered in the drilling. This work progressed while I was in the field, and I went by occasionally to keep a record of the results. Actually, not much was found on the Watson Tract to repay all the effort and expenditure -- just a few nodules of manganese and manganiiferous iron ore.

Early in the field season, I had a day's visit with Charles Butts, and with Arthur Bevan and Ray Edmonson of the Virginia Geological Survey, who were supposed to show me the different carbonate formations. Bevan and Edmonson showed up from Charlottesville, and I had expected Butts to be with them, but he arrived shortly after, having driven out alone from Washington. This surprised me, as he was now in his late seventies, but he was still very active. He had long since retired from the U.S. Geological Survey, but he was still retained as a consultant by the Virginia Survey. When I had seen him before in Washington he seemed very dogmatic, but a day in the field with him was a delight, as he was very mellow and tolerant. We looked at the carbonate exposures along the west side of the Shenandoah Salient, where they had been deeply cut into by the meanders of the Shenandoah River, and Butts showed me how to distinguish between the Elbrook, Conococheague, and Beekmantown Formations. I also took them by the drill holes on the Watson Tract, which had penetrated Shady Dolomite. Butts, in his Appalachian Valley map, had assumed that the Shady had been cut out in the Elkton area by a great thrust fault, because he had been unable to find any outcrops of the dolomite there.

For some reason, Mrs. Florey could not feed us that spring, but there were several other excellent boarding houses in town, which served hearty country meals. Toward the end of the spring we ate at the Matheney's which gave meals to the local school teachers. I remember that they frequently had custard and whipped cream for dessert, and I always had two helpings.

In April, we drove down to Amherst (near Lynchburg) to pick up John Rodgers and bring him back to Elkton. He was glad to be back with us, and it was good to see him again and tell him all that I had been finding. We continued together on our work on the Shenandoah Salient, and then went on to map the carbonate formations north and west of it, and I made long traverses around the different bends of the Shenandoah River. After our detailed mapping close to the Blue Ridge was completed, I made a reconnaissance west of the river, sketching in the outcrops of the carbonate rocks that I could see from the car.

By late in June, we had completed our assigned survey of the Elkton area, and the season was so far advanced that further field work would be impracticable. We returned to our house in Arlington, and I to the Washington office.

Throughout my field work, I had difficulties with Stose, who proved to be very cantankerous. Before I did field work in the Appalachians he had been very friendly, and was actively promoting me to be his successor as Map Editor. (None of the other Survey authorities warmed up to the idea, and in retrospect I think that I had a lucky escape). In the field work it was quite different! He was continually finding fault, and was suspicious of our field results. In this, he was aided and abetted by his wife, Anna Jonas Stose, who had done much field work in the Appalachians, and was much more prone than he to do geology by guess and by intuition. For example, she had decided that all ferruginous quartzites were not part of the normal sequence, but were parts of another formation that had been thrust in for miles from the east. She was determined to draw a great "Blue Ridge overthrust" along the entire length of the Appalachians, from Maryland to Alabama. But we had found a normal unbroken succession at Elkton from the injection complex out into the lower Paleozoic, and this was a black mark against us. Finally, the Stose's gave up their idea of a great overthrust, but it took some doing on my part.

Along in the summer, Miser informed me that he had taken over the Appalachian manganese investigations from the Hewett-Stose combination, which was a great relief to all us field men, as it had never worked well. Miser was to work closely with us for the next three years, and I treasure the memory of those days.

During the summer, Stose called me and Miser in to his office. In going over our mapping he had found a real defect (not just his and Anna's imaginings). I had assigned an inordinate thickness to the Harpers Formation in the Shenandoah Salient, as compared with the sequence south of Elkton. There seemed to be a continuous long sequence of dark siltstone from the quartzites of the Weverton below to those of the Antietam above, although it was true that some beds of ferruginous quartzite were interbedded in the lower part. Stose had decided that these ferruginous quartzites were upper Weverton. The three of us went out for a day in the field to check the idea, and found out

that it worked. So all our mapping in the Elkton area was rationalized, and Stose retired from the scene.

During the summer I began the task of coordinating and editing the data for the Tectonic Map, and began to see how it should be done. I transferred and replotted all the data on a U.S. base map. By the end of the summer I had made a good start, but there were several months more of work to do. This, the Survey authorities positively forbade me to do. The map was nothing, the strategic minerals work was everything.

During the summer I had a letter from Ed Owen, president of the American Association of Petroleum Geologists, expressing great interest in the project, and stating that, when copy was ready, the Association would take over and finance publication. Up to then, Wiser had been very suspicious and hostile to the project, looking on it as the pet of a Yale professor. But when I showed him the Owen letter he changed at once! He was always an ardent supporter of the A.A.P.G., and if the Association wanted something, he was for it too. From then on, he was a firm friend of the project and a supporter of my work on it; this helped greatly next winter.

That fall, it was planned that our manganese operations should be moved to a new area in northeastern Tennessee, where there were many good showings of ore. In september, we rented our house again and went first to Elkton, where I spent a month writing a report on the Elkton project. We didn't have as good luck with tenants this time; the only people who showed up were some rather shady characters, but we let them have it (more of this later).

The Bureau of Mines had given up the project on the Watson Tract, but made one more try on their own in the Elkton area. They put down a set of drill holes south of Elkton in an area that had been designated as "undeveloped tract No. 1" by Hewett and Stose at the close of the work during World War I. No showings of manganese existed here, but there was a well-marked syncline in the residual clay over the quartzite. Nothing came of this drilling either. The drill holes penetrated thick Pleistocene gravel with residual clay beneath, and in two or three of the holes went from this into unweathered Shady Dolomite. So much for the manganese reserves of the Elkton area, and so much for the "synclinal theory" of manganese ore accumulation.

Miser came out and read over the manuscript for the Elkton report, and liked it. We had a field conference of several days, with Knechtel, going over the manganese deposits of our two areas. Miser was able to point out many significant things about the deposits which we had not noticed. We went over Max Knechtel's area back of Waynesboro and straightened out many things. Max was rather overruled by the two of us, and in the end became quite hysterical, making a surprising set of accusations against me -- "You had all the advantages. You had John Rodgers for an assistant, and I didn't. You had a Bureau of Mines project, and I didn't. You got a topographic survey when you needed it, and I had to make my own topographic base!" And so on, and so on. I was disgusted, and never had any use for him after that. His work on manganese deposits was over, and he was sent off by Miser to work on other projects.

By the middle of October the Elkton area was finished, and I, Helen, and Gertrude bade a fond farewell to Elkton and headed south in our car for Elizabethton, Tennessee, which was to be our next seat of operations. Past the Virginia boundary at Bristol the "look" of the country changed. Instead of the lush farms and towns of the Appalachian Valley in Virginia, everything looked cruddier and the people more primitive. Elizabethton was a rayon-manufacturing town, everything revolving around the big Bemberg and North American plants in the west part of town. All the townspeople worked in the plants, and so did people for miles around in the countryside.

John had gone ahead of us and had made a start on the geology of Stony Creek Valley northeast of Elizabethton. A young geologist with the Tennessee Division of Geology, Herman W. Ferguson, was also there, and all of us liked him at once. Miser came down shortly after this, and for a time Walter F. Pond the State Geologist joined us. Miser was especially eager to placate Pond, who was a prickly character, suspicious of the Survey activities, so he spent much time working out an agreement with Pond regarding our project.

We all went around together, looking at the geology and the manganese workings. The scenery and the geology were quite fascinating. This was the area that had been made famous 40 years before by Arthur Keith in his Roan Mountain Folio (which Longwell had us study in our structural course at Yale). Keith had gotten many things out of

the area, but there were other details to fill in, and some of his interpretations did not accord with modern ideas. The area was a challenge, and I was eager to work out all of its geology.

Manganese mines were operating in many parts of the area, and we looked at all of these and talked to the operators. One mine, the Ham Greer Mine in Shady Valley, was something of a cause celebre. It had been located by R. U. Butler, a local operator, on some promising showings, and he had interested some outside investors. It was a big operation; a large open cut had been opened and a washer was in operation, but it had not made much money and was in trouble. We all went up to it one day to have a look. Miser told me, Rodgers, and Ferguson to make a survey of the workings, while he would inspect the supposed ore body, with the operators and investors at his elbow. These wanted us to have lunch with them at a nearby farmhouse, but the four of us drove several miles away to a cross-roads store for our lunches, and held a council of war. Miser said there was something queer going on here; either everyone was kidding themselves, or there was downright dishonesty. During the afternoon one of the investors announced that he was going back to Abingdon for the night, and would come back next morning to hear our verdict. Miser said, "We are not coming back next morning, and I will give you my verdict right now! You are losing \$1,000.00 a month, and my advice is to close the mine!" Everyone was angry and tried to dispute him, but his figures were irrefutable, and thus ended the Ham Greer Mine.

We were all staying at the General Taylor Hotel in downtown Elizabethton, but our quarters were too cramped for the family, so Helen found rooms in a nice house on a hill west of town, overlooking the Rayon plants. It was owned by Mrs. Mottern, who had recently separated from her husband, so her top floor was vacant.

We moved in, and all went well for a while. One evening, though, I came home and found Gertrude all bruised up. She had been told to stay away from the wall that led down to the underground garage, but had gone near it anyway to retrieve a doll. The result was that she fell off, landing on her face and breaking off two of her baby teeth. Helen had taken her to a dentist in Johnson City and found a dentist who patched her up. (On later visits he made a little plate to hold two substitute teeth, to keep her gums open until the adult teeth came in).

About this time Ringling Brothers Circus was playing in Bristol, and Miser urged all of us to go. We went; Miser carried Gertrude on his big back through it all, and I carried along ice for ice packs for her face.

Late in November we heard from our mysterious tenants that they were leaving, with some of their rent still unpaid. Helen decided to go back to the Arlington house, leaving me to finish up and follow her later, so I moved back to the hotel for the last few weeks of the season. She found the place to be something of a mess, and many bills still unpaid. The tenants had brought in a studio couch, which they had evidently rented as **hills** for it began to arrive. We confiscated it, however, as part compensation for the money owing to us, and had it for many years thereafter.

On Sunday, December 6, 1941, I drove north for home in our car. The traffic on the roads was heavy from long convoys of troops returning from maneuvers somewhere, so I had a taste of wartime conditions. When I got home I learned that the Japanese had bombed Pearl Harbor. Next day the United States declared war on Japan, and also on Germany, and we were now in World War II. I saw soldiers with machine guns on the roof, ~~of~~ buildings when I went on the bus to the office next morning, and all Washington had assumed a wartime aspect.

While we were still in the field, Miser brought news that the Chief Geologist had at last handed down his decision on the subject of the Names Committee meetings of the year before. He reversed nearly all of our recommendations; Pennsylvanian and Mississippian remained as series of the Carboniferous, and Wolfcamp Series was to be called "Carboniferous or Permian". Mr. Loughlin was very cozy with Jimmy Williams, and closely followed his wishes, rather than those of the majority of the Committee. Miser was as incensed as we were. My manuscript on the west Texas Permian was about to be sent off, and he instructed me to state in it, "The terminology here used is that preferred by the author," thus bypassing the Survey. All our efforts of the year before seemed to have been wasted.

That winter in Washington I was supposed theoretically to be working on my results of the strategic minerals field work, but with Miser's blessing I was actually completing the editing of the Tectonic Map. I completed this work by the end of the winter, and had it all in shape for the draftsmen. Lew Pusey and Porter Mattox were hired by the Association to draft the map in form for publication.

Early in March I drove south to Elizabethton, leaving Helen and Gertrude in the Arlington house. There had been snow in Virginia, and I was held up for a long time at one point by a truck jack-knifed across the highway. After that, the snow was less, and I finally got to Elizabethton late in the evening. John Rodgers was there with a new addition to the party, Laurence C. Craig, who had just finished his work at Columbia; his wife Rachel was with him. John and Larry had spent the preceding month or two on another manganese area near Cleveland. It was good to have a drink from the bottle I had brought with me, and to fall into bed.

The northeast Tennessee work now began in earnest. I started work with Larry Craig in Shady Valley, the next basin northeast of Stony Creek Valley. Rodgers began work in Bumpass Cove to the southwest of Elizabethton, and Herman Ferguson was at work in the valley north of Mountain City, where I went to see him occasionally. Craig proved to be a serious, steady geologist, good at field work, despite his little previous field experience.

New employees were added. First came Larry Smith, a big enthusiastic guy from Penn State. Later, Tom Hendricks, who was in charge of manganese operations in Washington, hired and sent down Warren J. Souder and Clemens A. (Clem) Nelson. I signed on the last two in Mountain City, and Ferguson and I took them, by way of indoctrination, on a long traverse back into the mountains. On the trip we killed a big timber rattlesnake which gave them a taste of the hazards of field work in Tennessee. Souder was bold and brash and rather deficient in training. He had graduated from Berea College in Kentucky and his chief claim to fame was that he had helped to bring in a small oil well on the Souder farm in western Virginia. Nelson was little and insignificant-looking, very bedraggled from a long bus ride down from Washington. He was a graduate of the University of Minnesota, and it was only later that his sterling qualities began to come out.

I put Smith and Nelson to work on the lower end of Shady Valley, with headquarters in Damascus, just over the line in Virginia. Damascus was a quiet little town, much more attractive than the towns in Tennessee to the south. They stayed at Mrs. Keebler's boarding and rooming house, where there were fabulous meals, and I loved to spend several days there on a visit.

Miser also put Ralph Miller to work on another manganese project in Glade Mountain south of Marion, Virginia, and John and I went up with Miser to help Ralph get started. Miser came down frequently to look over the work and advise us in tight spots, and to get a taste of country air. He was deeply involved in administrative chores in Washington, and had taken on the Appalachian manganese work partly as an excuse to get out of town occasionally.

My west Texas Permian work was published in the A. A. P. G. Bulletin in the spring, and this ended my Permian involvements for the time being. The A. A. P. G. also issued the Permian treatise in a hard-cover edition, but for some reason only made a few copies, so that it did not have a wide circulation.

In early May, I drove back to Washington to attend the meeting of the National Research Council and to discuss the Tectonic Map. It was good to see Helen and Gertrude again, as I had become very lonesome for them in Tennessee. Our stay in Elizabethton promised to be a long one; due to the wartime emergency there would be no summer break in the field work, and we were to work right through until the job was finished. Helen decided to come back with me. She rented the house to Mr. and Mrs. Wilson, who remained there until the following spring. Then she and Gertrude went back with me to Elizabethton.

Larry and Rachel Craig had rented an apartment with Mr. and Mrs. George Young, on a tree-shaded street not far east from the center of town. About the time we returned, Larry had been sent elsewhere on another assignment, so we took over the apartment, where we stayed until the following November. The Youngs were a congenial couple, and we enjoyed their company. George worked at the Rayon plant, and fancied himself something of an intellectual (very superficial, it turned out). Later, things soured a bit with them. George turned out to be an alcoholic who sometimes had mammoth drunks. In the end, we were glad to move elsewhere.

John Rodgers was working in the Bumpass Cove area, which produced not only great quantities of manganese ore, but also zinc carbonate ore. During the summer he was tapped by Charlie Behre for zinc work in Tennessee, and so left our party, although he came back occasionally to help out as a sort of senior advisor.

That fall, while we were at the Young's, we put Gertrude in her first school, in the first grade.

Early in the fall, Clem Nelson left us. His draft board was hot on his trail, but he was also offered an Ensign-ship in the Navy, which he was glad to accept as an alternative. He had really blossomed out in Damascus, had organized a baseball team there, and had been in amateur theatricals. We were sorry to see him go, but I was to see much of him in later years.

Souder was the "problem child" of the party. He was clearly not as qualified as the others in the field, and could not follow the map, so he frequently got lost. It was necessary for me to go out with him on many days, to keep him straight, so I had to neglect the other field men. He was pretty good company in the field, in an earthy sort of way, and had a huge fund of dirty stories. A complication was that Blanche Souder was a dedicated lesbian, and pursued Helen all over town, much to her annoyance. On one of Miser's visits I told him my troubles, so he transferred Souder elsewhere. He sent him to work with Knechtel, another "problem child," to work on a "challenging" project on the coking coals of Oklahoma. On Souder's last day in the field with me he disappeared for a long time, and finally came in with a traverse he had made to justify himself; I found that he had gone up the wrong valley, and plotted on the map the same data we had collected in another valley the day before! I heaved a sigh of relief when I saw the last of him.

In the fall, Larry Craig returned from his other assignment (I forget where) and we put him to mapping the southwestern end of Holston Mountain, which he did with his usual painstaking detail.

The other Larry (Larry Smith) turned out to be another "problem child." He was good in the field, and really covered the country, but he loved to pose as a big, awkward boy, exuding personal charm. He had evidently bluffed his way through Penn State with this pose ("Aw, prof, you wouldn't do that to me!"). His personal charm soon wore thin. When the field work was over for the winter, he and Ferguson took the Young apartment which we had vacated, and he was supposed to write a report on what he had done. Nothing was accomplished; he dawdled on trivia and spent most of his time day-dreaming and reading Time Magazine. I was thoroughly disillusioned with him, and my disillusion continues until today. In the spring he was transferred to a field party in southeastern Idaho, and as a parting shot sent all his belongings there by express, when he could have checked them on his railroad ticket; happily, this was disallowed and he was stuck with the bill.

In November, Helen found another place to live, in an upstairs apartment with the Jordan family in another part of town. I was to occupy the apartment until I finally left Elizabethton in the following May. In early December we went back to Washington for a family visit, riding up and back on the new Southern diesel train, the Tennessean. We stayed at the Mansfields. Conditions in Washington were not so good; the city was very crowded and it was hard to get around. It even took some doing to get off and take the train back to Tennessee.

After we moved in with the Jordan's, Helen became acquainted with a nice lady up the street, Mrs. Bremer, who had a little girl about Gertrude's age. Her husband was an executive in the Bemberg rayon plant, and was a native of Germany; most of the original personnel at Bemberg were Germans, because the process originated in Germany. There was very little anti-German sentiment in this country, in contrast to the foolishness of World War I, but it existed among the rednecks of east Tennessee, so the Bremer's were outcasts. Mrs. Bremer herself was from the Knoxville aristocracy, and they were both nice people, whom we cultivated.

Our field work virtually ended in December when winter came on, and there was much snow. It was time to start working up a report on our northeast Tennessee manganese work. However, there was still much interesting geology to do, especially in the south part of the big cove south of Mountain City, so at intervals of good weather Ferguson and I ran out the roads and sketched in the geology. Miser didn't like this additional field work, and wanted us to concentrate on report writing.

I put together geological maps of all the districts we had surveyed, and Larry Craig was set to work drafting them in form acceptable for publication. There were too many for him to do alone, so some of them were taken over by Helene McConnell, the lady draftsman of the Tennessee Division of Geology in Nashville. I made several trips there by train to supervise her work.

In February, just before one of these trips, Ferguson invited me on a long trip south of our area, to reconnoitre the geology and the manganese deposits. We looked at deposits near Newport, and went from Maryville south to a little known area of Rome and Shady back of

Madisonville. We decided to have a look at part of the Great Smoky Mountains, and the famed Ocoee Series of which we had heard so much. We drove from Maryville up the Little River and over the divide to Gatlinburg, which impressed me as a beautiful little town (we were to spend many years there later on). We found that the Ocoee was a sedimentary body like our own Chilhowee Group, but different, vastly thicker, and more confused.

From there, I went on to Nashville to check on the drafting. Just before leaving for our trip, I had received a mysterious summons from the Military Geology Unit in Washington to come in for a special job, and I had to make my way there after the Nashville visit. This involved a lot of doing. The trains were overcrowded, and Pullman accommodations were not to be had. I came back from Nashville to Johnson City in a day coach, and spent a night in Elizabethton. Then I went to the station in Johnson City to await the northbound train. When it finally arrived, very late, it was horrible! -- jammed with troops and other people, so that people were standing in the aisles. I finally found a corner of a seat in a washroom, where I had to spend the night.

I got to Washington worn out and went to the Military Geology office, where they seemed rather surprised to see me; my haste in coming had been a bit unnecessary. Their problem was to work up a landscape drawing of a landing site at Cove Point in Chesapeake Bay, where maneuvers were being held. A drawing of this, made from a map, might point the way for such drawings of potential landing sites overseas. I thought I could see how to do it, so was put to work.

The work was to be done at the Beach Erosion Board of the Army Engineers in the western part of the city, where coastal studies for military purposes were in progress. I got a room at the Mansfield's, and every day went out there on the streetcar. Krumbein, the sedimentologist from Northwestern, was working there. Every day we went up the hill to the cafeteria at the Army Map Service for lunch, and we became well acquainted. By the end of two weeks I had completed the needed drawings and went back to Elizabethton. I was intrigued by the Military Geology work and was willing to continue with it after the manganese work was finished; besides, it would give me an opportunity to be at home for awhile, after our long spells of wartime assignments elsewhere.

On April 1, I had another grade raise -- to Senior Geologist at \$4,600.00 per year. I felt like a plutocrat!

We had to make a concerted effort now to complete the manganese report by May, and I worked long hours at the desk. Ferguson, Rodgers, and Craig were also busy on the work, and I put all the results together and edited them. All this was a little too much for Helen so, when early in April the Wilson's said they were leaving, she decided to return to Arlington, to await my arrival later. Mrs. Bremer went up with her.

Robert A. Laurence had been taken onto the Survey from the Tennessee Valley Authority, where he had been an engineering geologist for years. He joined us during the closing stages of our work, and helped advise us on the final touches. (It was to see much of Bob in later years).

The report was finally completed. Miser had been worried that we would not finish and thought we were too relaxed, but I had a schedule rather clearly in my mind, and it worked out as I had planned. When the report was completed, Miser came down, and was joined by Pond for a few days, and they read through it and approved it with only minor changes. It was published by the Tennessee Division of Geology in 1944 as Bulletin 52. Even today it reads as a good report. While much of it consists of detailed accounts of the manganese mines and prospects, we were also able to include many of our ideas about the general geology and structure. The main exposition of these would have to wait, however, until the publication of Professional Paper 311 in 1960.

With the report completed, I put our belongings into the car and drove to Washington. Gas rationing had now been instituted. It had been rather relaxed in Tennessee, but was stringent in Virginia and the Washington area. I got extra ration coupons for the trip home. When I got near to Washington I found there was a gas famine, and all the service stations were out of gasoline. During our war years in Washington I tried to make do with the basic A rations, which allowed me only enough gasoline to drive to the office once a week, and to pick up the week's grocery order at Brooke and Harry on the way home.

In June, 1943, I joined the Military Geology Unit for work in Washington, with which I remained until 1945. There was a threat to these plans at first. An oil and gas program was being organized on the Survey, with Miser in charge, and he wanted me to go to West Texas. I hated to go back on my old friend, but I resisted. If nothing else, the thought of banging around the country again under wartime conditions, after having had several years of nomadic life, had no appeal. Besides, Bill Bradley wanted me very much for the military work. The matter was finally resolved in June at a meeting of Bradley, Miser, and the Chief Geologist, during which the respective spheres of influence of the two parties were agreed on, and I was off the hook.

The Military Geology Unit began informally early in the war, when it was realized that the Survey had many library resources of potential value to the military. After awhile it was formally organized with Wilmot C. (Bill) Bradley in charge. Funds were obtained from the Corps of Engineers, and a large staff was recruited -- partly from the Survey, partly from the universities, whose staffs were eager to get into war work. Most of those taken on were supposed to have considerable knowledge of foreign languages, in order to be able to read foreign publications. Reports were being prepared on different regions of potential combat, by "teams" recruited from within the unit for specific projects, and the reports were published as classified folios by the Engineer Corps.

There were old faces among the unit, and many new ones. Charlie Hunt had become second in command. Ken Lohman, the diatom expert, was also a top-notch photographer, and was in charge of photographic reproduction. Marie Secrist was handling library searches. Among the outsiders were Fritjolf Fryxell of Augustana College and Robert Reed, son of Ralph Reed of California, who had been in the Philippines and had gotten out on the last ship before the Japanese invasion. There were also Roger and Harriett Morrison, Louis Ray from Michigan State, Bill Putnam from U.C.L.A., Esper Larsen III and others from Harvard, and still others from Northwestern and elsewhere. Since we had to be isolated from the rest of the Survey, we formed a compact group, with much esprit de corps. Bradley did much to foster this spirit, and the unit was one of the first of the Survey organizations where everyone was put on a first name basis. On each one's birthday, he received a birthday card, signed by the staff.

Bradley wanted me to do landform drawings (awkwardly called "terrain diagrams") of foreign areas. I had had a good deal of experience in this, from my panoramic drawings in the field, and from making block diagrams of geological subjects. An instrument we used much was the isometrograph, which would convert map contours into isometric projection; from the contoured results, hill forms could be drawn in. The isometrograph had been devised long ago by C. K. Wentworth, and had been perfected by Bradley when he was making diagrams for his report on the geomorphology of the Uinta Mountains. We perfected it still more, with the help of the Survey instrument shop. Another device was worked up by Bill Bradley and Ken Lohman, using the principles I had worked out during my stay at the Beach Erosion Board. It would prepare scenes from maps in true perspective, and was named the scopograph. It was a big, awkward contrivance, hard to operate, and we gave it only occasional use. I also plotted land form maps, in which on a map the landforms were drawn with much the appearance they had in nature. In the end, I did little else than the landform maps, and covered enormous areas in the Far East -- for example, in the Philippines, in Japan, and a wide belt of the coastal area of China.

When I arrived some terrain drawing had already been started by Ruth Reed, Bob Reed's wife. She was a beautiful girl and very talented, and for a time she did very well. Later, her personality flaws began to come out. She lived by her emotions, and each job she did was an emotional jag. She was highly neurotic, and always ill from fancied diseases or physical ailments. Troubles with her increased and with my connivance Bradley finally persuaded her to leave, much to the relief of all of us.

We recruited some more help. Dorothy Wyckoff was brought down from Bryn Mawr, and made several excellent drawings, but was used more on other projects in the unit. We also recruited Edith McKee, a student at Northwestern, who turned out to be a broad 190-pound girl, who stayed with us until the end of the war work. Actually, she wasn't very good, and was stubborn, but she was cheerful, a hard worker, and got on well with the rest of the staff. (I have followed Edith's career through the years, and every year she sends me a Christmas letter telling what she is doing. She spent several years with Aramco in Saudi Arabia, has done consulting work in oil, and oceanographic surveys in the Great Lakes, among other things).

It was good to see Helen and Gertrude again, and to settle down for a few years in our own home. Since we could not get around much on account of gas rationing, we began to cultivate our own neighborhood more than ever before. We joined the Washington Golf and Country Club a mile down the road; we did not play golf or tennis, but we used the swimming pool and ate frequently at the dining room. We joined Rock Spring Congregational Church half a mile away, which we found very congenial; Mr. Hunter was an excellent minister. Soon I found myself teaching a Sunday School class of small boys; I wasn't very successful and the class drifted away, and was finally abandoned. I was made a deacon, and attended the board meetings of the church.

Our neighborhood had changed for the better, especially for Gertrude, for there were now many children of her own age. The next house up the street was inhabited by the Cottrell family, who had a boy, Lenny. Farther up the street was Commander Kenneth Knowles, who had a daughter Elizabeth. Down the street was a girl named Marlene. The three girls were a triumvirate, who called themselves Lizzard, Marly, and Girdle. Gertrude went to a school a mile up the road where her teacher was Mrs. Drees. She had a great trouble with her spelling for awhile; I remember a little composition she wrote about the red squirrel, which she spelled "rad srile".

Kenneth Knowles frequently picked me up in the morning on the way to work, and on the one day when I could drive in I reciprocated. In the evening, I rode home on the Arnold buses, which were crowded, and frequently late. Finally, I conceived the idea of riding the Cabin John streetcar to a point opposite Chain Bridge on the Washington side. From here it was about a 2-mile walk to home -- down to and across the bridge, up the hill beyond, then a level walk the rest of the way. Frequently, I didn't have to do this. A group from the Department of Agriculture would come by just as I was on the bridge, and would drive me the rest of the way. Their offices had been moved to Beltsville, Maryland, on the opposite side of the District, but some of them were still clinging to their homes in Virginia.

During 1944, I made two trips again to Tennessee, at the invitation of Mr. Pond, to inspect the work Herman Ferguson was doing. In May, I went to Elizabethton, and the next December to Newport. Pond had told Ferguson to finish the rest of the Johnson County Cove south of Mountain City, which we had only covered cursorily before. All the low ground in the southern part of the cove would eventually be covered by the lake behind the Tennessee Valley Authority's Watauga Dam. He had done his usual beautiful job of mapping, and had found out many interesting new things about the geology and structure. This time, I made sure of Pullman reservations before going. But all I could get coming back was a berth on the car that was put on in Roanoke, so I had to sit in the lounge car until midnight, then walk down and claim my berth for a grateful sleep for the rest of the way. When I was in Elizabethton I made a few calls on old friends, the Bremer's and the Jordan's, among others.

After the Johnson County work, Ferguson had been put on the barite deposits south of Newport, which were also in the Chilhowee rocks, the area being about halfway between northeastern Tennessee and the Great Smoky Mountains. Barite had been mined here in the past, mostly for a dishonest purpose -- as a food adulterant to add weight to the product; there were more honest needs for it now. He had worked out the stratigraphy and structure beautifully, and had found the same units and lithologies as we had seen farther northeast. At Newport, however, the Chilhowee was underlain by more sediments -- parts of the Ocoee Series. There was quite a group on this trip -- Bob Laurence and Deane Kent of the Survey, Pete Keyser with a private company, and maybe others. I found that my walks home from the streetcar had kept me in trim, and I could walk and climb with the best of them. After the others left, Herman and I spent a day reconnoitering southwest to and beyond the Pigeon River, to the northeast corner of the Great Smoky Mountains, looking at the Ocoee formations (mostly in the Snowbird Group). On the return trip, I found Bob Laurence on the train, and we shared a taxi to the office.

In 1944, a drastic shakeup of the Geological Survey occurred. Several years before, Mendenhall had retired as Director, and W. W. (Bill) Wrather had been brought in from the outside to fill his place; he had been in the oil business in Dallas and many of us knew him and were glad of his appointment. Nothing much happened to the

organization for a while, then he struck. Loughlin was replaced as Chief Geologist and was "kicked upstairs" to be "staff geologist." Bill Bradley was appointed in his place (a position he held until 1958). Charlie Hunt replaced him as chief of Military Geology. Hewett was removed as chief of the Metals Section, and was replaced by Tom Nolan.

Bill Bradley did a lot of reorganizing (not very effective, as it turned out). The chief benefit of the reorganization for me was that Loughlin finally began to read the Survey manuscripts that had piled up on his desk through the war years. He had the stubborn idea that he had to read critically and approve all the manuscripts from the Geologic Division before they could be sent on for publication. He finally set to work on the Guadalupe report that had languished for 5 years, and made many corrections, some pretty fatuous. Reports by other Survey geologists also began to move.

Miser's Oil and Gas program offered an opportunity to issue maps of my west Texas areas that had remained unpublished. He was glad to get them to make a quick showing on his program, and he was able to publish them quickly in his new Oil and Gas series; also I felt that my contributions would partly compensate for my deserting him in his program. I stayed down after hours into the evenings to draft the maps; I did the line work and the drafting staff added the lettering and the patterns. First I worked up the Sierra Diablo, which was published as Oil and Gas Map No. 2. Then I worked up a map of the Guadalupe Mountains, and finally one of the Hueco Mountains.

The Tectonic Map of the United States was finally printed and distributed in the fall of 1944. The contract for printing had been let by the A.A.P.G. to Williams & Heintz in Washington. But like many other firms at the time, they were deeply involved in war work, printing huge quantities of maps on their big presses for the Army Map Service. In preparation for printing the Tectonic Map I went over frequently to east Washington on the streetcar to confer with Bill Heintz. The big day arrived at last, and Longwell and I went over to witness the actual printing of the map on the firm's little offset press. It was a big day for both of us.

At home, Helen decided to take in roomers. It brought in a little extra money, and it was a real wartime contribution, with so many newcomers to Washington who had no place to stay. The people the first year came and went, were mostly uninteresting, and are now happily forgotten. From January to March, 1945, Brookes Knight stayed with us. Gus Cooper had found a research position for him in the National Museum, and he had come down from Princeton to occupy it. It was good to have Brookes with us, although living with him at close quarters wore a little thin after awhile; he became very bossy, especially to Helen. He decided to move down permanently from Princeton, so he and Madge bought a fine old house on the hills above the Potomac, near where I had been getting off the Cabin John streetcar to walk home.

After Brookes left us, Helen rented the room to Bill and Mary Hughes, our two most memorable roomers. Bill was a civilian flier, so was away for long periods; Mary was thus frequently at home. They were a gay couple, and great fun to have around. They stayed until the following September, and when they moved elsewhere we were sorry to see them go.

Germany finally capitulated in May, 1945 (VE Day), and the full weight of the war effort was turned on Japan. In July, Military Geology sent me on a special assignment to Fort Bliss, Texas. Invasion of the Japanese islands was imminent, and it was feared that Japanese troops would hole up in dugouts in soft tuffaceous sandstone. The problem was to find similar tuffs in this country where simulated maneuvers could be held, to find how to blast such works with artillery fire, etc. I was sent down to El Paso by plane on an American Airlines DC-3, which was my first long trip by commercial air transportation, and a new experience. At Fort Bliss, I stayed at the Officer's Club. One day, I went up in a military plane to reconnoitre the country, and looked at exposures west of Las Cruces, New Mexico, and at the Malpais lava flows north of Alamogordo. The next three days I went over these and other areas on the ground in military vehicles with Army drivers. Distances to the places seemed vastly greater than they did when simply flying over them, so this took a long time. I finally selected three likely areas and brought home the results. Probably the project was never activated, as Japan capitulated next month.

In August, an atomic bomb was dropped on Hiroshima, and Japan surrendered. The bomb caught most of the people in this country by surprise, for the secret was well kept, but I had developed a strong feeling that something big was impending:

(1) Before we left Elizabethton, Mrs. Bremer remarked that her sister had taken a job with "that big Eastman plant that was going up west of Knoxville" -- partly out of curiosity as to what it was all about; she found, however, that all her typing was in code.

(2) About this time a "trace elements program" was launched on the Survey; it soon became obvious that its primary objective was a search for uranium.

(3) On my trip to Newport late in 1944, I heard more about the Knoxville operation from Bob Laurence. It was styled the "Clinton Engineer Works" and big plants had been constructed at Oak Ridge which were chugging along night and day. Their mysterious feature was that big trainloads of ore were arriving all the time, but nothing came out; there seemed to be no product. Bob correctly deduced that they were working on atomic power.

(4) Many people all over the country were being drawn into some mysterious enterprise. Marshall Kay said that Schermerhorn Hall at Columbia was taken over, and that he was giving up his regular work. Edward said that what he was doing would exempt him automatically from the draft. When Edward came down for a visit, I told him about the mysterious plants in Tennessee, and he said there was another big one at Hanford, Washington, just like them.

When VE Day arrived in Europe, however, nothing had happened, and I began to wonder if it wasn't all a chimera. Then, a few months later came the event in Hiroshima and I, at least, was not surprised.

When VJ Day arrived, we were all caught short. All the stores and restaurants closed in celebration, and there wasn't much in the house to eat. Fortunately, the Country Club didn't close, and we went there with Mary Hughes for dinner.

Gas rationing was terminated abruptly, much to the relief of everyone. To celebrate, we went one afternoon with Gertrude and Mary Hughes to Great Falls, where Gertrude enjoyed rides on the merry-go-round and ferris wheel. The following weekend, we drove to Baltimore for a visit with Aunt Jessie and Aunt Bertha at their home in Towson.

The Military Geology work began to wind down. I was still working on a landform map of Manchuria, which was to be printed with the Manchuria folio. Publication of this folio was considerably delayed, but it came out eventually as an unclassified document.

I had made some trips to New York for visits to Columbia University. In December, 1944, shortly after the Newport trip, I had gone up to talk to the New York Academy of Sciences on our structural results in Northeastern Tennessee, and I spent the night with Marshall Kay at their home in Leonia, New Jersey. During this trip, Marshall told me about the extensive compilations he was making on North American geosynclines, and about his unsettling conclusion that there were no "borderlands" such as Appalachia. In May, 1945, I went up again at Prof. Walter Bucher's invitation, to talk to his class, and I spent the night at his home.

In December, 1945, the first post-war meeting of the Geological Society of America was held at Pittsburgh, and all of us from Washington attended. Jo Bridge was driving, and he took me and Ward Smith along. We went part of the way up and back on the new Pennsylvania Turnpike. It was cold and snowy, and the roads were icy, but Jo was a good driver, and we made the trip safely.

At the meeting, it was good to see old friends, from whom we had been cut off for several years. At one of the sessions, several of us gave papers on the results of our Military Geology work, including one by me on the terrain diagrams. The American Association of Petroleum Geologists was actively pushing a program of post-war research, and<sup>2</sup><sub>1</sub> I was on the Research Committee, I was singled out to take a part. At the Pittsburgh convention there was a preliminary meeting on the subject. I was supposed to do something about research in tectonics -- lay out a program, etc. This was to take a good deal of my time during the next year or so; I collected ideas from different people, wrote reports, and so on, but it was all rather futile, as well as time-consuming.

Sometime in the winter of 1945-1946 I went up to New Haven to confer with Longwell about a new and revised edition of the Tectonic Map of the United States. Much new information had come in since our copy for the 1944 map had been assembled, 10 years before, which might warrant revision, but after discussing it for a while we decided that the plan was still premature. (A new edition of the map

was only started 10 years later, but under other auspices). I also saw John Rodgers, who had been lured back from the Survey to the Yale faculty. On my way home, I stopped off for a visit with Edward and his wife Grace; he was now on the faculty at Barnard College, next to Columbia University, and they were living in an apartment in Morningside Heights. (I was to stay with them many times during the next 10 years, when I had business in New York).

The universities were laying plans for post-war expansion of their staffs, and I got many offers from different schools. Most of these could be laughed off, because the pay offered was the same or even less than what I was getting on the Survey. One offer was more tempting. I had a letter from Prof. Ball, who was about to retire at Northwestern University in Evanston, sounding me out about a professorship there. I thought about it halfway for awhile, but gave it up in the end; if nothing else, the idea of living again in the Middle West did not appeal to me.

While the war was still on, Charlie Hunt had made a big pitch about how we must not drift away after it was over, but should stay on the job and summarize what we had done for the edification of future generations. I was supposed to make a summary of the terrain diagram work. In spite of this, we rapidly began losing personnel. The people from the universities went back to their old schools; many of them expressed disillusion with the Survey, which was a surprise to me, but they had actually seen very little of the organization. To add to our disillusionment, "stay-with-the-ship" Charlie was one of the first to pull out. He had lost interest in the work, and was off to new projects in Utah. Without him, our whole involvement collapsed, and there was nothing more to hold us.

During the summer, I became aware that, as a wartime measure, the military had flown and photographed a swath of country for several hundred miles back of the Rio Grande (called the "Del Rio project"). I was eager to get the photos for west Texas, and Charlie Hunt got a huge hunk of them for me (from what funds, I don't know).

I set about studying them and began to trace the geology of successive flight strips in northern Trans-Pecos Texas. I continued this for a long time in the fall of 1945 and the spring of 1946, using as a guide our own ground surveys, and whatever other ground

data were in Survey files. The air pictures did much to amplify our observations on the ground in the northern Trans-Pecos area. They extended the geology and structure west and north from our Sierra Diablo work into the Diablo Plateau. In the Delaware Mountains, I found that the limestone layers in the Delaware Mountain Group that I had recognized and named along the edge of the Reef Escarpment stood out plainly, and could be traced southward (with minor fault offsets) for the whole length of the range, up to the Apache Mountains. I converted my tracings onto a map, and eventually covered two square degrees. The results were published several years later as an Oil and Gas Map.

I did all this at a time when I should have been working up a treatise on terrain diagrams for Military Geology, but this work bored me. I had decided, as an illustration of our methods, to make landform drawings of Fuji-Yama by all our different methods, but this became very tedious. I finally completed them, however, as well as some other sheets, and the results were deemed sufficient to fulfill my obligations. The sheets remained in Military Geology files until the mid-sixties, when they were finally taken out and published as an unclassified folio of the Army Engineers -- a gap of at least 20 years between submittal and final publication. I have been distressed at other times about long delays in publication of my work, but this one is a record.

On Easter Day, 1946, we decided to make a return pilgrimage to Elkton, partly for Gertrude's sake. There had been some changes in the little town, but not many. Mrs. Florey had given up Spotswood Tavern, and it was now used as a guest house for Merck & Co., who had built a new plant south of town. A big old frame hotel built by the Norfolk & Western Railway, which was vacant when we had been in Elkton before, had now been refurbished, and we spent the night there. Gertrude went around to all her favorite places, and had a visit again with "Shenefa" and "Rinsel-May".

In the spring, the first post-war Pick and Hammer Show was staged, which was designed as a satire on the strange "new Survey". At the last minute I was brought in to play the leading part -- I. M. Gullible, an old-time Survey geologist who is given a tour of the strange new Survey world. I worked hard for a few days memorizing my lines (there weren't many), and came off very well in the show. (As things turned out, this was my last Washington Pick and Hammer Show, either in the cast or the audience).

In the spring, I was appointed representative of the A.A.P.G. to the Division of Geology and Geography of the National Research Council, and in this new role attended their May meeting.

I had accumulated a huge hunk of annual leave during the war years, and in May I decided to take two months off to use up some of it. I stayed at home and enjoyed life. The first month I went to the Country Club swimming pool nearly every day, and read many books from the Rock Spring Church library. The second month, geology caught up with me again; I brought home the Guadalupe manuscript, which had at last come up for final revision, and read it carefully at leisure in the back yard.

Thought now had to be put on what I would do next. Should I go back to West Texas, or continue in the Appalachians? The West Texas air pictures had been so intriguing that I had a hankering to go back again. On the other hand, the Appalachians had many challenges. One of the ideas for post-war Survey work was investigations of the National Parks, and the Park Service had put Great Smoky Mountains National Park high on their list of priorities. The Great Smoky Mountains had been little studied since Arthur Keith's work more than half a century before, and he had left his work there unfinished and uncoordinated. The idea of working there sounded interesting, and it seemed to be a logical sequel to our wartime work in northeastern Tennessee. Eventually, I gave up the idea of a return to West Texas, and cast my lot with the Great Smoky Mountains.

During the summer, I began to think about personnel for the coming project. I tried to hire Herman Ferguson, my old friend from the Tennessee Division, but this didn't work out. Our personnel people would not give him the Civil Service rating he deserved, and he was chary about changing from State to Federal employment; besides, his wife Betty was a country girl, who didn't want to leave Tennessee.

One day, John T. (Johnny) Hack, who was temporarily running the Areal Section, asked me if I would consider Jerry Hadley. Jerry had done much of his work in New England, and had just come to Washington to help Johnny in the office, but he and his family had not found a place to live. And so, in this left-handed manner, Jerry joined the party. He was clearly well qualified, from his background of New England geology (as it turned out, much better qualified than I was on the difficult problems of Great Smoky geology).

# AUTOBIOGRAPHY

PHILIP B KING

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U. S. Geological Survey -- Third period (1946-1954)

In September, 1946, I drew out a Survey pickup truck, loaded it with field gear, and set out for a new season of field work in the Appalachians. Helen and Gertrude remained at home. I spent the first night in Johnson City, and next morning drove on to Gatlinburg, arriving about noon.

I went first to the Park Service Headquarters, an attractive stone structure set back from the highway, on the edge of the Park a few miles beyond town. I made myself known to Mary Lou Chiles, the Superintendent's secretary, and she took me to Arthur Stupka, the Park Naturalist, with whom we were to work closely. He, in turn, introduced me to Blair Ross, the current Superintendent.

Stupka gave me some pointers as to where to stay. This was still the tourist season, and the town was crowded with visitors. I spent the first night at a motel on the main street, and then moved to Mrs. Cox's on Roaring Fork Road, where I had half of a tourist cabin; I stayed there the rest of the fall. There were many restaurants in town, and I tried many of them, but I ate mostly at the Greystone Hotel run by the Whaley family, set back from the main street, which served hearty meals.

I had decided that the place to start geological work was in the central part of the mountains, near Gatlinburg. I would take the Gatlinburg 7½-minute quadrangle; the two adjoining quadrangles to the east and west, the Cartertown and Wear Cove quadrangles, would be worked by others of the party. Hadley would do the Cartertown quadrangle to the east, and Ferguson would do the Wear Cove quadrangle to the west. Ferguson had decided to join the project, but remained with the Tennessee Division; there would be a cooperative arrangement between the Survey and the Division, just as there had been in north-eastern Tennessee.

The others would not arrive for about three weeks, so I set out on my own to study the rocks of the Gatlinburg quadrangle. First, I ran out all the roads, locating and describing the outcrops, then I did the same on the foot trails, of which there were many. I thus built up a network of observations, but there were wide gaps in between which would have to be filled in later.

Each week, Stupka gave nature hikes for the tourists, and I went on all of them, as a way to get a general idea of the mountains. The chief emphasis on these was the natural history, and especially the vegetation -- the lush forests, and the many little ground plants. I had seen most of the same vegetation during the northeastern Tennessee work, but with no knowledge; here was a chance to really learn about it.

Bob Laurence had decided to set up a Survey office in Knoxville and had found quarters in the basement of the Post Office Building. He had hired a secretary, Margaret Gower, a nice little person. His Knoxville office was an enormous help to us during our stay in Tennessee.

The rocks around Gatlinburg were all sediments of the Ocoee Series. I found that the lower foothills on the north were fine-grained sediments -- siltstones and fine-grained sandstones, whereas those in the high mountains that rose to the south were coarse-grained sediments -- cliff-making coarse sandstones and fine conglomerates (later, we were to name these the Snowbird Group and the Great Smoky Group). One area in the foothills, Cove Mountain that rose west of Gatlinburg, was anomalous, as it was made up of coarse-grained rocks like those in the high mountains to the south. Arthur Keith, in the Knoxville folio, had shown it as a syncline, but its structure was obviously more complex than that, and was enigmatic.

The coves of the Wear Cove quadrangle to the west were open valleys formed of Ordovician carbonate rocks; the coves were windows in the Great Smoky thrust sheet.

I found that the eastern part of Cove Mountain, outside the park, had recently been timbered over, so numerous roads had been dug out to bring the logs to the sawmills. Most of the virgin hillsides in the region were covered by a veneer of colluvium and residuum, so that outcrops of the Ocoee rocks were not as abundant as those to which we had been accustomed in the Chilhowee rocks of northeastern Tennessee. The roads on Cove Mountain had cut through this cover, and showed nearly continuous outcrops, so one of my first tasks was to go carefully over all the roads on this mountain. (This was an excellent opportunity; within a few years the vegetation had all grown up again, the roads had slumped, and the outcrops were lost).

Presently, Hadley and his family arrived, and a little later Ferguson and Betty. Jerry's wife Charme was a cultivated lady, and they had a little girl, Gail, who was rather wild. The Hadley's found a house in the lower part of town, in the Medlin Addition, which took care of their housing problem. The Ferguson's rented Mrs. Cox's deluxe cabin.

Until the National Park was established in the early thirties, Gatlinburg had been an isolated mountain community. It was located in White Oak Flats, a cove or patch of open ground at the foot of the high mountains; Mount LeConte, only a few miles to the southeast, rise a mile above the town. The coming of the Park changed the community abruptly to a tourist resort. Three of the principal families, the Maples, the Whaleys, and the Huffs, had built big hotels -- the Gatlinburg Inn, the Greystone, and the Mountain View. The fourth principal family, the Ogles, ran the big general store. Lesser establishments had gone up along the main street, but there was still much open ground which gave the place an air of spaciousness. (The open ground would all be built up later, and today Gatlinburg is over-built and over-developed). The tourist season lasted from late spring to mid-September, and during the winter was nearly barren of visitors, so that everyone in town got to know each other well. During the twenties, a school had been established by the Pi Beta Phi Sorority, as a national charitable enterprise, to bring education to the mountain children. By the forties, this had outlived its original purpose, and the school had been taken over by the county school system. The Pi Phis still maintained a sort of quasi-control, and had a dormitory for mountain children who had come in from outlying areas.

On Thanksgiving Day, the Hadley's had the whole party for dinner, and also Gene Cameron and his wife; Gene was working the pegmatite deposits of the Bryson City district at the south edge of the mountains. After dinner was over, we went to the Park Service building and discussed what we had been finding, and showed Gene the rock specimens which we had so far collected.

I began to hear disturbing news from home. First, Gertrude had been sick, and Helen wore herself out caring for her. Then, she herself came down and was taken to the hospital, where big doses of penecillin pulled her through. From the letters, I didn't really realize how serious it all had been, and did not until I got home.

The field season was over in late November, and I packed up the pickup and drove home. It was good for all of us to be reunited. In the office, I did various things, mostly attending to final processing of the Guadalupe manuscript before publication. In January, 1947, I attended the regional meeting of the A.A.P.G. in Wichita, Kansas, going and returning by train. The A.A.P.G. had grown much, since the time of the national meeting of the Association which I had attended in Wichita 20 years before; now, the local meeting was larger than the national meeting had been. The chief purpose of my going was to attend another conference of the Research Committee, where we again discussed post-war research -- as usual, inconclusive, and happily my last.

Then, plans had to be made for more field work in the Great Smoky Mountains. Helen decided to go with me this time, and we put the house up for rental. A real estate man whose help we enlisted decided to take it himself.

In February, 1947, we set out in our own car; Jerry Hadley had been in town briefly and agreed to drive the pickup back to Gatlinburg. I had had the motor of our car overhauled, but on the way down I found we were continually losing water, so we limped into Gatlinburg. Later, I found that there was a leaky hose connection, which was easily fixed.

We went back to Mrs. Cox's and took her deluxe cabin that the Fergusons had rented in the fall. It had two bedrooms and a connecting kitchen, and was heated by electric radiators; this was T.V.A. territory and electricity was cheap, so that electrical heating was more feasible than in most areas.

Up to then, the winter had been open, with clear, sunny days, so we made the trip go Gatlinburg without incident. Next morning, however, it snowed, and continued snowing, and it turned very cold. Winter was really on us, and it continued snowing at intervals until April. We left the radiators on continuously, and did not open the windows at night. The highway over the mountain was closed for a time, and two women visitors were found dead in their car in the snow on the Little River road.

Mr. Ross, the Superintendent, was very solicitous of the Geological Survey party, and did everything he could to get us settled. He assigned us an office in the basement of the Park Service building, and found us office furniture. Jerry and I set up two desks, facing each other.

In the office, in the absence of field work, I started a report on the Elkton area. I had already turned in a report on the manganese deposits of the area, which was published as a Strategic Minerals Bulletin, but the editors had not permitted me to include much on the general geology. I felt that our general results were enough to warrant a more comprehensive report. I plotted up the geology on a base map that had been supplied to me, and started on a text.

In February, we went over to Bryson City in the snow to look at Gene Cameron's work. The going was slippery, and it was hard to stay on the road. Bob Laurence came with us. We looked at the pegmatite deposits, and the surrounding rocks, which were still a part of the Ocoee Series, but of higher metamorphic grade than those on the north side of the mountains. In the evening, the Camerons had us to dinner. They had felt very isolated, and were happy to have geological visitors.

For the spring field season, I continued work on the Gatlinburg quadrangle by myself. Jerry Hadley had hired as an assistant John Lydecker, a nice young guy. Ferguson had taken on as an assistant George Swingle, a promising graduate student from the University of Tennessee.

I extended my mapping into the high mountains in the south part of the quadrangle, formed by the massive coarse sandstones of the Great Smoky Group. My traverses had to be extended beyond the trails, up and down the steep mountain sides, which involved long climbs across the ledges. I found that the Ocoee sandstones, unlike those of the Chilhowee, had a much higher feldspar content, hence broke down greatly on weathering and were rarely continuous; one could thus work his way through them. When the visitors drove along the highways, they were impressed with the beauty of the country, and the abundance of rock outcrops. Actually beating through the country, off the roads and trails, was quite another matter! The glamor of the mountains wore off, and getting around in them was just hard, onerous work.

By the end of February, Helen had found better living quarters than our cramped cabin at the Cox's. Charlie and Eleanor Sims, at the other end of town, had an apartment over a garage in the back yard of their place. It was quite commodious, with a big bedroom, a large living room, and a glassed-in porch where we ate. We moved in, and lived there for nearly two years. There was a smaller downstairs apartment, which was taken for a time by Herman and Betty Ferguson. Helen really enjoyed Gatlinburg, and we decided to stay there until the end of the project.

Shortly after our arrival, we put Gertrude in the local school, in which she continued into high school. Education in Tennessee was old-fashioned and no-nonsense -- not like the "progressive education" so popular in northern schools. Up to the high school years, it was a much better way to acquire an education.

I had been notified by the Geological Society of America that I was appointed to their Publications Committee. During the preceding years, I had frequently served as a critic of G.S.A. manuscripts, and at some point had been dignified as a member of their Editorial Committee, a purely nominal honor. The Publications Committee involved real duties. In April, I went up to New York to attend my first meeting of the Committee at the Society House on the Columbia University campus. On this and later trips, I stayed with Edward and Grace, who lived a few blocks away. The committee consisted of three members, each serving a three-year term. The Chairman served his last year, and when he left the next in the line became Chairman. Morley Wilson was chairman that spring -- a fine old fuddy-duddy with the Canadian Survey, who had spent years on the Canadian Shield, and left it in a state of utter confusion. Next in line was Glen Jepsen, the vertebrate paleontologist from Princeton; I was the junior member. I went up and back on the train, and was pleased at how easy it was. The Southern trains had a Pullman car that went through to New York. Coming back, I took the car of the Birmingham Special, which left New York about noon. By evening, we were across the Potomac, out of the cities, and in open country. In the early morning hours our Pullman car was put off in Knoxville, and we could get up when we felt like it.

In May, we drove north in the pickup to Washington so I could attend the National Research Council meeting. Our tenant had told us that there was lots of room at our home, and that we could stay there. Home looked very good, and everything was about as we had left it. In fact, it seemed strangely deserted; and when our tenant came in at last he told us that his personal affairs had taken a nose-dive, that his wife had left him and he had been living there alone. Our few nights there were to be the last time we would sleep in the place. Shortly after we left, he wrote that he had rented the house for us -- to a South African couple who were in Washinton on some monetary commission. He rented it for the princely sum of several hundred dollars a month, which made me feel guilty, but the tenants told us later that their government gave them a generous cost of living allowance.

In June, Robert invited all of us down for a family reunion in Jackson, Mississippi. He had left the Shell Oil Company in Midland several years before, to go with the Superior Oil Company in Carlsbad. Later, he got disgusted with this company, and spent nearly a year on his own as a consultant. This hadn't worked out too well, and he got another job, this time with his old company, Texaco, in New Orleans. Afterwards, he was assigned to head their district office in Jackson, where they had bought a fine home on a big lot at the edge of town.

Helen, Gertrude, and I drove down in our car -- through the Appalachians of Tennessee, Georgia, and Alabama, to Tuscaloosa, where we spent our first night at an attractive motel in the pine woods at the edge of town. Helen and I rested on arrival, but Gertrude was very gregarious and went about the place getting acquainted with the other guests; then a wonderful southern dinner. Next day, we drove across the Coastal Plain to Jackson.

We spent nearly a week at Robert's place. Edward and Grace arrived shortly after we did, coming down by train. Shep Lowman, the current chairman of the A.A.P.G. Research Committee, had invited me to visit Houston, so I left the family gathering for a few days to make a quick trip there. I went on the Illinois Central to New Orleans, and from there on the Southern Pacific. The S.P. train was delayed for some reason in southwestern Louisiana, so we dragged into Houston quite late; it was good to get to the hotel. Next day, I spent with

Lowman, who had his office in a big barn-like building that housed the Shell Research Organization. Adkins was also there, working for Shell Research, and lo and behold, Edith McKee. Lowman propounded many ideas about A.A.P.G. research, which were probably all right, but I had lost most of my enthusiasm for the subject. We had lunch at one of the hotels, where there were other guests -- among others, Paul Weaver, and also Pinky Herring, my old friend and enemy from Marland days of 20 years ago, who was now a consultant at a Houston bank. Houston was very big, very busy, and very hot and humid. It was the only occasion when I spent much time there, and I left it without regret.

At the end of the day, Lowman and I, with Adkins in tow, went to his house and had drinks. Presently his wife, Katherine Lowman, came in. She was a lady columnist, who wrote self-help pieces for the women's page of newspapers; her syndicated piece appeared regularly in the Washington Star. After a quick supper, Shep took me to the station, and I was glad to get the train back to Jackson.

At the end of the week in Jackson, Robert proposed a grand tour. All of us drove to Vicksburg and then Natchez, both fine old southern river towns. Then we went on to New Orleans for several days. One of the high points of the New Orleans visit was a long sight-seeing voyage on a river steamer, up and down the Mississippi River water front. We had lunch at a French restaurant near the cathedral on Jackson Square. We spent the evening in the French Quarter, including dinner at Antoine's. Gertrude was bug-eyed by it all, and insisted on having a flaming desert. After dinner, I took her back to the hotel, and then rejoined the party. This New Orleans visit was a happy contrast to my dreary visit at the time of the A.A.P.G. meeting in 1938.

During the day, Robert was told at the Texaco office that he was being transferred to the New York office of the company. So he would have to leave the nice post he had had at the Jackson district office, and their beautiful home, in which they had only lived for a few months. He and Clara were very blue over the news, and it cast a pall on an otherwise gay evening.

We did some more sight-seeing next day, and Robert had still more things in mind for the days ahead, but Helen and I had had about enough of the heat, and our pocketbook was getting low, so we decided to quit and go home. Robert kept on going for a few more

days with Edward and Grace in tow. He gave us a key to the Jackson house, and we drove up there next day and had a good rest. Next day, we drove straight through to Gatlinburg -- a long day, arriving about midnight. It was good to be home again, and out of the steamy southern climate.

Edward and Grace stopped off to see us in Gatlinburg a few days later on their way home, and we took them on some trips into the mountains.

Ed Goddard, who had become Survey Map Editor, planned to spend a few months of field work in Colorado, and asked me to substitute for him during July and August. I decided to accept, as I would get a taste of what I had missed. Brookes Knight offered to introduce me at the Cosmos Club on Lafayette Square, so it was arranged that I would stay there.

Just before leaving, I received a message from Arthur Dean of the University of Texas, offering me a professorship there at a salary of about \$12,000 per year (my Survey pay at the time was about \$8,000), and asking me to come down at their expense for an interview. This seemed much more attractive than any of the other University offers so far, and Helen was also much excited over it. I decided to go down for the interview before going in to Washington.

I went down by train. A Pullman car was no longer put off in Austin, so I had to get up about 5:00 A.M. It seemed like I was reliving my arrival in Austin of 20 years before. I had breakfast at the same little restaurant across from the station, and with time to kill, I decided to walk to the University, looking at the old familiar places, and remembering the past. Arrived at the University, it was still early just as 20 years before, and I had to wait a while before Arthur Dean arrived at his office in the new Geology Building.

He explained that what they wanted was a professor in charge of graduate studies in geology. They were determined to really put the geology department on the map, after its years of somnolence, and to build up a good staff. The traditional hostility between the Department and the Bureau was now healed, and John Lonsdale, the new Director of the Bureau, was also strongly behind the move. Unfortunately, there was some foot-dragging among the old guard -- Professor Whitney, for one. Also, Fred Bullard was reported to be luke-warm about the offer to me; after all, I had been a mere kid when I was there before, when he was already a professor.

I was taken around for a series of interviews with different people, mostly geologists, but more importantly with Painter, the President, and several of the deans. I spent a pleasant evening with John and Edna Lonsdale. John made a strong pitch about the opportunity; before long, the old guard would all be gone, and I would have the department in my own hands, to shape as I wished. This didn't appeal to me as much as it did to John, as I had no ambitions or lust for power like he did.

By the end of the visit I was almost convinced, and said I would let them know after I got to Washington. One difficulty was easily taken care of -- my unfinished Great Smokies work -- I was told that I would not have to come until I had finished the Gatlinburg quadrangle at the end of the year. I rode back to Washington deep in thought, and when I got there I wrote that I would accept the offer.

But it all ended sadly. There was a long wait to hear the exact terms from the University, but while I was still in Washington, at the end of August, I finally received a letter from President Painter. The terms were somewhat like those outlined by Dean -- but were significantly different. My base salary for nine months would be about \$8,500 (just what I was getting from the Survey for 12 months). The rest of the \$12,000 I could get by working summers for the Bureau of Economic Geology. This wasn't my idea at all! I wanted time to think and write, to attend conferences like the upcoming International Geological Congress in Great Britain, and so on. I had no intention of working for the University 12 months out of the year, just to earn my supposedly princely salary! I wrote back indignantly that it was all off, and that I would not accept the professorship!

I had already cleaned out my Washington office at the Survey, and had asked to have my journals, books, and maps shipped to me in Austin. I now instructed the movers to ship them to Gatlinburg instead.

(There were many aftermaths of this adventure. A year later, I got another message from Arthur Dean, renewing the offer, and saying that I could come on any terms that I chose. But, once burned, twice shy, and I refused. A year or two later, Ronald DeFord accepted the post, which he held honorably until he retired a few years ago; he was in a better position than I was to do so, as he had an independent

income. The Department really built up from then on, and got better and better, so that it is now highly respected. And some years later, Arthur Dean was fired from the University, because of his long-standing overt homosexuality; I wouldn't have wanted to be around to help put a knife in my benefactor. And my visits to Austin through the years have always been most delightful).

My two months in Washington were something of a penance. I covered a lot of ground in the map editing, and had a chance to get acquainted with the administrative wheels of the "new Survey". Outside of our air-conditioned office building, downtown Washington was fearfully hot and humid. Outside of hours, mostly in my Cosmos Club room, I compiled old records of 19th Century mining activity to use in the Elkton report, and plotted "parent-materials" maps of the Gatlinburg and adjacent quadrangles from the soil surveys, as a guide for later field work. Not many people were about, and my social engagements were few. I had a compact to have dinner every week with Ted and Charlotte Cabot, two wealthy dilettantes from Boston, who had come down to Washington for some reason to take menial jobs with the Survey. Charlotte had been a secretary in Military Geology, and Ted was working in the Survey's Trimetrogon Project. They had a house on 29th Street, a few blocks above Gertrude and Harry's place. They were pleasant company, and after each dinner we sat and talked for a long time.

By the end of August, I was glad to start back to Gatlinburg. Eleanor Butler and Vergie Jussam from Military Geology saw me off on the train. It was hot and humid in Gatlinburg, too, but it was in the mountains, and away from the paving and asphalt of downtown Washington that made the place like an oven. Helen was very crestfallen when I told her about the outcome of the University of Texas adventure.

In early September, Robert and his family came through on their way to New York, and spent a few days with us. They had given up the Jackson position and Jackson home, and were on their way to New York and his new position; he has been in New York ever since. A little later, Longwell and John Rodgers came through with some students, and we showed them around the mountains, including a trip to Whiteoak Sink, where a waterfall cascades from a cliff of Ocoee rocks into a cavern worn in the overridden Ordovician limestones. Longwell was very garrulous, and on the morning he was to leave it was hard for him to get started.

Mapping of the Great Smoky Mountains began again during the autumn. I continued my traverses in the Gatlinburg quadrangle, as there were still many remote places back in the forest to go over. Jerry Hadley continued his work to the east, and had as his assistant a nice young guy named John Lydecker. John fell in love with a girl who worked for the Pi Beta Phi Sorority, and married her at the end of the field season, after which he obtained a job with Mobil in Midland, Texas. Herman Ferguson continued his mapping to the west, and had as his assistant George Swingle, an able chap who was a graduate student at the University of Tennessee in Knoxville. By the end of the field season, difficulties with the "cooperation" with the Tennessee Division of Geology began to be apparent, at Herman always took his notes and maps back with him to Nashville, so that we never had access to them. This problem was only resolved several years later, when Herman decided to give up further Smokies work, and at last turned his records over to me.

George Schoechle of Foreign Geology Branch conceived the idea that, as we were close to Washington, our project would be a good place to put foreign trainees. That fall he sent us Señor J. C. Granja from Ecuador. The idea was to put them to work, doing what we were doing, but this was not appropriate, as our work was pretty specialized, and involved hard slogging through the dense forests, looking for outcrops. Granja was a rather sad character. Jerry and I would take him in the field with us, but mostly as a companion, and he didn't get much out of it.

I arranged, therefore, to have him see the geological sights of east Tennessee. He spent a few days with the T.V.A. geologists in their engineering work at some of the dam construction in northeastern Tennessee, and got him a tour of the zinc mines. Partly to help him, and partly for my own education, we also joined some field conferences on the Ordovician in the Tennessee Valley. One memorable trip was near Friendsville southwest of Knoxville, where Gus Cooper demonstrated his conclusions on the Middle Ordovician. I became acquainted with the Lenoir and Mosheim at the base of the Middle Ordovician, and with the associated beds, which was a help later on.

During the fall, I completed the Elkton report and sent it to Washington in November. My books and maps arrived in the freight station in Sevierville -- four or five heavy wooden boxes -- and I brought them to the Park Service office and set the books up in the little anteroom next to our office quarters.

In December, the G.S.A. met in Ottawa, and we all decided to go north, Helen as far as Washington, I to go on to Ottawa. Something happened, however, I believe that Gertrude sprained her knee; she had the same defect that I had -- maybe it was hereditary. So we did not go after all. When we looked at the papers next morning we were glad we hadn't; the biggest blizzard in years had hit the Northeast. New York was in chaos, and the storm went as far north as Ottawa and as far south as Washington.

In January of 1948 we had a visit in the field with Harry Ladd, who was then Assistant Chief Geologist, and in February with Charlie Hunt. After the collapse of Military Geology, Charlie had gone off to Utah to work on the Lake Bonneville deposits of the Salt Lake City area. He had just been appointed chief of the General Geology Branch (the old Areal Section), and had gone into the job with his characteristic drive and enthusiasm for a new "challenge" (these enthusiasms never lasted, as past experience showed, and would show again). He had set up the Branch offices in the Denver Federal Center -- a World War II munitions plant west of Denver which was being remade into Federal offices. He was busy reorganizing the Branch, and was delegating authority right and left, giving everyone on the staff a title. I was to be "Geologist in charge of tectonic investigations." This would involve visits to field parties all over the country, and he gave me carte blanche to make these visits at will. In the next few years this resulted in some interesting trips, but it was a disaster for my involvement in the Great Smokies project, and I began to lose my grip on it.

During the spring field season I finally completed my traversing of the Gatlinburg quadrangle, so that the mapping was all complete. But the results didn't add up, and the geology remained enigmatic, and without any real interpretation. When the mapping of this quadrangle was completed, I began traverses into the Silers Bald and Pigeon Forge quadrangles to the north and south.

Things boiled up in the Publications Committee of the G.S.A. William Herbert Hobbs, the erratic genius of the University of Michigan, claimed that he had been misused by the Society, and his friends had made scandalous accusations. This came before the council at the Ottawa meeting, and the Publications Committee was ordered to go into the matter. A mass of documents was sent to each committee member for review, and we took up the whole thing at our March meeting in New York City. It seems that Hobbs had written an unconventional interpretation of the Channeled Scabland of Washington State, that the manuscript had received sad treatment by the Society, and that it was sent to Harlan Bretz and other enemies for review. In the end, Hobbs withdrew his manuscript and published it privately, with a denunciation of the Society.

The burden of the problem fell on Glen Jepsen, the current chairman, who worked hard on it, and used a great deal of tact and diplomacy. He worked out a revision of the rules for publication procedure, and this was gone over at a second meeting in New York, at which Hobbs was invited to be present.

That summer, I spent another two months as Acting Map Editor in Washington. This time, there was a change in weather at the end of July, and August was cloudy, cool, and reasonably pleasant. The map editing was interesting, as there were several big Professional Paper reports to go over, including Ed Eckel's on the La Plata Mountains, and Charlie Hunt's on the Henry Mountains. Outside of working hours there were several social events. Bunny Bowman, a draftsman friend from Military Geology, was married to Garn Rynearson of the Minerals Branch at her sister's country place southeast of Washington, and a big mob of Survey people came. Afterwards, I and some of the others went back to Ep and Marjorie Larsen's place in Washington for drinks and a long talk.

After the Washington stint I returned briefly to Gatlinburg, then west at Charlie Hunt's request. I went out to Denver by train. The railroads were making a big effort at post-war rehabilitation, and there were fine, beautiful, luxurious trains on the run from Chicago to Denver. I had never been in Denver before. Charlie Hunt met me with his station wagon, and put me up at their place in a big old mansion in downtown Denver. The office center was getting organized, and I spent some time there.

At the end of the office visit, I went with Charlie by plane to El Paso. Commercial passenger travel was just coming into its own, and Charlie was a great enthusiast for it. In El Paso, we were met by Fred Smith, who took us to Sierra Blanca, where he and Claude Albritton were at work with their assistants. He and Claude, and others from S.M.U., had done dissertation jobs in the area (which adjoined my own Sierra Diablo area on the west), for which they received Ph.D.'s at Harvard. Now, it was planned to put all this together on quadrangle maps, and a report for Survey publication. Fred had Mabel Claire, his two girls, and baby boy with him, living in an old adobe house.

At the end of this visit, I went east by train, stopping off in Midland. Ronald De Ford and the others there wanted to discuss plans for field trips for the El Paso meeting of the G.S.A. which was to be held in the autumn of 1949. I had dinner with Bert and Helen Hemphill (she was Dr. Sellards' daughter) and with John and Helen Lydecker (she was the Pi Beta Phi girl from Gatlinburg). They put me on the night train for the east, feeling very tipsy.

In Gatlinburg that fall, I made a reconnaissance investigation of the Richardson Cove quadrangle, northeast of the Gatlinburg quadrangle, where wide exposures of the foothill belt seemed to offer a solution of its many problems. I ran out the roads and many of the creeks, and got a general idea of the lay of the rocks and their structure, but not enough to be definitive. (Warren Hamilton mapped the area in detail several years later).

That fall, again, George Schoechle inflicted us with another South American trainee -- Ruy Ozorio Frietas from Sao Paulo, Brazil. Schoechle advertised him as a lot better than Granja -- "a producer" he called him -- but he was just as sad and difficult. Autumn was coming on, and the autumn leaves were never more beautiful. Frietas was a child of the tropics, and looked on the turning leaves with dismay. I took him with me on a trip to northeastern Tennessee. Rodgers had assigned two students to work there on dissertation problems -- Jean Lowry in the mountains east of Erwin, and Steve Oriel on the Hot Springs window. We were to meet Lowry and Herman Ferguson in Erwin. We had car trouble on the way, and there was a torrential rainstorm, so we got into Erwin late, wet, and tired. Next morning it was clear and cold, and there was a dusting of snow on the mountains. Frietas was shivering with cold, and thought the snow was

the beginning of winter. Next week, I had arranged for him to take a trip to the zinc mines, but he said forget it, he was quitting and going back to Washington. So we were rid of him, but with an after-taste of bad feelings; I wrote to Foreign Geology to quit sending these fellows to us. (They wouldn't give up; a few years later they tried to send an Egyptian to us, but all the party were against it; his dark skin would have created difficulties in red-neck East Tennessee).

In November I attended the G.S.A. meeting in New York City. Glen Jepsen and I presented our revised publication rules to the Council. The Council was full of cantankerous characters in those years, and the councilors offered so many changes and amendments that we felt our rules had been torn to shreds. There was an all-day symposium on sedimentary facies, with Longwell as organizer and chairman. Formal presentations were made, and a group of us were asked to present formal discussions. In addition, large numbers of others were permitted to get up and make remarks, some fatuous, so that it went on until late in the day. One of the evenings, Jim Gilluly gave his presidential address, which was an important and challenging discussion of the timing of orogenies.

After the meeting, there was a field trip to Dutchess County, led by Robert Balk who presented his work there. Robert and I went up by train to Poughkeepsie. As the bus was full, we rode with Scott Warthin, professor at Vassar, who was the junior member of the Publications Committee. Also with us was Robert Neuman, at that time finishing up at Johns Hopkins, whom we were hiring for the Smokies party the following year. Balk's area had many of the same problems as we were finding in the Smokies -- enigmatic rocks and complex metamorphism, so that the trip was enlightening.

Helen and I decided to move from Washington to Gatlinburg as our permanent home. A year or two before, I had told the Washington people that I was no longer entitled to per diem in Gatlinburg, and asked to be taken off. They responded by moving my official headquarters from Washington to Gatlinburg -- a more drastic solution than I had expected. But since they had so decided, why not take them at their word, and buy a house in Gatlinburg?

We looked at different houses for sale, some old and run down, some fairly new. Prices seemed high (we realized later these were just "asking prices", set by oversanguine or over-avaricious owners; actually, real-estate was very sluggish, as we found when we ourselves tried to sell some years later). Helen found a fine old sturdy house on a lane just off of Roaring Fork Road, owned by Arlie Watson, who worked for the Pi Beta Phi Sorority. He wanted \$12,000 for it, but it needed some going over inside.

We put our own Arlington house up for sale through Mr. Hoge, a real estate friend of Helen's. There was no trouble selling it for \$15,000 which, after the outstanding mortgage was paid off, gave us the money to buy the Arlie Watson place, and a thousand or two for remodeling. The final purchase of the Gatlinburg house was completed in March.

We set to work with the local carpenters to remodel the house. It was fun to take an old house and reshape it to our wishes. The partition between the two front rooms was knocked out to make one long living room and dining room. A fine big fireplace with heatolater was installed. In the back room that became my study, shelves were put up from floor to ceiling on one wall for our books. A new kitchen sink and refrigerator were bought and installed. An electrician went over all the old wiring, and installed fuse boxes. And so on, and so on.

In January, Helen went to Washington to superintend dismantling the Arlington home, and all our possessions went in a moving van to Gatlinburg, where I awaited them, and had them all installed before she returned. There was still much to be done about the house before it was perfect, and would have to be done later, but we felt well settled.

Late in January, 1949, I set out on a long trip to the west as part of my duties as "Geologist in charge of tectonic investigations". I flew from Knoxville to Phoenix, Arizona, in a DC-6, which seemed vastly bigger than the DC-3s in which I had previously flown. After a night in Phoenix, I went in a smaller plane to Las Vegas, where I was met by Chester Longwell. He was taking his sabattical from Yale to work the Colorado River valley south of Boulder Dam, which was soon to be flooded by another reservoir. He and his family were living in Boulder City, the "company town" next to Boulder Dam south of Las Vegas.

That afternoon, we looked at the geology of Frenchman Mountain near Las Vegas, then went to Boulder City where I was put up at a motel. Next morning, it was unexpectedly snowing, so we stayed in, and spent a little time in the offices at the dam. The weather began to clear a little, so next day we drove around to Muddy Creek Valley to look at the northern Muddy Mountains, which Longwell had studied in 1939. We saw a little that day, but next morning it was snowing again, so we had to give up and go back to Boulder City. The snowy weather was no mere fluke; a hard winter was upon us.

It was decided that I should give up the Las Vegas visit, and that I should go on to my next stop, with Foster Hewett in Pasadena. I went down by Union Pacific train that night. The weather was foul in the Los Angeles area, too; it had unprecedentedly snowed all over southern California, as far south as San Diego. Foster met me at the bus station in Pasadena and took me to their home on Rose Villa Street, not far from the California Institute of Technology. After Foster had been let out as Chief of the Metals Branch, he had spent nearly a year in Washington convalescing from some serious operations. Then he decided to come to California and undertake a big study of the Mojave Desert area, as an extension of his work on the Ivanpah quadrangle of 20 years before. He had hoped to lure me into the project, but I was already committed to the Smokes project, so didn't nibble. (The project gathered much steam during the next 8 years, and many people were recruited to work on it, with Ward Smith in charge.)

Foster had acquired a large collection of air photographs of the Mojave region, and I spent the next few days studying them indoors. The weather finally turned clear and crisp, and the San Gabriel Mountains rose beautifully above Pasadena, with the Mount Wilson Observatory gleaming white at the crest. (Every time I was back there in later years, the mountains were deep in smog). We finally decided that it was feasible to go out to the desert, and we set off in Foster's jeep.

But once beyond Cajon Pass, we found the whole desert was still in the grip of winter, with deep snow over everything. We spent a night or two in Baker, a dismal village on the highway at the south end of Death Valley, where we were joined by Ward Smith. We looked at a few places in the Ivanpah quadrangle, then at outcrops in the lower

country of southern Death Valley, including the foothills of the Avawatz Mountains. But finally we had had enough of the overcast and the cold, and drove back to Barstow for a welcome night in a warm motel.

Our next stop was a visit to Levi Noble at his Valyermo Ranch on the north side of the San Gabriel Mountains. Levi had been a part time Survey employee for years, but was something of a dilettante. He and his wife Dorothy for years had owned a prosperous fruit ranch at Valyermo, right on the San Andreas fault. Levi had two geological projects in mind -- Death Valley, and the segment of the San Andreas fault between Soledad Pass and Cajon Pass, and had done partial work on both of them. Some years before, Stose had helped him to write up the "chaos structure" of southern Death Valley. (Remember, also, Mansfield's halfway proposal to me that I work with him in Death Valley after the close of the Marathon work). Charlie Hunt (and maybe also Hewett) were after him to finish up his work on the San Andreas fault, and I was delegated by the Branch to help him.

Levi welcomed us at the ranch house -- a fine old comfortable place set in a grove of trees. Dorothy was away somewhere, but Levi cooked us an excellent lunch. I think Foster then went back to Pasadena (there was a coolness between them), but Ward and I stayed on for the night. In the afternoon, we went out to look at the fault zone. Its young tectonic features and topography, and its relations to the Pleistocene deposits, were quite impressive. There was still snow here, but not as much as in the desert to the north. Dorothy came in later, and was surprisingly friendly to me.

After another day in Valyermo, I went back to Pasadena, and Foster took me to a downtown hotel where I could get a morning bus to the airport for a flight to Denver. I was hoping to see the country from the plane, but there was cloud cover all the way. Fred and Mabel Claire met me at the airport, and put me up at their home in Lakewood.

I spent the next few days at the Branch office in the Federal Center, and attended a staff meeting. Denver was very wintry too, with much cold and snow. I took a train for home, but the trains out of Denver for Chicago were very late. It was good to be back in Tennessee again, after nearly a month away.

Bob Neuman arrived about the time I returned, and we set to work mapping the Silers Bald quadrangle south of the Gatlinburg quadrangle. This was wild, forested country, wholly in the park, and getting about was not easy. We worked on this into May, traversing all the trails, and made many long climbs in the mountains, but work was difficult, there were many geological problems, and we were unable to cover it all. Besides, I found that Bob had little taste for work in the wilderness; otherwise, we might have done some camping out in the more remote places.

In the spring, Frank Snyder of the Geology Department of the University of Tennessee organized a conference on the mineral deposits of the Southeastern States, to which many outside speakers were invited. I was to give the lead-off talk on the "Tectonic framework", Jerry Hadley presented the metamorphic problems, and Charlie Behre the general problems of mineral deposits; then followed papers on specific mineral deposits. I became interested in a reappraisal of the tectonics of the Southern Appalachians, and worked up so much material that I sent the long version to the A.A.P.G. Bulletin for publication. It was a very stimulating meeting, and was a start toward organization of a Southeastern Section of the G.S.A. a few years later.

Sometime during the preceding winter, I received a letter from John Maxwell of the Princeton faculty (unknown to me at the time) that they had been using my old International Congress pamphlet "Outline of the structural geology of the United States" in their regional geology course; that the existing copies were all worn out, and asking permission to reprint it. I replied that it was far out of date now, and that I would revise it. This was a rather foolish idea, as the job was far bigger than I fancied. However, I still had a great deal of accumulated annual leave that I could use for the purpose. In the end, the project mushroomed into a major enterprise. My review of the Southern Appalachians for the minerals conference was a good starter, however, and I set about giving the same treatment to the rest of the eastern United States and adjacent Canada. (More of this later).

I had criticized the inadequate Survey topographic map of Great Smoky Mountains National Park, and the Topographic Division indicated that they would do something about it. As a start, early in the summer, I started running out the trails in the western part of the park, and the rest of the party continued this work into the summer.

We had begun going to the Episcopal Church services in Gatlinburg. The Episcopalians were a small, but very select and congenial group. For a while they had evening prayer services on Sunday at the Methodist Church, but plans began to be made to build a church of their own, much abetted by Bill Cron, the Public Roads engineer who had his offices in the Park Service building. There were very few men in the congregation; at a meeting of the vestry I was appointed clerk. Most of the Gatlinburgers were either Baptists or Methodists. They were formally anti-alcohol (whatever the parishoners did in private). The Episcopalians were "drinkin' Christians," and so were the Catholics, of which there was an equally small struggling group; as such, we were viewed with suspicion by many of the natives.

In mid-summer, I went to New England at Jerry Hadley's invitation. I rode the New Haven train up the Connecticut Valley to Greenfield, Massachusetts, where Jerry met me and took me to his mother's place for the night, on a hill above the village of Bellows Falls. Next morning we drove north and northwest into Vermont, to a village on the east side of the Green Mountains, where we were to meet Wally Cady and a group of geologists for a field conference. Cady had covered the carbonate terrane of the Champlain lowland west of the Green Mountains for a doctor's dissertation at Columbia some years before. Now, he was extending the work across the Green Mountains into the metamorphic terrane of eastern Vermont for the Survey, out of headquarters in Montpelier. Others arrived: Al Chidester who was studying the ultramafics of eastern Vermont for the Survey, Marland Billings from Harvard, and various assistants. Tom Thayer came in later. Next day, we drove west over the Green Mountains into the Champlain lowland (which I had seen with Longwell in 1931), north to Burlington where we visited Doll the State Geologist, and the offices of the Vermont Geological Survey, then east to Montpelier and into the metamorphic terrane. East of the Green Mountains there was a great homoclinal sequence, facing east toward the Connecticut River, all Paleozoic from

Cambrian into Devonian, with a staggering thickness. Thayer's only interest was in the ultramafics, which formed small pods in the sequence, and we spent much time on them.

After the conference broke up, I went with Jerry to the Connecticut Valley, for the night at one of the towns. Jerry had covered the Mount Cube quadrangle on the New Hampshire side for his doctor's dissertation at Harvard, and only the New Hampshire part of the quadrangle had been published. He had made a start on the Vermont part (about a third of the quadrangle), and wanted to finish this part for publication also. We spent some time going over it. All the rocks in the quadrangle were metamorphic, but had been divided into various Paleozoic formations. After our visit to his area we went south, stopping off for awhile at Dartmouth College to see John Lyons of the geology department; I saw the famous murals on the walls of the library, which had been painted by a Mexican artist. Then I took the train back to Gatlinburg, feeling that I had gained a view of some of the problems of New England geology.

I had been home only a short time when, in August, I was off again by plane west to visit J. Fred Smith in the Sierra Blanca area. We spent nearly a week going over the area again, for which field work was about completed, then started off by car for Denver. On the way, we stopped off at Frijole, where Norman Newell and a big crew were working on the Capitan reef and associated beds, with funds from the Humble Oil Company. My Professional Paper on the Guadalupe Mountains had only been out about a year, and Norman had moved in to flesh out the details. (There were some bitter aftermaths of this, for me). Professor Bucher was a visitor at this time, too, and all of us climbed the trail up the face of the reef escarpment to the plateau on top.

After leaving Frijole, Fred and I drove north across eastern New Mexico for a night in Santa Fe. Next day, we stopped briefly in Taos and had lunch with Ted and Charlotte Cabot, then to Eagle's Nest, high in the Sangre de Cristo Mountains, where Fred's father and mother had built a resort. We spent several days there. Fred had to stay on for awhile, so I went by car to Denver with Mabel Claire and the children, and spent the night in their home.

After a few days in the Denver office, I flew to Los Angeles and had a long visit with Levi Noble at Valyermo. This time, we went over the whole San Andreas fault zone from Soledad Pass to Cajon Pass, and I made detailed profiles of the critical exposures adjacent to the fault, which later appeared in one of Levi's publications. The trip ended in San Bernardino, after which I took a bus to Los Angeles, and a plane home.

That fall, I accomplished little on the Smokies project. Most of my time in Gatlinburg was spent on the new work on U.S. tectonics, and the G.S.A. meeting in El Paso was coming up in November.

In November, I set out for the west again by train, first to participate in the pre-meeting field trip, then to the G.S.A. meeting. I was scheduled to lead the first part of the field trip on the pre-Permian rocks, which assembled in Marfa. There were many field trips in West Texas, with assembly points in the different towns, so Alpine and Marathon, which would have been more convenient for us, were spoken for. The field trip committee had their troubles, for they found after all arrangements had been made that this was to be the first week of the deer season, so that the area was swarming with other outsiders. All transportation was by individual automobiles, so there was a long unwieldy caravan. At Marathon, we looked at various famous localities, such as the Haymond boulder beds, and the picnic grounds south of Marathon.

We next went to Van Horn and made several stops in the area, then across the Diablo Plateau to the Hueco Mountains and into El Paso, where my duties on the trip were over. Another two days were spent in the Sacramento Mountains in New Mexico to the north.

Back at the convention, I presented the deliberations of the Publications Committee to the G.S.A. Council, and heard many interesting papers. I was assigned a room with Norman Newell, with whom I was on guardedly friendly terms. On the whole, though, the meeting left me with a taste of bitterness, which is hard to pinpoint now; I evidently felt that after my years of work in West Texas I had not been sufficiently recognized. To add salt to the wounds, when I got back home I received a letter from the local committee, which was nothing but a mimeographed form letter of thanks to all who had taken part in the arrangements.

At the close of the convention I stayed on for a day in El Paso to attend a meeting of the directors of the American Geological Institute, to which the G.S.A. had appointed me as their delegate. The A.G.I. was just getting organized, and the results of the meeting were rather inconclusive. I had foolishly agreed to take on chairmanship of their publications committee, thinking it might be something like the work I had done for the G.S.A., but it was something else. Earl Ingerson was pushing hard for a series of geological abstracts, with which I had little sympathy. I found that there were no operating funds, like those which the G.S.A. had so liberally supplied us, and I had no funds of my own to carry out what was expected of me. Early in the coming year I got out of it all, and resigned from my part in the A.G.I.

I was glad to leave El Paso and go on by train to my next stop of the journey -- to Los Angeles, where I met Foster Hewett in Pasadena. We set out for another long trip into the desert -- this time in far better weather than we had had the preceding January. We stopped again in Baker, and made several visits. We went with Lloyd Pray to Mountain Pass to the east, where he was starting a project on the radioactive minerals which Foster had discovered there; and with Don Kupfer to the Silurian Hills to the north, where he was doing a dissertation job on its complex thrust faulting. Following that, we went north to Shoshone, a little village east of Death Valley, which Levi Noble made his headquarters. Levi Noble met us there, and next day we looked at his "chaos structure" and other sights in the southern part of the Death Valley area.

I returned with Levi to Valyermo for a few days, and spent the time making drawings of the geological scenery around the ranch (some of them later published by Levi). Then I went home to Gatlinburg by plane.

The year 1950 was as complex and eventful, or more so, as the year 1949. Not much was accomplished by me on the Smokies project. I did a little work in the spring on the Pigeon Forge quadrangle to the north, and in the fall on the Kinzel Springs quadrangle to the west. Charlie Tooker, who had worked as assistant to Jerry Hadley in the spring of 1949, took on as a master's thesis problem the geology of Miller Cove with its Rome and Shady Formations, and worked on it in the summer of 1950. Bob Neuman and I went out with him frequently to keep him straight. Bob Neuman himself, after the purgatory in the wilderness of the Silers Bald quadrangle in the spring of 1949, went on to the subjects and kind of country for which he was better fitted -- the geology of the Ordovician in the north part of the project area -- in the coves or window areas, and in the Appalachian Valley beyond. I went out with him frequently.

I spent much time indoors on manuscript reviews -- Clyde Ross's report on Glacier National Park, Herman Ferguson's report on the Del Rio barite district, Levi Nobl's manuscript on the Pearland quadrangle, California, and shorter manuscripts by Sydney Furcron and John Rodgers on southeastern geology. Throughout my time in Gatlinburg I had no typing help (except for Margaret Gower in Knoxville), so all this required much time.

Many trips were made to other parts of the Southeast, usually in company with various others of our party -- Jerry Hadley and Bob Neuman, and also Bob Laurence and John Rodgers. Fortunately, there were no long, time-consuming trips to the far west that year.

(1) In March, we all went to Shelby, North Carolina, where Bob Yates, Bill Overstreet, and others, were mapping an area in the Piedmont for the Survey. Ernst Cloos of Johns Hopkins was also along.

(2) In May, many of us attended a field excursion of the Kentucky Geological Society into southwestern Virginia, where Byron Cooper demonstrated his work on the complex stratigraphy of the Middle Ordovician rocks.

(3) The same month, we went to Ducktown, Tennessee, where the geology was demonstrated to us by the geologists of the Tennessee Copper Company, including some time in the underground workings of the mines.

(4) In June, we were on a long trip into the Blue Ridge of central Virginia with Robert Bloomer and Harry Werner working for the Virginia Survey, starting at Natural Bridge, and going as far north as my old Elkton area. We stayed several nights at a wonderful old ramshackle hotel at Afton on the Chesapeake and Ohio Railroad just east of Rockfish Gap.

(5) The same month I was with John Rodgers and Herman Ferguson in the Mountain City area. John was working on a geologic map of all of eastern Tennessee, and wished to run out the traces of the faults east of Mountain City. I brought along our party's jeep, which was a great help in going over the mountain trails. On the way home, I drove south and across the Grandfather Mountain window of which we had heard much.

(6) In July, we went to Chatsworth, Georgia, at the invitation of Sydney Furcron, Assistant State Geologist, to look at the Chatsworth quadrangle that was being mapped by Arthur Munyan of Emory University for a thesis at Cincinnati University. We looked mainly at the Middle Ordovician rocks.

(7) In August, John Rodgers, Jerry Hadley, and I went to Cartersville, Georgia, to be shown around by Tom Kesler. (At the last minute, Bob Neuman wanted to come too, but I felt that too much time had been taken by everybody on trips, and told him to stay home; this caused resentment). Kesler had worked for a long time for the Survey

on the Cartersville area, but had left to go with a private company, operating out of Cartersville. He was a maverick, who had developed many "wrong" ideas while working alone, on the stratigraphy and structure. He denied the existence of the Cartersville (= Great Smoky) fault, and correlated the metamorphic rocks east of it with the non-metamorphic rocks to the west; he had peculiar ideas about the Weisner, Shady, and Rome Formations. On the other hand, his analysis of the geology in the ore pits themselves was most impressive. The Survey had just published his Professional Paper on the Cartersville area, with some misgivings. We went around and around on the problems, fortunately in a most friendly manner.

(8) In December, Jerry Hadley, Will Nelson, and I went again to Georgia in the cold, at the invitation of Furcron, to go over the work of another student at the University of Cincinnati, on the Murphy Marble Belt. This was being done under the direction of John L. Rich, who was along. He was a stiff-necked gentleman whom we saw a good deal of during our time in the southeast, as he was always down for some reason or another. He had just published two articles on fundamental subjects -- on a theory of sedimentation and on a theory of orogeny -- and he was continually applying one or the other of his theories, or both, to specific field problems, usually with disastrous results. We looked at the marble belt from Murphy, North Carolina, south to the marble quarries at Tate, Georgia. All of us were glad to go home and get out of the cold; it snowed during our return trip.

We also had a succession of visitors to Gatlinburg, and to the Smokies:

(a) In April, after the Shelby visit, Ernst Cloos and some companions came to the Smokies and we showed them the geological sights and problems. We felt honored to have the great Ernst Cloos visit us, and he was very sympathetic with what we were trying to do.

(b) At almost the same time, a group from Princeton came through on their spring field trip -- a dozen or so students, led by John Maxwell and Paul McClintock. We decided to have all of them to dinner at our house -- the Cloos party, the Princeton faculty, and the students. It was a big undertaking but it came off very well. We gave them ham, baked beans, and cole slaw. (Years later, I have met men in far places who said they were among the students who were there).

(c) In June, after our Virginia trip with Bloomer and Werner, we returned the hospitality by having them come down and look at the Smokies with us.

(d) At almost the same time, Jimmy Williams and Mackenzie Gordon arrived to have a look at the Carboniferous of the northwestern Great Smokies foothills with Neuman and me.

(d) In July, we were hosts to K. J. Neuvonen, a Finnish geologist whom Earl Ingerson had recommended, who was to collect specimens of the Ocoee rocks for fabric analysis. We went with him to various places in the mountains and foothills to collect rocks. (He later sent us a report on his petrologic work, but it was inconclusive, and not of much use to us).

(e) In December, we had a visit with a group from the Speleological Society, and went with them to the limestone caves in Whiteoak Sink and Tuckaleechee Cove.

In March, Helen, Gertrude, and I drove in our car to Westtown, Pennsylvania, to a Quaker school where we planned to send Gertrude the following year. She was finishing her first year in High School in Gatlinburg, and the possibilities for an education there were getting slimmer than in grade school; we decided that the best thing to do was to send her north. It was a pleasant visit, and more than confirmed our desire for her to go there. We also stopped to see Jessie and Bertha in Baltimore, and in Washington. Sometime during the trip, I went up for a day to Princeton to confer with Princeton University Press about my book manuscript on tectonics, and spent the night at Arthur Buddington's. All this city activity was hard on my nerves and digestion; we went home on the Blue Ridge Parkway, and a day in the quiet of the mountains did me a world of good.

Our car was now nearly 12 years old, and while it was still an attractive little vehicle, it was beginning to show many signs of age. Cars had been hard to get in the years following the war, but they had become plentiful again, and we decided to get a new one. We bought a Plymouth sedan at an agency in Knoxville; it was not as glamorous as our Ford convertible, but it was a sturdy vehicle which served us well during the next 5 years. Delivery was delayed by a strike in the Chrysler plants, so we did not finally have it until mid-summer.

In November, Helen and Gertrude went north with Bob and Arline Neuman to Washington. Bob was going to the G.S.A. meeting, and Helen for a family visit. Also, Gertrude had sprained her knee again, and

Helen took her to Dr. Hall, the bone specialist. Returning, she brought back with her Katherine Copley (the aunt who had lived with us for awhile in Arlington), with the hope that she would come and live with us permanently in Gatlinburg. She stayed with us through December into January and then went back to Washington. She was not as well as we had thought, and she died of cancer in 1951.

During the year 1950, my records show that I took 92 days of leave. Most of this was spent on the work on "Tectonics of Middle North America", and by the end of the year I had completed a manuscript and figures for all of the eastern part of the country up to the front of the Rocky Mountains. The much more difficult part on the Cordillera to the west was still ahead. About this time, Survey duties became so pressing that I could not continue for a while. Princeton Press was willing to publish what I had done, so I sent them the manuscript in December. (It was just as well that I quit when I did, as my knowledge of the Cordillera was slight, and much was yet to be learned about it anyway, as became evident in later decades. During the next year, I spent some time researching the Colorado Rockies and the northern Cordillera, but never got far into manuscript writing).

Steve Oriel (who had done the Hot Springs window for a dissertation at Yale) worked with Jerry Hadley in the fall of 1949, but left to go with an oil company in March, 1950. He was replaced by Willis (Willy) Nelson from Washington State, and he and Jerry made long camping trips into the mountains. Willy was a resourceful guy, good at radio and an ardent speleologist. Unlike me, Jerry was making great progress mapping the eastern part of the Great Smoky Mountains. He was not encumbered as I was with many outside duties, and the geological "texture" of the eastern Smokies was more "open" than in the Gatlinburg area, where all the structural and stratigraphic features seemed to converge.

The Korean War had broken out in mid-summer, and our troops were again in combat overseas. It was difficult to know what was ahead, and the status of our Smokies project was questionable. Toward the end of the year, Charlie Hunt asked us to summarize what we had done and put it away, to prepare for emergency work (one reason for my abandoning "Tectonics of Middle North America" half completed).

During the first few months of 1951, we did something to activate Charlie Hunt's directives, but eventually bogged down and went on with things about as before. I spent some time writing up our field excursions, and wrote a history of previous work on the Great Smoky Mountains, of which there were some interesting but very scattered records. Partly because of the Korean War, the Bureau of Economic Geology in Texas had become interested in the mineral deposits of the Van Horn area, and especially copper. I made a compact with Pete Flawn of the Bureau to write a joint report on the area. This seemed like a reasonable personal contribution to the Survey's involvement in the war effort; besides, it would give me an opportunity to write up all the work I had done on the Van Horn Precambrian rocks 12 years before.

Also, I received for review Foster Hewett's long manuscript on the Ivanpah Quadrangle, California, and I spent nearly 3 weeks on it, hiding away in a workroom in the attic of the Park Service building. It wasn't very well written or well organized, and I went over it in detail. Unfortunately, Foster didn't take criticism gracefully, and acted as though I had it "in for him", so the whole effort ended sadly.

In March, another Southeastern Minerals Conference was held, at Emory University in Atlanta, and all of us went. (There had been a second conference in Kentucky in the spring of 1950, which I had not attended). I gave another paper on tectonics in the Southeast, which presented some good ideas and some half-baked ones; fortunately, I never worked it up for publication.

After the conference, Furcron conducted a field trip into northern Georgia, and there was a large attendance the first day. John Rich was along, with his usual perverse ideas. The party broke up near Toccoa, at the north edge of the state, but there was more to see, so Jerry Hadley and I stayed on. Furcron took us for the night to a disreputable motel in Toccoa, where I have never before or since spent a more miserable night (there were plenty of good hotels in the area). Next day, we went to the summit of Brasstown Bald, in an area that had been mapped as Carolina Gneiss, but which was obviously metamorphosed Great Smoky Formation. In the evening, we put Rich on the train at Athens, Tennessee, and went home ourselves.

Late in April, I started off on another long trip to the west. I took the train to Van Horn, where I was met by Pete Flawn and an assistant, and we spent nearly a week going over the Precambrian rocks, preparatory of writing our report.

Then I went by train to Los Angeles, and by another train to Lancaster, where Levi Noble met me and took me to Valyermo. We had a pleasant time together for a few days. One day, we went to the Cajon Pass area, to look at the work done by Robert Yerkes on the crystallines for a master's thesis at Pomona College. But then a crisis developed in the Noble family -- some relative was on the point of death in the east -- so the rest of the visit had to be called off. This was to be my last professional visit with Levi, although I saw him several times later. The pressure of Great Smokies work prevented me from making any more long trips to the west. Fortunately for the San Andreas fault project, Harry Ferguson was available. He was about the same age as Levi and his wife had just died, leaving him at very loose ends. He came out and spent several months at Valyermo and gradually became his old self. He and Levi worked up the Valyermo Quadrangle for publication, and also a general account of the fault zone for "The geology of Southern California", being prepared for the G.S.A. meeting in Los Angeles in 1954. These two reports, and that on the Pearland Quadrangle, were all we ever got out of Levi on the San Andreas project.

Levi and Dorothy drove me to Los Angeles, and arranged for me to spend the rest of my time in southern California in Claremont, where I was a guest of John Shelton of Pomona College. I spent several interesting days in the field with John -- to the Palos Verdes Hills to see the little inlier of Franciscan metamorphics, with lunch at Knott's Berry Farm on the way back; and to the south slopes of the San Gabriel Mountains above Claremont to see the older metamorphics. One afternoon, I talked to A. O. Woodford's class at Pomona College.

I took the Union Pacific train at Ontario for Denver, and spent the day watching the scenery go by in the Mojave Desert. Next morning, I woke early and saw Great Salt Lake at sunrise -- my first view of it. That day, I had my first look at southern Wyoming, and got into Denver in the evening, where Robby Robinson met me and took me to the Olin Hotel in downtown Denver.

I spent a week or so in the Denver office, and one evening I talked to the Rocky Mountain Association of Geologists, giving the same paper as the one at Emory University. I was ready to go home, but Jim Gilluly persuaded me to spend another week with him in Utah and Nevada, and see more country. The Intermountain Association of Geologists was holding a field conference in the House and Confusion Ranges in western Utah, assembling in Lemhi. We drove out in the jeep that Jim was to use in the field that summer. It was an interesting trip, but uncomfortable from the dust of the long line of cars during the daytime, and nights sleeping in the open in bedding rolls; I did not have an air mattress and the ground was very hard.

At the close of the conference we went west with the Union Oil Company crew to Ely, Nevada, where they had a big apartment, and there was a welcome chance for a bath. The Union people were John Hazzard, Bill Moran, and Bill Easton, and various assistants; they were working on eastern Nevada, during the brief flurry of oil exploration there. Leaving Ely with the Union people, we went west through Eureka to Cortez, with Jim demonstrating the geology along the way. They left us there, and Jim and I went on alone together to Battle Mountain.

Next day or so, Jim showed me the geology of the Battle Mountain area, and then the northern part of the Shoshone Mountains (Mount Lewis quadrangle), where Jim was starting a field project. We then drove west through Edna Mountain and Golconda to Winnemucca, where he left me. I had obtained a good view of the complex geology of north-central Nevada.

The Western Pacific Zephyr came through Winnemucca at 11:00 P. M. that night, and I took it back to Denver. On arising, we were just leaving Salt Lake valley, and all day we went across the plateaus of Utah and Colorado, and through the Rocky Mountains. It was a thrilling experience (I have done it several times since). Finally, we went through the Moffat Tunnel, coming out in a snowstorm, and down the Front Range into Denver.

Charlie Hunt met me at the station and took me to his place for the night. The train got in at 6:30 P.M., and I had not obtained an evening meal on the train, expecting to eat in Denver. But once home, Charlie settled down to talk, and I swore to myself that I would not ask for his hospitality, empty though I was. He was full of a grandiose scheme to convert our Smokies project into some kind of regional review

of southeastern geology. Happily, nothing came of this; shortly after I left, the Chief Geologist diverted him to some kind of administrative chore, and Robby Robinson took over as Acting Branch Chief, and we were back again to a more normal course.

It was a delight to be on the train for home; when we were leaving Chicago, I had several long drinks in the club car, and had quiet satisfaction in knowing that my long trip was over. Back in east Tennessee it was hot and humid, and it was hard to remember that I had seen snow in the Rocky Mountains only a few days before.

In early June, shortly after my return, Jerry Hadley and I drove to Shelby, North Carolina, where the Yates-Overstreet party were finishing their project. The high point of the trip was an excursion into the Kings Mountain belt east of Shelby, with its display of complex formations, and a visit to Kings Mountain Park, where there were exhibits commemorating the Revolutionary War Battle of Kings Mountain.

I spent most of the summer marking my geological work of 1938 of the Van Horn Precambrian on enlarged air photographs. The Texas Bureau had promised to convert these into a geological map of the area, for our projected report on the Van Horn Precambrian.

Charlie Hunt had decided to have Bob Yerkes work with Levi Noble on the San Andreas fault project in California, but assigned him for the summer to the Smokies party, where he worked with Jerry Hadley in the eastern part of the mountains. (The deal with Levi fell through, but at the end of the summer Bob went back to California, where he has continued his geological work ever since). That summer, also, Bob Neuman hired as field assistant Andy Griscom, a promising graduate student from Harvard. (Like Yerkes, I have seen Griscom many times since that summer; they are both now at Menlo Park).

In July, Robby Robinson visited the party for a few days in his new role as Acting Branch Chief, and in August we had a visit from Gerry Richmond, who went over with us the Pleistocene deposits of the mountains. His perception of these deposits was most surprising, and he gave us much help on the Pleistocene problem.

During the summer, Helen and Gertrude rented rooms to tourists in our house every night. Gatlinburg was crawling with summer visitors, and the hotels and motels were not sufficient to hold them all, so they were happy to refer the visitors to us and to others willing to

handle the overflow. Helen and Gertrude wanted to earn enough extra money to pay for clothes and other things that Gertrude would need during her coming school year at Westtown, and made \$500.00 or so. (Next year, enough more motels had been built so that the demand for extra rooms was over -- happily for me).

In September, Helen, Gertrude, and I went north in the Plymouth to Westtown, and saw her installed in her new school. After that, we went up to Mamaroneck for a visit with Robert and Clara, and one evening saw "The King and I" on the stage in New York; we also went down for a day for a visit with Edward and Grace in Morningside Heights, and it brought back memories to Helen of how she had lived in the neighborhood as a child long ago. From New York we went for a visit with Dorothy and Jerry Weikel in Bethlehem, Pennsylvania. Jerry was a cousin of Helen's and a vice-president of Bethlehem Steel Company. They had a fine country home in Saucon Valley south of Bethlehem, and when we arrived the National Golf Tournament was about to finish up at a nearby country club. Jerry took us over to see the final match, with its hoopla of press and television coverage. We then went back to Gatlinburg, and to a home that seemed empty without Gertrude. Gertrude had decided that from now on she would be called "Trudy", so as to avoid such nicknames as "Gertie", and I shall refer to her by this name from now on.

Shortly after our return, Jerry Hadley had the Smokies party (especially Bob Neuman and me) on a field trip to the eastern part of the Great Smoky Mountains, and especially the work he and Yerkes had accomplished during the preceding summer. Things had really begun to shape up in his area, and he was able to draw definite conclusions about matters that had always eluded us before. I saw lithologies like some I had seen farther west, whose stratigraphic and structural relations had always been elusive.

After the trip to the eastern Smokies, Bob Neuman gave me a written report on what he and Andy Griscom had done the past summer in the western Smokies. I was much incensed by it, because he included a lot of half-baked speculations about Great Smokies geology which ran counter to my own ideas. Bad feelings had been growing between us, for he had become increasingly aggressive and was forever pushing speculations which I felt had little basis. I sent it back to him with scathing

comments. It was several years before he quieted down again, and he got off his high horse, so that we could become friends again.

Despite this quarrel, in October Bob and I went together to Columbia, South Carolina, to attend the annual field conference of the Carolina Geological Society. This society existed only for their annual field trip, and had no other meetings. We looked at rocks along the southeastern edge of the Piedmont, and at the adjacent overlapping Cretaceous. On the way home, the two of us looked at the little metamorphosed or deformed rocks of the Carolina Slate Belt at several localities east of Charlotte. At the meeting, at Bob's instigation, I offered an invitation to the Society to hold their next field conference, in the fall of 1952, in the Great Smoky Mountains, which was accepted. (This proved to be more of an undertaking than either of us had bargained for, as next year's record will show).

Almost as soon as our return, I drove south in our jeep station wagon with Kefton Teague of the Survey's Knoxville office, to Chatsworth, Georgia, again at the invitation of Sydney Furcron, to look at the Ocoee-like rocks of northern Georgia. We saw some peculiar rocks along the mountain front north of Chatsworth, including a remarkable boulder conglomerate, and then went east through Ellijay to Dahlonega, looking at geology on the way.

I had decided that fall to do Great Smokies field work on a new basis, and to really accomplish something after several years of virtual inaction. The weather was too uncertain for continuous field work, yet there were many fine days right through the winter, when I had previously stayed inside. I decided to do field work all fall, winter, and spring, but to go out only on the good days, using the bad days for the inside work, of which I had too much. The belt of rocks north of Wear and Tuckaleechee Coves, between there and Chilhowee Mountain, was very confused and made little sense, and I was anxious to go over it in detail. Ferguson and Swingle had been over the part in the Wear Cove quadrangle several years before, but although they had made many traverses, they had not made an acceptable geologic map. Ferguson had at last sent me his field sheets, and I was able to get started on the work. I started at the western end of the belt, and by the end of the winter had covered only a few square miles, but I had results of permanent value.

During inclement weather, I stayed indoors and started writing the report on the Van Horn Precambrian, and finished my part of it the following April.

In November, I went with Bob Neuman to central Tennessee, to see the work being done by Louis Conant on the Chattanooga Shale, which was being carried out because of its uranium potential. We ranged widely over the area, looking at Chattanooga outcrops in many places. As I recall, the big problem was that Conant thought he had found a residual soil on the Ordovician rocks just under the Chattanooga, and that we concluded the "soil" had another origin.

In November, also, I attended the meeting of the G.S.A. Council in Detroit. I was now to be on the Council for a three-year term, and so was Glen Jepsen, my old companion of the Publications Committee. After I had finished on the Publications Committee two years before the G.S.A. had put me on the Penrose Medal Committee. This had not involved any meetings of the committee members, merely extensive correspondence. Now, I was back on the circuit of attending meetings on G.S.A. business.

In December, I spent nearly a week with Bob Neuman in Maryville, where he was working on the Middle Ordovician rocks along the northwestern fringe of the mountains. After Christmas, all of us attended the A.A.A.S. meeting in Philadelphia, where there was to be a program of papers on the Southern Appalachians. I believe that several of us gave papers, but I can't recall them now. There was an evening get-together at Bryn Mawr College; we went out from town on a bus, and it was my only view of the place. On the way back from the meeting, I stopped off briefly in Washington on business.

Trudy was away at school, and decided to stay in the north at Christmas time, so Helen and I had a lonely Christmas.

The year 1952 was another complex and busy one, with only moderate amounts accomplished on the Great Smokies project, and much time spent on other things.

In March, Hal Malde arrived to work with Bob Neuman, and late in the summer Richard Goldsmith and Warren Hamilton were assigned to the Smokies project. Goldsmith was to work with Jerry Hadley in the eastern part of the mountains, and I assigned Hamilton to the Richardson Cove quadrangle that I had been over in reconnaissance the year before, and which still had many mysteries, the solution of which would go far toward clearing up the problems of the foothill belt of rocks.

I finished my work on the Van Horn report in April, and the whole manuscript (my part and Flawn's) was reviewed, corrected, and approved, and sent off to the Bureau in May. The Bureau was really serious about prompt publication; I read proofs in September, and the work was published early in 1953.

I reviewed some manuscripts for G.S.A. and A.A.P.G. -- one on Appalachian geomorphology by Ver Steeg, and one on "Appalachia" by Ver Wiebe. Both were terrible -- badly written and with half-baked ideas. I gave them "the works" and this must have killed them, as they happily never saw the light of day. (I heard later that Ver Steeg had died shortly after, and maybe my unfavorable review helped to kill him, but I have no regrets).

A new faculty member of the geology department at the University of Tennessee was Fritz Kruger, and Helen and I liked him and his wife at once and saw much of them. He started a once-a-month geological seminar at the University which was a great stimulus to geology in the Southeast and drew a large attendance; people came up from Georgia and other places. Our group took part in the meetings, and were often on the program.

In April, I attended the G.S.A. Council meeting in New York City, and talked to Walter Bucher's class at Columbia University. On the way home, I stopped off in Philadelphia and went out to Westtown for a few day's visit with Trudy.

The Southeastern Section of the G.S.A. was organized formally at last, after several years of planning and discussion, and Clarence Moody, a Louisiana oil geologist, was elected the first chairman. For the following year, I was put up as chairman; I had protested that my time in the Southeast would soon be over, but this made no difference to my friends. The first meeting of the new section was held at the Hotel Roanoke, in Roanoke, Virginia, and all of our party attended. There was a dinner meeting of the new officers, at which we discussed many plans. After the meeting, Byron Cooper took us on a field trip along the Pulaski fault. Walter Bucher was along, and was his usual enthusiastic self.

Aunt Jessie had had a cataract operation, and in March Helen decided to go north to Baltimore to care for her and Bertha, so I was left alone in the house for several months. This was to be the first of her many errands of mercy, which took her away from home and from me for considerable periods during succeeding years. Later on, I discovered frozen foods which were just coming into the market, and did very well by myself. But this time, the food situation was not so good; she left me a roast of beef, from which I sliced pieces every evening and made a sort of stew.

Trudy was home for the summer, but did little except lie around and read, much to her mother's distress. We decided to repaint her room (the downstairs back bedroom). After it was done, the rest of the house looked shabby, so I did the hall, then the bathroom, my study, and the kitchen. There were many cracks in the wood paneling, which made us a prey to mice and cockroaches, so I calked all of these with plastic wood. Then, I went on and gave the same treatment to the stairs and the two upstairs bedrooms. I worked on this evenings and weekends, and it was late October before it was all done. In the meantime, we had had asphalt tile laid on all the floors. Once done, the old house was revitalized, and very attractive.

We had several notable visitors. In April, while Helen was away, we had a visit from J. V. Harrison and his wife Janet from Oxford University. I was not crazy about the proposed visit, but we received virtual orders from the Chief Geologist to entertain him. The rest of the party would have nothing to do with it, and said it was my problem. I went down to the bus station to meet them, not knowing what kind of people to expect. I was much surprised to find an older gentleman, looking much like George Bernard Shaw, instead of some callow young Britisher. The rest of the party were entranced with the Harrisons. and Jerry Hadley made a big play for them. We showed them the geological sights of the Smokies. I had to leave to attend the G.S.A. Council meeting, but Jerry Hadley and Bob Neuman were happy to entertain them a few days more. (I saw Mr. Harrison and his wife several times during my subsequent visits abroad, and he and Janet entertained us in Oxford in 1964; I was saddened a few years ago to hear of the death of both of them).

In September, I had a phone call from Pat Graves, an unknown, who said she was with Life Magazine, and was researching for a series of articles on the earth sciences for the magazine. She said she was referred to me by the Washington office of the Survey, and wanted to come down and see me. Once again, I didn't know what to expect when I went to meet her at the Mountain View Hotel. I was delighted to find a very beautiful and personable young lady. We took her to dinner that evening to the Buckhorn Inn, out in the country, and on subsequent days I took her on several long trips -- to the western part of the mountains; south to Whiteside Mountain in the North Carolina crystal-lines (with Helen); and north to the Thorn Hill section of Paleozoic rocks. I hope I gave her some ideas about geology; when she came she supposed every hill was an anticline. She was back again in late November with a Life photographer, but they did not accomplish much that time, because there had just been a heavy snow and all the electric power in Gatlinburg was out. The articles came out the following year, and were well received by the public.

Much of the rest of the summer and fall were spent in preparation for the Carolina Geological Society excursion in November, and most of the burden fell on me. Bob Neuman (who had originated the idea of the excursion) was spending the summer in the west, working in Jim Gilluly's party in the Mount Lewis quadrangle, Nevada; I thought that contact with another Survey group might help cure him of some of his bumptiousness, and I guess it did some.

During the summer, I worked with Jerry Hadley on a text for the guidebook. Then there was a problem of how to get it printed, along with its maps and figures. I wanted to have it issued as a Survey circular, but met with stone-wall opposition from the Branch in Denver, and especially from Robby Robinson. The Denver people were not amused by our plans for the excursion, but I had been committed and was obliged to see it through. There was quite bitter feeling between me and Robby, the start of my alienation with the Denver office group. The Denver office finally consented to make masters of the pages for mimeographing, and the Park Service in Gatlinburg agreed to run off the edition. The Branch of Technical Illustrations in Washington agreed to draft the geologic map in form from which prints could be run off, so we were on our way.

I had Seahorn & Kennedy, the Knoxville blueprint outfit, make the ozalid copies of the map. I had our local printing firm print an attractive cover, and I bought binders for the guidebooks. All this had to be at my own expense, to be recovered from the field trip fees (they were, with a little to spare).

Then, there were a host of other arrangements -- a registration place and dinner at the Greystone Hotel, a special account at the bank and a night depository key, contacts with the Tennessee Highway Patrol and the Park Service rangers for escort service, and a thousand and one other little details.

The big event finally came off early in November, and 210 people turned up for it. There was a dismayingly long line of private vehicles, and quite a job parking them at the stops, with much help from the escort service. Stephen Taber, the grand old man of South Carolina geology and his wife rode with Helen and me in the lead car. I believe we took two days, the first day on the north side of the mountains, the second day on the south side, where Jerry Hadley did the leading. At the last stop, everybody cheered, and we knew it had been a success.

Almost as soon as this was over, the G.S.A. met in Boston, and I had to go north to the Council meeting. I missed all the field trips because of this; before the meeting, there was an excellent trip across New England which Jerry Hadley attended, and which I was sorry to miss.

During the meeting I had a talk with Bill Johnston of Foreign Geology Branch. An International Geological Congress was scheduled for Mexico in September, 1956, and they wanted to have a geologic map of the country ready for it. My geologic map of northern Mexico, compiled in 1940, had been published in Mexico in 1948, and had been a great success. Why shouldn't I go to Mexico under Foreign Geology auspices, and help the Mexicans assemble a map of the whole country? Superficially, it was an intriguing idea, and I consented to go when the Smokies work was over. (For further developments, see the next few years).

I came home feeling much chastened, and was happy to drive back to Gatlinburg by a roundabout road through the park, and soak up some of the peace and quiet of the forests and mountains, and the mountain air.

In January of 1953 we had another visit from Charlie Hunt. He had finished his administrative chore for the Chief Geologist, and was now back as Chief of General Geology. It turned out not to be a very friendly visit; he seemed to feel that the Branch had gone to pot in his absence, and he had blood in his eye. He said we had spent half a million dollars on the Smokies project with nothing to show for it. He talked over plans for winding up the project, and scattering us to the different main office centers. He said that we had worked long enough on pure research, and that on our next assignments we would be penalized by being put to work on down-to-earth economic work. He said many other things with great authority, as though he was passing on to us the prevailing Survey policy; later, I came to realize that he was merely expounding his own ideas and **prejudices**. There was at least one constructive result: We all agreed that, instead of planning one big coordinated report on the geology of the Great Smoky Mountains, we should each write a separate report on the areas we had covered. This made much sense, as we each had our own fund of information, and in some cases quite different interpretations. For some reason, Charlie took a dislike to Warren Hamilton, and told people after he left us that he was going to haze him and make things hard for him, until Warren was thoroughly humbled. (He never was).

Much of our difficulties with the project actually came from Charlie's own erratic direction. Making me "geologist in charge of tectonic investigations" was a blow to my part in the project, as it took me away from it for long periods into other parts of the country. And his directive at the end of 1950 to wrap up the project and put it away had caused us many months of delay. The only tangible result of this "war work" had been my report with Flawn on the Precambrian of the Van Horn area, which was now published.

Charlie went on from us to visit the Florida phosphate project, still like a mad bull, and told the men there that their project was a failure too, and that he was going to reorganize it along new lines.

I brooded resentfully for awhile over the visit, and finally wrote out what I felt and took it to Washington and read it to the Chief Geologist. He received it with little comment. I found out afterwards that just before my visit, Charlie had gone on a rampage and had left the Survey in a shower of sparks, insulting the Chief Geologist and everyone else. He had blown his top over the phosphate work, and had left in anger because he could not get his way. He had gone on to be manager of the American Geological Institute; as usual, his enthusiasm for the A.G.I. didn't last, and in a year or so he was back again on the Survey, chasing a new bubble.

Charlie, in his grandiose plans for revamping the phosphate work, took Hal Malde away from us and sent him off to South Carolina. Hal had been working with Bob Neuman, and this made us short-handed. I appealed to Denver, and they sent down Willy Nelson again. Bob and Willy set about covering the western part of the Great Smoky Mountains, which they completed in about a year.

After Hunt's visit, Helen and I drove south for a vacation with Brookes and Madge Knight in Florida. They had given up everything in Washington and had built a house on Longboat Key, facing the Gulf of Mexico not far from Sarasota. It was a pleasant visit. We went out several times with Brookes in his boat, in the lagoon and in the open gulf, observing the marine life. Helen and I went one evening to a performance in the little Italian-style theater put up by the Ringling Circus people, and we drove south to Venice to visit Percy and Mame Finney (Harry's twin brother). The only sad note was that on the way back when we stopped at a traffic light in the next little town beyond Sarasota, we were "rear-ended" by a carload of drunks. The back end of the car was much battered, and Helen got a whip-lash in her neck which plagued her for the next several years.

Hardly was I back in Gatlinburg before I left on a trip to Mexico. Bill Johnston wanted me to go down and investigate the situation about the project for a geologic map of Mexico, and had found funds in the G.S.A. for the trip, which was supposed to coincide with a meeting of the local petroleum geologists. I went down to Mexico City by American Airlines plane, changing in Dallas. Carl Fries, in charge of the local Survey office, met me at the airport after I got through customs, and took me to a hotel on the Paseo de la Reforma. Next morning, I walked over to the Survey office, which was in a building that housed the other U.S. missions. I went to the opening session of

the petroleum geologists meeting, and made many other visits -- to the offices of Petroleos Mexicana, to the Instituto de Geologia, and many others. I had lunch one day with Everett De Golyer, and with his assistant Jack Dunlap, and one evening I was invited to a dinner at the Marengo Restaurant, attended by various prominent petroleum men from the States, and assorted Mexican politicians (I felt quite out-classed). I met Zoltan de Cserna, a Hungarian expatriate who had gone to Mexico to make a career in geology, and one afternoon called on Mrs. Townsend and her sister, two rich ladies from Gatlinburg who were spending the winter in a pension in the city.

I formed a dim view of Mexico City, compared with the States. There was a persistent and overpowering smell, composed of sewage and rotten garbage. Most of the buildings in downtown Mexico City were leaning at crazy angles, due to settling on the soft lake clays beneath. The Instituto de Geologia building, a glory of the Diaz regime at the turn of the century, was now decrepit and run down, and set in the middle of an enormous slum. I found in talking to Carl Fries that American aid in Mexico looked far different out at the end of the street car line than it did to Bill Johnston in his ivory tower in Washington.

I went home still planning on the Mexican project, but with more awareness of the difficulties, and with misgivings as to what was ahead.

In the spring, I started writing a manuscript on the geology of the Gatlinburg quadrangle, to put my accumulated knowledge into coherent form. I also went over the Ferguson notes, and wrote up summaries of what he and Swingle had found to the west. I also became interested in working up maps and a report on our results of the early forties in northeastern Tennessee, and I made several trips there to fill in details that had been omitted during the earlier work -- first near Damascus, and later in the Elizabethton area.

The new Southeastern Section of the G.S.A. met in Nashville in April. Just before the meeting, Anna Hietanen came down from Washington to Gatlinburg, and all of us went with her to Jerry Hadley's country on the North Carolina side of the mountains. Jerry and Anna kept up a stream of talk about petrogenesis which was over the heads of the rest of us. Helen and I took Anna along with us when we drove over to the meeting, and she tried to talk petrogenesis to me also, which made it hard for me to concentrate on the driving.

The meeting was held in the Hermitage Hotel in downtown Nashville, but I decided to stay at a motel farther out. This was probably a mistake, as it kept us away from people at the meeting, and as the motel accommodations were not as good as I had expected. This was Clarence Moody's year as chairman, and he did everything he could to make the meeting a success. He had Roy Hazzard come up from Shreveport to give a long talk on Coastal Plain geology, and imported Francis P. Shepard from California to give the evening talk on oceanographic work. Helen and I went to the evening banquet, during which I made a little speech as incoming chairman, pushing hard for more geologic work in the Southeast, with some swipes at the Colorado Plateau mentality that was then dominant on the Survey.

Later in April, I went to the G.S.A. Council meeting in New York, and when there went down for a day to Princeton. On the way back, I stopped off in Philadelphia for another visit with Trudy in Westtown, and in Washington to attend a meeting of the Geologic Names Committee, at which it was finally decided to recognize the Mississippian and Pennsylvanian as geologic systems.

During the winter and spring, I continued my field work when I could on the foothill belt north of Wear and Tuckaleechee Coves, and made much progress. In May, Jerry Hadley had all of us (Neuman, Nelson, Hamilton, Goldsmith, and me) over for a view of what his party had been doing in the southeastern part of the Great Smoky Mountains. We spent several nights at a cabin on Ravens Fork, and another night in Waynesville.

Jerry had decided that it would be best for him and his family, with a growing girl, to move to Denver, so they left that summer. From then on, Warren Hamilton was the only one with me permanently in the Gatlinburg office. However, Jerry was back in later seasons for field work, and Bob Neuman came in occasionally from Maryville.

I spent the summer on the northeast Tennessee material, in the Public Roads drafting room in the basement of the Park Service building, plotting the geology on metal-mounted sheets, and then drawing a whole series of cross-sections, 36 in all.

I wanted very much to bring to a final conclusion our pre-War Sierra Diablo work, and at the end of the War put in official requests for a report on the fossil collections by the Paleontology and Stratigraphy Branch, and to the Topographic Division for better topographic base maps. Nobody paid any attention to either request, although I repeated it several times, and that summer in disgust I wrote to the Chief Geologist requesting that the project be declared officially abandoned. This sort of thing "wasn't done". Many good projects of the Survey in the past had not been finished, but had just been allowed to wither and disappear, with nobody saying anything. The Chief Geologist replied that NO, we can't abandon the project, and he ordered the paleontologists to get to work at once on the fossils. Pres Cloud was chief of the Branch at the time and huffed some, but he and his crew got to work, and in about a year furnished me with a set of fossil lists. (The topography was another problem, which was solved eventually, after a fashion, but it was nearly 10 years before I could find time to settle down to report writing).

Trudy had begged us to let her spend the coming summer at one of the Quaker "work camps", where a group of young people worked together to improve some benighted area. There was to be one "work camp" in the Cherokee Indian Reservation in nearby North Carolina, and I hoped that she would go there, but to my disgust they assigned her to Richmond, Indiana.

She was home from school about a week in June, and then we put her on the train for Richmond. She wrote us very long letters about hers and their activities. They were remodeling and redecorating houses in a negro slum in Richmond (an area I had not known about when I was there as a boy). The camp group was a mixed bag, many nice straight kids, and some wild radicals. Some of the girls among the latter dated the local negro boys, and it became clear that the group was arousing consternation, and even dangerous resentment among the local townspeople. Finally, by early August, Helen and I had had enough, and told Trudy to come home (several weeks before the end of the camp). This caused some flap among the Quakers, but we had our way. We had to get up in the early morning hours to go into Knoxville to pick her up at the station.

Trudy had only been home for a few weeks when Helen got a call from Washington, reporting that Gertrude Finney, he aunt, was very ill, so Trudy and I put her on the plane for the north. With her mother gone, Trudy took over very efficiently (unlike previous times when others were to do the work), cooked the meals and kept the house. At the end of the month, Helen phoned that Gertrude had died, and asked that Trudy join her in Washington. I put her on the plane, too, and again was alone in the house.

In late August, when I was at work in the drafting room on the northeast Tennessee cross-sections, a strange visitor came in, a woman who asked in a heavy accent what sounded like, "Is Kelley here?" She turned out to be Irene Helen Sribny, who had been a student at Louisiana State, and was on her way to do more work at Columbia University. She had planned to meet a chap named Goedicke from Texas A. & M. for a 10-day coming trip together in the Smokies. She was a Ukranian Jew who had somehow survived the holocaust of World War II, moving with her family from Germany to Austria, to Italy, always one jump ahead of the Nazis. Goedicke showed up; it seemed he had been a student at Chapel Hill, North Carolina, before going to Texas, and had done some work in the Grandfather Mountain area. He said he didn't believe the area was a window, and further that he didn't believe the northeast Tennessee cross-sections I was drawing, so that took care of him. Sribny said she was looking for a thesis problem, so I loaned them a planning report I had written on the Smokies. They looked around in the mountains, and came back later to say she had decided on a thesis problem in the Smokies. She said she could sense the geology through her skin, and in the contours of the hills and the trees. I was appalled. They disappeared at last, and fortunately I never saw either of them again, or ever heard of them.

During the autumn, I resumed my field work. This season, from October into April, was to be my last in the Smokies. I completed my review of the foothill belt, and rationalized the geology as far as I could. I also set about checking the mapping by Ferguson and Swingle in the Wear Cove quadrangle south of the coves, verifying the contacts and the faults, and bringing more order into the picture. Indoors, mainly at home in the evenings, I began writing a Professional Paper text on northeastern Tennessee, which was completed the following spring.

Helen and Trudy attended Gertrude's funeral, and her burial in Arlington Cemetery. Harry Finney was now alone, and at very loose emds. He made sizeable gifts of money to Helen and Trudy, and they went with him on a visit to Salley Beauchamp and her family, his niece, in New Jersey. From all reports, they had a big time. From there, Trudy went back to Westtown. Harry decided on a long visit to New Mexico with a nephew, and took Helen with him. They stayed for several weeks at the La Fonda Hotel in Santa Fe and I began to wonder whether I had lost Helen for good. Finally, she was able to get away, and I met her at the plane late in October. Harry talked big for awhile about living permanently in New Mexico and Arizona with the nephew, but in the end came back to Washington again, and to the house at 1322 Twenty-ninth Street.

It was wonderful to have Helen home again, after the long separation. On the first evening, I fixed the dinner, demonstrating what I had learned from the frozen foods that were just coming on the market -- steak, french-fried potatoes, tomato salad, and blueberry pie.

In November, the G.S.A. meeting was in Toronto, and I went up early by train, via Detroit, for the Council meeting. At the meeting, I saw briefly Bill Putnam of U.C.L.A., my old friend of Military Geology days; the meeting was casual and I thought little of it. After the meeting there was a field trip to Sudbury, which I attended as my first opportunity to see Canadian Shield geology. We all went up on a night Pullman from Toronto. I brought along my Tennessee field clothes -- including a duck coat and shoe-pacs. I found that the locals were dressed about as I was, but other people from farther south were not as prepared for the early northern winter. We looked at the surface geology of the Sudbury basin from south to north, and spent a day underground in one of the big nickel mines, where they had considerably hosed down the drifts so that we could see the ore. There was a dinner for the group one evening, and I walked back to the hotel with George Tunell of the U.C.L.A. faculty.

The proposed Mexican project deteriorated. I had waited impatiently all year for some definite word and plans from Bill Johnston and Foreign Geology Branch in Washington, but nothing happened. Finally, I decided to appeal for information from an outside source, and wrote to my old friend Jack Dunlap, who worked with De Golyer, whom I had seen in Mexico City in February. He replied that the situation was very bad

there -- jealousy between the different Mexican agencies, and a general hostility toward geologists from the States -- that I would be sticking my head into a hornet's nest by going. With that, I called off all plans of going to Mexico.

Helen and I were feeling rather blue over this when, one Sunday, I got a letter at the Post Office from Bill Putnam of U.C.L.A., whom I had seen the month before in Toronto. He wrote, "Why don't you come out for a year to U.C.L.A. as visiting professor, and try it out?" I took this home to Helen and we were both intrigued. I wrote Bill I was very much interested, and would like to hear more.

Helen and I both decided we needed a change of scene, and had become increasingly oppressed with the way things were going on the Survey. It seemed as though I was at outs with the whole Smokies party; we had been together too long and antagonisms had built up. When I tried to enforce discipline, they all fled to Hadley, who had no authority. I had become increasingly at outs with the Denver group in General Geology Branch. They seemed to act as though they were the Survey and spoke for the organization, and I didn't see eye to eye with them. (Actually, there were many other parts and factions in the Survey, even in the Geologic Division). And the University of California was a much bigger, more stable institution than any other of the universities whose offers I had considered.

Harry Finney came down for a Christmas visit, but was very restless, and didn't seem to approve of our way of life, so he went back to Washington in about a week.

The first half of 1954 was to be our last in Gatlinburg, and we were to move on to an unknown future. During 1953, and continuing into 1954, we made continued efforts to sell our house, but with no success. Many people professed great interest, and some wanted to buy, but when we investigated the latter they all turned out to be dead-beats -- either with no funds, or a long record of bad debts. It gave us a new insight into the towns-people, who seemed outwardly prosperous and were riding around in fine automobiles. Among the most plausible was Jimmy Carter, the local mortician and son of the Baptist minister; our grocer told us that he owed six months on his grocery bill. Then there was the Pelton family recently arrived from Knoxville, who went to our church and were pillars of respectability. Their social references in Knoxville

were impeccable, but the Knoxville credit bureau reported a long record of bad debts, so we called off dealing with them, to their consternation. Various people said that east Tennesseans with money were waiting in the wings to come through with a cash offer at the last minute, but none materialized before we left Gatlinburg. We finally rented the house to the Blake family, who were soon to arrive for duty with the Park Service.

From March 8 to April 13, Helen went again to Baltimore for another errand of mercy to care for Jessie and Bertha, going and returning by plane.

The Southeastern Section of the G.S.A., with me as chairman, was due to meet at the State University in Columbia, South Carolina, in April. Larry Smith of the university was to be in charge of local arrangements. I assumed that he would also be responsible for the program, but he rebelled and dumped all of this on my lap. I was in despair, as I had enough responsibilities; I settled down to write a whole series of letters, soliciting papers for the program, which Margaret Gower typed for me. Abstracts began to trickle in, then became a flood; we had to turn away late comers because the program was full. The Southeastern Section was on its way, and was a success!

Helen returned just in time to go with me to the meeting, and we drove over in our car; we had a pleasant time of it. Morgan Davis of the Humble Oil Company volunteered to contribute a company movie about reefs. For the evening lecture I asked Stephen Taber, the grand old man of South Carolina geology, to speak; he talked about a late submergence of South Carolina, which seemed to be mostly wishful thinking. The drive to and back was delightful, as the spring was never more beautiful.

In May, I started packing so that we could vacate Gatlinburg. At the office, I took down the boxes in which my books and journals had come from Washington, wrapped all the books and other possessions in the boxes for shipment. I was not to open the boxes again for two long years, until we finally arrived in the Menlo Park area in California.

The Gray Van movers arrived late in May and emptied the house, and the van went on its way to Denver. I put a few dabs of paint in places in the empty house, and left it bright and shining for the Blakes. Our friends Dorothy Lexau and Ethel Trainer put us up for the night at their place up the hill, and next morning we left in our car from Gatlinburg, never to return.

It had been a hectic eight years in Gatlinburg, full of much joy, and much frustration. I might have accomplished much more on the Smokies project had it not been for the many interruptions and digressions on other duties, but after eight years my own fund of information on the Smokies was actually quite sizeable. The digressions, especially during the preceding three years, had been great and frustrating. Everyone wanted my services -- the Survey, the G.S.A., the Episcopal Church -- and I realized that each group was asking for things with no realization of my whole picture. Added together, they made a burden almost too much to carry.

We drove across the mountains, past Asheville where we bought a bottle of whiskey. I had been too busy during the last few months to make a trip to buy liquor, so it was a welcome thought to have it in the car. Then, we headed north on the Blue Ridge Parkway which by now had been built through to Asheville. A drive along the parkway, through the quiet mountains, would be our farewell to the Southeast.

We spent the night at The Bluffs Lodge, a concession on the Parkway in northern North Carolina, set on the edge of the Blue Ridge Escarpment, with great views across the lower country below, extending off southeastward. We had a welcome drink in our room, then an excellent dinner, and retired for the night for a well-earned rest.

We followed the Parkway next day to Roanoke, then over more prosaic highways into Washington. We spent a few nights with Brookes and Madge Knight, then a few more nights with Harry Finney. Helen visited Gertrude Finney's grave in Arlington Cemetery. Leaving there, we stopped off for a brief visit with Jessie and Bertha in Baltimore, then drove to Mamaroneck to visit Robert and his family; one day we drove down to New York to visit Edward and Grace. Then it was time to go to Westtown for Trudy's graduation.

We spent several days at Westtown and attended the graduation ceremony, where Trudy received honors. I met Jim Shuster, a boy of whom Trudy was very fond, for whom I took an instant dislike, and was glad when he went back to Philadelphia. Anyway, he had been a useful boy-friend during her high school years, and helped bring her out of her adolescence. I helped her dismantle her room, with its three-year accumulation, and loaded most of it into our already crowded car.

Next day, we drove up to the nearest interchange on the Pennsylvania Turnpike and I asked, "Which way west?" So we were on our way to new lives and an unknown future.

U. C. L. A. interlude -- 1954-1956

On the trip west, Helen was alarmingly ill, which made our traveling difficult. We spent our first night in Washington, on the west edge of Pennsylvania, next night in Indianapolis, another in Chillicothe, Missouri, and the last night at Bird City, a dreary little town in the plains at the western edge of Kansas. Going through Kansas, there was a detour, and I was suprised to realize that we were rerouted through Sabetha, which I had visited as a boy when Grandmother Lash was living there. Sabetha was a quiet little town, which looked much the same as when I had been there years before.

We drove into Denver next day, and to Robby Robinson's home in the north part of the city. Robby had shown some contrition for our mutual hard feelings, and had offered his house while he and Rhoda were away for the summer. Arriving, I reached in the glove comparmnt for the key, but it wasn't there; I realized with horror that I had put the key in a desk in my study, and that it was now in the moving van on its way to Denver. We had to break a window to get in. I drove over to the Survey office and got my mail. We were to spend two long months in the Robinson house.

Trudy had made her own arrangements about college plans. With her Westtown credentials, she received acceptances from Pomona College in Claremont, Stanford University, Mills College in Oakland, and Reed College in Portland. Without consulting us, she had decided to go to Stanford. I had hoped that she would go to Pomona, a smaller school of which I had seen and heard much. Stanford was a bigger place, and much more expensive, which would make it harder on our budget.

My negotiations with Bill Putnam had turned out very well, and by summer it was all decided. In July, I received my first month's salary check, for \$750.00, from the University of California, which embarrassed me, because I had not even started there or been in Los Angeles. I found that I was being paid for 9 months of work, which was pro-rated on a 12-month basis. I arranged with the Survey to go on 9 months leave without pay during the academic year, which would leave my Survey status intact. During the summer I received a letter from Ada Nisbet of the English faculty at U.C.L.A., who was going on a year's sabbatical in England, offering to rent her apartment at 11,963 Gorham Avenue. This sounded good, and we took it.

I started on a summer of work in the Denver office, but had only been there a week when there was alarming news from home. Helen was flat on her back in bed. She had been hanging clothes on the line when something snapped in her neck, and she could not move. We consulted various doctors with no result, and finally found one doctor who diagnosed her trouble. It stemmed from the whiplash in her neck during the auto collision in Florida in 1953. He prescribed a collar to take the weight off the vertebrae, and finally late in the summer she began to be on the mend. But it made a sad summer for Trudy and me. Trudy, as before when responsibility was thrust upon her, took over nobly, and kept house and did all the cooking.

At the office I submitted the Professional Paper manuscript on northeast Tennessee, and fed it into the mill. (It had many vicissitudes after that, and was not finally published until 1960).

Ed Goddard, in his work on a new geologic map of North America, appealed to me to put together the Southern Appalachians. For the map, he had been relying on contributions from the different States, but in the Appalachian region, things were so broken up between the different States that the contributions made no sense. I compiled a contribution on the 1:5-million final scale. Particularly troublesome was the Piedmont region of North and South Carolina. I found that there were a considerable body of data available in scattered form, admittedly not of the best, but which had never before been put together, so I made a larger scaled compilation of this region, and then wrote a text to go with it. An excursion across the area was planned before the G.S.A. meeting in New Orleans in 1955, and the field trip committee said they wanted the map and text for their guidebook. One of the results of the compilation was that it became evident that the Piedmont in those States was divisible into well-defined "belts" that had not been clearly recognized before. With the help of Bill Overstreet, I named them the Brevard belt, the Inner Piedmont belt, the Kings Mountain belt, the Charlotte belt, and the Carolina Slate belt. These names have stuck, and continue in use today. I had had no real experience in Piedmont geology, and was merely an "outsider looking in", but in later years it had been necessary for me to continue this kind of compilation, culminating in the U.S. Map of 1974.

Toward the end of the summer, Helen felt well enough for us to go to a dinner party given by Ed Lewis, who with a boy-friend shared an old house in Golden (west of Denver), and we had a pleasant evening.

The summer was drawing to a close, and Robby Robinson returned. We spent our last evening, with Trudy at Eliche's Gardens, an amusement park in west Denver, where Trudy rode on the roller coaster. We had a fine dinner at an old-fashioned restaurant in the Gardens, with an after-dinner drink, a "stinger". It was a gay evening, but with a touch of sadness.

None of us had much warmed up to Denver, which was big and rather barren, in its high, semi-arid setting. There were miles and miles of little brick houses, in which we could not conceive we would ever want to make our home. The summer was fearfully hot -- unusual, they said --and water was scarce and lawn watering was severely rationed. And we felt depressed and uprooted in general, after our pleasant home in Tennessee, and very uncertain about our future. I am still convinced that some of Helen's disability that summer arose from our condition.

At last, on Labor Day in September we could get away, and Helen and the rest of us felt better the moment we shook Denver from our feet.

We drove south along the front of the Rocky Mountains, through Colorado Springs and Pueblo, and over Raton Pass into New Mexico. Through correspondence with Fred Smith, I learned that his mother and father were still running their place at Eagle's Nest in the Sangre de Cristo Mountains, and we made this our first night stop. We were there for several days, and one of the days we drove down to Taos and saw all all the sights of the old town, and the Taos Indian Pueblo on the outskirts.

Leaving Eagle's Nest, we went back through Taos and down to Santa Fe. Helen wanted to linger here, to relive her visit there with Harry the year before, but the town was crowded and I was against it. We went on to Albuquerque and had a noon lunch at a Mexican-style restaurant in the old part of town, then west on Highway 66. I desired to show the family Acoma Pueblo, which I had visited with Stuart Northrop's group in 1929, so with some trepidation we turned off on the unpaved side road 10 miles south to the pueblo. We reached the

place safely and climbed the steep trail to the pueblo on top of the mesa. The Indians charged us a fee, but were otherwise very hospitable and let us look around. We were impressed with the stark church, with skulls on top of the surrounding wall. Returning to the highway, we spent the night at a motel in Gallup.

Next day we drove west to Flagstaff through the Painted Desert, and turned north across the desert, then west. We looked at the deep slot cut by the Little Colorado River, then climbed to the forested upland of the Kaibab Plateau, and had our first look into the Grand Canyon itself at Desert View. We had a picnic lunch there, and Trudy wanted to delay awhile so that she could draw the whole complex panorama! We had to dissuade her, and go on to El Tovar, where I had obtained reservations at Grand Canyon Lodge, on the brink of the canyon. We spent the night there, admiring the view in the ever-changing light.

The day after, we drove south past Prescott, and down a series of giant steps into the low desert of southwestern Arizona. The Arizona desert vegetation was familiar to me from old times, but Trudy was fascinated as it was all new to her, and had to have it all explained -- the dust devils, the saguaros, ocotillos, and chollas. It was a long hot ride west across the desert. On the long upgrades the motor thermometer rose gradually almost to boiling, then went rapidly down again on the long downgrades. We crossed the Colorado River at Blythe, and were at last in California. Then another long stretch of desert until we came down into the Coachella Valley at Indio.

It was fearfully hot in Indio, being near or a little below sealevel, but our motel was set in a grove of date palms. After a comfortable night there, we drove on into Los Angeles, through Palm Springs and San Bernardino. The desert air was crystal clear, and I looked forward to showing Helen and Trudy the backdrop of the San Gabriel Mountains rising above the orange groves, but a little beyond San Bernardino we ran into the smog blanket. I had not seen it in my previous visits, so it must have become worse during the preceding five years.

Once in the great city, we navigated with the aid of a street map, and reached the Nisbet apartment on Gorham Avenue without much trouble. We were welcomed by Florence Much, the apartment owner, who lived in a house on the grounds, and we went to the apartment, which was spacious and much to our liking. Miss Nisbet had left us a nice note, a few groceries, and a bottle of whiskey.

That afternoon, I drove over with Trudy to the U.C.L.A. campus and to the Geology Department, where I was welcomed by the departmental secretary and got my mail. Bill Putnam was there, and I saw him briefly. There was still a week or two before classes would start, so the place was very quiet.

Trudy was due shortly at Stanford, so after a few days we drove north. We went on U.S. 99 over the Tehachapis to Fresno, where we were to visit with the Sheppard family -- Fanny Sheppard was another of Helen's aunts, who had been living in California ever since she was married. We stayed at a comfortable motel, and in the evening drove over to the Sheppard's home, where I met Fanny's husband Shep and her daughter Frances and her husband Aaron. It was quite a family reunion, and there was much to drink, so I went back to the motel feeling very tipsy.

It was hard to get going in the morning, feeling as I did, but we went on nevertheless, and reached Stanford about noon. We found Trudy's dormitory room at Roble Hall, and she was welcomed by the other girls. Having already spent three years in boarding school, she fitted right into the routine. Some of the other little new girls, who had never been away from home before, were crying.

I stopped by the Geology Department (which I had visited in 1931 when I saw Bailey Willis), and had a brief visit with Charlie Park. Helen and I looked for a motel to spend the night. Rickey's had been recommended to us, but they were all full. There were few other places. (The long line of motels along El Camino were not built until a few years later). We finally got a room at Travel Lodge -- spartan, but clean and adequate.

Next day, we were up early for our return. We had breakfast in Gilroy, and drove down Highway 101 through coastal California. I remember Gaviota Pass, where the highway was very narrow at the time, and the rocks were turned up steeply; then along the seashore past Santa Barbara and into Los Angeles, where we arrived about 4:00 P.M.

My duties at the University were to conduct a graduate seminar in structural geology (= tectonics) which ran through the year, and in the spring a lecture course for undergraduates on the Geology of North America. The latter was ordinarily given by Clem Nelson, who was away that year on a sabbatical. There was thus not much to do in the fall semester, and I had much time for my own work. Fifteen or twenty students registered for the course, and I put them to reading and reporting on areal geologic reports and maps of interesting areas. A feature of the course was a semester term paper. Ordinarily, the professors at U.C.L.A. looked over the term papers and graded them, with little comment. I found that they were all badly written and I decided to give them "the works", going over them and editing them like they were a Survey manuscript. This was a shock to the students, but in the end they came to like it and expect it. (The same seminar had been conducted by Jim Gilluly when he was at U.C.L.A. in the forties; he ran it with an iron hand, but I was more gentle).

I met the other members of the geology faculty, and eventually we met their wives. Bill Putnam I already knew; he was currently Chairman of the department. There were also Cord Durrell, George Tunell, Willis P. Popenoe (Parky), John Crowell (who had just returned from a sabbatical in Europe), Dan Axelrod (the paleobotanist), two Canadians, Ken Watson and Don Carlisle, and last but not least Ulysses S. Grant IV. It was a congenial group, not unlike an assemblage of a better class of Survey geologists. Only Dan Axelrod was aloof from the rest. We occasionally saw Dave Griggs and George Kennedy of the Institute of Geophysics who were quartered in the basement of our building. Dave was snooty and supercilious, and George was flip and outgoing. Bill Putnam, who was a firm adherent of the old geological verities, hated them both.

Ulysses Grant was a real character, something of an alcoholic, and the grandson of the President. Shortly after our arrival, the Grants gave a big reception to introduce us at their large house on a hill overlooking the campus. After we arrived, I found that, contrary to my idea that my stay would be temporary, everyone assumed that I had come permanently, and made every effort to convince me to stay.

Columbia University in New York was celebrating its bicentennial year, and in October their Geology Department held a mammoth symposium for the occasion, rather inappropriately entitled "The crust of the earth". I was asked to give a paper on "Orogeny and epeirogeny in time", so flew to New York on a long weekend, staying as usual with Edward and Grace. It was a huge gathering, with many distinguished participants, some from abroad. The weather was very hot and sultry, which gave me so bad a cold that, when the time for my paper arrived toward the end, I did very badly. The last evening, we were taken by bus to Lamont Geological Observatory on the opposite side of the Hudson, at Palisades, New York. We had a tour of the establishment. Refreshments were to be served in a tent pavillion on the grounds, but there was a drastic change in the weather; a hurricane blew in and the tent was torn to shreds and virtually carried away. Everything had to be moved hastily to more cramped quarters indoors. During the evening, all the people on the program were given "medals", really commemorative silver plaques. I never thought of them as real "medals", but many of the others did. The flight going and coming was my first cross-country non-stop plane trip; I had hoped to see the country but had inside seats both ways, so got only glimpses.

In November, the G.S.A. meeting was in Los Angeles, and was held in the Statler Hotel downtown. It was to be my final year on the G.S.A. Council, and from then on I had no more G.S.A. duties. I had feared that there would be more, but an anti-Survey faction had arisen in the Society, so that for a time nominations from the Survey for office were at a minimum. At the evening meeting, which Helen attended with me, I made the presentation speech for Arthur Buddington to receive the Penrose Medal. (I had served that year as chairman of the Penrose Medal committee, as a duty on the Council).

Sometime in October or November, we had our first rain. It had not rained in Denver all summer and the California days had been so continuously sunny that I thought it never rained there. The rain was a pleasant surprise, and felt very good. It was our introduction to the "rainy season" in California.

Trudy came down from Stanford during the Thanksgiving holiday, and again at Christmas. I drove across town in heavy fog to Glendale Station, where she came in on the "Starlight" or night coach train from the north, along with a horde of other college students.

The week after Christmas, Helen, Trudy, and I went on a holiday trip to Death Valley. Accommodations in the valley were all taken, so we stayed at the hotel at Death Valley Junction east of the valley. One day we drove south along the length of the valley. Next day, Trudy wanted to see a ghost town, so we drove north to Rhyolite, where we were disappointed to find only a few walls remaining. Then we went west down into the valley, and back south. At some point on the trip, Helen and Trudy cavorted across the sand dunes in the valley.

Between semesters in January, 1955, Helen and I went north for a visit with Trudy at Stanford. On the trip, we tried different routes. Going north, we turned east at King City and drove down the valley along the San Andreas Rift to Hollister. Coming back, we followed the coast road (California Highway 1) along the steep cliffs overlooking the sea, and spent the night in Buellton, from where we went over in the evening to Lompoc for a visit with Helen's friend of Washington days, Juliet Arnold, who was teaching school there. On the trip, we visited various of the old mission churches. We had taken the precaution of getting advance reservations at Rickey's in the Stanford area, so lived quite luxuriously. One day, Trudy took us to San Francisco by train to see the sights, and we rode around on the cable cars. We returned from our trip having seen a lot of California, and filled with a yearning to live in the San Francisco area, rather than in Los Angeles.

We had come to Los Angeles with a set of prejudices against it, and after living there discovered that most of them were not so. Actually, Helen especially found the city quite exhilarating. Later on, a new set of dislikes, more real than the first prejudices, built up. We enjoyed the movies on Hollywood Boulevard and in Beverly Hills, and on weekends we drove down to the beach in Santa Monica and took long walks along the sea -- excellent therapy when we were tired and discouraged. Los Angeles traffic was exhilarating -- fast and well disciplined.

But Los Angeles was no longer the exotic never-never land that I had seen on my first visit in 1929; it was just another big sprawling city. The fake Spanish architecture had gone out of style, and the few remnants of it that survived looked odd indeed. The air was no longer crystal clear; the smog blanket was persistent, and very heavy at times. And Hollywood Boulevard, the glamor street of 35 years ago, was now sadly run down; the city had moved on past it, leaving second hand stores and other junk in its wake.

We looked at real estate, and were appalled at the prices (especially coming from a depressed real estate market in Tennessee) -- more than \$30,000 even for old run-down houses. All of west Los Angeles had been built up in past decades, so there were no new houses. Construction of tract houses was proceeding merrily, but far away to the southeast. None of this appealed to us in our hopes for a new home in the west.

With the spring semester at U.C.L.A. my real work at the university began, because, besides the graduate seminar, I had the undergraduate lecture course. I thus had no spare time of my own, except for a welcome break of a week at Easter recess. For the graduate seminar, I worked up topics in theoretical geology, submarine geology, and the like -- things that I had been unable to read up on myself during my years of work on the Survey, and always wanted to. The undergraduate lecture course sounded interesting, but it was a required course, so the caliber of the students (though upperclassmen) was fearfully low, and it was an effort to teach them anything.

However, along in the spring I realized that I had a new basis for continuing my work on the subject of the tectonics of Middle North America, but in a different mold than the published book. I therefore began carefully to save my lecture texts. Thus was born "Evolution of North America."

In April I had become very discontented with our car. It was only 5 years old, and in excellent condition, but it was stodgy and outclassed by the automobiles in common use in Los Angeles. We went around to the local Plymouth agency and selected a white station wagon, which gave us both a lift. It had automatic shift and directional signals, both missing on the old Plymouth.

The semester was drawing to a close, and I decided that one year was not enough to get the full flavor of university life, so I arranged to stay for one more year. Sometime in the spring, however, I received confirmation from the Survey and Gilluly that we could move to the Menlo Park office as my permanent headquarters, and with that assurance I never wavered in a plan to return to the Survey eventually.

After the ordeal of reading the semester's term papers in the graduate seminar was finished, we put all our belongings in the new station wagon and headed north for Stanford, where we picked up Trudy. While in the area, I went one afternoon to the Survey office in Menlo Park to look the place over. It was a single hoxy building with much land around it on which other buildings would eventually be put up. The only geologist who was around when I called was Dick Sheldon, who gave me a tour. After the visit, Helen and I looked at new houses that were being put up in the vicinity, but we had no ready cash at the time, and were unable to think of buying.

From Stanford, we made our way east by various stops and detours to the Survey office in Denver where I would spend the summer. The first stop was to visit Cord and Helen Durrell at Blairsden in the northern Sierra Nevada, where they had a cabin in which they always spent the summer. It was our first look at the forests of the northern Sierra. We had dinner with them that evening, and moved on in the morning. The next stop was at Battle Mountain, Nevada, where I would visit Jim Gilluly and his field party. Jim came by in the afternoon and took me out to see a window area in the west part of the Mount Lewis area, and in the late afternoon we drove over the mountain to his tent camp and had dinner with the party. We drove back to Battle Mountain after dark over the uncertain roads. We had left Trudy in Battle Mountain to amuse herself for the evening.

Next day we drove through Salt Lake City to Heber for the night, and had a pleasant dinner at a place outside of town by some hot springs. The following day we went across the plateaus and the Rocky Mountains to Granby in Middle Park, and finally down the mountains into Denver.

For the summer, we had rented the place of Dwight (Rocky) Crandell in Lakewood, who was in Washington State for the summer on Suurvey field work. It was a pleasanter place than Robby Robinson's cramped little house in Denver, with a wide lawn and many trees. We much enjoyed our stay there that summer.

At the office, I spent the summer on the maps for the Sierra Diablo report. My problem of a topographic base had been solved after a fashion. We had acquired the work sheets for the Army Map Service quarter-million Van Horn sheet, enlarged to 1:48,000. Contours were on 100-foot intervals and were seemingly rather crude, but they were accurate as to form and position, and were capable of many improvements by me. I also had the air pictures, and I set to work tracing the topography and geology on them. I then began to put the results on metal-mount copies of the A.M.S. topography. (I took all this back to Los Angeles with me, and continued the work during the fall semester).

During the summer we had a get-together of the Great Smokies field party -- Jerry Hadley and Warren Hamilton of the Denver office and Bob Neuman out from Washington. George Cohee also attended. At the meeting we made our final decisions on stratigraphic nomenclature of the Ocoee Series. One evening we took Bob and Jerry to the opera house in Central City to see a play, "Bus Stop." We had been to the opera house earlier in the summer with Trudy to see "Iolanthe."

Trudy was still mooning over Jim Shuster, and was determined to go east and have it out with him. We put her on the plane, and hoped for the best. She was gone for about 10 days. She got Shuster out of her system, and then went on for other visits with friends, and came back feeling much differently than when she had left.

At the end of the summer the Blakes wrote us that they were giving up the Gatlinburg house, and we were in a quandary about our unwanted real estate "back home." Then we had a letter from the Trammels across the street offering to buy the house for cash -- the long-awaited East Tennesseans that we had hoped would come forward before this. Of course, they wanted a rock-bottom price, so we told them \$8,000.00, which was the amount for which the place was insured -- a loss for us, of course. We were leaving Denver next day, so it was all very chancy. We telegraphed John Morrell, the Park Ranger and a pillar of our church to complete the deal for us. He did nobly, and it was all settled by the time we reached Los Angeles. I put the

\$8,000 into savings bonds, to await our eventual purchase of a home in California. It was a big load off our minds, and we were free at last of our Gatlinburg obligations.

The summer was over, and the Crandell's would be home by Labor Day, so we set off on our return to Los Angeles. We drove west over the Rocky Mountains and through Glenwood Springs for our first night in Grand Junction. Then we went west across the plateaus, and south along the valleys between the High Plateaus. We made a brief side trip to Bryce Canyon Park, and to a night stop at the little old-fashioned Mormon town of Kanab. Next morning we went south through the forests of the Kaibab Plateau to Kaibab Lodge, a pleasant collection of cabins at the north edge of the National Park. Leaving our things there, we went to the north rim of the Grand Canyon and had lunch at the hotel. After lunch, Trudy and I walked down a ways on the trail into the canyon, but there was not time to do more than go to the top of the Supai Formation. After our walk, we drove along the road along the canyon rim, looking down from various overlooks, and returned to Kaibab Lodge for the night.

Next day, we went west, stopping at Zion Canyon where we had lunch at the Lodge, and walked up the canyon for a ways under the towering sandstone cliffs. We went on to St. George for the night, the last Mormon town in Utah, with its great temple. From then on the trip was anticlimax. We had lunch in Las Vegas and went on to San Bernardino for the night. We were back in Southern California again ("the Southland", they called it), and into the ~~f~~ag end of a heat wave.

We went on back to Los Angeles, where we had rented a house for the year from the Robertsons at 10,639 Wellworth Avenue, in Westwood, nearer the University than the Nisbet apartment of the previous year. The Robertson's were an old couple, he with the Chemistry Department, and they were going on a world cruise. The house was old and run down (about the vintage of our Arlington home of the thirties), but was nicely built and with much room -- two bedrooms and baths in the back, and a little study behind the living room. Our next door neighbor turned out to be Mrs. A. O. Thomas, widow of the paleontology professor of my days at the University of Iowa, who still took the Iowa City Press-Citizen.

The University work in the fall semester was light -- the graduate seminar pretty well ran itself. I was delighted to find that Celeste Engel, Al Engel's wife, was taking graduate work at U.C.L.A. that year, and had registered for the seminar. Being a mature person, she added stability to the class, with a feminine touch. Cord Durrell was giving a course on the geology of California, which I sat in on in the afternoons. With much time available, I completed the Sierra Diablo maps in the study at home -- topographic and geologic.

In October, the Permian Basin Section of S.E.P.M. held a field trip to the southern Guadalupe Mountains which I was invited to attend, and did on a long weekend. I went by train to El Paso, where I was met by a member of the local committee and driven to Carlsbad. On the way, we stopped for beer at Pine Spring Camp, and Mrs. Glover greeted me as an old friend. The first morning of the trip was spent in McKittrick Canyon, with Wallace Pratt playing the genial host. In the afternoon, we went around to the old Williams Ranch at Bone Canyon. I took the party up the canyon to Bone Spring, then to the top of the bench to the north, where we could look north and see the Delaware Mountain sandstones overlapping the rising surface of the Bone Spring Limestone.

The second day, we went north along the west side of the mountains into the El Paso Gap quadrangle, New Mexico, which adjoined my work on the north, and which had recently been mapped by Bill Boyd (as a thesis for Norman Newell at Columbia). The stratigraphy changed rapidly, almost at the State Line, so that my familiar units to the south had changed character, or were replaced by other units -- added to which the whole Brokeoff Mountains area was sliced by a multitude of little faults. I had to leave the party before the day was over, as I was taking an evening train out of Van Horn, so one of the geologists drove me down. I was the only passenger to get on at Van Horn. It was good to settle down in a roomette for the ride back to Los Angeles.

During the spring semester, as during the preceding year, the University duties closed in on me, and I had no time of my own. My seminar had become increasingly popular, and 32 graduate students registered, which I had to divide into three sections. This at least had the advantage of my getting to hear three different reports on the papers assigned, which gave me an idea of their contents without having to read them carefully myself. We went over various subjects that were to assume greater importance in later years, when plate tectonics came

without our trying to account for them -- submarine geology, magnetic striping, and the like.

The undergraduate lecture course was as time-consuming as ever, and the students were even dumber, if possible. It was good discipline, however, to try to write out explanations of complex subjects for so unreceptive an audience. More than ever, I had the future "Evolution of North America" in mind, and to complete the subject I wrote out the texts of several lectures that I knew there would not be time to give.

Cord Durrell's course on the Geology of California continued into the spring, but changed into a field course -- a series of long (Friday to Sunday, or Friday to Monday) trips to accessible places in south-central California. I decided to go along. We slept out in the open on bedding rolls, and cooked our meals on Coleman stoves. It was hard to get used to sleeping on the ground, but after a few nights I slept like a trouper, and only woke when I heard the pots and pans rattling when the cooks started to fix breakfast.

On April 6-9 we went from Shoshone east of Death Valley, across Death Valley, past Darwin to Owens Valley, and the front of the Sierra Nevada below Mount Whitney. On April 20-22, we went across the southern Sierra Nevada past Isabella Reservoir to the Bakersfield area. We saw the ground that had been broken up along the White Wolf fault during the Tehachapi earthquake of a few years before. On May 4-7 we went through the oil fields of the southern San Joaquin Valley, into Cuyama Valley, and down the Santa Maria valley to Santa Maria, with my first look at the mysterious Franciscan Formation. We looked at the rocks at coastal exposures near Point Sal south of Santa Maria, and those of the Sierra Madre above Santa Ynez Valley. Finally, we examined the rocks of the Santa Ynez Range, and along the coast to the south. We returned from each long trip tired, dirty, and constipated, but we became a congenial group before it was over. I got a good view of southern California and its geological sights, many of which I have not seen since.

In the seminar, I also gave a field trip each fall and spring of each year -- in the fall to the San Andreas fault zone near Valyermo, and in the spring to the great fossil landslide at Blackhawk Canyon on the north side of the San Bernardino Mountains. The second autumn, Trudy was home on vacation from Stanford, and went with us on the trip to the Valyermo area.

Our finances during the two years at U.C.L.A. were difficult and precarious, and we were continually overdrawing our joint checking account. My pay was much more than I was getting in Gatlinburg -- monthly pay from the University and Survey pay during the summers, but most of it went, and it was hard to save anything. There was Trudy's tuition and expense at Stanford, storage on our belongings at the warehouse in Denver, rent on our apartment and house in Los Angeles, and many other things. And we got little credit on income tax -- no deductions for mortgage interest or taxes, etc. When spring came, it seemed as though all my surplus on the double pay was confiscated for taxes. Once I got back to the Survey routine in 1956, the taxes became much easier.

Bill Putnam was incensed that I had decided to go back to the Survey; for some reason he thought I was coming permanently to U.C.L.A. Neither he nor anyone else on the faculty understood our problems about housing; most of the staff had bought their places in the thirties or forties, when houses were only moderately expensive. So when I finally left for good there was a tinge of bitterness, which fortunately wore off in time.

The spring semester of 1956 drew to a close at last. There was the final long chore of reading the 32 term papers in the graduate seminar. In June, I was finally able to pack all our possessions, at home and at the office, and send them north by Bekins moving van.

Finally, one morning we were able to say goodbye to Los Angeles for good, and start off in the station wagon, well loaded with gear. There would be a gap of a week or so before we were due in Menlo Park, and we decided on a vacation. On one of the trips with Durrell I had seen Mattie's Tavern, an old-time hostelry near Solvang in the Santa Ynez Valley, and we made reservations there. It proved to be everything we had hoped for -- quiet, shady, and relaxed, run by a kind and hospitable older couple.

We made a few trips to places I had seen during the spring field trips. One day we went to the nearly deserted beach at Point Sal. Another, we went up a forest service road to a lookout overlooking the Santa Ynez Valley. On this trip, we must have hit a rock, for the manifold of the Plymouth broke and made an awful noise. From then on, we stayed close to the tavern, and just relaxed and rested. I used the time to catch up on my voluminous correspondence, mostly Survey

business, which I had been unable to touch all spring because of my heavy teaching schedule. We had expected to stay a day or so at Rickey's after we got north, but learned that they were all booked up, probably on account of Stanford graduation, so we decided to stay several days longer at the Tavern.

Finally, we set off for the north, and to a new life with the Survey at Menlo Park. We had to stop at Santa Maria for awhile for emergency repairs on the manifold, so it was late in the afternoon before we finally arrived in our new area.

U.S. Geological Survey -- Fourth period -- 1956-1973

For the summer, we had rented the home of Bill and May Bell Pierce on 14380 Manuella Avenue in Los Altos Hills. This was a strange area for us, as we had never been into the "back country" off the main highways, but with the aid of a map we found it without too much trouble. The Pierce's had left that morning; Trudy had already brought her things down from Stanford and was awaiting our arrival.

Bill Pierce had been with the Survey in Washington a little longer than I had, and like me had decided to move to the new office center in Menlo Park for his final days on the Survey. They had just built the house, and had only been in it a few weeks before it was time for his field season to begin in Wyoming, so we came into a brand-new house. Los Altos Hills was an area of rather open country, with large lots and widely spaced houses. Within a few days our belongings arrived from Los Angeles by van, so with what we had we moved in and felt quite settled.

Next day, I went to the Menlo Park office of the Survey, and found a place to work. The building was small and cramped, because of the many new people who were arriving, to await the construction of the larger office building that was to go up next door. "Next door" was a weed-grown empty lot, on which the foundation of a new building had just been laid out. Bill Pierce had been using a little cramped cubby-hole in the old building and I moved in there for a few days. Paul Bateman, in charge of the local Minerals Branch office, then invited me to move upstairs and use a desk of one of his men who was in the field for the summer. When the Minerals Branch people began to return, I had to move on, and eventually ended up in the "lecture room" where nearly a dozen of us had desks -- all people arriving who were awaiting construction of the new building. There, all of us stayed until the following February.

Sometime during the preceding year, Jim Gilluly had called me in Los Angeles from Denver, proposing that my next Survey job should be a compilation of a new Geologic Map of the United States. The previous geologic map had been compiled under the direction of George W. Stose, and was distributed at the time of the International Geological Congress in 1933, but was now far out of date. A new version was thus

long overdue, and when I got back on the Survey it was with the intention to work on this project. As things turned out, however, it was to be nearly ten years before the project could actually be started. For one thing, I had two long reports to complete -- that on the central Great Smoky Mountains and that on the Sierra Diablo -- and there were to be other distractions, as set forth later.

At the office, I laid out the Great Smokies material, and picked up again the text that I had started to write in Los Angeles. Through the years at U.C.L.A. my editorial sense had been sharpened by reading all the student term papers, so I went over what I had written most rigorously.

At home, I set up an office in the little study next to the living room, and spent the evenings all summer going over the texts of my accumulated lectures on the "Geology of North America", putting them into a single narrative for the eventual "Evolution of North America". By the end of the summer I had a combined manuscript that I could pass around to various other geologists for critical reading and review.

Trudy planned to stay at home that summer, and to take her work at Stanford during the summer term, then in the fall term to get a job. I took her down to the Survey for job interviews, and she was hired by the Alaskan Branch as a draftsman, to go on duty at the end of August. It was her first paying job and she was very proud to qualify, but she was rated at GS-1, the lowest possible category.

Helen set about looking at houses for our eventual home. A few old run-down houses were selling for between \$10,000 and \$20,000, but we wouldn't have them. The really attractive places were all \$30,000 or more -- steep by our Tennessee standards -- but unlike those in Los Angeles we felt they were homes that we could love and spend years paying for. We liked the area, and were eager to settle down in it.

Helen looked at various places advertised in newspapers, and went out with a succession of real estate men. We looked at a place in Atherton which was very attractive, but it was selling for \$50,000 and had a large landscaped grounds that would be hard to keep up. Helen went out much with Mr. Binney of Cornish & Carey, but became suspicious of him because she felt that he had misrepresented some of the places offered. She decided to try another agent, so one Saturday morning we went out with him to see other houses. However, Mr. Binney had asked

for one more chance, to show us a house in which he had part interest. This turned out to be a house at 670 Covington Road in the south part of Los Altos a few blocks from the Rancho Shopping Center. It was owned by Col. and Mrs. Winters, and they were selling it for a firm price of \$28,500 -- a ridiculously low price at it turned out. The Colonel was a great putter-together of things, and once he was settled he was restless for a new challenge and was eager to move on.

We went with Mr. Binney to see the place, and felt instinctively that this was it. It was large and nicely built, only 6 years old, and the whole place was landscaped and with fruit trees planted in an orchard in the rear. When we got home and talked it over we decided to buy; we had only been looking at houses for about a week, but had done the job. I was instructed to make a down payment to hold it, so I went to the bank the next morning and sold several thousand dollars of our savings bonds to deposit with Cornish & Carey.

There was a little flap, however. Helen talked next day with the other real estate man of Saturday, who told her that we had made a very bad buy, and when I got home that evening she was hysterical. This was sheer malice on the man's part, but it took some time to quiet Helen down and to investigate further. None of the man's allegations had any foundation.

The Winters wanted to stay in the house until the end of the summer, which was all right with us, because we were well fixed at the Pierce's for several months. I planned to make a \$10,000 cash down payment -- \$8,000 from the Gatlinburg house, \$1,000 saved in Los Angeles, and \$1,000 borrowed from my Mutual of New York Life Insurance. I was embarrassed at so puny a sum, but learned that it was much more than was usually put into purchases in the area. It was arranged for the rest to be covered by the Marble Mortgage Company. Some good fairy must have been looking after us, for the mortgage terms were very generous --  $6\frac{1}{2}\%$  interest -- and interest rates were going up; they became much higher almost at once.

Jessie King had died that spring on March 26; Bertha had died in June of the year before. We learned that her estate would be settled that fall, so I was not especially worried about the money I was borrowing.

We set about buying a stove, refrigerator, washing machine, and many other things for the new place -- all to be delivered at the end of the summer.

During the summer I made some trips. In early August (6-10), I borrowed a Survey car and drove over the Sierra Nevada to the east side to visit Clem Nelson at Lone Pine in the Owens Valley. I went with him for several days to see the work he was doing on the Lower Cambrian in the Inyo and White Mountains to the east. This was an important sequence (Walcott had named it the "Waucoban" years ago), and Clem had been working on it patiently for a long time. We saw many interesting areas, including a trip over the crest of the White Mountains (as high as the Sierra Nevada, but more barren), and down a steep jeep road into Owens Valley; and one south to the bottom of Saline Valley (a creepily lonely place).

In late August (24-28) I flew north to Eugene, Oregon, where I was met by Parke Snively, who wanted to show me the coastal country of the Northwest. We went from Eugene to Newport on the coast, where he was spending the summer with his family. Then north through Washington State to the Olympic Mountains where Bob Brown and Howard Gower were at work. The Olympics and the coast to the north were especially exciting. The mountains were appallingly steep, and as densely forested as the Smokies -- or more so. At the end of the trip we went back via a succession of ferries across Puget Sound to Seattle, where I took the plane home (we got there just in time for take-off).

Moving day arrived at last, on Labor Day in early September, 1956. The Winters had vacated, and during the day I brought down all of our things from the Pierce's, in many trips by car. The new stove and other appliances were delivered and set up. During the summer our household goods had been brought out from Denver and stored in the local Bekins' warehouse. They were not delivered until late in the day; it was good to see our things again, which we had not seen for two long years. (When everything was set up, I looked in the desk drawer of the case in my study, and sure enough, there was the key to the Robby Robinson house in Denver, which we had missed on our arrival there two years before!)

The International Geological Congress met in Mexico City that September, and although I had anticipated attending two years ago, I now decided not to go. The time coincided almost exactly with our moving into our new home, and I could not stand the expense, on top of our moving expenses.

On October 1-3 I went on a trip with Loren Clark to the Mother Lode belt of the Sierra Nevada, and on October 7-20 on a long trip east. I spent the first week in Washington on various conferences -- on illustrations for the Northeast Tennessee Professional Paper, with George Cohee on plans for a new Tectonic Map of the U.S., with a group to make plans for the proposed Geologic Map of the U.S. (premature, as it turned out), and with Bob Neuman on Great Smokies terminology. The second week was in Denver, where I talked of Great Smokies problems with Jerry Hadley and Warren Hamilton, and with Ed McKee on plans for a Permian paleotectonic folio. I also supervised the shipment of my boxes of Survey things that were stored in the Denver office to Menlo Park.

When I was in Washington I had dinner at the Cosmos Club with Brookes and Madge Knight, who were up for a visit from Florida -- the last time I would see them. I also called on Harry Finney, who told me that he was going to give us \$5,000. That, and the Jessie King estate which was settled that fall to give us \$4,512, gave us nearly \$10,000 with which to get started on our new home. I repaid the insurance loan, and paid in several thousand dollars on the mortgage. The rest all went for a thousand and one little unanticipated expenses incidental to getting settled.

On January 22, 1957, after what seemed like an interminable wait, the new office building was finally ready for occupancy. During the preceding months we had been able to go into the partly completed building and look at the rooms that were to be assigned to us, with their rough cement walls and floors. Then there was a long period when the inside work had been completed, but before the building could be inspected and the contract approved. Finally, the big day arrived, when the geologists could occupy their rooms. Movers were provided to bring in possessions, but all of us were too impatient, and carried over many of our belongings ourselves by hand. My room was 2017, a large one on the second floor, facing the back, which I was to occupy for nearly 20 of the next succeeding years. I brought over my journals, books, and maps, and spent weeks putting them on the shelves and into the map drawers.

Once settled, I continued work on the Great Smokies report, and its maps and figures. Also, a new Tectonic Map of the United States was being planned by the American Association of Petroleum Geologists and the U.S. Geological Survey, this time under the direction of George Cohee. As before, different parts of the country were assigned to various compilers, and this time I was asked to compile the Appalachians. I spent much time that spring on assembling this area, and plotting it on the final scale.

We decided that, since it would be some time before our long reports on the Great Smoky Mountains could be published, we had better get out a preliminary journal article on the Ocoee stratigraphy. That spring, our group did much correspondence on the matter, and there was quite a wrangle as to what form it should take, as each man in the party had different ideas. An added complication was the classification of the Chilhowee Group, which had always been called Cambrian (with which I strongly agreed). Warren Hamilton wanted to call it Precambrian, and to my dismay I found that he was supported by Jim Gilluly, who was very dictatorial on the matter. When the article was finally finished, late in May, the Chilhowee went in as "Precambrian(?)" -- an abortion that took a year or more to straighten out.

Another enterprise came up which had a seemingly innocent beginning, but which became larger and larger, so that it took much of my time later in the year. On August 26-27 there was to be a symposium at Stanford, sponsored by the Society of Systematic Zoology, on "Origin and affinities of terrestrial and fresh-water faunas of western North America", and I was asked to give an introductory paper on the geological background -- on "Evolution of modern surface features of western North America". I spent much time on this during the summer, and on the manuscript and illustrations in the fall, after the meeting was over. This work was not a complete loss, as much of the material could be used again in my book on "Evolution of North America", on which I was busily at work outside of working hours.

On March 22, 1957, there was a severe earthquake, which shook our new building where I was at work. This was the most perceptible earth shock that I have felt since we came to California, before or since. The actual effects in our area were rather slight, but there was severe damage in South San Francisco to the north. The epicenter, for a change, was on the San Andreas fault, on which there had been but little movement since the great earthquake of 1906.

Between terms at Stanford, March 19-24, Trudy decided to have her knee operated on, to alleviate her congenital defect, which was giving her constant sprains. The advantage at this point was that she could get most of her surgical and hospital expenses paid by the university. The operation was a success, in spite of Helen's and my misgivings, although she had to spend several weeks in a cast; she has had no further trouble with her knee since then.

On April 18-21, the Cordilleran Section of the Geological Society of America met at U.C.L.A. in Los Angeles, and I went down and back in a Survey car, along with four other Survey geologists. I stayed at the Grant's home during the meeting; Charlie Hunt, who had come out from Denver, was also a guest there. It was like "old home week" for me, and a chance to see people and places familiar from my two years stay there. There were many parties -- a big cocktail party at the Grant's the night of the banquet, another later at the Dave Griggs's, and dinner at the Tunell's.

On May 25-June 3, I made a trip to the Denver office, going and coming each way by train. Going east, I rode the California Zephyr -- a wonderful way to see the country as I had found in 1952, on the way back from my Nevada trip with Gilluly. One of the days in Denver, I attended a meeting of the Paleotectonics group, helping plan for the folio on the Permian System. The rest of the time was spent with the Great Smokies party -- Hadley, Neuman, Hamilton, and I -- putting in final form our preliminary paper on the Ocoee Series. It was a cantankerous, contentious session. Our years in Gatlinburg had been too close, and there was an atmosphere of hostility and suspicion -- especially for me. (Fortunately, this atmosphere gradually cleared in later years). Sentences of a proposed text were read and argued over, until we could reach some kind of an agreement over the wording. Finally, we had it all done.

For me, the most constructive result of the trip was that Jim Gilluly informed me that I should have my own secretary-typist at Menlo Park. This would be a great boon, because I was the only member of our branch at the Menlo Park office, and I was at the mercy of the good will of the other branches for the most elementary services. The idea became activated during the summer, and after many job interviews Barbara L. Patrick was hired, and began work August 12. (She had heard about the job earlier and actively sought it, as it meant a raise from GS-3 to GS-4, but many formalities had to be gone through before she got it). Barbara was a red-head and very able; she made my office sing. She stayed with me only a year, to my distress, and then went on to a better grade in another Survey office. (One of the other girls said later that she was a "job-jumper".)

Trudy spent the summer in England, with a group called "Experiment in International Living", in which each of the young people lived for a while with a family in a host country, then went on a hiking tour of England together. She was gone from June 18 to September 20. Helen and I thought we could relax on expenses while she was gone and enjoy ourselves a little, but we were sadly disillusioned. I found that she had overdrawn her checking account by several hundred dollars, and after I made that up, her unpaid bills began to come in, which took us all summer to pay for. That fall, we made her get a loan from Stanford to pay for her fall term, and she had to make it up with her own earnings next year.

I made several field trips that summer. This was to become a pattern during the next 10 years, as it was desirable for me to see as much of the country as possible, in preparation for the work on the eventual Geologic Map of the United States.

Between July 6 and 15, I went with Paul Bateman on a long inspection trip to eastern California and across Nevada. We looked at work being done on the east side of the Sierra Nevada by Dean Reinhart and King Huber, and by Jim Moore and Cliff Hopson; we also visited the Pine Creek tungsten mine which Paul had studied earlier. Then we drove across Nevada to the southern Snake Range, to visit the party of Alan Griggs and Don Whitebread. We slept in the open by their trailers, but were kept awake all night by ravenous mosquitos, so it was good the next night to sleep indoors at a motel in Ely. From there, we went west to Eureka, and spent a day with Tom Nolan, who demonstrated the geology

of that classical area. Our last stop was at Lovelock to visit the party of Bob Wallace, Norm Silberling, and Don Tatlock in the Humboldt Range. Then we went back across the Sierra Nevada by the next pass south of Lake Tahoe, and home.

On July 31 to August 4, I visited the Redding area in northern California, to see the work of John Albers and Porter Irwin. I drove up with Edgar Bailey, who showed me the geology along the western side of the Sacramento Valley, and the Mesozoic rocks later called the "Great Valley sequence." Out of Redding, we looked at the Paleozoic and Mesozoic rocks of the southern Klamath Mountains, including a long boat ride up the newly impounded Shasta Lake. I drove back early on the morning of the fourth, to escape the daytime heat of the Sacramento Valley, and was happy at last to come into the fog and cool air of the San Francisco Bay area.

On October 23 to November 1, I made a trip to Texas, going and coming by train. A field trip to the Glass Mountains by the West Texas Geological Society was scheduled, with me and Gus Cooper as invited leaders. The weather started out fine, but the last part of the trip was spoiled because a "norther" blew in, with low clouds and cold winds. At the end of the trip I went back to Austin with Pete Flawn, and we discussed plans for a project on the "Ouachita system" in which I was to take part during the next few years.

In the fall, on top of my other obligations, the compilations which had been made for a new Geologic Map of North America came to me for review. This had been assembled by a committee of the Geological Society of America under the direction of Ed Goddard, now at the University of Michigan. I found many things to criticize, for the whole thing had been put together with little coordination -- "a horse put together by a committee." Many faults were shown in "faultless" Indiana, and none at all in much-faulted Utah and New Mexico. Formations did not match across state lines, and so on. I gave the map "the works" and probably did it much good. When it was published 10 years later, I was embarrassed to see my name listed among the authors, whereas I had been merely a destructive critic.

At Christmas time, we gave a mammoth egg-nog party for the Survey group in our home, and about 60 people attended.

Throughout the year, I had worked along outside of office hours, on "Evolution of North America", and in December I took large amounts of annual leave to draft the accompanying figures. I completed the whole job toward the end of the year, and on January 2, 1958, mailed the first half to Princeton University Press; the last half was sent on February 3.

During the first half of 1958, I worked steadily on the report on the Central Great Smoky Mountains, and finished most of the text by the end of August. During the first part of the summer I spent nearly a month reviewing the manuscript by Hadley and Goldsmith on the Eastern Great Smoky Mountains. There was a great mass of good information, but it was turgidly written, so that it required much editing and comment. (Hamilton's report on the Richardson Cove and Jones Cove quadrangles had been read by me the year before).

There were also manuscript reviews on other subjects -- the report on the Sierra Blanca area, Texas, by J. Fred Smith and Claude Albritton; the Denali fault, Alaska, by some of the Alaskan geologists; the Bannock thrust, Idaho, by Armstrong and Cressman; the Permian correlation chart, assembled by Carl Dunbar; the Moose River synclinorium, Maine, by Arthur Boucot; and various others.

On March 17 I received unannounced a package of maps from the U.S.S.R., sent by Professor A. A. Bogdanoff of Moscow University. There was a geologic map of the Soviet Union, and a tectonic map. Since the captions were all in Cyrillic lettering, it took some time to get them translated and to understand them. The tectonic map was puzzling at first, as it was done according to quite different principles from our own tectonic map of the U.S. During the summer I wrote an appraisal and sent copies to our own tectonic map committee. On a chance, I also sent a copy of the appraisal to Bogdanoff, and to my surprise received a nice reply. Thus began a long friendship, first by correspondence, then by personal contacts abroad, which only ended with his death in September 18, 1971.

In March, I went down for a day to Los Angeles to attend the meeting of the U.S. Tectonic Map committee, which was held during the national A.A.P.G. convention. I did not stay for the meeting, but came home that night on the train.

From March 26-30 I attended the meeting of the Cordilleran Section of the G.S.A. in Eugene, Oregon. First, I went up by plane to Portland, and went on a one-day trip up the Columbia River gorge. After the meeting, I drove back with Edgar Bailey and Porter Irwin. We stopped to look at outcrops of the Mesozoic rocks near Roseburg, then over the Siskiyou Mountains into northern California, spending the night in Eureka. The rains had settled in, and most of the trip was in pouring rain, but I got many good ideas about the Franciscan complex.

On April 12-19 I drove into southern Nevada. The first few days Hank Cornwall and Frank Kleinhamper demonstrated to a group of us the work they were doing on Bare Mountain south of Beatty. Then I went south with Chester Longwell to the Las Vegas area, where he was at work with Bill Hayes. Chester went out of his way to show me around, and we went to all the places that he had intended to show me during the snowy visit of 1948, and more. We saw many places in the Spring Mountains west of Las Vegas, and in the Muddy Mountains to the east, including his upside-down "anticline" and the Buffington window. We looked again at the area around Frenchman Mountain, and went south of the Colorado River into Arizona to see his great fossil landslide, looking much like the Blackhawk Canyon landslide in California, but bigger. Unfortunately, I was not taking photographs, so I kept no picture record. This was doubly unfortunate because the desert was never more beautiful than that spring, with all the desert flowers in full bloom. At the end of the trip I had a long, wearisome one-day drive back to home base.

When June came, Trudy graduated from Stanford, and we went to the graduation exercises. I had rather hoped that she would get academic honors such as Phi Beta Kappa, which she was quite capable of doing, but she had not tried hard enough. She had spent her last two years as an art major. Immediately on graduating, she applied for another job with the Survey, and was taken on as a draftsman again, this time with a rating of GS-3. She used the money earned that summer and through September to pay back her loan from Stanford for her fall term of the year before.

All was not well with my secretarial help. Barbara Patrick had found a better job elsewhere in the Survey, and left me on August 19. I had to go through the agony again of a new set of job interviews, and finally picked Mrs. Adele Kenneley, who came from outside the Survey. She had a most impressive record, and had evidently done a lot of good work when she was younger, but she didn't work out. She was married now and not in good health, and had a little girl to worry about, so she was constantly staying out sick. To add to complications, I had to leave almost at once for nearly a month on a long trip to the east, so I never really got her into the swing of things. I left her with a big pile of Great Smokies manuscript to do while I was away, and she did this beautifully. On October 10, she finally announced that she was leaving, with the added sting that she didn't think much of the job. So I was back again where I had started.

Fortunately Ruth Cobb ("Twitch") who had worked for a long time for the Survey decided at this happy moment to come back to the Survey, and I hired her with delight. She was a hunchback, and was married to Ed Cobb of the Alaskan Branch, and knew her way around the organization. She was my faithful helper for nearly two years.

About the time Barbara was leaving, plans were developing for me to make a long trip to the east, mainly to New England, and she spent much of her last time with me telephoning about arrangements. I had decided to make the whole trip by train, and to stop off on the way to see Glacier National Park in Montana, so arrangements became quite complicated.

I finally got away on August 31, and went north to Portland, arriving in the morning. This meant a long wait in Portland next day (Labor Day), as my train out did not leave until 9:15 that night. I sat in the station most of the day, reading books I had brought with me. My train left at last, and all next day we went across the mountain country of northern Idaho and Montana (my first and last view of it). I alighted in the evening at the station on the west side of Glacier Park and spent the night at the hotel in the park. Next day we were driven by bus across the mountains on the "Going-to-the-Sun" Highway, and north to Many Glacier Lodge. This trip I began to take kodachrome photographs with a little Brownie Starflex camera, and I got some very good pictures here, and to many other places I have visited in later years. I went on tours around Many Glacier and saw

much of the famous Belt Series and the Lewis overthrust. On the last day we went south to the hotel by the railroad on the east side of the park, and took the evening train for the east. Although I had no geological guidance in Glacier Park, the rocks and structures were clearly displayed, and I got a lot out of the visit.

There was a long tedious train trip from there into Chicago, and another one from there to Boston, where I arrived on another Sunday, several days later. Here, I found that nothing worked any more; trains which were in the timetables back home no longer existed. So I had to wait most of the day in Boston station for an evening train to Bangor, Maine, where I was to meet Bob Neuman. Bob had volunteered to organize a trip to show me Maine geology -- an area in which he had been working after the end of the Smokies project. Bob and Lou Pavlides met me at the station in the evening at Bangor -- a faithful pair who had wondered where I was and what had happened to me.

Next morning (September 8) we drove north to Greenville on Moosehead Lake and the start of the week's go-round on northern Maine. At one time or other on the trip we were joined by Art Boucot, Arden Albee, and Andy Griscom. We went west for several days, then east around the north side of Mount Katahdin, then north to the "potato country". Some of the rocks were enigmatic, others had a rather clear stratigraphy.

Leaving the northeast corner of Maine on September 14, Bob and I drove into French Quebec to Cabano, where we joined a group of geologists of the Quebec Bureau of Mines who were mapping in the area -- Beland, L'Esperance, and their assistants. Art Boucot had come up to advise them on their stratigraphy, and Fitz Osborne of Laval University had come over as a senior supervisor for the Bureau. Fitz had been at Yale when I was there in the twenties, and it was good to see him again. He said, "Well, Phil, I'm glad to see that you have lost your Iowa accent." From Cabano, Art, Bob, and I went with Beland eastward along the south shore of the St. Lawrence Estuary, then south to Lake Matapedia (where Geoff Crickmay had done his doctor's dissertation at Yale when I was there in the twenties). On September 19, we left French Canada at last, and I bade farewell to the hated French language which was all that was spoken there.

Bob and I went back to Maine for a night at Patton, where he had made his headquarters during his field work, and next day he took me

back to Bangor, where I was to wait all day to take a night train to New York. During the last day or so together we talked about the Smokies problems, and found that our ideas were converging along lines which were somewhat at variance with those of the rest of the party.

I spent September 20-22 in New York, staying with Edward and Grace. One day I went down to Princeton, and was given galley proof of Evolution of North America, which I began reading with delight. Another day, I visited Robert and Clara in Mamaroneck.

On September 23, I took the train for home, a tedious ride of several days. It was good to be back again, after my long safari, but California was in the middle of a late heat wave.

I found that Trudy was finishing up her work for the Survey, and had decided to live for a while in San Francisco. She had found an apartment in a ratty building on Post Street near Van Ness, and went out job hunting in the city. She found a job with the Bureau of Public Roads, now with a rating of GS-5, and lived there for over a year alone.

On October 15-17, just when Mrs. Kennely was leaving, I went again with Loren Clark to the Sierra Nevada, this time to the northern part of the Mother Lode belt, as far as Downieville and Sierra City.

On November 4-14 I made another trip to the east, attending the St. Louis meeting of the G.S.A., and on to Washington. At St. Louis there was a meeting of the Tectonic Map Committee. In Washington, I spent my time on the text and illustrations of the Northeast Tennessee report, which was finally moving after long inaction. Manuscript processing had deteriorated on the Survey, and strange and wonderful things were being done to my materials. The manuscript was put in the hands of Rosa Tisdale, a determined and rather ignorant lady, who had set about virtually rewriting it, despite the fact that it had been approved for publication by the Director. In places she changed my "granites" to "granite deposits." and committed other horrors. (She should have been fired, but was allowed to resign and go with the Army Map Service). The illustrations had been equally mishandled by a multitude of "know-it-alls" and took much time to straighten out again.

In January, 1959, I finally completed the interpretive chapter for the Great Smokies report, after much thought and effort, and the manuscript was finished at last. I then sent it for review to the other members of the field party, who read it during the spring and summer.

I got a very sour review from Hadley, and an especially vicious one from Hamilton; the bad feelings in the party still survived, especially toward me. I asked Hal James, who had the office next to me, to go over it again, and it fared better. I finally submitted the revised manuscript to the Branch in the fall, and it was promptly approved and sent forward by Fred Smith, who was now Chief of General Geology (Jim Gilluly had gone on, to be Chief of the Fuels Branch).

With the Smokies report out of the way, I started at last to write the Sierra Diablo report (20 years after completion of the field work), and continued on this through 1960. It was good to get onto this long-awaited task. I sent some of the writing to my old field companion Brookes Knight in Florida, and was happy to know that I could give him an idea of my intentions; he had always had a deep interest in the project, and had been distressed at my long delay in doing something about it. Unfortunately, he did not live to see its completion, as he died in February, 1960.

Pete Flawn was planning a big report for Bureau of Economic Geology publication on the "Ouachita system" (= orogenic belt), making extensive use of subsurface data, and he began sending me a succession of chapters for my reading and critical review. Later, he asked me to prepare several chapters of the report (see 1960).

I received page proof of "Evolution of North America" in January, 1959, and the book was published in April. In October, I received my first royalty check, for the munificent sum of \$1,200.00. I had visions of getting quite an income from it, but subsequent checks were half as much or less. The book got some nice reviews in peripheral journals, but I was distressed to find that none of the well-known geological journals in America (the American Journal of Science, the Bulletin of the A.A.P.G., or the Journal of Geology) ever reviewed it; this may have been an accident, but it cut me deeply. The book was a "sleeper", which gradually made its way on its own merit. Colleges began requiring it for class use, and in 1961 it was translated into Russian, where it had a wide sale. Nearly twenty years later, when I was already at work on a revision, copies of the original edition were still being sold, somewhat to my embarrassment.

In the spring of 1959 I went on a one-day field trip with George Schlocker and Davey Jones to see the Franciscan rocks of the San Francisco area, and in November the Pick and Hammer Club staged a much larger field trip to the same area.

The Cordilleran Section of the G.S.A. met in Tucson in early April. Before the meeting there was a one-day trip over the Santa Catalina Mountains, but it was rather inconclusive because the leaders had data only from student theses on small areas, and had not been able to put together the big picture on the metamorphic complex. After the meeting, there was a three-day field trip on "General geology of southeastern Arizona" to be led by Evans Mayo. This sounded interesting, so I went, but it was a disappointment. Most of the stops were out in the valleys, where the leaders arm-waved about the geology in view in the distant mountains. Moreover, Bob Smith, an aggressive guy from Chevron Oil Company, virtually took over the stops from poor wishy-washy Mayo, and filled the air with his prejudices. Bill Moran of Union Oil Company was along, and was good company for me. We stopped one night in Douglas, and in the evening many of us went over the border to Agua Prieta for dinner. On my return to Tucson I had a morning's visit with Carl Fries, who had come up from Mexico for a year at the University, and he took me to the plane at the end.

From April 20 to 25 I was in Washington, mainly to attend a Names Committee mtg. on the base of the Cambrian. We had at last smoked out Jim Gilluly and his arbitrary ideas on the base of the Cambrian and the age of the Chilhowee Group, which had beclouded our work in the paper on the Ocoee Series in 1957. Much of this was due to the aggressive actions of Pres Cloud; I had been unable to make an impression on the matter. Early in the meeting, Jim announced a recommendation that all beds with fossils should be classed as "Cambrian" and that strata beneath down to a natural physical boundary should be called "Cambrian(?)" -- which was what I had wanted all along. This pretty well took care of things and knocked the wind out of further argument, but we continued to discuss the matter the rest of the day.

The spring before, when I had been absent from the Le Conte Club meeting, I was put up for president without my knowledge (the only geological office I have held in the Bay Area). The other officers for the year were to be John Harbaugh of Stanford and Garniss Curtis of Berkeley. We had an organization meeting in the fall, and Garniss proposed that, contrary to the usual pot-pourri of papers on the club programs, we should have symposia. He suggested one on the Great Basin, so I set about organizing it for our meeting in Berkeley in December. I went through the tortures of the damned getting speakers, but it all fell into place eventually. Besides local speakers, Carl Hubbs agreed to come up from San Diego and talk about the vanished Pleistocene lakes and rivers of the Great Basin. The meeting was a great success, and we determined to have another symposium at our spring meeting.

On December 8 I received from George Cohee prints of the new Tectonic Map of the United States. He was very proud of the completed job, but to me the results were highly disappointing; the quality of the data shown, and of the drafting were far below those of our first version of 1944. Nobody else in the Menlo Park office liked the results either. I hung the map on the tack board in the hall, and people who came by would yelp and scream in indignation, or (unkindest of all) just laugh. Many people volunteered changes or improvements, and I myself went over the map inch by inch, making hundreds of comments and corrections.

I sent all this back to Washington at the end of the month, ending my remarks with "George, I'm awfully, awfully sorry." It must have been a real blow to dear old George, but he took it very gracefully, and invited me to come in during February and go over the map with him, and to plan what next should be done (more about this later).

On the evening of December 12, Trudy phoned us that she and Daryl Reagan had been married, and were at the home of Mr. and Mrs. Helm in Palo Alto, asking us to come over. Trudy had met Daryl when she was at Stanford, and he had just finished as a graduate student in physics. She had a number of other casual boy-friends in school, but in the last few years Daryl had been the only one. Marriage had seemed increasingly likely, but I guess they shied away from a big formal wedding (some of the Survey wives were trying to plan one for us), so just went ahead with it and presented us with the accomplished fact.

Daryl had just returned from a physics laboratory in Oxford, England, and was currently employed at the Lawrence Laboratory in Livermore. Trudy disposed of her apartment in San Francisco, and they made their first home in a rented house in a run-down section in Oakland, where he could commute to Livermore, and she to the University of California in Berkeley, where she was taking art courses. In May, 1960, they bought a house in Livermore.

Through the year 1960 I worked as time would permit on the Sierra Diablo report, but there were many interruptions from other things. Pete Flawn's Ouachita report was progressing, and he asked me to do two chapters. One was on the history of the Ouachita system, the other on the extension of the Ouachita rocks and structures east of the Mississippi River. The latter was mainly on the subsurface -- a subject on which Pete was mainly concerned, but he said that the eastern part was more than he could handle, and he asked me to relieve him. This was an interesting and challenging subject, as it involved the possible relation between the Ouachitas and the Appalachians, but it was difficult for me to get information from the distance of Menlo Park. Paul Applin, who was our Survey geologist in Jackson, Mississippi, was a great and good friend in the enterprise, and he dug up a great deal of information for me. He also called my attention to a recent Survey Bulletin and map, which was a register of all wildcat wells drilled in Mississippi, compiled by Helen M. Beikman and Sophie Drakoulis, two young Survey ladies who were unknown to me. I finally succeeded in assembling data of sorts on most of the drilling that had been done between the Ouachitas and the Appalachians, and was able to draw some conclusions, mostly quite new to me, at least.

During the last half of April, I worked on a long manuscript by Charles A. Ross on the Wolfcamp Series of the Glass Mountains. This had been written for a doctor's dissertation at Yale under Carl Dunbar, and had been submitted to the Geological Society of America for publication as a memoir; it was sent to me for review by the Society. I found it brashly written and full of questionable ideas, although it had been read and approved by the Yale faculty. I decided to give the kid the shock of his life, and a taste of the cold outside world, so "gave it the works". Neither Dunbar nor Ross took this gracefully, so there were some unpleasant repercussions, but when the memoir was finally published a few years later, I was pleased to see that my recommendations had been followed, after all.

On February 22-27 I went to Washington at the invitation of George Cohee to go over the Tectonic Map. He had decided to junk the newly printed version that I had received in December, and to start over for a final edition, with better editing and with the addition of more data. We talked about this for several days, and I agreed to work over some of the more troublesome areas myself, which I did in succeeding months.

On April 5-8, I went to Ohio State University at Columbus to give a set of lectures -- the "Bownocker lectures", they were called, which were given every year by an invited speaker. I talked on the Capitan reef, on low-angle thrust faults, and on the Great Smoky Mountains. I was taken on a succession of visits to different members of the staff, and quite talked myself out. I was also entertained at luncheons and parties, the last evening at a cocktail party at Ed Spieker's -- made somewhat uncomfortable because of his well-known alcoholism.

The Le Conte Club was coming up again for its spring meeting in April, and all my worries for the fall meeting fell on me again. John Harbaugh suggested a symposium on the San Andreas fault, which I liked, but to our surprise we ran into vehement opposition from the other officer, Garniss Curtiss, who said that he and the other people at Berkeley didn't want to hear any more about the fault. John and I went ahead anyway. After much struggle we got a good program together. John Crowell came up from Los Angeles to talk, and I got a retired Rear Admiral from the Coast and Geodetic Survey to talk about geodimeter work along the fault; I don't recall the other speakers. The spring meeting was at Stanford. I enlisted Helen to give the ladies' tea at our home, and a large number of them attended. So my year as President of the Le Conte Club ended up as a success, after all.

The Cordilleran Section of the G.S.A. met at the University of British Columbia in Vancouver, and I went north to see new country. Before the meeting there was a one-day field trip up the coast along the Coast Batholith to Squamish, and after the meeting there was a three-day field trip northeastward into the Interior Plateaus as far as Kamloops. We crossed the northern Cascade Range just above the international border, and returned down the gorge of the Fraser River. I saw many significant rock formations and was impressed with the incredible height and steepness of the mountains. We got back to

Vancouver very late in the evening, and I was glad I had a hotel reservation. Next morning, I took the plane home, with fine views of the volcanoes of the Cascade Range.

The summer of 1960 the International Geological Congress was to meet in Copenhagen, Denmark, and many of us in Menlo Park (as well as geologists from all over the country) decided to attend. In the fall of 1959 I received intimations that I would be sent as an official delegate from the Survey, so I began making plans. Travel arrangements for the Congress were being made by an outfit called "European Traveling Seminar", being run by a Dane, Arne Sorensen, which issued glowing prospectuses, but they proved to be very inefficient, so between them and our own Foreign Geology Branch it was all very chancy up to the moment of leaving. It was to be my first trip abroad (outside of Canada and Mexico). I had expected that Helen would go with me, but she backed out at the end -- a shame because this was the best European trip I have had. Since this was to be a memorable trip for me, I kept a detailed daily record, which I have preserved elsewhere, so here I will give only an outline of the trip.

The "Seminar" advertised tours before the Congress~~es~~ and I signed up for a 5-week tour in July. After all the initial confusion, this worked out very well. Our tour leader was Douglas Berggren, a young philosophy instructor at Yale, who was a bit precious, but quite helpful. On June 25 we flew in a cramped plane from New York to Glasgow, toured the northwest Scottish Highlands, went by train from Edinburgh to Birmingham, England, by bus to London through Stratford, Oxford, and Windsor. After a few days in London we flew to Amsterdam for a tour of Holland, then by plane to Paris for several days. We flew from Paris to Switzerland, had a one-day trip through the Black Forest in Germany, then across the Alps over St. Gottard Pass on a marvelously clear day to Lugano. From here we went to Milan and across the Lombardy plain to Venice, then north through the Dolomites to Bolzano. From there we cross<sup>ed</sup> the Alps again over Brenner Pass to Innsbruck, then to Munich, to Salzburg, and back through Munich, across Bavaria and down the valley of the Neckar River to Heidelberg. Here, I made the acquaintance of our Survey Military Geology team who were at work there, and got filled in on the geology we had been over. From Heidelberg we went down the Rhine (part of the trip by boat) to Cologne, and by plane on July 30 to Copenhagen and the end of the E.T.S. tour.

It was good to have a few day's rest and inaction in Copenhagen, to recuperate from our dizzy pace across Europe and catch up on my notes, but in the end I was glad to move on. On August 4 I went by train to Oslo, Norway (with a ferry trip from Denmark across to Sweden). In Oslo I joined the Congress excursion across south-central Norway. During my stay in Oslo, I met by pre-arrangement with George Cohee who had just arrived from the States, to join another excursion. He told me about impending developments on international tectonic maps (more of this below). We had a 10-day trip in Norway under the guidance of Professor Trygve Strand, and were impressed with the beauty of the country. A good many American geologists were in the party, including Max Crittenden and Bill Rubey.

At the end of the excursion on August 13, we went south by train, but Max Crittenden and I got off at Lillehammer and were met by Mabel and Beth Crittenden. In the early morning hours we drove south along Lake Mjosa to look at the Eocambrian Moelv Tillite, and then on through Oslo. The next day we drove across southwestern Sweden and back to Copenhagen, to the start of the Congress.

I attended some of the technical sessions, but then became involved in the meetings of the Subcommittee of the Tectonic Map of the World, where it was my pleasure to meet Professor Bogdanoff at last. I found that our American delegation was under pressure to produce a Tectonic Map of North America, and the general sentiment (from the Chief Geologist on down) was that I was to be the one to do it. This was a setback, because I was nearly ready to start work on the long-planned Geologic Map of the United States. This would now have to be postponed again, so I consented to work on the North America map with reluctance.

One of the other highlights of the Congress was a dinner party given by Lauge Koch at his apartment for a selection of foreign geologists. He was the famous Greenland geologist, and Director of the Danish East Greenland Expeditions. At the party I had an opportunity to meet John Haller, a young Swiss geologist, whose publications on Greenland I had been receiving.

The Congress was over at last on August 24, and I flew back to the States in another cramped plane, with touchdowns in Greenland and Winnipeg. The plane had been much delayed, so I finally got home very late. It was good to see Helen again, and to give her my two principal gifts from abroad -- a peridot ring from Amsterdam, and a cameo bracelet from Venice.

When I got home I learned that Trudy had had her baby, who was born the last week that I was away. She named him Jeffry Reagan. Helen was glad that she had not been abroad with me, so that she could be on hand for the great event.

During the preceding spring, the Geologic Division of the Survey had been reorganized, but actual activation of the reorganization had been postponed until everyone got back from Europe; now it was upon us. The old economic divisions of Fuels and Minerals, and most of the others were abolished, including my own little General Geology Branch. All the projects were regionalized -- into Eastern, Rocky Mountain, Southwestern, Pacific, etc. branches. Like the others, I floundered in a sea of uncertainty; I lost my ties with our Denver office, and I did not fit into any of the regional categories. For administrative purposes, I was formally attached to the Pacific Branch, headquartered in my own office center. This at least made it possible to go directly to our Branch Chief, who was Parke Snavely. For nearly 20 years I had always been away from my Branch office, in some distant place, which had created many problems.

Twitch stayed on for awhile that fall, but had become bored with Survey work, and finally left. Getting another typist for me was now in the hands of the administrative office, and early in 1961, Ted Sumida gave me Marion Bristol, a little twittery woman who was very officious. She finally left in June, 1962, much to my relief (but that is getting ahead of my story).

I spent much of the fall organizing my notes on my trip abroad, and sorting my many photographs and postcards, all of which I had bound up into three volumes for permanent record.

The national meeting of the G.S.A. was to be in Denver in early November, but rather than go to it, I decided to join a field conference in Arizona a little later, on November 8-15. The principal objective of the conference was to go over the problems of the Apache Group and the overlying Cambrian, a subject in which I had long been interested. Most of the participants were members of the newly created Southwestern Branch -- Cy Creasy, John Cooper, Medora Krieger, Andy Shride, Ron Wilden, and Don Peterson; Bob Wallace was along as Branch Chief, Pete Palmer was paleontologist, and Kathleen Tagg (later Montgomery) was Medora's field assistant. We assembled in Tucson, and that evening watched TV on the Presidential election returns -- Kennedy vs. Nixon.

We started in the south near Benson, and worked our way north as far as the Sierra Ancha and the Salt River Canyon. We looked at sections in each geologist's area in turn, thus getting a quick review of the whole picture. This was quite enlightening to us outsiders, and also to the local geologists, who had never viewed the regional relations themselves. We could see gradual changes in the Cambrian northward, and the unconformity between it and the Troy Quartzite. The diabase was shown to be intrusive into the Troy, but not into the similar-appearing overlying Cambrian; in places the basal Cambrian contained diabase clasts. I saw again the Sonoran desert vegetation, so different from any other that it seemed to belong on another planet.

Through the first half of 1961 I continued work on the Sierra Diablo report, and I transmitted it at last on July 20. It gave me a peculiar feeling. Up to now, I had always had a great backlog of old field work results and notes still to be worked up. Now, they were all finished, and there were no more. There was still much Survey work for me to do, but it would all be on map compilation -- on the Tectonic Map of North America, and on the Geologic Map of the United States. On the day that I delivered the completed Sierra Diablo report, I got a peculiar phone call from Washington. I had tried for years to get the pre-War Sierra Diablo project officially recognized by the "new Survey," but they went on blithely as though it did not exist. Now, someone in Washington had discovered its existence at last, and wanted to give it a project number! I was pleased to tell them that of that day the project was over, and had been completed!

The Northeastern Tennessee report was published at last as Professional Paper 311, and I received my copies at the end of April. It had been seven long years since I had submitted it in the summer of 1954, and it had been through many vicissitudes -- manhandling by many busy hands, separated by long periods of inaction during which I fretted and stormed. Report processing by the Survey at this time was in a very bad way; fortunately it then became better, and I have had little such trouble and frustration with my later reports.

Proofs of the revised copy of the new Tectonic Map of the United States were received in early April, and I spent nearly three weeks going over it inch by inch, and making corrections of errors. My work on this tectonic map was mainly a labor of love, since I anti-

cipated little credit, and was content to have George Cohee receive primary listing. I simply wished to have the new tectonic map come up to the same standards as our original one of 1944.

I made some trips during the spring. On March 25 to April 1, I attended the meeting of the Cordilleran Section of the G.S.A., which was held at San Diego State College, where there was a very active geology department. Before the meeting there was a two-day field trip across the Peninsular Ranges of San Diego County, very competently organized and led by Gordon Gastil. After the meeting there was a two-day trip to Baja California -- my only opportunity to see this fabled land. We went south along the Pacific coast, with a night at Ensenada, and next day went still further south. The country was much like coastal southernmost California, but still mainly empty and unspoiled.

From April 10-22 I returned again to the Southeast, the occasion being the meeting of the Southeastern Section of the G.S.A. in Knoxville. Before the meeting a group of us gathered in Gatlinburg (my last visit to the place) -- Jerry Hadley, Bob Neuman and me of our original party, as well as Bob Hernon and some others. Hernon had been working in the Ducktown area and had developed some heretical ideas about Ocoee stratigraphy (largely unjustified, as it turned out), and the main purpose of our visit was to show him what we had worked out in the Smokies. During the meeting, Bob Neuman and I took another day to go back to the Smokies and show them to Tom Nolan, the Director.

After the meeting there was a one-day trip to the west edge of the Valley and Ridge province, where George Swingle demonstrated the complex structures he had been working out. After that, a group of us went with Bob Hernon to the Ducktown area for two days to see his results. He had spent a season or two there, carefully traversing the roads and creeks, and measuring innumerable strikes and dips, but he was clearly out of his element in this region of complex metamorphic Appalachian structure, and unaware of its pitfalls and nuances.

After this rather unfortunate field conference, Bob Neuman, Bob Laurence, and I drove south into Alabama to see the famed Talladega Belt. We spent our first night in a fine old southern hotel in Talladega, famed for its lavish meals, and next day drove into the belt itself. The Talladega Belt had had a sad history; Butts had originally interpreted it as a simple southeast-dipping homocline of great thickness, but later detractors had tried to introduce all sorts of complications. We concluded that the relations were about as Butts had said originally, and that the Talladega was a sequence of Paleozoic age, of a different facies than that of the adjoining Valley and Ridge province. One afternoon we searched the fields in the area of the Erin Slate, where Carboniferous plant fossils were supposed to have been collected, but found none. Next day, we looked at the Jemison Chert farther south, and found Devonian brachiopods just as had been reported. In this southern area, however, we saw many curious rocks and formations that had never been adequately described before.

Leaving the Talladega Belt, we went into Birmingham, where Wiley Rogers of the faculty of Birmingham Southern College had offered to put us up for the night in the dormitory. We spent next day with Dick Sheldon, who was doing a Survey job on the sedimentary iron ores of the Birmingham area, and that evening, to repay Wiley's hospitality I talked at the college on the geology of California; it was improptu, and not a too well organized talk. Next morning I took the plane for home.

On May 11-14 I went by invitation to the University of Oklahoma at Norman to give talks. I talked on the Capitan reef, on low-angle thrust faults, and in the evening on the geology of California (better organized and illustrated this time). I had visits with people on the staff of the department and the Oklahoma Survey, and there were some social events.

The Professional Paper manuscript by Bob Neuman and Will Nelson on the western Great Smoky Mountains was received in the middle of May, and I spent nearly a month reviewing it critically. This was the last of our long reports on the Great Smoky Mountains, and thus brought our project finally to an end. It was to be several years, however, before our reports were finally all printed.

On June 12, 1961, I finally made a start on the Tectonic Map of North America. My first objective was to assemble the sub-sea contours of the ocean areas around North America, which had never been adequately represented before, and I spent much time in the ensuing months assembling up-to-date data, and plotting them on the North American base map at 1:5-million.

On August 26-30, Bob Wallace, as Branch Chief, was to go to Nevada to inspect the field parties there, and took me and Ted Sumida along. We spent several days with Fred Smith and Keith Ketner in the Carlin area, and then a day or two each with Hal Mazursky and Pat Muffler in the Cortez Range. On October 2-6 I went again to Nevada to see the work being done by John Albers and his assistants in Esmeralda County; Pete Palmer was also along, and we spent much time collecting Cambrian fossils. We spent our nights at Goldfield, the county seat, now virtually a ghost town. On the way back, I drove into Yosemite Valley for my first look at the place, and had lunch at the lodge there before going home.

On September 26, the text for the new Permian paleotectonic folio was sent to me for review. It was nearly a thousand pages long, and had been put together from contributions of the many team members with little overall review and editing. I found it pretty awful, and gave it a scathing review, crossing out many pages as superfluous padding, recommending in the end that the whole thing be junked, and that the maps alone be published. Tom Dutro, the other reviewer, was equally unsympathetic. The text was very different when it was finally published a year or so later, thanks to our reviews.

On October 26-November 9 I went east. The first objective was to attend the "Grand Appalachian Excursion" conducted by Byron Cooper across the Appalachians, to end at Cincinnati in time for the G.S.A. meeting. I went east by train -- a slow pokey ride by the Southern Pacific to New Orleans, then by the Southern to Winston-Salem, North Carolina. I took along a stack of old New Yorkers to read on the train. On the field trip Cooper elaborated on his heretical ideas of Appalachian structure and stratigraphy. Most of us just "took it" with little comment, but John Rodgers argued it all vigorously.

At the meeting I was glad to have an opportunity to talk with Cliff Stockwell, my counterpart on tectonic map work with the Geological Survey of Canada. After the Cincinnati meeting I went into Washington, where a group of us conferred on plans for the Tectonic Map of North America. It was good to take the plane home at the end of this long trip.

Early in 1961, I had a letter from Professor Kalervo Rankama of Finland, announcing plans for an ambitious set of volumes on the Precambrian of the World, and entreating me to prepare a chapter on the Precambrian of the Southeastern United States. In the latter part of the summer I compiled all the available data and wrote it up, finally sending it off to meet the deadline at the end of the year. This was not to be the end of it, alas, as my chapter, and Prof. Rankama's whole enterprise had a sad subsequent history. Many of the proposed contributors never finished their assignments, so that the volumes that were finally published were very fragmentary. My own chapter underwent a whole series of revisions, mostly to bring it up-to-date, and it was not finally printed until the summer of 1970.

In December, Parke Snavelly proposed that we take on a draftsman for the North America project. The Honolulu Oil Company in San Francisco had disappeared through a merger, and many of its good people were without employment, including their head draftsman, Robinson Easterbrook. Parke proposed that we take him on, and in order to make the job attractive to him fought with the personnel people to get the job set up as a GS-9. Bob came on duty December 11, and set to work on the North American data I had already compiled.

During 1961, Betty Avery, a young rich widow from Fresno moved into the house across the street. She and Helen became good friends at once, and she began having us over for dinner, as she was very lonely. We liked her very much, and she liked us, so that we saw much of each other for the next two years, until she finally moved on to a new life in Carmel.

On January 21, 1962, it snowed in Los Altos, and all over the low ground in the Bay Area -- our first and only time.

One February day in 1962, when I was peacefully at work in the office, I received a phone call from Bill Johnston of Foreign Geology Branch, reporting that the Survey desired to send me to a tectonics conference to be held in Prague, Czechoslovakia, that spring. A trip to Europe so soon after the trip of 1960 was unexpected, and a trip to a communist country behind the Iron Curtain would be something new. I began to make plans. I had learned that my friend John Maxwell was spending another year in Florence, so I wrote to him to arrange for a visit to northern Italy after the Prague meeting.

I set out from home on March 20, and spent several days in Washington getting instructions from the Survey and from the State Department. The Survey gave me a heavy roll of the newly printed Tectonic Map of the United States to take to the meeting to present to those present. On the evening of March 23, I finally struck off into the unknown, by plane to Europe. When I changed to a Prague plane in London, I was delighted to find two Britishers whom I had met in 1960 (Fred Dunning and Frederick Trotter) were also bound for the conference, which did much to relieve my uneasiness and sense of loneliness in a strange venture.

The conference had been called by the Subcommittee for the Tectonic Map of the World, primarily to discuss the Tectonic Map of Europe that was nearing completion, but to go into various other tectonic matters of general interest. A rather small group was present, no more than 30 or 40, and the conference met in the newly built International Hotel in the northern suburbs of Prague. The conference lasted nearly two weeks, counting several days of field trips at the end. Being a small group, discussions were freer than in other foreign meetings I have attended. Each remark was translated in turn into the other languages, so there was an opportunity for me to take down most of what was said. Professor Bogdanoff could not attend, but he sent me, through the other Russians, a strong and cordial invitation to visit Moscow and the U.S.S.R., which I took home with me, determined to do something about it.

There were various entertainments for the visitors (a night at the ballet, a cocktail party, trips to museums, etc.), and during my free days I had an opportunity to wander about in Old Prague, the central part of the city, which I found thoroughly fascinating.

At the close of the conference on April 6, I took a plane to Italy, with a change in Zurich. I went to Rome and by train to Florence, where I eventually connected with John Maxwell. John took me on a merry chase, a highly concentrated view of the northern Apennines, ending up in Milan. I went back to Florence for a few days more of sight-seeing, and then by train to Rome for a few days more of it, finally returning by plane to the States. The trip had been shorter than the one in 1960, but equally rewarding, and filled in places I had not seen on the previous trip.

I was only home about a week when I was off again, this time to attend an S.E.P.M. field trip to the Sierra Diablo in Texas. The field trip was unusually well planned and organized, and gave me an opportunity to demonstrate to the local geologists what we had worked out during our Survey project of the 1930's, on which I had just completed the Professional Paper report. The last evening of the excursion, I was presented with a plaque stating that I was made an honorary member of the Permian Basin Section of the S.E.P.M., which quite overwhelmed me, because it was the first specific honor I had ever received. It also helped to assuage the bitterness that I had felt at the time of the G.S.A. meeting in El Paso in 1949. At the close of the field trip I went to Austin for a few days for a visit with Pete Flawn and others, then took a train from San Antonio, arriving home again on May 5.

While I was on the field trip, Trudy had her second baby, whom she named Russell Reagan.

Bob Easterbrook had made much progress drafting the Tectonic Map of North America. He completed the sub-sea contours and made a start on the land areas, where I had begun assembling the easily available data from our own Tectonic Map of the United States. Bob did excellent work, but was clearly too good for the position and was discontented. In early July he transferred to the Branch of Technical Illustrations, and I was without a draftsman again.

Marion Bristol's daughter was married in June, so she lost her reason for remaining in the Bay Area, and decided to move back to Utah. She left on June 26, so I was temporarily without a typist. Ted Sumida, however, found another girl, Lida Groll, who had worked for the Survey in Denver, and she began work for me on July 16, 1962. Lida had been here ever since, and has been my faithful helper all these years. After a slow start, our friendship deepened, and she, Helen, and I are very close friends.

Long ago, in Austin in 1927, I had attended the lectures on "physical geography" (= geomorphology) by William Morris Davis. I had taken careful notes which I had preserved through the years, but they were in chaotic form, and I had always planned to work them up in a more acceptable manner. I finally decided to do this in 1962. I did this work mostly outside of office hours, but I stole time at the office to work on the drawings. I started the work in January and completed it by the end of October. When finished, I made three xerox copies, one for each Survey library, and I had all four copies bound up in book form.

Helen had committed herself to a visit with Mrs. Reagan, Trudy's mother-in-law, who was teaching school near Longview, Washington, so I was obliged to drive her up. We were away from July 6-9, and we much enjoyed the trip up and back. While there, Mrs. Reagan took us around Longview, a rather grubby sawmill town, and we met all the other relatives.

Without a draftsman, I set about exploring North American geology, trying to decide what tectonic units should be shown on the projected map, and blocking them out. By fall, I had been over all of North America in this manner, and had put together a colored preliminary map -- just showing areas and not structural features. I sent the results to Washington for George Cohee to take to Paris for the next meeting of the Subcommittee on the Tectonic Map. (The Chief Geologist, in his wisdom, decided to send George Cohee to this meeting instead of me). The colored map made a great impression in Washington -- too much so, as many of the higher-ups concluded (disconcertingly) that the North American tectonic map had been finished, whereas I knew that there were still several years of work ahead to get it right.

I took several field trips in the fall of 1962. On October 6-8, I joined Chester Longwell in Las Vegas, where he wanted to demonstrate to some of us (John Crowell, Roland Von Huene, etc.) the true nature of the Arrowhead fault, a transverse structure separating the southern and northern Muddy Mountains. His published interpretations had been contested by critics, who had made other interpretations. He had done some more work and had found, in fact, that the geology of its critical western end was different from what either he or his critics had supposed.

On October 19-20 a group of us went on a field trip out of Bakersfield sponsored by the San Joaquin Geological Society, across the Temblor Range and the Carrizo Plain, along the San Andreas fault, ending with a barbecue dinner at a ranch near Cholame. We were very late getting home.

On October 30-November 4, I went again to West Texas at the invitation of the West Texas Geological Society to attend a field trip in the northern Guadalupe Mountains, mostly north of where I had mapped in the thirties. I saw some new country, and took some hard hikes over country in which I had been before (in the head of McKittrick Canyon). The last day, we had a trip through Carlsbad Caverns, and into the lower levels to which visitors are not ordinarily admitted. I flew out to Midland, and took the train home from Pecos.

Late in the season, from November 26 to December 1, I went to Nevada. Ralph Roberts wanted to demonstrate to a group of us Survey geologists his ideas about the Antler Orogenic Belt and the Roberts Thrust. We went as far east as Elko, and worked back through successive localities to the Sonoma Range. Being late in the season, the days were short and very cold, and there was light snow now and then.

The year 1963 was one of continued effort on the Tectonic Map of North America. I was now compiling the land areas -- during the spring data on the United States, and by autumn Mexico and Central America.

At the meeting of the Cordilleran Section of the G.S.A. on April 8, I gave a paper on the Tectonic Map project. I had to leave before the meeting was over, however, to go east to attend the meeting of the Southeastern Section, which met in Roanoke, Virginia. Wally Lowry of the V.P.I. faculty had organized a symposium on the tectonics of the Southern Appalachians, and I was invited to give the lead-off paper. Other papers followed, including one by Jerry Hadley on radiometric dates, and a final one by Byron Cooper, who presented again his agnostic ideas. After the meeting there was a one-day field trip to the eastern side of the Blue Ridge led by J. A. Redden. Next day, a group of us drove to Washington (Neuman, Dutro, Hadley, and Espenshade), and Gil Espenshade showed us something of the complex geology of the James River synclinorium. In Washington, I received the edited copy of the Sierra Diablo manuscript, which I started reading. On April 19-21 I rode the train home -- by Zephyr from Chicago.

On May 31 to June 21 I went to Alaska on a tour devised for me by George Gates, in order to get acquainted with the geology of this part of the world for representation on the Tectonic Map of North America. Ernie Lathram was sent along as a companion and mentor. We started in the Alaska Panhandle and worked north. We saw some of the sights around Juneau, then to Anchorage, then northeast to the Wrangell Mountains, where we got within a few miles of Orange Hill, a mineralized area in which Harry Finney had invested a great deal of money. Then to Mount McKinley National Park and into Fairbanks. After some trips out of Fairbanks, we went northeast across the Arctic Circle to a field party in the eastern Brooks Range, where I spent several days with Bill Brosge riding over the country in a helicopter. We then returned to Fairbanks and flew home, returning late in June. After the perpetual daylight in Alaska, it was good to see the sun go down in the evening. We had the guidance of many Survey geologists at work in Alaska (Art Grantz, Clyde Wahrhaftig, Florence Weber, Bill Brosge, and others), and the trip was most rewarding for my purposes.

In July, Daryl Reagan found a new job, with the Stanford Linear Accelerator<sup>Center</sup>, which was being built on the Stanford grounds under the direction of his old professor, Panofsky. Trudy and Daryl therefore moved from Livermore and found a house on Moreno<sup>Avenue</sup> Street in Palo Alto, where they have been ever since.

In July, I was told that a talented girl, Ann LeGullion, who was with B.T.I., desired a change, and it was suggested that I take her on as a draftsman, to replace Bob Easterbrook. I had been doing without drafting help for a year, and the work was piling up, so that a helper was much needed. Everyone sang Ann's praises, so I was quite convinced that I had acquired an exceptional person. She was therefore taken on July 22 for a 30-day trial period. During this period she went at the work with the greatest interest and enthusiasm, so she was permanently hired as a GS-7.

After the trial period was over, apathy set in, and I gradually became disillusioned, in spite of myself. Ann turned out to be a hopeless neurotic, a goofer-offer, an intriguer with no conception of normal work habits. I fumed inwardly for months, unable to make my feelings felt. But somehow, the work got done by both of us.

A Russian course was offered by the Survey as part of an in-house training program, and I took it through November and December. In view of my actual and potential Soviet connections, it seemed like a good idea. But eventually the complexities of Russian grammar and declension became too much; I was just too old for another language. Trips away were coming up in January, 1964, so I finally dropped the course at the end of the year, with regret mingled with relief. I had made only a modest start in the language, and in the Cyrillic alphabet.

My work of compilation of Central America and the Caribbean was progressing, and various friends suggested that I contact the Chevron Exploration Company (= Standard Oil Company of California), with offices in San Francisco. I went up to the city for several days in November and December to look over their records. The staff were quite willing to let me look over their maps and records, but when I asked for copies of the most useful ones the higher-ups in the company were more cagy and I got little. Nevertheless, the staff put me onto several maps that I would not have known about otherwise and which were not generally available at the time -- geologic maps of Cuba, Venezuela, Colombia, etc.

From January 11-24, 1964, I went to Guatemala and Mexico to get data for the Tectonic Map (one reason for dropping the Russian course). In Guatemala I was guest of Gabriel Dengo and in Mexico of Zoltan de Cserna and Carl Fries. I spent a good deal of time studying maps and records at Gabriel's home (more productive than at the Chevron office), and he took me on several trips in Guatemala -- west to Lake Atitlan, and north to Coban. In Mexico I also spent some time at the Instituto de Geologia in University City, and Zoltan showed me some of the sights of the Mexico City area, such as the Pyramids of Teotihuacan. At the end, Zoltan and Carl took me on a four-day trip south to Acapulco, across some of the more problematical parts of Mexican geology. I was delighted with Guatemala, but less impressed with Mexico. I returned home by plane from Mexico City on January 24.

I had only been home a little while when I was off again to West Texas on February 2-8. N.A.S.A. was looking for training areas for the astronauts, and was considering the Marathon country and the Big Bend country. I went down by train to Alpine, where I was joined by Al Chidester from N.A.S.A. in Houston, Bill Muehlberger of the University of Texas, and others. We looked at the various famous places in the Marathon area, and I had an opportunity for the first time to take color pictures there. We also visited the "astrobleme" of Sierra Madera, and made a long trip through the Big Bend.

On Good Friday, March 27, I finally had it out with Ann LeGullion, and expressed a few of my long pent-up feelings. She was to scribe the contours for the southern sheet of the Tectonic Map, and said she was an expert at such things and was eager to start. Once started, however, she found nothing right, and began her usual stream of complaints. I decided that "this was it," went down to the washroom and put some water on my face, and when I came back I said mildly, "I'd like to tell you your worst fault; you are always complaining." The results were catastrophic! She went into hysterics and began screaming accusations at me. I just took it and went home. On the way, I began to laugh, and a great load fell off my mind. (That evening, reports from Alaska began to come in of an enormous earthquake on the south coast -- the famous "Good Friday Earthquake." I am almost convinced that I caused it!).

Ann didn't know what she was in for. The following Monday I recieved the annual performance rating form for her. On most of the elements I gave her average (= satisfactory) marks, and none better, but I rated her "unsatisfactory" on "work habits." When she got her copy at the end of the week she announced that she was going to resign. Our personnel people said, "For Gods sake, get her to put it into writing before she changes her mind!", which we did. She added a final sting on her resignation form in the space for "reasons for leaving," that she was leaving because of "impossible working conditions," which amused all of us. (A year later she tried to get on welfare, saying that she lost her job because of "impossible working conditions," but the Survey authorities fixed that.)

Ann stayed on, however, and finished the scribing job, so it was not until June 17 that I saw the last of her. I was determined to have no more of drafting assistants!

Between May 17 and June 1 I went east. First I went to the A.A.P.G. meeting, which was being held in Toronto, Canada, which featured a program on "basement rocks", which was an important item in the Tectonic Map. Following the meeting, there was a delightful trip into the Grenville area -- a nearly unspoiled piece of country that was very beautiful in the springtime. I then went to Ottawa for several days of conference with Cliff Stockwell at the offices of the Geological Survey of Canada. Finally, I stopped off for a few days in Washington and came home on the train -- with another ride on the Zephyr.

The seventh congress of the International Association for Quaternary Research (INQUA) was to meet in the United States, and a volume was being planned on the Quaternary of the United States. I was solicited to prepare a chapter on "The tectonics of the Quaternary", and I set to work on it in April, finishing a manuscript late in July. Quaternary geology was rather unfamiliar to me, and I received much help from Dave Hopkins and Clyde Wahrhaftig. The editors of the volume objected to some of the concluding parts of the article, so I had to make revisions in the fall, and finally sent the manuscript off for good in December.

On June 21, when I was at work alone in the office, Don Peterson called. He was currently in charge of T.R.U. (Technical Reports Unit) of the Geologic Division. He said that there was a lady in his office whom I might be able to use as a draftsman, adding, "She's fifty-three and a grandmother -- but she's awfully good looking!" I said to send her over for an interview -- and so Trudy Edmonston entered my life!

She was Gertrude J. Edmonston, who had come out from Washington to the Menlo Park center looking for a position. She had worked for many years for the Topographic Division in Washington, but she found that this division had no openings at the moment, so had come over to try her luck with the Geologic Division. She had recently divorced her husband (an alcoholic), and various personal problems had impelled her to try for a new life in California. Her daughter Joan had decided to come to the area to live, to await her husband who was in the Marine Corps overseas in the Pacific area.

She made a good impression during the interview (quite aside from the "looks"). She seemed talented, able, and willing, so it was arranged that she should come on duty August 3, 1964. We gave her Ann LeGullion's rating of GS-7, but with the thought that we might bring her up to Bob Easterbrook's rating of GS-9 if she made good. As usual when I was without help, work had begun to pile up. Cliff Stockwell's data on the Canadian Shield was beginning to come in, plotted on 1:1-million sheets, which needed to be replotted on our North American base at 1:2 $\frac{1}{2}$ -million, so I put Trudy to work on this.

It had been Ann's custom to drift into work about 8:30, or three-quarters of an hour late, then to wander about for another half hour until she was at last able to settle down to work. This was annoying, but it at least gave me several hours to myself at the beginning of the day to collect my thoughts. On August 3 I therefore was a little disconcerted when Trudy showed up for work right on the dot. There were to be no more early morning meditations by me!

In mid-summer I received word that the Survey had decided to send me to the International Geological Congress in India, which was to meet in New Delhi in December, 1964. Up to then, we had been told that few people would go to this congress officially, and not to plan on attending. The news came awfully late; there would be no chance to apply to the Congress for field trips, and applications for hotel reservations were chancy. I was disgusted, and thought of tossing it all back at them, but thought better of it, and started making plans.

Since I would miss out on field trips in India, I thought I might visit Japan on the way home, and persuaded Helen to meet me there after the Congress and have a tour together -- then to come home by ship, on the President Cleveland. So this was all worked out, with the help of our travel agency.

On September 11-25 I took a brief vacation, and Helen and I went to visit Betty Avery, who had by then given up her Los Altos house and had moved to a rented house in Carmel close to the beach. It was a fine relaxing time, free of the worries of the office work. On the way home, we went south along the coast highway and had a trip through the Hearst Castle.

A few days later, Helen went east to Washington for 5 days. While she was away, Trudy Edmonston invited me to dinner, thus beginning our long social friendship. As an inducement, she said, "I'll give you a drink!" She and Joan had rented a house in the Menlo Park area. With them was the other daughter Terry, a 13-year old but large for her age. I asked her how she liked California, and she said "I hate it!" which ended my conversation with her for the evening.

On October 25-28 a group of us from the Survey went to a meeting of the Canadian Institute of Mining and Metallurgy in Vancouver, which was to feature papers on the tectonics and mineral deposits of the western Cordillera in Canada. I was slated to give a general paper on the tectonics of the whole Cordillera, and the other Survey people gave papers on local areas.

The India trip finally got under way on December 11, when I left San Francisco for New Delhi on a Pan-Am plane. I had rather foolishly decided to fly right through (with brief stops in Honolulu, Tokyo, Hongkong, and Bangkok); the schedule didn't look too bad, but we were flying with the sun, so 8 or 9 hours were added to the scheduled time. We left San Francisco in the morning, and arrived in New Delhi at sun-up next morning, December 13.

I had been given a reservation at the Ashoka Hotel, which turned out to be one of the better hotels in New Delhi, and the one at which many of the foreign visitors were staying. The first morning I went over to headquarters to register, then back to the hotel for lunch and to sort the papers received. I then forced myself to lie down, and I was wearier than I realized, for I came awake again when it was dark and 8:00 P.M. in the evening. I went down for dinner, and thus ended my first day in India.

During the sessions I attended mostly the meetings of the Sub-Commission on Tectonic Maps. Bogdanoff was present and presiding, and it was good to see him again and to know him better. Through the year 1964 I had completed the south half of the Tectonic Map of North America, and had sent a hand-colored copy for display at the Congress.

There were various official entertainments, including a reception for the Americans by the U.S. Ambassador at the American Embassy. On Sunday, many of us hired automobiles and went down to Agra, a hundred miles to the south, to see the Taj Mahal and other famous buildings. Robert had come to the Congress, and went with me that day.

When the Congress was over, I flew to Tokyo on December 23, where Helen met me as planned at the Okura Hotel. We had arranged for a two-week tour, really a rather special one, as we had throughout our private escort, and usually a limousine and chauffeur. The morning after our arrival we went by the Bullet Train to Kyoto, where we spent Christmas, with various sight-seeing trips in the area. We returned by plane to Tokyo, then by train to Nikko in the snowy mountains to the north. Our last trip was west from Tokyo to Hakone, in the heart of a volcanic region. Thence, our limousine took us to Yokohama and to the dock where we boarded the President Cleveland on January 6, 1965.

It was wonderful to relax in our room on the ship and know that our travel worries were behind us. For the next 10 days we just relaxed. I read books on the deck that I had never had time to go into before, and we drank and ate. We stopped in Honolulu for a day and Helen and I took a tour of the city and its surroundings. We reached San Francisco at last on January 18 (I had been away for more than two months). Trudy and Daryl met us at the dock and drove us home.

Once back in the States, I had to leave within a few weeks for a trip to New England (February 9-18, 1965), which I had agreed to do by correspondence in the fall. It was planned that I would give talks at the four colleges near Amherst, Massachusetts -- Amherst, the University of Massachusetts, Mount Holyoke, and Smith. I had a sheaf of lectures and a box of slides with me, and I gave one lecture at each school. After this circuit, I was driven to Boston, visited the Harvard campus in the afternoon, and spent the night on the Wellesley campus, where I gave another talk. Next day, I took a bus down to Woods Hole for a visit with the Survey people there, and the day after a train to New Haven, where John Rodgers put me up in his quarters on the Yale campus. I gave most of my prepared lectures again to the group at the Yale geology department. Following that, I went down to Mamaroneck for a visit with Robert and Clara, and then another visit with Edward and Grace in New York. It was good to take a plane for home on February 18, and settle down for a while.

During the year 1965 I worked along on the northern sheet of the Tectonic Map of North America. I compiled Greenland, Iceland, the Arctic Islands, Newfoundland, and the Canadian Shield. I also made a start on Alaska. The only untouched remaining area was the Canadian Cordillera, for which the Canadian data had not yet come in; work on this would have to be postponed until the year 1966.

Ever since my return from Czechoslovakia in 1962 I had been thinking about plans for a trip to the Soviet Union, but there were many frustrations. During 1963 the idea was being handled by Bill Thurston of the Director's office of the Survey, but with minimal success. The only concrete thing he came up with was a supposed legal opinion that I could not accept my book royalty while I was in the Soviet Union. (All enterprises in the U.S.S.R. are owned by the state, and I as a government employee could not receive money from a Foreign Power, much less a hostile one!) Bill seemed to think finding this out was a real achievement. (But during the summer of 1965 I got a ruling from the State Department that it was O.K., so I went ahead and claimed it). It became obvious that Bill Thurston was more of a roadblock than a help, so I cut off further dealings with him and in 1965 began to try other means, on my own.

I tried the National Science Foundation, without success. I got nowhere with the National Academy of Sciences (I was not an Academician, and neither was Bogdanoff for that matter). The C.I.A. was definitely interested, and I had frequent visits from a Jerry Rubin the local C.I.A. representative, but this would be the Kiss-of-Death, so I avoided this entanglement. I had much correspondence with the Inter-University Travel Committee with offices in Bloomington, Indiana, and got much good advice and moral support, but no financial help.

The break-through came in April, when I received a letter from Prof. Bogdanoff outlining what I might do in the U.S.S.R., including not only lectures at the Universities, but attendance at a two-week colloquium sponsored by his Sub-Commission, to be held at Tbilisi, Georgia, with field trips in the Caucasus. The last seemed to be something that the Survey might support, so I asked and such support was miraculously forthcoming! (Miraculous, because an official trip of this kind was frowned on in succeeding years, and I found later that the British Survey would not sanction a trip to this colloquium by their staff).

Honors began suddenly to fall upon me in 1965. Early in the year, I learned that I was to receive a Distinguished Service award (with gold medal) from the Department of the Interior. The awards ceremony was held in Washington in mid-summer, and I went in for it on July 15-17, on one of the hottest and most humid periods of the Washington summer. And in April I received an announcement from the Geological Society of America that I was to receive their Penrose Medal at the annual meeting in Kansas City in November. This last seemed almost unbelievable, as I had always thought it was unattainable -- considering the distinguished roster of previous medalists.

On June 19-26 Helen and I spent a week with Trudy and Daryl at the Stanford camp on Fallen Leaf Lake, on the east side of the Sierras near Lake Tahoe. I took many walks by myself into the surrounding mountains, and a good time was had by all of us.

With the go-ahead from the Survey, I spent much of the rest of the summer on plans for the trip to the U.S.S.R. I corresponded frequently with Prof. Bogdanoff. I submitted to him the titles of possible lectures I could give there, and he selected ten -- all on tectonic subjects. (I was disappointed that he was not interested in any on the Permian that I suggested). With this list in mind, I began composing the texts of the lectures, and in having slides made for them. I asked him about the book royalty, and he sent me instructions as to how to write to the publishing house, stating that I would be in Moscow in the fall to claim the money. The project for our tectonic maps of the U.S. for our National Atlas had come up, and I assembled the data for these as something Trudy could work on while I was away.

I fully expected that Helen would go with me to the Soviet Union, but in the end she got cold feet (perhaps just as well, for the trip did involve many hardships). As an alternative, I proposed that we should spend two weeks together in Scotland and England first, before I went off into the unknown, and she was happy to fall in with this. (In the end, she stayed on alone in England for another 5 weeks, until it was time for me to return from the U.S.S.R.).

We were off at last on September 3, flying via T.W.A. to London and then to Edinburgh. I had written to the British friends that I had made, saying that we were coming, hoping for their hospitality --- Tony Hallam in Edinburgh, Frederick Trotter now retired in Gloucestershire, Professor Harrison in Oxford, and Fred Dunning in London. All responded beautifully.

In Edinburgh I rented a car, and we drove north to Inverness, retracing the route of our bus tour of 1960. I wanted very much to see something of the geologically famous Northwest Highlands, so from Inverness I drove northwest one day in the rain and mist to the west coast, seeing much of the country and a little of the geology. We then returned to Edinburgh where Tony Hallam entertained us for a day.

We then took a train to Gloucester, via Birmingham, and were met by the Trotters, who put us up for two days at their old house in a village in the country. Next, we took a train to Oxford and spent several days there, with Prof. and Mrs. Harrison's kind hospitality. From there we went by train to London for our last few days together. We toured the British Museum and saw other sights, and I looked up Fred Dunning at the British Survey. He and his wife had us to dinner one evening at a restaurant in Soho.

Our idyllic vacation together in Britain was over, and on September 19 I put Helen on a plane for Paris, where she would spend a few days before returning alone to England. On September 20, I set off alone on the flight to Moscow, and to a very different and crueller world.

The flight to Moscow was uneventful, almost prosaic. I was met at the airport, along with some other arrivals, by Prof. Bogdanoff and some of his cohorts, and we were taken to the Minsk Hotel in downtown Moscow, where the other attendees of the Tbilisi colloquium were staying. We spent a few days in Moscow -- one day of sight-seeing with the group, and another day on business of my own -- at the University, at the American Embassy, and to the publishing house where I was given 1,000 Rubles in crisp new bills to represent my book royalty.

Our group flew together to Tbilisi. Moscow had been dark and gloomy, and it was a pleasure to come down into sunny Georgia. We were in Georgia for two weeks, September 23 - October 9. Meetings were held in a big public building down the street from our hotel, and were broken in the middle by a 5-day field trip across the Caucasus. The conference was not as rewarding as previous international meetings I had attended; too many miscellaneous Soviet people came so that it was unwieldy, and too much of what was said was said in Russian and French, and not enough in English.

It was hard to leave the sunshine of Georgia on October 8 and to go back to gloomy Moscow. The day after our return it snowed; they said this was very unusual this time of year, but it was a presage of approaching winter. I was in Moscow for two weeks, until October 23. Nearly every day I went to the University, and gave my lectures on tectonics in the afternoons. This was a rather confining schedule, but there was at least enough time for a few visits, to an art gallery, to the St. Sergius Monastery outside of Moscow, and to some evening entertainments. This time in Moscow, I stayed at the Ukraine Hotel, on the Moscow River west of the center of the city, within walking distance of the American Embassy. My last afternoon in Moscow I was given a reception by the Rector of the University, during which he presented me with a bronze plaque -- the Lomonosov Medal. This seemed like a rather empty honor, but I have learned that it is not often given to visitors.

I left Moscow by train on October 23 for Leningrad, where I remained until October 31. Leningrad is farther north than Moscow and farther west, but for some reason the weather was more pleasant and winter farther away. Unlike Moscow, Leningrad had much antique charm and lived up to its reputation as "the Paris of the North". Here, I was guest of Leningrad University and gave two of my lectures there, and also another at the Institute that was the successor of the old Imperial Geological Survey. My hosts showed me the sights of the city, including two mornings at the Hermitage Museum, and trips to the Palaces at Peterhof and Pushkin (= Tsarske Seloe).

My time in the Soviet Union was over at last and on November 1 I flew to Finland and eventually to New York. I was glad that I had gone, but I was happy to leave, as it had been a rather grim experience with much hardship. My contemporaries among the Russian geologists were the soul of hospitality and kindness, beginning with Professor Bogdanoff, but there were long dreary periods between their entertainments when I was on my own. Even the airport in Helsinki seemed like it was in a different world.

(Other geologists from the States had been to the Soviet Union before me, but I was among the first of the modern group, and I started a whole succession of younger people on the visit. Most of them saw more country than I did, and had a better time of it -- partly because they took the trouble to learn something of the Russian language).

Arriving in New York, I spent several days with Edward and Grace at their apartment, where I gradually recuperated. I was delighted to get a phone call from Helen in Washington, and to learn that she also was safely back in the States.

On November 3, I flew to Kansas City to attend the G.S.A. convention, and finally had my reunion with Helen. We took little part in the meeting, and mostly sat in our hotel room and talked, comparing our adventures. Our big evening was the night of the banquet, when I was presented with the Penrose Medal. After receiving it, I made a probably too long acceptance speech, telling about my career. I did this mainly for a purpose -- to set myself apart from the little clique of Yale "wonder boys", such as Rubey, Gilluly, and Bradley, who for so long had dominated Survey and Society affairs, and with whom the unknowing public would inevitably confuse me.

Returning home at last on November 6, we found our place to be much the same, after more than two months away. It was good to be back home and at the Survey office, among my own people again. I decided to kiss all the girls -- and I was especially happy to give Trudy and Lida a big kiss.

I spent a good part of November and December writing up the records of my travels, organizing my photographs, postcards, and other documents, and in the end had four bound volumes.

The year 1966 was devoted to completing the Tectonic Map of North America, especially the parts in the western Cordillera of Canada and Alaska. To firm up the compilations, I made another trip to the Geological Survey of Canada in Ottawa from February 28 to March 4, where I conferred with Cliff Stockwell and other members of the staff.

Our Plymouth Station Wagon was now 12 years old, and while it still ran well it was outmoded in appearance, and was showing signs of age. Sometimes it would stall unaccountably, mainly because the automatic choke would stick and flood the motor. We decided it was time for a new car, and looked at various ones. Helen had her heart set on a convertible (she had never gotten over our Ford convertible of 1938), and we finally got a Buick Skylark convertible, of which we became very proud.

A spinoff of my honors of 1965 were others, especially election to the American Academy of Arts and Sciences (a sort of consolation prize for never having been elected to the National Academy of Sciences).

With completion of the Tectonic Map of North America, I set about during the summer to assemble the data on North America for the smaller-scaled Tectonic Map of the World, to fulfill our obligations to the Sub-Commission for the Tectonic Map of the World -- a frustrating task because of its skewed, "squirrely" projection. Trudy drafted this up neatly, and we sent it off to Prof. Bogdanoff in Moscow on March 10, 1967. (Completion of the World Map has been delayed as a result of Bogdanoff's death in 1971, and it is still pending today, in 1977).

The Commission for the Geological Map of the World was scheduled to meet in Paris in June, 1966, and the Survey decided to send me, Doug Kinney, and Phil Guild. At the last minute, I called off my part of the trip; there would be little for me to report, as the Tectonic Map of North America was not yet completed, and as I had only just started to assemble copy for the World Tectonic Map.

Besides this, we learned that Edward, Grace, and Andy were coming through just at that time. Edward's sabbatical at Barnard was due to arrive, and he decided to spend the coming year in Australia, at the New England University in Armindale, New South Wales. When they arrived we had them at our home for a few days, and showed them the city before they took a plane to **Australia**.

On August 2 and 3, Lida had Trudy, Helen, and me to her place over the mountain in La Honda. She had bought a little house there of which she was very fond, and she has lived in it off and on. (She is renting it out now). We sat around and drank, went down to the beach in the afternoon, and had a generally gay time.

I became interested in Helen Beikman's career. She had become stuck in T.R.U. as a map editor, and wanted to better herself. The Geological Society of America had advertised an opening for chief editor, and she applied with the support of all of us in Menlo Park, but this position did not materialize. In August, when I was thinking about her situation, it suddenly occurred to me that she would be a natural to work with me on the impending U.S. Geologic Map. This project had been long delayed, and as there were not many years left before my retirement, geological help was obviously needed.

There were several hurdles to surmount to put this idea into action. First, I had to convince Helen to be my helper, then the two of us had to convince the higher-ups of the Survey on the wisdom of the move. The hurdles were all surmounted, and it was agreed that she would start work on the project at the end of the year.

Partly to amplify the background for the North America map, I attended the meeting of the Geological Association of Canada, which met in eastern Canada on September 8-16. First, there was a field trip in New Brunswick, operating out of Fredericton. The weather was much drier <sup>than</sup> when I had been in nearby Maine 8 years before, so it was easier to get around. Then we went on to the meetings in Halifax, Nova Scotia. After this, there was a 2-day field trip in western Newfoundland. I saw much geology that was very helpful to me.

Shortly after my return, I went east again on October 9-11, to attend a conference at Rutgers University in New Brunswick, New Jersey. This was the bicentennial year of the University, and the title of the symposium was "What's new on earth? The megatectonics of the continents and the oceans." They had invited a distinguished group to give papers on a variety of related topics, and I was asked to present the tectonics and geophysics of Eastern North America. For this purpose, I worked hard to assemble data and put them on a

series of maps. Many interesting presentations were made. This was immediately before the final flowering of the concepts of plate tectonics, and the reality of continental separation. Some of the speakers were already quite convinced; others of us were reserved in our judgements, and others were still rejecting it.

Before going home, I spent two days in Washington, conferring on the final stages of the North America Tectonic Map, and on planning for the impending U.S. Geologic Map. At some point in one of these conferences, which Hugh Miser attended, he looked over his glasses at me and said, "I hear Helen Beikman is going to be working with you --- She's a good girl!" This was a memorable and quite justifiable remark, which I have long remembered.

The G.S.A. convention was held in San Francisco on November 11-16, and I drove up and stayed at a hotel (the Olympic near the Hilton). I attended a field trip north of San Francisco before the meeting, and there were many cocktail parties. The most memorable one was given by W. H. Freeman and his wife at **their** apartment the night before the meeting. Bill Freeman was a sort of hanger-on on the fringes of geology. He had founded the very successful W. H. Freeman & Co., the scientific publishing house, but had somehow lost control of it, and was looking for some new enterprise. An interesting, and very select group of geologists attended.

The last part of the year was spent making color-out sheets of the different parts of the North American Tectonic Map, to be sent to Washington as a guide for the illustrators, so that preparation for printing could begin.

On February 14-17, 1967, I flew to Texas. The Midland group was giving a series of lectures on tectonics by invited speakers, and I gave the same talk as I gave at Rutgers the fall before. This was out of context of the Rutgers symposium, and may not have been entirely what they wanted, which was somewhat embarrassing, because admission was charged. At any rate, I had an opportunity to visit again with the geologists there at various social gatherings.

After the Midland visit, I went with Dan Bridges to Chihuahua. He had done a dissertation there on the older rocks for a University of Texas degree, in an area along the Conchos River east of Chihuahua City, near Mina Plomosas and Placer de Guadalupe. He was now with the Shell Oil Company at Midland, but couldn't let his area go, and went back frequently. (The Shell people finally became impatient with his inattention to his regular work, and shortly after I was there they transferred him to the Gulf Coast). He wanted to show off his area again and get advice, and had brought along a group of geologists, mostly locals, but including Kasper Arbenz, a really sharp senior geologist with Shell. We spent our nights in the city, which was bad planning, because it meant a long trip each day to the area over bad roads. He had clearly found out many interesting things about the Paleozoic and lower Mesozoic of the area, but was hampered by lack of adequate base maps, and obviously there were many problems still unsolved. The trip was interesting, and gave me a view of new country, but the results were inconclusive.

Helen Beikman started her work on the U.S. Geologic Map in January, and I finally got started in February. For my own work, I decided to begin on the West Coast and work east (the reverse of Stose's procedure for the U.S. Map of 1932). I started on the published maps of California, making a compilation (with Trudy's help) on a 1:1-million scale. In March, April, and May, I followed the same procedure for Nevada, Arizona, and Oregon.

Trudy Edmonston had been living in an apartment near the Survey, but decided to buy a house. In April, she found a nice little place on Kendrie Street not far from the office, and with the encouragement of Lida, Helen, and myself, made the purchase.

We had heard that Professor Bogdanoff's younger son, Alexei, was spending the year in this country and would be out our way, so we offered to put him up. He was a medical biologist, not a geologist. On a rainy night on April 23 we received a phone call from him out of the blue, and went down to the San Antonio shopping center to pick him up. He had with him a young Russian colleague, Sergei Shastokov, and they stayed with us for three days. Their main contacts were with the Stanford Medical School, where they seemed to be held in high esteem. On their last day, the wife of one of the doctors offered to take them on to their next destination.

In June, I began to write a text, or "treatise," to go with the North America Tectonic Map (later to be published as a Professional Paper entitled "Tectonics of North America").

Work on this and other things went slowly, however, because I had decided to paint the house. The house had been painted a dull gray when we bought it, and was clearly in need of brightening up. I did the job on a series of long weekends (Fridays to Mondays or Tuesdays), and it went on all summer, to the end of August -- 35 work days in all, including 20 days of annual leave. In September, I went on to paint the tool shed in the back yard.

The South American countries were planning a tectonic map of their continent, and were to hold an organizing and planning meeting in the fall of 1967. There had been rumors that the Survey might send someone to the meeting, but I put the matter out of my mind. However, in August I was informed that I was it, and would have to go -- the only foreign travel that I did not much want to undertake. The meeting on the map was to be held in Montevideo, Uruguay, on October 20, following a U.N.E.S.C.O.-sponsored symposium on continental drift that had been organized primarily by J. Tuzo Wilson.

I spent much of September and October preparing for the trip. I had two booklets made up -- one on tectonic maps in general, the other specifically on our North American map -- based on excerpts from the "Treatise" that I was writing. These were printed and bound, with Lida's great help, and sent off to be handed out to participants of the meeting. My travel agency helped greatly on travel plans, even though they got little out of it.

I set off on October 12, on a Pan-Am flight to Buenos Aires, where I spent a day before my short flight to Montevideo. This was my first trip to South America, or to the Southern Hemisphere, but most of the flying was at night, so I saw little of the country. In the Southern Hemisphere it was mid-spring rather than mid-autumn, so the climate was much the same as the one I had left in California.

The travel agency had made reservations for me at the Victoria Plaza Hotel, facing the main square in downtown Montevideo; the meetings were being held several miles away at the Park Hotel on the water front -- a run-down hotel and casino. It had been arranged that I would attend the continental drift symposium that preceded the tectonic map meetings. This had a large attendance, including many

American geologists that I knew, as well as representatives from many other nations. The sessions were very interesting and enlightening; by now, the plate tectonics concepts had taken firm hold.

After the symposium was over, the attendance at the Tectonic Map conference was smaller. Prof. Bogdanoff was much in evidence, as well as Marcais, Frances Delaney, and Choubert from Paris, representing the World Map Commission. Zoltan de Cserna had come from Mexico with his new wife. One of the pleasures of the meeting was to know Sobarhan Singh, the government geologist from Guyana, of Indian descent, who spoke fluent English and was as much confused as I was by the sea of Spanish, Portuguese, and French languages. A surprise of the meeting was to see again Senor Granja, the Ecuadorian who had spent several months with us in Gatlinburg in 1947; he seemed almost pathetically glad to see me again. I presented our North America Tectonic Map, of which I brought a hand-colored copy, and Choubert presented the Tectonic Map of Africa. The problems of a Tectonic Map of South America were much greater than for the North American map, because the continent was divided up between a multitude of countries, partly competing and antagonistic, but plans were set up to get started on the map.

When the conference was over, I flew to Rio for several days. Zoltan and his wife went with me as far as Sao Paulo. In Rio, I was guest of Max White and the other geologists of the American A.I.D. staff. Rio was all I saw of Brazil, and this was not the best, because the weather was hot and steamy, with much rain, so that the sensational peaks around the city were mostly in clouds.

I was rather glad to start for home, but there were obstacles. When I went to the airport for the morning flight on October 27, I was informed that the Pan-Am plane would be delayed for hours, so I was taken back to a downtown hotel and told to wait. It was a frustrating day, but we finally got off in the evening, more than eight hours late. The plane was full of a horde of American tour groups -- a most scummy lot. We finally reached Miami on the morning of the 28; and I wearily set out on another flight for home. It had not been a very stirring trip for me, and I was glad that it was over.

Returning home, I spent a little time on the records of the trip, and then got back to writing on the "Treatise", the text of which I finished in January, 1968. The illustrations took a while longer, with Trudy's help, and the whole thing was finally transmitted in May.

Between January 6 and 23, Helen was in Washington, but found the visit frustrating because of the snowy, wintry weather. I stayed at home alone, in the cold.

Nikita Bogdanoff, Prof. Bogdanoff's older son whom I had met in Moscow, had indicated his desire to spend some time in this country and to see our geology. It was not possible to obtain for him any foundation grant, but I went around to various schools and received offers of support during his visits to them. In particular, Clem Nelson of U.C.L.A. came through splendidly with funds to cover his visit to southern California. On February 13, he came north to our area; I paid for his plane ticket and we put him up at our house. On February 17 and 18 a group of us from the Survey (Clark Blake, Mike Churkin, Edgar Bailey, and others) took him on a field trip into the northern Coast Ranges north of San Francisco. Nikita lectured at Berkeley and Stanford, and one day I took him on a visit to the University of California in Santa Cruz. He left on February 27.

Color proof of the North America Tectonic Map arrived in March. It had taken heroic measures on the part of Doug Kinney and myself to get B.T.I. to push it through, but we wanted a printed copy to take to the International Geological Congress in Prague in August. That same month I received the printed copies of Professional Paper 349-C, on the Central Great Smoky Mountains, and with that my obligations on the Smokies project came to an end.

Bill Muehlberger of the Geology Department of the University of Texas in Austin had written me proposing that I give two weeks of lectures at the University. It was a regular custom to have outside geologists come to the department to give short lecture courses. Compensation was to be \$1,500.00, which I took without compunction; I had learned that the Survey was going to send me to the Prague meeting as an official delegate, but with no financial help; the Texas money would help pay for my trip abroad.

I went down on March 23 and was in Austin until April 5, giving my lectures on various tectonic subjects, and attending the usual round of parties and entertainments given by my kind hosts. From April 6-9, the students and faculty went with me on a field trip to West Texas, with the usual stops in the Marathon Region and the Big Bend, ending in El Paso.

From there, I flew to Tucson to attend the meeting of the Cordilleran Section of the G.S.A., and to go on a field trip afterwards in the Tucson Mountains. My travels ended on April 16, when I returned home after nearly a month away. Trudy met me at the airport, and it was good to see her again and to have her affectionate welcome. She said I looked wonderfully fit and complimented me on my tan, which I had acquired on the desert field trips.

I resumed work again on the U.S. Map, and went over my previous compilations on the Southwestern States -- California, Nevada, Utah, and Arizona.

A part of July, however, was spent in preparation for the impending trip to Europe. On July 29, I left via TWA for Zurich. Ken Hsu, who was at the Geological Institute in Zurich, had invited a group of us American geologists on a field trip through the eastern Swiss Alps, as a prelude to the International Geological Congress in Czechoslovakia. There was a selected group of attendees, including Jim Gulluly (with Enid), John Maxwell, Clarence Allen, and others. Most of the guidance was by Rudy Trumphy of the Institute, who demonstrated very clearly the complex stratigraphy and tectonics of this part of the Alps, from the Molasse on the north, back into the Pennines and the Austro-Alpine nappes. The country was beautiful, despite frequent rainstorms, and I came away with pleasant memories of Switzerland.

Our trip lasted from July 31 to August 6, and ended again in Zurich. Next morning, I flew to Vienna, which I reached in pouring rain. I phoned the Austrian Survey, to which I had been given names, and they were the soul of hospitality. Mr. Holser of the Survey spent the day showing me around the city (spoiled by the bad weather), and helped me to get on to Bratislava next day.

On August 8 I went by bus to Bratislava, to join the Congress Excursion in the Carpathians. Wally Cady and his family, who were going on the same excursion, were also on the bus. The Excursion party addembled at the Carlton Hotel in Bratislava. There were 7 Americans, 6 French, 2 Germans, 2 Soviets, 1 British, 1 Norwegian, and 1 Pole. In addition, the trip was encumbered by 9 wives and dependents. Our leader was Michel Mahel, a Slovak, who had three or four assistants. Our objective was to be a general examination of the Carpathians in Slovakia. (This was only about half of them; the other half was in Poland to the north, which I expected to see on a post-Congress Excursion, but this was not to be).

Czechoslovakia was undergoing a rejuvenation, after long years of Communist domination. The old guard had been thrown out of the government, and reforms were being made by Doubcek, the new leader, and his cohorts. A spirit of optómism was in the air, marred by rumblings of displeasure from the Soviet Russians. By the beginning of August, however, everything seemed to have been settled in favor of Czechoslovakia.

We spent a day sight-seeing in Bratislava, and next morning visited Mahel's Geological Institute. Bratislava is a very ancient city, formerly Pressburg in days of the Austro-Hungarian monarchy. I was surprised to learn that it, rather than Budapest, had formerly been the administrative seat of Hungary. Now, it was the capital of the Slovakian part of Czechoslovakia.

Our trip in Slovakia lasted 10 days. We zig-zagged across the country, north to south, several times, seeing much geology and many mountain groups, but I must confess that I obtained no coherent picture in the end, comparable to the one Trumpy had demonstrated for us in Switzerland the week before. I was surprised to find very little Alpine scenery; the mountains had been worn down to stumps, despite their Mesozoic and Cenozoic deformation. (There may be more younger, rugged mountains in Poland to the north, but we were destined not to see them). Between the mountains, there were broad areas of plains. All this was unexpected, since the Carpathians are on the orographic continuation of the Alps.

Anyway, the country was beautiful and peaceful. Flocks of white geese were everywhere in the fields. The little towns, many of them of historic interest, were mostly grimy and grubby, with local industries, but there were some beautiful resort hotels in the higher mountains, at some of which we stayed. Troops, and even police, were not in evidence. The only time we saw one was when a highway patrolman stopped and scolded Mahel for parking our tour bus too far out on the paving.

The excursion ended in Kocice, a city at the east end of the Slovakian Carpathians, where we toured the local Geological Institute, and had a farewell dinner with our leaders. As the senior man there, I was supposed to lead off with a speech of thanks. I did this very badly; the Europeans who followed me did much better, but they are more accustomed to these kinds of amenities.

On the morning of August 19, we flew into Prague. It seemed much gayer than when I was there six years before. Partly this was because this was the time of year for leaves to be on the trees, which did much to brighten the place. There were also many more automobiles on the streets. Czechoslovakian flags were everywhere, and the communist inspirational posters had disappeared. The Congress was being held at a Technical University not far from the International Hotel where I had stayed in 1962; it seemed very new and shiny.

My reservations were in the Hotel Pariz, an older structure downtown, a little way from the center. No bathroom, but plenty of other conveniences, and two good restaurants. Next day, many Congress attendants arrived, who were also assigned to the Pariz.

I spent Monday and Tuesday mostly getting ready for the Congress -- making a hundred and one arrangements. I went to the opening meeting of the World Map Commission, and some other meetings, and saw the travel people about my trip to Poland after the Congress.

Wednesday morning all this was changed. The Russians had struck during the night, sending their tanks and troops into the city and the rest of the country, and the "Czechoslovakian Spring" came to an end. All day, we Congress attendees were marooned in our hotel, watching the tanks and troops maneuver in the streets.

On Thursday, an effort was made to resume the Congress. Buses were run to take people from downtown hotels to the meeting. I went to the World Map Commission meeting, where I presented the Tectonic Map of North America. Many of the participants droned along on their reports, oblivious of the situation, but it all seemed unreal, and I had no heart for it. During the day, there was a meeting of the American attendees, and there was much brave talk -- don't give up the ship, stay with it to the end as a gesture of support to the Czechs, and so on.

At the end of the day, a tremendous crowd waited outside the meeting halls, waiting for buses that did not appear. I decided to walk back to town, accompanied by two friends. At the hotel, I found that most of the American group were preparing to leave next morning, by bus to Germany. But I remembered the injunction to "stand by the ship." Anyway, I was alone and had no dependents to worry about, and I had not yet arranged to have my Congress proceedings volumes sent to me. So I said goodbye to the evacuees Friday morning.

The only way now to get to the Technical University was to walk, which I did -- long and hard, since it was mostly uphill. I got my proceedings volumes safely in the mail, and looked around for a meeting of the American group that had been scheduled. None of the leaders showed up -- only Dick Armstrong and his wife, Bob Neuman, and Bill Berry. We had a dreadful feeling that we had been deserted. In another hall, an assembly of the Congress was in session, with many speeches by foreign delegates praising the Czechs and their Congress. This was to be the closing meeting of the Congress; all the rest was called off.

I walked back to town in drizzling rain, and went to the office of Cedok, the Czech travel agency, and bought a rail ticket to Vienna for a train that evening -- a long tedious process. Then back to the hotel, which I found locked, dark, and nearly deserted. They let me in and I went to my room to pack -- throwing away many things, but still with heavy baggage.

When I came downstairs the few hotel people about told me it was impossible to go to Vienna; the station was surrounded by tanks. They said the American Embassy had been notified of my presence, and that I would be evacuated in the morning; the only thing for me to do now was to go to bed, which I did, supperless, and with a cold coming on.

Next morning (Saturday), the hotel people told me to call the American Embassy, but all I could get over the phone was Czech and broken English. So the girl at the desk called the Embassy herself, and informed me that I would soon be picked up. The Embassy station wagon arrived, driven by a man who spoke no English. He took me through the torn-up streets and around tanks to an outlying station where there was a train for Germany standing on the tracks. He took me and my bags to the train and found me a seat in a compartment; I wanted to give him money, but he would take none.

The train sat for another hour or more, and filled up completely, with many people standing in the aisles. To my surprise, there were many Americans from the Congress on the train, so I was not the last one out, after all. Next to me sat Marie Segrist, who worked for the Geological Society of America in Washington. I was worried about my train ticket; I had a ticket to Vienna, but this was the wrong train. Anyway, no one collected our tickets in Czechoslovakia. Contrary to my fears, the train started at last, and went right through. In Germany we bought our tickets for the rest of the way; I bought a ticket to Nuernberg, the first German city, and wondered what I would do when I got there.

But at Nuernberg station the U.S. Army was on hand to care for the refugees. We were taken to the U.S. Army barracks on the edge of the city, where we were given coffee and sandwiches, interviewed by intelligence officers, and were able to send messages home via the Red Cross. My message to Helen, I found later, was glad tidings indeed, for Helen had about given me up for a lost soul. I spent the night at the barracks, and stayed up very late to record the crowded events of the last few days.

Next day, I went downtown and found a room in a German hotel. Many of the Americans from the Congress were there also. Alas, the cold that I usually get on my travels had arrived, and I was tired and ill as well. I decided that I had had enough of Europe, and might as well quit and go home. (A good many other people from the Congress stayed on in Europe for a while longer).

Next day (Monday), I went to the T.W.A. office and had my tickets changed for a trip home next morning, and on Tuesday flew back to California. Helen and **Trudy** Edmonston were waiting to meet me at the airport and took me home, to recuperate from my travels.

I worked on records of the trip through September and October, then started again on the U.S. Map, determined to work right through on it, without the many interruptions that had plagued me for the last year or so.

On December 20-23, we were visited by Dorothy and Howard Biggs from Washington; Dorothy was Helen's first cousin, and had been married to Howard since 1939. After a few days at home, we took them to Lafayette across the Bay for a day with the Sheppard family -- Aunt Fanny, her daughter Helen, Helen's husband, and their children. It was a big load off our minds when we finally dropped the Biggs' at the San Francisco airport next morning.

There was a sad aftermath, for Helen had picked up a germ at the Sheppards, and was sick almost from the moment we got home. She was flat on her back at home all through January (1969), and it was not until February that she could get about again. There were several parties during January, and I had to go alone.

I had about finished the western region for the U.S. Map, as far as I could carry it, so I started on the Appalachians and New England. I found that compilation of this part went faster than I had expected, for many good source maps were available. But the Piedmont region of the Southern Appalachians remained enigmatic, and serious work on it would have to wait until another year.

In order to get more information on the Piedmont region, I decided to go to the Southeast, April 9-16, to attend the meeting of the Southeastern Section of the G.S.A. in Columbia, South Carolina, and to go on afterwards to the University of Georgia at Athens. The meeting featured a program on Piedmont geology, from which I had great hopes of getting tangible map information, but it was a disappointment. Everyone talked at length about little areas and problems, and no general picture emerged. There were a few glimmerings of future information. I heard about age dating being done at Chapel Hill, and a little useful mapping was being done in northwestern South Carolina around Clemson, and in the Alabama Piedmont, but compared with the whole region they were a drop in the bucket.

I then went on to the University of Georgia. Vernon Hurst, then Chairman of their Geology Department, had urged me when I had seen him at meetings to come by Athens for a visit, talking grandly about how much information he and his colleagues had obtained on the Georgia Piedmont. This proved to be a chimera also. Actually, a few counties had been mapped under his direction, but most of the Piedmont in the state was still a blank; they had not even mapped the home county around Athens. The faculty all seemed to be off chasing other bubbles, including high-pressure rock mechanics. I gave a few talks at the Department to repay their hospitality, and went home with little to show for my trip.

On April 27, we gave a huge garden party in our back yard---our last big entertainment. About 100 people came, our Survey friends and Trudy's friends. We had sandwiches, and a bar was set up, for which we hired two bartenders.

Helen Beikman and I had planned for a long time to go to the Denver office of the Survey to obtain information and advice for the map, especially in the Rocky Mountain region. We finally made the trip together on May 10-16. I knew that the runs of the California Zephyr would soon be discontinued, so at my urging we went to Denver by train, so that I could show Helen the scenery and geology on the way. Once in the Denver office, we fanned out on various visits -- I to get data on Idaho and western Montana, Helen on areas to the east and southeast. Unlike my inconclusive trip to the Southeast, this one was most productive, and we brought back material that would keep us busy for the next several months.

Work on the U.S. Map continued through the summer and fall. Our large-scale compilations were all reduced and generalized, and were plotted neatly by Trudy on the final 1:2 $\frac{1}{2}$ -million scale. We had by now finished a large part of the area of the map; only a few difficult areas remained -- notably the Piedmont, the Precambrian, and the Cenozoic volcanics in the West. There were simply no ready sources of information on these at that time, and it seemed best to let the map lie fallow for awhile, until more information turned up.

Helen's part of the work on the map was about over, and I had wondered about what she should do next, to continue the skills she had developed. However, the powers-that-be suggested that she should go on and make a Geologic Map of Alaska, as a companion to our map of the Forty-eight States, so she got started on this in early 1970.

On the last day of the year 1969 I felt very ill, and it soon developed into a bad case of the flu. I was sick at home until January 11, running a fever, and with little appetite for food. When I got on my feet again, I found that I had lost nearly 15 pounds. Losing weight is always something to strive for (at least for me), but this was a drastic way to do it. On January 12, I finally was convalescing and started back to the office again. Trudy and Lida were very attentive, and came for me and took me home each day (wonderful loving girls!). I sat at my desk propped up with pillows, and Trudy brought me hot chocolate now and then. After a week or so of this I felt no better and developed pains in my back. I went to the doctor at last, and he diagnosed it as a touch of pneumonia. His prescriptions knocked this out, and late in January I was on the mend at last; it had been a long siege.

During this time, and until mid-February, I worked on definitions for the A.G.I. Glossary of Geological Words. A new and much expanded edition of the Glossary was in the making, and in December I had received cards for most of the words on tectonics. During my illness I worked these over, rewriting and perfecting the definitions, and discarding some of the words. I was pleased to find, when the glossary was published a year later, that most of my work had been accepted.

I had been invited to be one of the speakers at a conference on tectonics sponsored by the University of Alberta at Banff in May, and in February I started assembling data for my lectures. I worked on these off and on through March and April. I had hoped to be able to take Helen with me to Banff, to give her a little taste of Canada, but this didn't happen, as we shall see.

On March 16 word came that Helen was needed in Washington, so our life that spring was rudely interrupted. I put her on the plane, and was at home alone. I would not see her again until June 11, long after the Canada trip was over. After Gertrude Finney died in 1953, Harry had been living in the house at 1322 Twenty-ninth Street, with

two lady friends to watch over him, but his health had gradually deteriorated; he was in his late eighties. Helen went to look after him, and in the end he was finally persuaded to give up the house and move into a retirement hotel, The Hermitage. But this took a long time, and it was a lonely period for Helen and me.

On March 22-28 I went to the Hayward meeting of the Cordilleran Section of the G.S.A., including a 4-day field trip beforehand through the southern Coast Ranges, led by Bill Dickinson and Ben Page of Stanford -- all very stimulating. On April 8-10 I went to San Diego for a visit to San Diego State College and to Scripps Institution of Oceanography; while there I gave some of the lectures that I had been preparing for the Banff meeting.

The Canadian trip finally came off on May 2-14. I went by plane to Vancouver, then on the Canadian Pacific train to Banff. The train left Vancouver in the evening, and next morning we were in Revelstoke on the west side of the Purcell Mountains. I had a fine view from the train of these mountains, of the Rocky Mountain Trench, and of the Rocky Mountains before reaching Banff in the early afternoon.

The meetings were to be held in the Center for Continuing Education and School of Fine Arts, a group of buildings in the pines on a hill above the town, with fine views of the surrounding mountains. Here, the Extension Department of the University of Alberta staged various conferences. It turned out that our own was rather small potatoes; a huge lawyer's conference was running concurrently, and others were coming in as we were leaving. Everyone slept in an attractive dormitory building, and had their meals in a big dining room that served hearty food.

The three featured speakers on our program were me, Rudy Trumphy the Swiss geologist, and V. V. Belousov the Russian geophysicist. Each of us gave five lectures through a five-day period. I talked on various phases of the tectonics of North America. About 50 geologists were registered for the course, mostly petroleum geologists from Calgary, but many other Canadians, and a sprinkling of geologists from the States.

After the conference, there was a field trip along the highway across the mountains into the Shuswap metamorphic terrane west of Revelstoke -- the same route I had followed on the train trip to Banff, but in poorer weather. At the end of this trip, we three speakers were dropped off at Lake Louise, and Professor Charlesworth took us

on a personally guided tour along the Rocky Mountains to Jasper, and then into Edmonton. In Edmonton we were guests for two days at the University of Alberta, and then I flew home. I had much enjoyed the many Canadians, and I gained a fine picture of the Canadian Rocky Mountains.

Trudy Edmonston met me at the airport. She was deeply engrossed in plans for her daughter Terry's wedding, which was to take place the following week. She was determined to make a big production out of it. She and the older daughter Joan were busy making Terry's wedding dress. She was having carpets laid in the Kendrie Street house, and she had ordered a champagne fountain. Terry was determined to marry a drip of a boy, Stormy Weems, who was hardly worth all the trouble. An enormous church wedding was staged at a little Catholic Church nearby. Trudy had imported her ex-husband, Carberry Edmonston, to stand up with her at the ceremony. He was barely sober, and retired in a drunken stupor afterwards, and we saw no more of him.

Lida and I went to the wedding, and spent the afternoon at the festivities at the house, loading up on champagne. I went home feeling quite squiffed, had a blueberry pie, and fell into bed. The marriage lasted only a year or so (despite Trudy's big sendoff) -- the boy was a no-goodnick, as everyone could have seen from the beginning. Terry drifted on to other liasons, sometimes married, sometimes not, and she is now out of my life.

Helen came home at last on June 11, having missed all the events at home of the spring. It was wonderful to see her again.

On July 4, "Krody" Krodel died. He was Lida's third husband, and they had been married about 8 years, but had never gotten along. Lida had been very mean to him. He keeled over on a fearfully hot day, while Lida was on a holiday at Lake Tahoe, and it took some time to locate her. We all went to the funeral with her, and it was a strain.

A project of the Geological Society of London had been banging around for the last few years. They wanted data on all the orogenic belts of the world, and the Survey had asked me to do the western Cordillera in the United States -- "just a month or so of Phil King's time". A little study of their questionnaire made it evident that this was not a questionnaire, but a career -- involving several years of compilation and effort. I had refused to do it, and nobody else on the Survey would touch it. But I got appeals from my British friends

so I decided to give it a try during the summer of 1970.

I worked on this project during June, July, and August, and assembled much interesting information, but the project was far from completed at the end of August, so I gave it up, much to the distress of the Geological Society of London. It would have to be written off as an aborted project. In September, it was time to get back to the U.S. Map project, which had lain fallow for the first eight months of 1970.

The first problem to take up was the Precambrian. Partly because of the work on the U.S. Map, a Penrose Conference of the Geological Society of America had been called for late September (September 18-25). The conference had been organized by Pres Cloud, who had arranged for it to meet at the University of Wyoming camp, high in the Medicine Bow Mountains west of Laramie, Wyoming. I spent the first part of the month getting ready for the conference. Since I was to be away, Helen decided to go to Washington at the time for two weeks.

Charlie Anderson ("Andy") and I flew together to Laramie, where we and other arrivals were driven to the mountain retreat. Two or three dozen attended, mostly Americans and Canadians, some from overseas. Many interesting topics were covered. I presented the Precambrian classification we had used in compiling the U.S. Map, which was well received by the Canadians, but met with a disappointingly sour reaction from Hal James, our Chief Geologist, and other Survey brass; evidently they had had no idea of how much information was available, and to what extent the different areas of Precambrian could be correlated with each other. (Hal James afterwards set up a committee to examine the question, and the committee verified most of my conclusions). Arrangements for the conference were being handled by Dorothy Palmer, a nice lady from the G.S.A. Headquarters, and we took an instant liking to each other. Early winter had set in in the high mountains, and it snowed several times when we were there, and it was snowing when we left.

On returning, Andy and I took a taxi down to Menlo Park from the San Francisco Airport, and I was greeted there affectionately by Trudy, who took me home. Next day, I spent the afternoon with her and her friend Mildred Kerr, and I took them to dinner. This marked

the high point of my friendship with Trudy, and our "special relationship." From then on, our friendship deteriorated; personal and family problems began to distract her, and we became more distant.

On October 11-16, I was off again, this time with Helen Beikman to Austin, Texas. Up to now, we had left most of Texas blank. A new compilation of the geology of the state was in progress, under the direction of Virgil Barnes of the Texas Bureau of Economic Geology, and we wanted to use as much of this as possible. We settled down in Virgil's office at the Bureau and made tracings of the compiled sheets, then scrounged around the records for data on areas not yet covered. As before in Austin, we were given a round of social entertainment, and felt that we were welcome indeed. We brought back enough data to keep up occupied for quite a while.

My thoughts then began to turn to another of our big problems -- the Piedmont region. A field conference of the Alabama Geological Society was planned at the beginning of December, under the leadership of Bob Bentley and Tony Neathery, and I decided to go. First, it was arranged that I would go as a visitor to Clemson University in South Carolina, and to West Georgia College at Carrollton, Georgia, under the A.G.I. Visiting Scientist program. Here, I gave some of my lectures, and saw some of the geological work that the staff were doing. Then, all of us went on to the Alabama excursion. I was in the Southeast from November 29 until December 6, and learned much about the new developments in Piedmont geology which would be of great use for the map.

Following that, I flew to Denver for a few days to attend some of the meetings of the Survey's Precambrian Committee. Much was decided about a new Survey classification of the Precambrian. On December 8 I flew home.

At the end of November, while I was away, Trudy Edmonston had a hysterectomy operation. The after-effects were severe, and she was sick at home through December. On many days, I would go over at noon to visit her and comfort her. It was not until January that she was able to return to work. It seemed to have some effect on her disposition, which became more difficult; my Helen passed on the opinion that women never had the same disposition after this operation. (About a month later, Helen Beikman had her hysterectomy, and it didn't seem to affect her disposition any).

In December, I worked over our accumulated data on Texas and the Piedmont. I had appealed for help on the Piedmont area from our Eastern Branch, and a savior appeared in the person of Michael Higgins, who volunteered to help in gathering and interpreting information. Fortunately, he had been permitted to spend part of the year 1971 at the University of California in Santa Barbara to work on radiometric dating of Piedmont rocks with G. R. Tuttle, so I visited with him there on January 27-29, 1971, and we accomplished much in our discussions; we added to it later when he returned the visit at Menlo Park.

I continued my work on the Piedmont through the spring of 1971, and as a final verification decided to attend the meeting of the Southeastern Section of the G.S.A. at Blacksburg, Virginia, on May 5-10, with a field trip afterwards into the Blue Ridge area, led by Doug Rankin. It was good to see the Southeast again, and all the Southeastern geologists, and I came back with much useful information for this part of the map.

The eastern half of the U.S. Map was now finished, and during June and July I colored a final copy, which was sent east to be reviewed by Survey geologists in that part of the country.

Our Buick convertible was now 5 years old, but there were reports that American manufacturers were going to terminate the production of convertibles that year, so we decided to buy a new one, which we obtained on June 26. It was an excellent automobile, which we still have.

On September 20 we received word from Moscow of the death of Professor Bogdanoff. He was a great and good man, and everyone who knew him all over the world felt his loss keenly. He was a few years younger than I was.

Helen Beikman decided to accept a foreign assignment, and went to Liberia on the Survey A.I.D. program from October until February, 1972.

Having completed the eastern half of the U.S. Map, I started in the autumn of 1971 to put the final touches on the western half. The remaining problem on the western half was the Cenozoic volcanics, and with much help from the Menlo Park geologists I was able to do this.

The eastern half of the U.S. Map came back from Washington in December, with a dismaying number of corrections, especially from the New England geologists. I felt that most of these comments were nit-picking, but some valid improvements were made. I was not looking forward to this kind of review of the much more complex western sheet, and George Becraft, during a visit, expressed equal distaste for it. We decided to simplify procedure on this sheet -- to have meetings of interested geologists in Menlo Park and Denver, where they could look over the map and make general comments. We held such a meeting in Menlo Park in early March, 1972, and later that month Helen Beikman and I went to the Denver office where we followed the same procedure. In mid-April, George Cohee came out to Menlo Park and went over the Legend with me in behalf of the Geologic Names Committee. With that, all reviews of the map were completed, and we were ready to perfect a final copy which could be exhibited at the International Geological Congress in Montreal in August of that year.

On April 1-8 Helen and I went to Yosemite Valley for a week's vacation with the Reagan family. We stayed in cabins at Yosemite Lodge, and had a fine time exploring the valley. The Reagan's often went there, but this was the first visit for Helen and me, except for my brief visit on the way back from Esmeralda County in 1961. Because we were early in the season, the valley was not as crowded with visitors as it would have been later in the summer.

Later that same month, April 23-28, I went on a float trip to the Grand Canyon. The local Pick and Hammer Club had organized a group of nearly 50, mostly Survey geologists and their wives, to make the trip. We assembled at El Tovar on the south rim, and were ferried in small planes to Lee's Ferry, the start of the trip down the river. We **slept** out three nights under the stars on our bedding rolls; fortunately, the weather was fine the whole way, with not a cloud in the sky and a full moon at night. It was a wondergul adventure, and was an excellent opportunity to see the rocks of the canyon bottom, only dimly visible in looks from the canyon

rim, such as I had had in 1954 and 1955.

At Phantom Ranch, about half of us left the party, to be replaced by a new contingent, who would continue to the lower end of the canyon. The part which I had seen had all the interesting geology; from there on the geology would be less interesting, and the attraction of the rest of the trip would be mainly in running the many rapids.

From Phantom Ranch, we were to walk out, making the 5,000-foot climb to El Tovar on the Bright Angel Trail. This walk was as great an adventure as the river trip, and it involved quite a bit of doing on my part. I left the float party about 10:00 A.M., and it was after 6:00 P.M. before I finally arrived at the top, very weary. Some of the group, more able than me, made the trip more quickly, but there were stragglers behind us, too. I made the last half of the climb with Bill and Mary Brosge, who kept about the same pace as I did, and were good company. In fact, the whole trip was a wonderful opportunity to mingle with my fellow-Survey geologists, and we ended up with a fine esprit-de-corps.

On May 4, not long after I returned from the canyon, Helen had to go east to Washington to look after Harry Finney; she was away until July 16.

During the time she was away, I was well occupied hand-coloring final copies of the U.S. Map -- one copy to be sent for exhibit to Montreal, another for the Branch of Technical Illustrations; these were sent to Washington early in July. I then started a text to go with the U.S. Map, and during the hot days in July I worked in the library, researching the history of previous geologic maps of the United States.

Helen returned on July 16, bringing Harry Finney with her. It had been decided, partly at his request, that he would abandon the Hermitage and come out and live with us. My quiet life of the last two months at home was interrupted; rooms had to be shifted about, and a boy brought in to cook the meals.

On August 7, I left all this to go to Canada for the Geological Congress. First, I flew to Edmonton to join pre-Congress Excursion A-66. This was to be a 10-day overview of the Canadian Arctic by plane. Nearly 75 people had signed up for it, and we traveled in a DC-6. We saw a great deal of the geology of Arctic Canada from above, photographing the sights from the plane windows. We had one memorable day in beautiful weather flying up the east coast of Baffin Island. The rest of the days the weather was mixed, with much cloud cover and fog. We therefore had to spend more time than we would have wished in our dormitory at Inuvik on the Mackenzie Delta. Our farthest north was above the 80th Parallel over Ellesmere Island. All told, we got as much out of it as could be expected, on a scheduled trip with limited time.

Returning to Edmonton on August 19 was an anticlimax, back from the Arctic weather to the hot summer on the western plains. From Edmonton our group, and the returnees from other excursions, flew to Montreal for the Congress sessions. Attendance at the Congress was enormous, and I didn't enjoy it very much; added to which I was no longer accustomed to the humid, rainy summer weather on the East. I spent much time at the map exhibits, which were very fine. The U.S. Geological Survey had **an excellent** display of maps, among which **our own** Tectonic Map of North America and the Geologic Map of the U.S. had prominent places. I also attended the sessions of the Commission for the Geologic Map of the World, but the meetings of its Subcommittee for Tectonic Maps was a disappointment. Professor Bogdanoff was gone, and Professor Khain of Moscow had taken his place -- a well-meaning, kind-hearted man, who did not run things with a firm hand.

It was a welcome break to go on a 2-day excursion to Quebec City, where the geology of this classical area was well demonstrated by John Riva. Quebec was scenic and historic, both in North American history and in the history of geology. Once back in Montreal, I decided to cut the rest of the Congress meetings short, and returned home on August 29.

Things at home had not gone so well. Caring for Harry was more of a burden than Helen could handle, and she was ill. Finally, on September 12, she found a nursing home not too far away that would take him in (Sunnyview Family Home), and we moved him there.

Back at the office, I worked on records of the Canada trip in September, and then started assembling records (correspondence, memos, etc.) of the U.S. Map project. I also started work on another project. Alan Nairn and Francis Stehli were editing a series of volumes on the Ocean Basins and Margins of the World, and they asked me to contribute a chapter on the Appalachian and Ouachita orogenic belts for the volume on the Gulf of Mexico and Caribbean. I roughed out an account of the data, and then put it aside to wait until the following spring.

On October 16-21 I went to Washington on final business of the U.S. Map. I spent much time with Betty Zimmerman and others of B.T.I. who were designing the map for printing, and had a pleasant lunch at the Cosmos Club with Dick Sheldon, the Chief Geologist, and others of the staff; during the lunch it was decided to add submarine contours to the map area surrounding the United States. Unfortunately, the change of scene gave me a bad cold, and I was sick at home for a few days after my return.

November, December, and January of 1973 were spent on the text that was to accompany the U.S. Map, and the first installment was completed in February.

On April 2-8 there was to be a meeting of the South-Central Section of the G.S.A. at Little Rock, Arkansas, that was to feature a symposium on the Ouachita Mountains, and I was invited to give a paper on the Marathon Region. The prospect of this meeting was the reason for deferring final work on the chapter in the Nairn-Stehli volume that I had started in the fall. The session was interesting and informative, and there were field trips into the Ouachita Mountains before and afterwards.

Before I left, Harry Finney had gone into a steep decline, obviously terminal; he was about 95 and had lived well past his time. He died on April 3, while I was away. I was sorry not to be home to stand by Helen, but we talked on the phone nearly every evening, which probably cheered her up some. She had the body cremated, and after I returned took the ashes to Washington for burial in Arlington Cemetery beside the grave of Gertrude Finney. (On May 22, Trudy Edmonston's ex-husband Carberry died in Washington, so much of our familiar scene was passing away).

After the Little Rock meeting, I completed the text of my paper on the Marathon Region, and then went on to complete the chapter on the whole Ouachita orogenic belt for the Nairn-Stehli volume. Trudy beautifully drafted the figures for each of these papers -- the last big job she was to do for me. I sent off both manuscripts early in July, but it would be several years before they were published.

On June 30, I officially retired. My 70th birthday would not be until September, but there were certain advantages to my pension if I chose June 30 as the retirement date. After that, I continued my Survey work on a W.A.E. (when actually employed) basis in order to complete my projects.

The retirement day passed without fanfare, and I wondered whether my leaving the Survey would go unnoticed. But on my birthday, September 24, a group of my Survey friends and colleagues gave me a luncheon party, and in the afternoon there was a much bigger gathering in the patio, with cake, presents, and speeches, so I was given a fine sendoff.

With the prospect of having to vacate my Survey office quarters, I began to plan for a workroom at home. We had a competent architect, Erwin Meissner, design a structure to be put up in the back yard at home, and construction on this began in 1974. Fortunately, the various sums that came to me in settlement of my retirement were sufficient to pay for the whole thing in cash (more than \$20,000).

After my retirement, Trudy Edmonston moved to another office, so we saw less of each other than before; she finally left the Survey for good several years later, and is now living in Half-Moon Bay. A little later, I was moved from our big office, which I had occupied ever since the building was built 20 years before, but the Survey allowed me to move to a smaller office on the first floor.

On November 26, we received word that Edward had died while on Sabbatical in England. This was a blow to all of us, as he was 12 years younger than me.

In July, I began the second installment of the text to go with the U.S. Map -- an account of the Precambrian of the United States. I finished the text at the end of December, 1973, but preparation of the accompanying figures required many more months of work during 1974. I was to complete two more manuscripts to go with the U.S. Map -- one on the Paleozoic and Mesozoic, the other on the Cenozoic. These texts have been published as Professional Papers 901, 902, 903, and 904.

This completes the record of my life and career through my 70th year, which is now four years ago. I have done many things since then, some interesting, others dull, but I would prefer at this time to end the record of my life at this point.

# AUTOBIOGRAPHY

PHILIP B KING

VOLUME 3 APPENDIX

Descriptions of the Texas  
Towns + wildlife are  
a valuable addition to  
my <sup>mental</sup> picture of the place.

Enjoyed Schucart's letter of  
encouragement! Also your  
acceptance speech for the  
Pewrose medal.

Enclosed is a little nostalgia  
piece. I wrote 4 years ago  
about Tennessee, after hearing  
the mourning doves in the  
back yard.

## WILD PLUM PICKING

It takes energy to keep up with the rank vegetation hereabouts. Over there is a field they forgot, with second-growth beginning, sprouts of briars. Somewhere nearby are the old gravestones we found once, not very legible anymore, since someone chose the local shale to carve on. A child can carve it.

A little dusty from the dirt road here, and hot, I'm standing in the thicket of high plum bushes and other weeds, some with thorns, some smelling of hay. These bushes seem permanent: could be, this patch was never cut over. My mother is out of sight, filling her bucket, watching out for copperheads as well as rattlers. (Do water moccasins live just in the water?) I don't bother to taste what I'm picking. I know it's tart.

Locusts wheeze, the afternoon shadows get heavy. The mourning dove's round jug-whistle call repeats and repeats. Plaintive flatted notes in a cozy Tennessee thicket. Through my blanket of solitude comes the thin sound of hillbilly radio music from the only house downhill: <sup>U</sup><sub>A</sub> racous but sweetly predictable. From this distance there's a Darlin'-Cory-graveyard-ground lonesomeness to it.

The Huff girls taught me to carry the mourning dove sound with me, to make an oval jar-shape with my two hands and blow in the hole between my thumbs. By blowing harder and softer I can get all three of the notes to come out.

But in later years only the cry of the throaty dove itself will conjure up this scene again.

## FIRST IMPRESSIONS OF WEST TEXAS -- 1924

### Lomita to San Angelo, July 16, 1924

From Lomita, which is about 50 miles west of Temple, to Brownwood, we follow the divide to the east of the Colorado River, which is also the escarpment of the Comanchean which dips eastward; the westernmost of the rocks of this age have been stripped away, laying bare a great area of Pennsylvanian and Permian rocks, which reaches down to the Central Mineral Region not far to the southwest.

The country is beautiful and only partly cultivated; for long distances there is nothing but low oak scrub interspersed with grassy patches. The fields when planted are large, and are kaffir corn, corn, and cotton. The topography is fairly rough, with occasional steep-sided, flat-topped buttes, most of them densely wooded, rising above the general level. Occasionally a broad, shallow valley stretches down toward the Colorado, giving a vista of far-distant purple skyline, and nearer at hand, a view of mile on mile of rolling slopes mottled green and brown by the scrub and dry grass, and darkened by patches of cloud shadows. All this is evidently grazing country.

This is the type **section** of the Comanchean rocks; it was from Comanche County, a few miles to the north, that R. T. Hill first defined the name. In cuts and in outcrops on the distant hillsides we can see the flaggy marine limestones of this series; while it is evident that more massive beds are the resistant strata which cause the flat-topped buttes. Occasionally in the lower ground red shales are seen, which become increasingly prominent as we approach the lower country. Whether these are Pennsylvanian or Comanchean is not known to the writer.

Occasionally one sees bits of old Texas in characteristic bits of architecture, such as porches extending from the eaves to the ground, glass cupolas, etc., and sometimes, nestling against a butte, one sees a many-winged structure, no doubt a ranch house of the earlier days.

The towns have a typical Texas spaciousness about them, with widely scattered houses set down here and there in the scrub, and broad dusty streets, white from the limestone country rock. The stores are often of brown stone, otherwise they are weatherbeaten frame. Sometimes the sidewalks in front are roofed over as a protection from the continual sunshine. Many have false fronts and all look picturesquely discouraged. One does not see many negroes, but the color line is sharply drawn; waiting rooms at the stations are divided, not by sexes, but instead there is a "negro waiting room" with a "white waiting room" set primly as far as possible from it.

Before reaching Brownwood there is a gentle descent to the valley of Pecan Bayou, a branch of the Colorado. The Comanchean strata are broken into numerous detached, flat-topped, steep-sided hills, while the valley bottom is in Pennsylvanian.

Brownwood is a town of about 8,000; it is a junction and distinctly more modern than the smaller places described.

Beyond Brownwood the line enters another group of rugged hills, a Comanchean outlier. On the east face of these hills, above Brownwood, considerable excavations have been made and a plant, probably a cement plant, is in operation. Within the hills the topography is rough and broken, with country rock at or near the surface. The limestones stand out as massive ledges or lie in huge blocks and slabs upon the hillsides.

The vegetation undergoes a gradual change. The scrub oak still continues predominant, but occasionally mesquite bushes, and prickly pear in the more rocky places, are to be seen, as well as the yucca with its sharp-pointed leaves and tall central stem. The mesquite stands out by the silvered appearance of its leaves and by its gnarled and scraggly branches.

Beyond Bangs the Comanchean hills are passed and open out into the plain beyond, breaking into promontories and isolated hills. The plain is underlain by Pennsylvanian and has a notable red color to its soil; it is for the most part under cultivation.

Santa Ana is a town newer in appearance than those previously passed, built up to the base of a series of high, flat-topped buttes several miles in extent composed of Comanchean rocks. The slopes are sometimes as great as  $45^{\circ}$  and several massive ledges outcrop around

their sides. The more shaly and sandy portions are quarried in the northeastern part of town where a considerable bare face has exposed pink, buff, and gray sediments.

Toward the southwest the country slopes away to the Colorado valley, giving long and distant vistas of brown hillsides dotted with scrub.

Westward the country is low but rugged, with hillsides covered by flaggy fragments of Pennsylvanian limestones and but scantily overgrown by dry grass and scrub.

Beyond Valera and from there clear to San Angelo the country is a level rolling upland upon which cultivation and fenced property appear in patches, the rest of the country being covered with dry sparse grass. Near San Angelo the cultivation largely disappears, giving place to mesquite bushes which occur in close patches interspersed with dry brown grass.

At Ballinger the uplands are broken by the broad gentle valley of the Colorado River, in which exposures of the Permian appear; this consists of red shales and gray, brown, or maroon massive rocks, probably either limestone or sandstone. The grayer beds are with difficulty distinguishable from the Comanchean except that in a general way they are not so massive, and being weaker weather into smaller pieces.

Beyond Ballinger we begin to see, far to the westward, the escarpments of the Edwards Plateau composed of Comanchean limestones. Far to the west-northwest a range of hills runs out from the plateau, breaking into knobs as they terminate to the eastward. More to the northwest and very much farther away are other hills, probably of the same system, but north of the Colorado, among them, is a conspicuous conical butte, evidently nearer than the others.

As we approach Niles the escarpment to the southeast of San Angelo appears very far away, forming usually an even rim on the skyline, but at times broken by notches and peaks.

As communications have become better throughout the country regional characteristics of people, architecture, and customs become obliterated, so that today it is scarcely possible to pick out some regional characteristics of small towns which could not be duplicated elsewhere. Still, in this particular portion of Texas there are local features and habits which have not been destroyed despite a superficial layer of uniformity and modernism.

The typical West Texas small town is a spacious affair, as if the inhabitants had felt imbued with the roominess of the country. Its streets are broad, and laid out in a rectangular pattern; their white dust overgrown, and soon dying out in the scrub which surrounds the town. The weather-beaten frame houses are separated by big vacant patches, also claimed by scrub and weeds, and dry grass. The store buildings all look picturesquely discouraged with their faded signs and barren windows, and usually of one storey, long and low, built of brick, sheet iron, or frame. The wide sidewalks in front are mostly roofed over clear to the street, the signs being placed on the outer edge or else hung from the ceiling of the overhang.

San Angelo is a larger town than most of the rest, and despite a modern six-storey hotel and new bank buildings is still quite typically West Texas, and away from the newer buildings one comes upon older structures which have so far escaped the comparatively recent expansion of the town. The court house, opposite the hotel, is an old-fashioned but dignified structure of brown stone, surrounded by trees. Near the southeast edge of town are the remains of old Fort Concho, formerly an army post on the route from El Paso to the east. Here one sees the old half-ruined brown stone barracks, stables, and better built officer's quarters, all arranged about a weed-grown plaza and drill grounds. Some of the old buildings are now used by more or less disreputable lumber yards, junk dealers, and squatters, but for the most part the old fort has been abandoned to decay and neglect.

San Angelo to Rankin, via the Orient Railroad,

July 17, 1924

Half a century ago the idea for the Kansas City, Mexico, and Orient was first conceived -- a railroad to run from Kansas City southwest to Topalobampo on the west coast of Mexico -- a magnificent idea, one of those daring enterprises of empire building for which American business of that day was noted, and one which looked very pretty to British investors who bought the stock. The line was never completed, due to lack of funds and the death of its builder, and today one finds it a halting, staggering road, bankrupt these many years and sunk into a leisurely resignation. It now consists of one line from Wichita, Kansas, to Alpine, Texas, and two short fragments in Mexico -- one out of Chihuahua, and one on the Pacific Coast.

Throughout Texas there is but one train a day each way, and west of San Angelo this is mixed freight and passenger, having what is probably the worst and most uncertain passenger service of any line in this part of the United States.

The railroad guide will tell you that the west-bound train will leave at 6:25 A.M., but on arriving at the station it will be found to leave anywhere from an hour to two hours later than that. At Rankin it is due at 1:40, but may arrive as late as 8:30 P.M. It is, of course, primarily designed for freight service, which probably explains matters, but the general opinion here is that the trainmen have a sinecure and know it, so that they consequently take things easy.

The line still has hopes of reorganizing, and completing its route across Mexico. There is considerable talk of its being taken over by the Southern Pacific or the Rock Island, besides which representatives have visited each county to ask for a decrease in assessments, in order that it may continue operations.

Going southwest from San Angelo on the Orient there is a gentle descent from the station to a broad plain which slopes south toward the middle fork of the Concho River, and the town is soon hidden from view.

All about are miles and miles of scrub and mesquite- covered grassland, stretching away southwestward and southward to the Comanchean escarpment. The Permian redbeds underlying the region form red and pink soil in which are mingled crumbly fragments of lime which in cuts are seen to be interbedded with the redbeds. Occasionally broad-bottomed arroyos and washes are crossed, and toward the Concho broad-bottomed draws appear and small badland topographies all giving good redbed exposures. At one place cobbles were seen on the surface (Quaternary gravels? or conglomerate in the Permian?).

Near the Concho are cotton fields and cultivation, but after climbing the other side mesquite, grass, and cactus again appear, and fenced fields are no longer seen.

Tankersley, a small station south of the Concho is typical of many such places, with about three store buildings and one or two houses set down in the scrub without apparent arrangement and from which many dirt tracks branch out and disappear in the mesquite. Set apart from the rest at some distance is a rather imposing but considerably dilapidated ranch house.

Several miles beyond this place the highway joins the railroad from the north, and follows it in a general way from here to Rankin and beyond. From San Angelo to this place the road ascends to a gap between a long promontory of Comanchean buttes, and some of its outliers and descends again to the valley of the Concho, which runs back and is hemmed in by the Edwards Plateau. The railroad, however, follows the tributary valley of Spring Creek, a pretty running stream with trees along its borders.

The country undergoes a gradual change, the valley gradually rising and becoming more gently sided, and at Mertzon we are definitely in the Comanchean.

The road follows the south wall of the valley at some distance from the railroad, rejoining it again at Mertzon. About three miles southeast of the latter place it passes through Sherwood, a small county seat town off the railroad, the cupola of its stone court house having a dummy clock with its hands painted on the dial pointing perpetually to 8:20. At the creek crossing at Mertzon beds of gray limestone conglomerate are exposed, of Tertiary origin. Along the stream are groves of box-elder and cottonwood, the last trees to be seen.

Beyond the latter town there is a gradual climb to the rolling uplands of the Edwards Plateau, and at Monument Station the country is of gentle relief, sweeping up to low buttes. The whole region becomes more arid -- the grass brown and the mesquite more sparse; densest on the shallow washes and bottom lands, thinning toward the hills.

Big Lake is probably the most considerable settlement in the region; it is a cattle town but owes its present prosperity to the oil field to the west.

The typical Edwards Plateau is a gently rolling surface with no buttes or hills rising above it, and broken by shallow basins, some many miles across in which there are rarely shallow lakes. One of these a number of miles west of Big Lake is about half a mile in length. Bedrock lies very near the surface in the higher ground, and for long distances the ground is strewn with limestone blocks. The vegetation is sparse, but quite generally grass covered, and in the basins there is a considerable growth of mesquite and other scrub.

At Big Lake a divide is crossed, all the country draining eastward toward the Concho and westward toward the Pecos.

A few miles before reaching Best a low divide is crossed, and the railroad skirts the south side of a depression many miles across in which the oil field is located. Rigs are scattered at wide intervals across the country, their gaunt forms breaking the monotony of the interminable scrub.

The two settlements of Best and Texon are glaringly new, with many tents, and the air is filled with the sound of hammers and the smell of raw lumber, as bare frame structures are rapidly erected. In places the earth is brown where the crude petroleum has soaked into the dusty soil. One gets whiffs of its unforgettable odor, mingled with that of hydrogen sulfide, which the oil contains, and one hears the heavy gurgle as it is run from the hastily constructed pipe lines into tank cars on the siding.

The country continues much the same to within a few miles of Rankin, when it becomes more dissected, being broken by valleys which extend down to the Pecos about twenty miles distant. The town is situated near the head of one of these, and consists of about half a dozen stores and a score of houses.

Desert scenery, southwestern Upton County

Last half of July, 1924

At Rankin and to the westward the country becomes even more barren than in the Edwards Plateau to the east, passing into a true desert in topography, vegetation, and culture, the explanation probably being the better runoff and saturation of the alluvium-filled valleys which here begin to open out and which drain down to the Pecos about twenty miles distant, in comparison to the level uplands of the plateau where impervious strata lie everywhere near the surface.

A newcomer from more fertile regions finds much to see and wonder over along the desert roads which branch out from town -- jackrabbits, incredibly lean, with black-tipped ears; long-legged, ungainly black birds, road-runners or chaparrals; lizards which scurry to cover like ground-squirrels; and occasionally a big black tarantula ambling along in the dust.

Less commonly white-bellied hawks and great black buzzards soar overhead, attracted by some pool of water, or some dead animal; and little fat cottontails scurry away. Coyotes and rattlesnakes are reported to be about too, but they usually keep out of sight; and certain wild localities up under the overhanging caprock are the lairs of bobcats and javelinas, the local wild boars.

The vegetation consists of fairly close-set low bushes with some mesquite and prickly pear, and an extremely sparse dry growth of wiry grass on the sandy earth below. Plants hitherto not seen are the Spanish dagger, related to the yucca; and century plants with great heads and sharp-pointed leaves. Sometimes these dot the valley floors in great abundance, standing like grim and silent sentinels above their humbler, more scraggly neighbors. Occasionally in some rocky wild locality one comes suddenly upon a colony of that strange and wierd plant called the "devils flower-basket" (= ocotillo), whose gnarled, waxy-surfaced, woody branches rise from their central root, bare except for tiny leaves and thorns. Very rarely, the more familiar cedars are found, stunted, contorted, and half-dying in the rigor of their environment.

The topography of the region is controlled by two more massive beds of limestone, which are separated by thinner-bedded and more argillaceous layers of lime, and underlain by sands and shales. The upper of these, nearly 60 feet thick (= Middle Caprock) lies near the top of the Edwards and forms a sharp prominent capping of the higher buttes and broad plateaus. Nearly 250 feet below this is the lower series of resistant ledges, which form lower hills and benches. The cap ledge often overhangs, forming cave-like recesses and in the dimple-like gullies which come down from it there are clusters of dark cedar scrub, reported to be sustained by small seeps and springs.

The region is cut into broad, steep-walled valleys miles across, which rise gently toward the rock benches that hem them in. From these open broad, sack-like tributary basins into which short gullies have poured their detritus to form alluvial fans. These basins rise imperceptibly from their openings headward so that rock benches which form conspicuous promontories above the valley at their mouths 75 feet or more high are gradually approached headward, until they are engulfed by the mass of detritus.

Looking out from one of these promontories we see the desert in all its beauty. The desert plants, set with almost mechanical regularity in the gray rocky soil, extend down the gentle slope to the broad floor of the plain; their clumps merge with distance into an endless sea of vegetation, dull green for the most part, but with long streaks and patches of red-purple, brown, and blue-gray. Now and again the plain is streaked by the darker coloring of a cloud shadow. By imperceptible degrees it approaches a butte and suddenly passing over it, engulfs it, making it stand dark and cool in contrast to its gleaming neighbors. Now and again dust whirls rise in the plain and after a few moments of rapid rotation, expire. On hot days, dozens of these can be counted from one view.

Here and again a higher butte rises above the benches on which we stand, as perfect in detail as objects near at hand, but showing its miles of distance by the lowering in chroma of its coloring; all its shadows are softened from black to blue, and the glaring yellow-buff of its illuminated surfaces are grayed and dimmed.

As sunset approaches, the harshness of the outlines are softened, shadows merge with sunlight on the far-away hills, rounding and smoothing their contours; the shadowed recesses of the rocks assume a blue opalescence, and long after the valleys are immersed in cool shadows the sharp caprock of the distant mesas gleam faintly with a pale pink.

#### From Rankin to Midland -- 1924

From Rankin the Midland road runs in a general northerly direction across typical Edwards Plateau country. About eight or ten miles from Rankin, Upland is passed, which ~~is~~ the former county seat of Upton County, but is uninhabited; all that remains standing is the empty stone courthouse, set down in the monotonous rolling plain. From Rankin to this point there is a gradual ascent, and many broad, deep valleys are crossed, in which it is often necessary to cross bare rock ledges. From the divides, one catches glimpses of vistas of high plateaus and broad level-floored valleys to the south and west, but beyond Upland these are left behind, and the plateau extends in all its monotony nearly to Midland, the road being a mere track winding through mesquite and grass, and occasionally passing a ranch or going through a fence line. This is typical West Texas cattle country, the whole region being used for grazing, although this industry has received a considerable setback economically during the past few years. Rock outcrops are few, the whole surface being covered by a sandy loam, usually buff in color, though occasionally tinged with red, possibly derived from wind-blown material from the redbeds to the north. Sometimes fragments of crumbly lime are mingled with this material.

About fifteen miles from Midland the country becomes more rolling and several limestone ledges are crossed in a gradual descent. The soil becomes a red sand with rounded masses of lime occasionally protruding through it, either of the Comanchean or of more recent origin as a caliche precipitate. Farther on true "redbeds" appear and the limestones are left behind. This is the southern margin of the Llano Estacado, and its contact with the Edwards Plateau; we have seen the first of that heterogeneous aggregate of deposits which mask the surface of this great area; of which one of the most characteristic is the type of sandy-loamy red clay through which we now pass, which is here probably derived from the Basement Sands of the Comanchean. It weathers into small badland topography with low vertically walled gullies. The nature of its soil seems to be more favorable for holding water than the plateau country to the south, for with nearly the same rainfall there is cultivation of cotton, corn, and other crops around Midland. Where there has been cultivation, the wind has sorted out the sands, depositing them in low, red, ripple-marked dunes on the lee sides of the fields.

#### Midland to Monahans, March 21, 1925

From Midland to Douro we cross a level rolling country with broad, south-trending swales many miles across, giving views of long blue expanses of monotonous even skyline. The country is covered with a red soil, sometimes predominantly sandy, at others clayey, which apparently overlies shattered, irregular layers of caliche frequently exposed in excavations for highway surfacing. Before reaching Odessa there is some cultivation and plentiful grass, but west of this place all fields disappear and the face of the country is covered with a dreary growth of scattered mesquite, some greasewood, and short, sparse grass.

The country rises by degrees to Douro (elevation 3,000 ft.) and two miles beyond we reach the rim of a west-facing escarpment with a rapid descent to lower country beyond (= Concho Bluffs). This escarpment is composed of the lowermost beds of the Fredericksburg Group, several of whose prominent beds outcrop here and are exposed in the railroad cuts, below which there is a well-marked layer of the upper-

most horizon of the Trinity. The strata appear to have a dip of from 30 to 50 feet to the mile to the east, and from 60 to 75 feet of material is exposed.

The limestone beds are fossiliferous and are called "Gryphea conglomerate," closely packed with fragmental shells of the oyster Gryphea, in a matrix of buff to white crystalline limestone; some of the crystals reach 1/16 inch in diameter, though the majority are finer. There is considerable brown ferruginous staining and some filling of small irregular cavities in and around the shells with earthy limonite. The bed of Trinity exposed is a dense, hard, fine-grained, cinnamon-brown sandstone with many minute glistening surfaces, possibly cleavage planes of calcite, and a scattering of dark-colored, pea-sized grit. It breaks under the hammer into jagged-edged chips.

Beyond the escarpment the country slopes downward for several miles into a broad, southeast-draining depression in which is Judkins. West of this place we enter the sand hill region -- a broad belt of dunes fifteen miles across which extends from here to the outside of Monahans, and whose irregular, peculiarly broken, hummocky surface, with occasional gleaming spots where the sun has picked a bare area forms a characteristic skyline, recognizable even from a distance. The sand is fine and varies from red to buffish-pink in color; some parts are fairly well covered by a growth of purple scrub, and yellow-green yuccas, and a little grass, but considerable areas, especially toward the west, present little but bare ripple-marked surfaces. Between the dunes are peculiar, aimless, sharp-sided depressions.

### The Sierra Madera -- December, 1924

The Sierra Maderas are a small circular group of sharp-pointed peaks about two and one-half miles across, which rise from their surroundings nearly 1,000 feet, and attain an altitude of 4,600 feet on their highest peak. The mass lies about 17 miles due south of Fort Stockton and 9 miles north of the northeastern foot of the Glass Mountains; it consists of an upthrust outlier of Permian limestone, surrounded by Comanchean rocks. The northern foot of these mountains were visited by the writer during the month of December, 1924, and some observations were made and samples taken, but no detailed studies were attempted either of the stratigraphy or of the structure.

From a distance the mountains are seen to be covered by a dense, thick growth of grass and cedars, pretty effectively masking the underlying structure, but in small patches more resistant ledges are seen to stand out and to be dipping at high angles. On a north-trending spur from the main group of peaks a prominent bed was standing nearly vertically, but more generally dips appeared to range from  $45^{\circ}$  to  $60^{\circ}$ ; but in such confusingly various numbers of directions that no definite idea of the general structure could be obtained. On the northeast side of the mountain the beds appeared to dip toward the north.

The first outcrops of Permian are found immediately on reaching the lower flanks of the group; away from them, slope wash and detritus with many large and small blocks of limestone cover all the bedrock.

On weathering the rock assumes on its surface the dead-gray color characteristic as well of the Comanchean, but examination quickly reveals its different nature. The rock is intensely hard, compact, and heavy, markedly different in these respects from the surrounding formations. Bedding is poorly indicated, the whole mass seemingly permeated by shatter-like joints, although at times it is definitely discernable by cracks parallel to it. The limestone is olive-gray to blue-gray in color, of very dense texture, whose minute crystallization is only apparent under a lens. Very often it is monotonous in its lack of structure, but some portions are of either conglomeratic or brecciated nature, with strikingly irregular weathered surfaces which show a confused mass of angular fragments ranging from  $1/8$  to 1 inch across, and which are more resistant than the matrix. Some of this

conglomeratic material has been stained, possibly by mineralization, and has assumed a salmon-pink to pale vermillion color. Considerable gray and white chert was noted in masses up to 3 inches in diameter with an almost vesicular structure; it has now largely been recrystallized into a fine-grained quartz which is in notable contrast to the lack of crystallinity of the limestones. No fossils were noted, except for some doubtful test-like bodies about 1 millimeter in diameter. In the wash several fragments of red quartzitic sandstone were noted, but nothing of this sort was seen in place.

Professor Schuchert to P. B. King, April 14, 1926

Directions for collecting Permian fossils in the Glass Mountains:  
To Philip B. King and brother:

I am mainly interested in the Permian faunas, and as the formations above the Word are thought to yield few fossils my interest centers in those from the Word to the base of the Permian.

As there is a time of orogeny followed by erosion after Gaptank time I am interested in fixing the time of this movement. Therefore we need large Gaptank collections and chiefly from the upper Gaptank. As the upper limit of the Gaptank is variable from place to place it is necessary for you to carefully note this fact and to locate accurately this in the section the fossils collected from place to place.

Upper Gaptank collections.-- Look especially for shale or muddy limestone places where the fossils weather out free. In such places get all the fossils you can since many of these are not only fine but what is more valuable, are new species. This is especially true of the gastropods.

Apparently good Gaptank fossils can be had from Wolfcamp northeast to Gaptank. Udden's best collections are from the vicinity of Gaptank (several places here), and from Wolfcamp. By all means collect the ammonite fauna (extensively) at the base of the hill marked 4752 on Hess Quad., which is about five miles northeast of Wolfcamp. Here there is an uppermost thick limestone (?50 ft.) beneath which are softer shales and limestones (?200 ft.), a second thick limestone (? 20 ft.) and again softer limestone and shales (? 130 ft.). It is 60 feet beneath the second limestone that the small ammonites occur. They are common in the dark limestone secured from loose blocks down the hillslope. I wish you would collect from the shale of the upper Gaptank zone by zone since it is possible that other ammonite horizons may occur, in which case we would learn something about the evolution of them before Permian time.

Note that at this locality 5 miles northeast of Wolfcamp the upper Gaptank is about 200 feet thicker than at Wolfcamp, and thicker than anywhere else. This is the conclusion of Grant and Blanchard.

Wolfcamp formation.-- As this is the first Permian formation it should be most extensively collected from, and from zone to zone. In having large collections we can then trace the evolution of all stocks throughout the remainder of the Permian; not only this but also see what their time relations and fossil affinities are with the upper Gaptank.

A fine Wolfcamp place for fossils is northeast of Wolfcamp about one-half mile, on the hillside northeast of a dry gulch. Here also occur small Richthofenias in single free specimens and in clusters overgrown by ? bryozoans. Fusulines and Schwagerina are common here. Close collecting is required here.

Hess formation.-- Schuchert collected nothing from the Hess. Udden's finest locality for free fossils in great abundance is  $2\frac{1}{2}$  miles N  $65^{\circ}$  W of Wolfcamp. It is replete with free but small crinoidal columnals and other parts but these have no value and need not be collected. Udden's collection has two cigar boxes full of this crinoidal matter. Many of the fossils here are semi-silicified and will etch out. The fauna as a whole looks much like the upper Gaptank, but is clearly Permian.

What is the relation of the Wolfcamp to Hess formation? Are they continuous deposits?

Cuesta north of Wolfcamp.-- Udden collected here large Productus cf. ivesi, large Aulosteges with very high cardinal ventral area, large Enteleles, large Lyttonia and small Fusulina elongata. The association looks like high Permian; what is the formation? As P. ivesi is a characteristic of the Kaibab (Grand Canyon of Colorado) fossil this fauna may be most valuable in Permian correlations. Therefore, try to locate this zone and collect from it.

Clay slide of Leonard formation.-- Given as 2 miles northwest, or 2 miles west-northwest of Iron Mountain. One of the labels says about 300 feet below the basal limestone of the Word, at base of Udden's clay slide, 2 miles north-northwest of Iron Mountain. Here again Productus ivesi is common, and the fossils are free. At foot of clay slide occur in nodules large ammonites (Perrinites). By all means find this locality and collect extensively from it.

Another label says that the same fauna occurs in heavy ledges top of Leonard Mountain on north side, and in thinner ledges on top of the heavy ones.

Leonard formation.-- On Widen's Ranch occur large and free Waagenoceras hilli and strongly bundled Spirifers like S. cameratus. If you can locate this place get some of these things.

Word formation.--  $\frac{1}{4}$  mile north of Word Ranch occur siliceous masses (chert) in a residual clay that have fine Richthofenias (long), large and fine Lyttonias, and large Fusulina elongata. We must have some of these fossils, as they are the most typical Permian fossils. Besides, we want to compare them with the same things from India.

Finally.-- The above plan is of course extensive and the \$200 given you may not pay for the plunder secured. Rest assured that if your success is good, I will not forget you financially.

In a young man's vocabulary there should be no such word as fail (Richeleu), and accordingly success brings more success; God gives to those that have. Good luck to the Kings!

Yours truly,

Charles Schuchert

Peabody Museum, Yale, New Haven, Conn.

Austin, April 14, 1926.

WESTERN UNION

Gvt. NL = F  
Washington, D. C.  
May 22, 1930

Received at 18 East Congress Street  
Tucson, Arizona

Philip B. King  
University of Arizona  
Tucson, Arizona

Practically certain that you will be assigned to completion of  
Marathon area and hope that allotment can be made this fiscal year  
so that work can start in June STOP More definite advices later.

Mendenhall

Letters from W. A. J. M. Van der Gracht, 1931 - 1933

Mr. Ph. B. King  
Bureau of Economic Geology  
Austin, Texas

Schloss Hainfeld  
Post; Feldbach, Steiermark  
May 20, 1931

Dear Mr. King:

I thank you very much for your kindness in sending me a copy of your splendid new treatise on the Geology of the Glass Mountains region, as published in Bulletin 3038 of the University of Texas.

I also thank you for your kind criticisms and suggestions regarding my manuscript on the Permo-Carboniferous Orogeny of the southern United States. This manuscript is an abbreviated portion of a more extensive treatise, which is now in press and is being published by the Royal Academy of Amsterdam. These suggestions and further detail comprised in your bulletin came extremely handy, since these data could still be made use of in the final proofreading here, as well as for the manuscript to be published in the AAPG Bulletin.

It was extremely gratifying to me to find, that working entirely independently of each other, we have come to such very similar conclusions as regards to the general lines of the late Paleozoic orogeny in the southern states. I have been pondering over this problem since the last fifteen years, but only gradually deep drilling and new work in the exposed areas have procured additional facts, and the solution of the many puzzling problems seemed available. I gave these ideas in abstract before a meeting of the Academy in February of last year, and offered it in abstract before the March 1930 meeting of the AAPG. Since that time, new facts, including particularly those procured by your own work in the Marathon region, though giving additional detail and changing some minor aspects of stratigraphical correlation, have procured additional confirmation of the general thesis.

It will be a pleasure to me to offer you a copy of my European treatise (in English language) as soon as it is published. Here in Europe, we all felt a great need for a comprehensive treatise of this sort, since these American mountains are such an important item in the general facial structure of the Northern Hemisphere, but what is to be found in European textbooks so far is most incomplete, antiquated, and partly entirely erroneous.

I was very pleased to hear from Stille, during his recent visit in Austin, that he agreed with me on the general lines, after a personal visit to some of the localities. I have not seen him yet, since his return, but hope to have this opportunity before long.

Will you also, please, thank Mr. C. L. Baker for his kind suggestions, and again give my kindest regards to Dr. Sellards.

Very sincerely yours,

W. A. J. M. van Waterschoot van der Gracht

Maastricht, 21 Hubertslaan  
December 24, 1932

Mr. Philip B. King  
U.S. Geological Survey  
Washington, D.C.

My dear Mr. King:

I am exceedingly grateful for the advance copy of Guidebook 28 of the next XVI International Geological Congress, in which you give a magnificent synopsis of the main structural features of the North American Continent. I consider this a masterly piece of work and I heartily congratulate you with it. You have succeeded in condensing an enormous amount of information into a very handy little space, and still performed the job in a very readable manner.

I certainly look forward to your treatise on the Permian Stratigraphy of West Texas, and hope that it may appear before the Congress.

It is my intention to offer a short paper, giving a summary of my treatise on the late-Paleozoic mountain ranges, to the Congress. Unfortunately I will not be able to attend in person, since I cannot possibly absent myself from my present job, directing our Bureau of Mines here.

At present I am also trying to find an opportunity to do some research work on the late Paleozoic foreland here in Europe. Conditions here are exactly the reverse from those in North America. Here the central and front ranges of the late-Paleozoic ranges are beautifully exposed, but the foreland is mostly hidden under an enormous blanket of Mesozoics and Tertiary in the North-German Plains, Holland and the North Sea. Still, the data we have about it indicate that conditions on it are very similar to those in the American Midcontinent, only it is broken considerably by enormous faults. So in the same manner as I used our knowledge of the Paleozoic mountains of Europe in trying to picture what the buried ranges in America might be like, I intend to use our knowledge regarding the American Midcontinent Plateau to draft a picture of the buried Plateau-foreland of northwestern Europe. I may succeed in getting connected with rather extensive explorations for petroleum which are planned in southwestern Germany and in Holland, which will necessitate to extend our knowledge of the buried foreland considerably, both by geophysics and drilling.

I have a very few copies of my treatise on the late-Paleozoic orogeny as published by the Academy here, and will be very pleased to send one to Dr. Arthur Cooper at the National Museum.

I also suggested to the secretary of the Academy that they place the book on sale somewhere in the United States, and will write to Mr. Hull of the Assoc. of Petroleum Geologists whether they would care to be bothered with it.

Repeating my thanks, and extending the best wishes of the season, notably for much further successful work during the next year, I remain

Cordially yours

W. A. J. M. van Waterschoot van der Gracht

Maastricht, June 14, 1933

Mr. Ph. B. King  
U.S. Geological Survey  
Washington, D. C.

My dear Mr. King:

In grateful recognition of your having so kindly sent me an advance Copy of your splendid guidebook 28, I am enclosing a copy of the paper I am sending today to Mr. Mendenhall, on the late-Paleozoic Orogeny in the North American continent, intended for presentation to the International Congress (Abstact was sent before).

As you will see, I abstained from adding a general map of the late-Paleozoic mountains, but refer to your map in Guidebook 28, which is very complete, clear, and amply sufficient. I only added a detail map of the interrelations of the Wichita and Ouachita systems in Oklahoma and Texas, which is the key region and, naturally, is not treated in so much detail in your general synthesis.

The three tables referred to in the text are more or less identical to the Tables I and II of the A.A.P.G. Bulletin paper (vol. 15, 1931, pp. 991-1057) and Table XV of the Royal Academy paper (Vol. XXVII, no. 3, 1931).

I regret very much that I will not be able to present the paper in person, and add some eventually asked further explanations, in a possible discussion. If there is a chance to have the paper presented at the meeting (when probably it will have to be somewhat abbreviated), there is no person who could do it better than either you or Dr. Miser, who are both so familiar with the subject, and I would be very much indebted to either of you, if it could be done. The subject attracted considerable attention in Europe, and so our visitors from abroad might be much interested to have something said about the subject at the meeting itself, and be able to ask some further questions, rather than to have to wait for the publication of the Compte Rendu. It would then give them an opportunity to discuss the matter further with the American geologists who know the region, and possibly to give it some attention during the excursions.

Nothing could illustrate a presentation better than a slide of your own structural map, and possibly one of my detailed map of the Wichita-Ouachita interrelations.

Regretting very much that I will not be able to see you, or Dr. Miser in Washington this summer, I remain with kindest personal regards

Cordially yours

W. A. J. M. van Waterschoot van der Gracht

United States Department of the Interior -- Geological Survey

November 23, 1933

Memorandum for the Chief Geologist:

There is transmitted herewith a manuscript, with illustrations, by Philip B. King, entitled "Permian stratigraphy in trans-Pecos Texas". I have read this manuscript carefully in its preliminary draft and we have since discussed together many phases of it. Mr. Miser and Dr. Girty have also read it and held many discussions with Mr. King about it. He has also sent the manuscript to a number of outside geologists whose reactions to it are revealed in the attached correspondence. In the light of the above mentioned discussion and criticism Mr. King has carefully revised his paper, and it has been passed by the Committee on Geologic Names. Mr. Miser and I have each read it carefully in its revised form and Mr. King has made still further revisions.

It seems to me that Mr. King has given unusual care and attention to the preparation of this paper, which I regard as a distinct contribution to Texas geology. He desires to publish it in the Bulletin of the Geological Society of America, and I recommend that permission for such publication be granted.

G. R. Mansfield

Geologist in Charge

Section Areal and Nonmetalliferous Geology

June 8, 1933

Memo by N. H. Darton

On paper by Philip B. King on Permian of trans-Pecos Texas

This paper has my unqualified endorsement as a correct statement of all available data bearing on the problems of classification and history of the strata. It presents good reasons for making a separation of Permian and Pennsylvanian on a practical basis. Possibly this basis is somewhat arbitrary from a paleontologic point of view but considering the indefiniteness and changes in this country and abroad the selection of a definite separation plane is most important for classification and mapping. The plane here set forth is readily recognizable in western Texas and accords with the classification which I extended south from New Mexico. There for many miles the Abo redbeds separate the Permian limestone succession from the Pennsylvanian limestone succession, and thinning out near the Texas line the two limestone successions with their separating unconformity extend south into the Hueco Mountains in Texas, as described by King in this paper. It is equally clear that farther east in New Mexico the Permian limestone succession, of which the age is not questioned, grades into the limestones of the Guadalupe Mountains, the Carlsbad limestone member constituting their upper part.

N. H. Darton

May 31, 1933

Memorandum to Mr. King:

I have handed you the manuscript on the Permian stratigraphy of the trans-Pecos region which you asked me informally to criticize. I have read the manuscript with reasonable care and made a few pencilled notes on the margin which you will probably be willing to consider.

My feeling is that your subject is so large in scope and so complicated in detail that it deserves, indeed, requires a discussion of more than 100 pages. You have given a readable exposition of your opinions and have offered an explanation of a good many things, but have furnished the reader with scant grounds for accepting your opinions or believing your explanations. As a matter of fact, I do not accept or believe a number of them, but I cannot permit myself to criticize until I have examined the evidence in my possession, and it is largely because of the lack of stated evidence that my comments are necessarily so general. The correlation of the rocks of the different mountain areas of the Trans-Pecos region seems to be basic to your entire treatment, and the paleontologic evidence is basic to your correlations. Although you seem on page 89 to make a point of having presented such evidence, the few fossil names with which your pages are here and there embellished might have been omitted without being greatly missed.

George H. Girty

Geologist

P.S. -- Possibly I have been too downright in these comments but I feel that your paper is overweighted with opinion and deficient in a foundation of evidence.

G. H. G.

July 14, 1933

Memorandum for Mr. Miser:

I acknowledge the receipt of Mr. King's manuscript dealing with the Permian Stratigraphy of the Trans-Pecos region and I desire to thank you for referring it to me officially, following a tradition which I believe should generally be observed. It happens, however, that I had already read this report at Mr. King's request, and it has not seemed necessary to read it again.

I think that the report is well written and that it contains information of value. The weakest element, and it seems to me to be very weak, consists in the correlation of horizons in the isolated mountain uplifts. These correlations are much more a paleontologic than a stratigraphic problem, a truth which the reader learns to appreciate from the repeated references to variations in the character and thickness of beds in their lateral extent. These correlations still lack in any substantial degree the authority of paleontologic evidence, and that which is cited in the report is negligible. I heartily approve, therefore, of Mr. King's not carrying into other areas the formation names of the Glass Mountains section. If, however, any of these correlations are at fault, I cannot but think that some of them are, the generalizations based upon them are to that extent impaired.

I avail myself of this opportunity to correct a fallacy which has several times appeared in print and which Mr. King repeated in his report, to the effect that the Capitan limestone is equivalent in part to the Delaware Mountain sandstone. Mr. King may have revised the paragraph dealing with this subject at my instance, but the explanation of this anomaly should be made of record. As explained to me by Mr. King (for the facts known to me made the statement incomprehensible) it was found on tracing the Capitan northward that some of the limestone beds in the lower part of the formation were replaced by beds of sandstone. These, on account of their lithologic character were classed as Delaware Mountain formation -- therefore the Capitan in part was equivalent to or correlated with the Delaware Mountain formation.

George H. Girty  
Geologist

Study of Permian stratigraphy of trans-Pecos Texas  
a project proposed by Philip B. King

(1) Abstract of project

The writer proposes to continue his investigations of the Permian rocks of trans-Pecos Texas by a study of a Permian section of major importance. Topographic maps have only this year become available for the district in which this section is exposed, and for the first time it is possible to study it in great detail. The writer proposes to spend five-months in field work, and asks for an appropriation of \$1,096.50.

(2) Statement of project

The Permian problem.-- Since the time of Girty's monumental work on the faunas of the Guadalupe Mountains (1908), the problem of the American Permian, and more particularly the Guadalupian phase of the Permian, has been much debated among stratigraphers and paleontologists. Is the Guadalupian faunas a facies of the more typical Carboniferous, or is it a stratigraphic entity, younger than the better known faunas? How, precisely, did the Paleozoic era come to an end in the southwestern United States?

Since the time of Girty's first studies, much further work has been done on the problem. This later work has raised certain problems regarding the Pennsylvanian-Permian boundary, but it has also shown that there is a great thickness in trans-Pecos Texas of marine strata which are younger than any strata of Pennsylvanian age, as that series is commonly defined. Some geologists have suggested, with considerable justification, that the Permian section in this area can appropriately be made the standard of reference for the series in North America.

Recent work on the Permian series in the region has shown many complex faunal and stratigraphic features. Lithologic changes in the series are very abrupt and marked. In places, sandy and shaly strata were deposited, in others limestone masses, partly of reef origin, and in still others redbeds and saline deposits. These changes in facies, bewildering as they may seem at first, were not without systematic arrangement. The marine sandy and shaly beds appear to have been laid down in an arm of the sea which projected into trans-

Pecos Texas. The seaway was nearly encircled by limestone reefs, beyond which lay an area which received chiefly redbeds and saline deposits. These lithologic changes are strongly reflected in the faunas, and the relation between the two makes the region a remarkable laboratory for the study of ancient marine ecology.

The problems of the fossils and the physical stratigraphy of the Permian series in trans-Pecos Texas deserve detailed study. Such studies make the Permian section of the region more useful for correlation purposes, and add to our knowledge of the history of the closing stages of the Paleozoic era.

Work on the problem by the writer.-- Since 1925 the writer has been studying the geology of trans-Pecos Texas, and during most of this time his attention has been centered on the Permian rocks.

During this time, in association with his brother (R. E. King), the Permian area of the Glass Mountains was mapped in detail, and the results were published by the Bureau of Economic Geology of the University of Texas. Later on, the Permian rocks of the Diablo Plateau district (Sierra Diablo and Hueco Mountains) were studied, but the detailed results of this work have not yet been published. Part of this work was done for the Texas Bureau of Economic Geology in company with R. E. King, part for the U.S. Geological Survey in company with J. Brookes Knight.

During the past year the writer has been assembling the available information on the Permian of trans-Pecos Texas, in order to make a general summary of the problem. The detailed observations in the Glass Mountains and Diablo Plateau were utilized, as well as reconnaissance observations in other areas, and the published results of other geologists. This paper, "The Permian stratigraphy of trans-Pecos Texas", is now ready for publication.

The Guadalupe Mountains.-- The first work on the Permian rocks of trans-Pecos Texas was done in the Guadalupe Mountains, and it is from this district that most of the material for Girty's Guadalupian fauna was collected. Since Girty's work, further studies have been made in the area, partly by the U.S. Geological Survey, but even more intensively by geologists working for oil companies. This work has revealed many complex stratigraphic features, which are of the sort characteristic of the Permian as a whole. A complete understanding of these features has not been possible, however, since no careful

paleontologic work has been done in the region in recent years, and since no topographic base whatsoever was available on which to plot observations.

To the writer, in his work on the Permian problem, the Guadalupe Mountains are of more than ordinary interest. The region contains the type section for the Guadalupian Permian, and needs restudy, so that correlations between it and other sections in trans-Pecos Texas may be better established. In these mountains is the great mass of the Capitan limestone, which is similar to the limestone reefs which he has studied, but much thicker and larger. Moreover, the mountains possess several features which furnish information not available in the Glass Mountains: (a) northward in the Guadalupe Mountains, away from the trans-Pecos Permian seaway, there are continuous exposures into rocks of saline and redbed facies; (b) east of the mountains there has been intensive drilling, and it is possible to link the surface exposures with well records, and thus establish correlations with the buried Permian salt basin; (c) the upper rocks of the Glass Mountains are dolomitized and generally unfossiliferous, whereas those in the Guadalupe Mountains are locally very fossiliferous.

Investigation proposed.-- The previous lack of topographic maps in the Guadalupe Mountains has prevented the making of detailed geologic observations. During the past year, however, the U.S. Geological Survey has mapped two 15-minute quadrangles which cover the most critical part of the area. The region is now ready for detailed study, and the writer wishes to work there as a continuation of his investigation of the Permian series.

The detailed study should be carried out along several lines: (a) Careful collection of fossils, bed by bed, and locality by locality; (b) the measurement of numerous stratigraphic sections; (d) the study in three dimensions of the form of the various types of deposits, particularly of the limestone reefs, with the aid of the topographic map; (e) careful mapping of traceable beds on the topographic base, in order to correlate sections and show the form of the various deposits; (f) study of the structure, not only to facilitate correlation of the beds, but also to determine to what extent structural lines have influenced Permian sedimentation.

It is proposed that these investigations be made in the southwestern Guatalupe Mountains. Work would be begun in the vicinity of El Capitan, which is the original district studied by Girty, and the area of type sections of the formations involved. Detailed studies would be extended from this district northward and southward along the escarpment. Perhaps the whole eastern half of the quadrangle could be covered in a field season, but a small rather than a large area would be studied because of the detailed work necessary. A study of this area would not solve all the problems of the Permian, but it would provide a solution for some of the questions, and would lead to a better understanding of the type section of the Guadalupian.

Availability of the problem.-- The region has been studied by various oil geologists, but most of their work has been completed. None of them contemplate any further scientific work in the area, and several of them would welcome further work of this sort. The region is not of economic importance, and any study contemplated would not be with a view of locating mineral resources. The non-economic character of the project prevents it from being seriously considered by the Geological Survey on account of the present reduced funds for geologic work.

### (3) Plan of work

Base map for field work.-- The writer is informed that the plane table sheets of the topographic map will be inked this winter, but will not be printed. It will be possible to have photographic copies of the map, and in the estimate of expenses an item for reproduction of the map is included.

Field plans.-- It is proposed that five months of field work be done in the area, starting from the type section of the Guadalupian, and continuing the studies away from it for as great a distance as time will permit.

It is proposed to do the work in the spring of 1934, probably starting in February. During some time in this fiscal year, the writer will probably have to take a furlough without pay from his regular position on the Geological Survey. Rather than remain idle, he would prefer to go into the field.

The writer plans to do most of the fossil collecting himself, and in order to guide the field work, hopes to be able to make fairly accurate field identifications of the more important fossils. In addition, he proposes to avail himself of the services of a paleontologist for about a month in the field. He has talked with a number of paleontologists connected with either the Survey or the National Museum in Washington, and finds that several would be interested in doing the work. The final selection of the one to go will depend on the availability of the different men at the time. He proposes that the paleontologist join him after about two months of field work have been completed; he will then have the important collecting localities well in mind, and the paleontologist can then amplify the writer's own collections at the places where it is most needed.

Headquarters will be made at a ranch centrally located in the area (Frijole), which takes in guests at moderate cost. Field work will mostly be carried out on foot, with the aid of an automobile, but the less accessible parts of the district will be reached by horseback and short camping trips. The writer will use either his personal automobile or one belonging to the Geological Survey, if such is available. In either case, the expense to the Society will be the same. Geological instruments can probably be borrowed from the Geological Survey.

Office work.--- After the completion of the field work, the writer will prepare a geological report on the area.

Most of the fossils collected will be turned over to Dr. G. H. Girty of the Geological Survey for study. He has the largest pre-existing collection from the region, which includes the type specimens of his Guadalupian fauna. In some of the groups, including the important stratigraphic markers, the fusulinids and ammonoids, a specialized knowledge has arisen. Specimens of these groups will be sent to experts in these fields, probably to Dr. C. O. Dunbar of Yale and Dr. A. K. Miller of the University of Iowa.

It is probable that the writer will be permitted to prepare the report in the office in Washington in connection with the Permian work on which he is already engaged. The writer plans to have most of his important report work out of the way before leaving for the field for this project. It is expected that a finished report can be turned in about a year after leaving the field.

#### (4) Nature of the report

The written report will probably be 150 to 200 pages in length. It will deal entirely with stratigraphy, and will describe the stratigraphy and paleontology of the formations and members involved. There will also be a discussion of the principles of sedimentation and its relation to structural features and faunas. The section will be correlated with Permian rocks of other areas. The report will be illustrated by appropriate maps, sections, and photographs.

It is possible that the report could be published by the Geological Survey, but the limited funds of that organization would mean a long time delay. For that reason, it is likely that the Survey would not object to the publication of the report in the monograph series of the Geological Society, or in any other place which the Society would designate.

#### (5) Estimated expenses

For carrying out this project, if approved by the Society, the writer requests that \$1,096.50 be set aside, to be used in the following manner:

##### (A) General field expenses:

(1) Photography (films, developing, printing, etc.).....	\$25.00
(2) Reproduction and enlarging of base maps.....	35.00
(3) Freight charges for fossil collections.....	25.00
(4) Ordinary automobile expense (oil, gasoline, greasing charges), 5 months at \$20.00 per month.....	100.00
(5) Repair work on automobile (including purchase of one or more new tires during field work), 5 months at \$15.00 per month.....	75.00
(6) Purchase of camp supplies (cot, portable stove, etc.)	10.00
(7) Miscellaneous (cord, wrapping paper, lumber for packing boxes, occasional hire of horses, etc.).....	<u>20.00</u>
	\$290.00

(B) King's field expenses:

(8) Railroad and Pullman fare, Washington, D. C., to El Paso, Texas, and return.....	\$180.00
(9) Meals en route.....	12.00
(10) Subsistence (board and room at ranch, camp provisions, etc.), 5 months at \$60.00 per month.....	300.00
(11) Hotel bill, on business trips to town.....	15.00
	<u>\$507.00</u>

(C) Field expenses of paleontologist:

(12) Railroad and Pullman fare, Washington, D. C., to El Paso, Texas, and return.....	\$180.00
(13) Meals en route.....	12.00
(14) Subsistence (board and room at ranch), 1 month at \$55.00 per month.....	55.00
	<u>\$247.00</u>

(D) Expenses in office:

(15) Thin-sections of clastic rocks, 10 sections at 75 cents apiece.....	\$ 7.50
(16) Photographic work (enlargements, making of prints for reproduction, etc.).....	15.00
(17) Railroad and Pullman fare, round trip to New Haven, Conn., for conference with Dr. Dunbar on fusulinid collections.....	30.00
	<u>\$ 52.50</u>

Total expense for project.....\$1,096.50

(6) Schedule of payments

The writer suggests that items A, B, and C be paid in equal installments, one at the start of the field work, and the other two aand one-half months later. At the close of the field work, the funds will be fully accounted for, and any balance returned to the Society. Item D could be held till the office work was begun, and could be paid when the bills were presented.

(7) Other sources of funds

The writer has no other sources of funds, besides his salary on the U.S. Geological Survey. During a part of the field work, as stated above, he will probably be on furlough without pay. The writer could not finance even a part of the project outlined.

About one month ago the writer, along with some other members of the U.S. Geological Survey, submitted outlines of projects to the National Academy of Sciences. Presumably this project is on file at their offices and will be considered by them. This project is similar to the one outlined above, but was less fully considered. It calls for three months of field work with no assistance from a paleontologist at a cost of about \$500.00. The writer now believes that work for this shorter time would not do justice to the project, and he would much prefer to do the work on the more ambitious scale outlined above. If, by chance, funds should be allotted by both organizations, the writer would accept only one.

(8) Published results

The following are the principal publications of the writer which deal with his work on the Permian problem. This list includes some papers as yet unpublished.

- P. B. King and R. E. King, 1929, Stratigraphy of outcropping Carboniferous and Permian rocks of trans-Pecos Texas: Bull. Amer. Assoc. Petrol. Geol., vol. 13, p. 907-926.
- P. B. King and R. E. King, 1931, The geology of the Glass Mountains: Univ. of Texas Bulls. 3038 and 3042.
- P. B. King, 1932, Limestone reefs in the Leonard and Hess formations of trans-Pecos Texas: Amer. Jour. Sci., 5th ser., vol. 24, p. 338-354.
- N. H. Darton and P. B. King, 1933, Western Texas and Carlsbad Caverns: 16th International Geological Congress Guidebook 13.
- P. B. King, Permian stratigraphy of trans-Pecos Texas (100 page manuscript, now ready for publication, and soon to be submitted to the Geological Society of America for publication).
- P. B. King and J. B. Knight, Geology of the Diablo Plateau (in preparation).

Respectfully submitted,

Philip B. King

Assistant geologist

U.S. Geological Survey

THE GEOLOGICAL SOCIETY OF AMERICA  
419 West 117th Street  
New York

October 24, 1933

Mr. Philip B. King  
U.S. Geological Survey  
Washington, D. C.

Dear Mr. King:

I am glad to be able to inform you that the Council of the Geological Society of America, acting in accord with the recommendation of the Committee on Projects, has approved a grant of Eleven Hundred Dollars, in support of the request presented by you, known on our records at Project No. 143, covering continuation of investigation of the Permian rocks of trans-Pecos Texas. The full amount requested by you has been authorized.

This advance notice carries a request for certain specific information which we shall incorporate in an agreement of contract to be prepared as promptly as practicable. In the meantime, of course, you are justified in proceeding with your plans.

Sincerely yours,  
Charles P. Berkey  
Secretary

October 31, 1933

Memorandum to the Chief Geologist:

You will see by the enclosed letter from Dr. Berkey that my application to the Geological Society of America for \$1,100 to continue field work on the Permian of west Texas has been approved. In case you wish to refer to it, I am also enclosing a copy of my application. The funds provide for field work of five months in the Guadalupe Mountains, as well as for some items in the preparation of a report, and for the expenses of a paleontologist to help in the collecting for one month. I think the sum is adequate for doing a very good job in the region.

As you know, the Guadalupe Mountains are one of the most important districts in west Texas for a study of the Permian, and a detailed knowledge of the region will throw much light on the stratigraphic relations, distribution of faunas, and principles of sedimentation in the entire region. The plan for working there strongly appeals to me personally, but it will also be of much value to the Geological Survey, if the Survey still wishes to continue its research on the Permian problem.

The funds from the Geological Society will cover a large part of the project, but the encouragement of the Survey would also be needed in several respects. I am, of course, fully aware of the uncertainties now existing in the Geologic Branch, and the difficulties of making any promises or plans for the future. It would, however, be desirable to outline the help needed from the Survey, and to get an opinion from you as to whether such would be possible, if the circumstances were favorable.

The matters which I would like to bring up are: (1) Salary: The field plans call for five months of field work, or two months longer than the estimated furlough time; could my salary be paid while I am in the field, for the time over and above furlough time? I plan to leave in February, and if necessary could finish the work before the end of the fiscal year. If circumstances would permit, however, I might wish to stay on during a few weeks or months of the following fiscal year. (2) Writing of the report: After the field work is done, I would like, during the following fiscal year, to spend part of my time here in Washington writing the report. This does not need to be decided definitely until the time arrives, and

it is possible that I might be able to work on it some outside of office hours, if other work was urgent. (3) Equipment: For the work, I would certainly need a Brunton compass, and possibly other instruments, and it would be desirable for me to borrow them from the Survey. (4) Automobile: If possible, I would like to use a Survey truck. If necessary, however, I could use my own car, but the paint, upholstery, and possibly other things might come out the worse for the wear, whereas a truck adapted for field work would be little damaged. If a Survey automobile were used, I would return it in good condition, since provision is made in the fund for repair work.

As I have said, I am very much aware of the present uncertainties in the Geologic Branch. The project is one in which I have a great personal interest, and one which, I think, will further the Survey's research work on geology. At the same time, I realize that I must be ready to do those things which are for the greatest good of the organization, and I will hold myself in readiness to perform any work which you desire.

Respectfully yours,

Philip B. King

From Geological Society of America Bulletin, v. 76, no. 12, p. P-223-231

PRESENTATION OF THE 1965 PENROSE MEDAL TO PHILIP B. KING

Citation by J. Hoover Mackin:

Mr. President, Ladies, and Gentlemen:

It is my pleasure to present Philip King for the Penrose Medal -- a token of recognition by the Society of "outstanding original contributions or achievements which mark a decided advance in the science of geology."

Because I have regarded his work as a standard of excellence since my student days, and because I'm now pretty well along, it has seemed to me that this honor is long overdue. Actually, at 62, Phil is a youngster in the distinguished company of Penrose Medalists. A glance at his record indicates that he has been on the precocious side throughout his career. His first paper on the Glass Mountains in west Texas was published when he was 23 years old; he was just 28 when the comprehensive Glass Mountains report appeared. His reputation as a top-notch field geologist was established by his Marathon Basin paper was published when he was 34. We commonly think of regional syntheses as the province of older men, but Phil was only 30 years old when he prepared his "Outline of the structural geology of the United States," for the 16th International Geological Congress. He has continued at the same pace. The esteem of his peers is indicated by two other high honors that have come to him recently -- the Distinguished Service Award of our Interior Department, and the Lomonosov Medal of the University of Moscow.

Generations of University of Texas field course students have swarmed over the map of the Marathon Basin, but their instructors tell me that these eagerbeavers can change none of Phil's lines. I have used the map in class for years, and have recently had the good experience of walking over the ground with it in hand. The geological patterns are graceful because they portray the structure, not only where the contacts are exposed -- this is merely a matter of accuracy of location --but where they are concealed; they are drawn by a sensitive hand controlled by a sort of reverent understanding of the meaning of the lines, in the tradition of our Geological Survey and

the Surveys of the British Isles. The understanding that gives life to the map patterns starts with the mechanisms of origin and transportation of the sediments and the physical and biological environments of deposition, continues through the diagenetic changes that transform the sediments into layered rocks and the response of the heterogeneous sequence of strata to constantly changing stress fields at different depths in the crust during successive periods of regional deformation, and through the erosional power that finally lay the contorted strata open to the sky. If the geology is to be expressed by the work, these physical, chemical, biologic, and geometric relations must be seen in historical perspective by one man, on the outcrop, as the lines are drawn.

This is what I mean when I say that Phil King is a field geologist. We are all aware that geology is now in the midst of a great swing to the quantitative, and that the progress of the science depends increasingly on electronic gear that makes possible measurement of parameters that were beyond our reach, or not even known, a generation ago. But these new capabilities in the acquisition and processing of data, marvelous as they are, surely do not justify deprecatory references to mapping and field work generally as "old-fashioned descriptive geology." The attitude implied by this catch phrase, which I have heard three times in the last month by people in influential positions in the Earth Sciences, is as poorly balanced as the attitude that regards the new methods as inconsequential frills. It will take many different approaches and habits of thinking, and close and cordial co-operation between all Earth Scientists, if we are to solve such durable problems -- if I may borrow a word from Francis Birch -- as the mechanisms and causes of crustal movements. Geologic maps provide data that are basic to this and nearly all other major problems of the Earth. Mapping by the King breed of field geologists, will be just as essential, 100 years hence as it is now, to the advancement of our understanding of the origin and evolution of this planet, and its neighbors in space.

Phil moves easily from scales of inch to the mile or better to continental scales. His early "Outline of the Geology of the United States" mentioned a moment ago, was followed in later years by two books "Tectonics of Middle North America," and "Evolution of North America" -- which are known the world over as models of regional synthesis. Tectonic theorizing is a heady wine which, especially if it is spiked with some well-chosen numbers, tends to be habit-forming; the addict prefers to have both his feet off the ground and never permits discordant facts to limit the sweep of his generalizations. Phil's imagination is as active as any other tectonist, but is disciplined by a knowledge of the literature and first-hand observation equaled by few people. Characteristically, he shifts from the broad brush in analysis of regional relations to matters of fine-line detail as editor and compiler of geologic and tectonic maps of the United States and North America; each of these unlike skills feeds on the other.

Phil is much more concerned with geology than with geologists -- he prefers rocks to most people. He would be a total loss as an administrator, and he couldn't care less about committee work; even his stay in Texas failed to bring about a talent for politics. Some of his colleagues think that his insistence in doing his work and nothing else is downright unsocial. But people who know him well know that beneath that crusty shell he is as shy, and generous, and humble as the man to whom I now present for the Penrose Medal. Mr. President, may I present to you Philip B. King, recipient of the 1965 Penrose Medal.

Response by Philip B. King, Penrose Medalist

Mr. President, Ladies and Gentlemen:

When I was planning the remarks I was to make this evening, I intended to preface these remarks by telling you of my surprise and astonishment that this highest honor of the geological profession should be bestowed on me, and my deep humility at receiving this honor. Then I decided to look up what the previous recipients of the medal had said during the last 10 or 15 years, and they all said that! I guess we'd better forget that part. Nevertheless, I would like to express my gratitude to the Geological Society of America and to all of you here for this most heartening display of your confidence in me and my work.

This award has brought back many memories of earlier times. Many previous Penrose Medalists have been prone to reminisce, and, heaven help me, so am I. I hope you will bear me out while I tell my story.

These earlier times were not all "peaches and cream" for me. Some of the times were happy and exciting, others were discouraging and frustrating. But running as a bright thread through these memories of happy and discouraging times are memories of many older geologists who offered this younger geologist a helping hand, very often at times when his discouragement was greatest. I'd like to tell you about a few of these.

In the early Twenties I entered the University of Iowa at Iowa City and during my freshman year took the beginning course in geology. For several succeeding years I could not pursue geology further, because there were irreconcilable conflicts between courses that it was very necessary for me to take and courses that I would have wished to have taken.

Even before I entered the university I was reading many books on travel and exploration; I guess it was an escape from the humdrum Iowa world in which I lived. I continued this reading during spare hours of my university years. Many of the books that I wished to see were in the library of the Geology Department, and I spent much time there, browsing among the shelves.

One day, Chester K. Wentworth, on the staff of the Geology Department, came through the library. He said, "Who are you? I've seen you around here a lot." After we had introduced ourselves, he continued, "I'm giving a field course at Baraboo, Wisconsin, this summer. Why don't you come along?"

I did go along, and so had my first introduction into field geology. Wentworth proved to be a hard taskmaster. In field camp he made us wash our own clothes, and when the camp was moved 15 or 20 miles we had to walk, continuing our geological traverses as we went. He was also rigorous in prescribing our methods of geologic work. We must keep clear, meticulous notes, from day to day and from place to place, recording them with a hard pencil, carefully sharpened. During my subsequent years as a geologist I retrogressed into many slip-shod methods, but as time went on I worked back into more precise methods by trial and error -- into methods that were very nearly those which Wentworth had prescribed to us in the first place.

Unforgettable was my first day in the field at Baraboo. Wentworth marked on a topographic map the route my partner and I were to follow -- a road in the Baraboo Basin that went up a grade, at the crest of which we were to diverge through the woods and climb a high hill. When we returned in the evening he asked us what we had seen. "Well, Mr. Wentworth, we looked at everything along the road and up the hill, and all we could see was just a lot of sandstone." Wentworth looked disgusted. Much later I learned that we had started near the base of the Cambrian and had ended in the St. Peter Sandstone, the middle of the Ordovician, thus crossing the entire Paleozoic sequence in this part of Wisconsin. Much of this sequence consisted of various kinds of sandstone, to be sure, but it also included several significant marker beds of limestone and shale. When the Baraboo field course ended, I was not doing much better; I was by no means a field geologist and had merely been exposed to field geology.

We'll skip a few years and pass on to the mid-Twenties, when I arrived in Austin as a young instructor in the Geology Department of the University of Texas. At that time the department had a visiting professorship filled from term to term by various distinguished geologists from different parts of the country. When I arrived, the visiting professor was Charles Schuchert who had retired from Yale University some years before.

The previous summer, my brother Robert and I had done field work and fossil collecting in the Glass Mountains in the western part of the state. It was our plan to write the results of this work as a couple of master's theses at some Midwestern university, then go on to oil company jobs or to college teaching in the same region. There had been much talk among the Texas geologists about the Glass Mountains since J. A. Udden and Emil Böse had made a reconnaissance there about 10 years before. Many geologists went to the mountains and looked around, but nobody was doing much about the area. Geologic work wasn't being planned and staked out in those days the way it is now. The Glass Mountains were a geological "open range" so we just moved in and started to work.

It turned out that Schuchert, the visiting professor, was also interested in the Glass Mountains. He had stopped there a few years before on his way to Arizona and had spent enough time in the mountains to convince himself that they held the key to the stratigraphy of the marine Permian sequence of North America. The main reason he had accepted the visiting professorship at Austin was on the chance that he could find a young geologist who would make a survey of the Glass Mountains.

Incredible things began to happen. Schuchert decreed that our work in the Glass Mountains must continue. Schuchert arranged with Dr. Sellards for the Bureau of Economic Geology to allot a modest sum for our field expenses, and he himself agreed to buy all of Robert's fossil collections for the Peabody Museum of Yale University. We were no longer to plan for obscure master's degrees at some Midwestern university, but were both to go to Yale as graduate students, where we would work for doctor's degrees -- our dissertations to be on the geology and paleontology of the Glass Mountains.

Dear Professor Schuchert and his faith in young geologists! At the time he was arranging all this he scarcely knew me, and he knew Robert not at all. He was buying a "pig in a poke" -- two "pigs" in fact. I hope that in later years he felt that he had been justified in the inspired decisions he made about us.

The one-term visiting professorship brought other notable people to Austin. A few terms later it brought William Morris Davis, at that time retired from Harvard and lecturing in the West at different colleges and universities -- a self-styled "geographer" whose great work was actually in the field which we now call "geomorphology."

Davis was then at the height of his attainments. He had evolved his system of "geography," by which every landform could be classified on the basis of "structure, process, and stage," a magic formula which seemed to unlock all the problems of surface geology. I enthusiastically attended his lectures as an auditor, knowing that I was at the feet of an inspiring lecturer and a truly great man. Davis's inspiration made converts of us all, and I still use his "geographical" system in my later geological work. I refuse to go along with the later generation of detractors who deride his system and who want to toss it out of the window entirely. Nevertheless, with the greater knowledge of our times, I am quite aware that parts of the system are much too naive and oversimplified.

Quite aside from this, I was inspired by Davis's exquisite rendering of landforms by pen or pencil, even by chalk on the blackboard. When I had been at the University of Iowa I took as many courses in art as I took in geology and the other sciences, and on graduation I had even thought of becoming a professional artist. But this seemed to be a chancy undertaking, with no assurance that it would be much of a mealticket. My first opportunity for a paying job was in geology, and a geologist I became. Yet I regretted that I possessed an ability now gone to waste; Davis's own skill in landform drawing showed the means by which I could use this ability in my geological work.

Even more importantly, Davis demonstrated to us the virtues of the old-style qualitative methods of geology -- how to arrive at valid scientific conclusions by processes of rationalization and deduction. At that time, as now, inroads were being made into conventional geology by quantitative methods borrowed from mathematics, physics, and chemistry. These were all very worthwhile, to be sure, but I had no talent for them whatsoever. Davis demonstrated to me that, for a few decades anyway, I could still do useful geologic work by means of qualitative, rationalistic methods.

Another geologist of the Austin days was Walter Scott Adkins, paleontologist and stratigrapher of the Bureau of Economic Geology. When I arrived in Austin, Adkins was in Europe, studying the Mesozoic under the guidance of Spath and other immortals. I met Adkins later in the Bureau office in the basement of "B Hall", one of the more dilapidated university buildings that has long since disappeared from the scene. He was emerging from his office, a strange unkempt figure, with a cigar in one hand and an ammonite in the other, with shoelaces untied or missing (he always considered shoelaces as a bourgeois impediment to clear thinking). He said gruffly, "I can't make head or tail of this beast. Tell me what you think about it!" This was a queer kind of request to make of a young geologist who still had difficulty in telling an ammonite from something else, much less one ammonite from another. But it did illustrate the deep humility with which Adkins always looked upon scientific matters.

As time passed, Adkins and I had many fruitful sessions together -- in his office talking about a wide range of geological problems, or in the field examining the marvelously fossiliferous Mesozoic rocks of Texas.

In Iowa, where I learned geology, stratigraphy was very provincial, and little thought was given to its world-wide significance. Such arcane terms as Portlandian, Cenomanian, and Bajocian that had evolved in Europe had little meaning to us, and might just as well have been words in Swahili or some other barbaric language. But under Adkins' guidance these barbaric terms, as well as the stratigraphic stages they represented, and the fossil zones that compose them, took on real meaning. I began to grasp how they transcended the provincial stratigraphic concepts of Middlewestern United States.

During our second or third summer in the Glass Mountains, Robert and I received a telephone call from nearby Alpine, Texas, from a very pleasant lady who said she was Mrs. Darton and who told us that Nelson Horatio Darton would very much like for us to visit him. Darton was one of the staff of the U.S. Geological Survey, and was then working on the big new geological map of Texas, compiling the part which lay west of the Gulf Coastal Plain.

Darton was a legendary figure, who had joined the staff of the U.S. Geological Survey only 2 years after it was founded. We knew of his geological work in the Black Hills, and over enormous areas elsewhere throughout the West. I'll confess that we had chafed at some of his rather crochety interpretations that we had found in his publications, but we really didn't understand the circumstances under which he had made them. This aside, we knew it would be most interesting to meet the man himself, so we drove to Alpine at the first opportunity.

We found Darton plotting his field results in the bedroom of a country hotel -- a lean aristocrat, bronzed by the sun, wearing a white shirt and the famous aritst's tie that was his lifelong trademark. He was assembling the geology of a 30-minute quadrangle, mostly from his own observations, and marvel of marvels, a topographic base map of the same area at the same time! From what we had seen of the area on the ground, both base map and geology rang true to us.

The west Texas work of both Darton and ourselves continued through several more field seasons, and we had many pleasant visits with him and Mrs. Darton in the various little country towns. Occasionally, too, we made field excursions together. Unforgettable was one into the Franklin Mountains north of El Paso on an August day. Darton wished to resolve the stratigraphy of the western slope of the mountains, where the rocks had heretofore been mapped as the vast indivisible "Hueco Limestone", all of Pennsylvanian age.

We drove the car to the end of a dirt track and continued into the mountains on foot. Robert and I ran hither and yon, like two fox terriers chasing rabbits, chipping the rocks, picking up a fossil here and there. But it looked as though poor Darton wasn't going to make it. The day was fearfully hot, and the sun beat down unmercifully. Darton set off so slowly that he seemed exhausted at the start. Now and then we ran back to offer him some canteen water or ask if we could help, but he refused.

Time passed, and Darton continued at the same steady pace. We had drunk all our canteen water, and by the time we reached the far end of our traverse, well up on the mountainside, we were thoroughly pooped. Darton revived us somewhat by giving us the cans of fruit juice which he had brought along for himself. In the evening, after we had staggered back to the car, he took us to one of the little Rio

Grande valley towns and bought each of us an enormous memorable milk shake. We had had an embarrassing lesson about the way a great field geologist conserves his energies in the field.

Despite these misadventures, our little expedition produced significant results. The three of us were able to prove that the supposed indivisible Hueco Limestone of the Franklin Mountains was composite -- that it contained units not only of Pennsylvanian age, but also of Devonian, Mississippian, and Permian ages.

I'll skip a few years when I was a graduate student at Yale and an instructor at the University of Arizona, and will pass on to the early Thirties when I arrived in Washington as a young Survey geologist. These were the years when the Great Depression which so affected all our lives began, but this doesn't much concern the present story.

Aside from its regular duties, the staff of the Geological Survey was deeply involved in preparing for the 16th International Congress, which was to be held in the United States in 1933. Harry G. Ferguson (or Fergie, as we all call him) was Assistant Secretary and one of his tasks was to plan the field trips which were to be made all over the United States by delegates to the Congress. Also, plans had to be made for a series of guidebooks which were to explain the field trips.

One day, as an outgrowth of my Texas field work, I was doodling at my desk on some of the regional implications of this work, marking on a map structural trend lines which sprawled in various directions across Texas and Oklahoma. Fergie happened in on another matter. Seeing the map, he said, "We have been needing some kind of a tectonic sketch to illustrate our Midcontinent guidebook. Would you polish up this map so that we could use it?"

Then, a further thought, "I'm looking for a general text describing the structural geology of the United States to be published with the guidebooks. I've thought of various authorities who might write it -- Lawson, Chamberlin, Daly, and others -- but many of them are too busy with other Congress matters, and I doubt if some of them could put together just what we want. Why don't you do the job for us?"

Dear Fergie! Again an older geologist was inspired to "buy a pig in a poke," hardly knowing the qualifications of his young candidate. I was breathless at his dazzling proposal, yet I was confident that I could do the job -- a confidence which surely must not have been shared by my older and wiser colleagues. I'll say in my defense that I had made a hobby of regional geology ever since I had heard Arthus Keith lecture on the subject in Austin a few years before, and that my interest in this hobby had grown while I was studying stratigraphy and structural geology under Carl Dunbar and Chester Longwell at Yale.

Fortunately in Washington in those days, the complete staff of the Geologic Division of the Survey was on the fourth floor of the old Interior Building, so I had a wide range of talent to draw on. Merely by walking down the hall, I could obtain advice from experienced geologists about almost any part of the country. Despite this wide range of talent, much of the work of these geologists was on their local areas, and they were not giving much thought as to how these areas fitted together.

My quick survey of the whole United States brought out many gross features -- the central stable region, the bordering mountain belts, and the geosynclines from which they arose. Many of these features were already well known, but I could also sense other features that were as yet little appreciated. Many of these features were more fully documented later and given formal names, such as "eugeosyncline" and "miogeosyncline."

Somehow, for better or for worse, I was able to put together a manuscript and map on "An outline of the structural geology of the United States" in time to meet Fergie's deadline for publication as one of the Congress guidebooks. Through Fergie's encouragement, what had hitherto been a rather incidental hobby became a formal part of my professional work -- the synthesizing of the geology of large regions, or "tectonics" as we have come to call it later -- and so it has remained with me to this day.

We'll leave the story here, and I'll give my salute to the many older geologists (including many I couldn't mention here), who have given their help to this younger geologist at needful times, and who often set him off into paths that he had not even anticipated. May their tribe continue and may it increase, to the benefit of future generations of young geologists!

Now I wonder why I am reminiscing, for the things I have talked about don't seem very long ago. I can't picture myself as an old codger, telling the younger generation about a world utterly different from their own. The nearly half a century since the close of the First World War has wrought momentous changes, yet the fabric of our society and the geological profession which must operate within that society have not changed very much. Then, as now, young people went to college, obtained a succession of degrees, then went into some kind of a profession in which they hoped to make a name for themselves in the world.

It is true, of course, that our ways of looking at things are now more sophisticated. For one thing, we have vastly more geological information, thanks to the eager investigations of a horde of younger geologists. We have many more geological maps, and many more data about many parts of the earth, and these data confirm or deny many long-cherished geological theories. And there have been great changes in technology. Thus, the old-time company geologist with his plane-table, searching for surface structures, is as extinct as the dodo, and has to be replaced by a different breed. By geophysical means we are now probing the deeper layers of the crust, determining the geology of the ocean bottoms, and obtaining isotopic ages for hitherto undatable rocks.

But the old challenges remain. The work of the field geologist continues, because there are areas still unstudied, or areas that need new study. And the tug-of-war between the qualitative and quantitative methods for the study of the earth will be with us always. The vast contributions that have been made to an understanding of the earth by quantitative methods are part of the story of the earth, but they are not the whole story. Despite the ebullient assertions to the contrary, our ambiguous earth has too many variables for it ever to be generalized into a few algebraic equations.

I offer my respects to both the old-style qualitative methods of study of the earth, and the new-style quantitative methods. My only personal regret is that I cannot understand and appraise the one as well as I can the other. The future lies with the geologist who can comprehend both, and who can combine the many threads from both into some grand and convincing synthesis. May there be many future Penrose medalists with these attainments!

From Modern Men of Science, second ed., McGraw-Hill, 1970?

(Written by P. B. King)

King, Philip Burke American geologist, Born Sept. 24, 1903,  
Richmond, Ind., U.S.A.

King's interests and methods of scientific inquiry were those of a field geologist, who obtains his data from the rocks exposed at the Earth's surface and from them derives inferences as to the history and behavior of the Earth. He was especially interested in the sedimentary rocks, and interpreted the environments in which they were originally laid down and by relation of these environments to tectonic movements were taking place in the Earth's crust.

King did his early field work (1925) in the Marathon region, an area of about 1,600 square miles in the trans-Pecos part of Texas, where varied rocks and structures that were formed during Paleozoic time have been stripped of the cover of younger strata that conceal them elsewhere in this part of the Southwest. Because of the semiarid climate the Paleozoic rocks are well exposed, making the Marathon region a virtual outdoor laboratory for the investigator.

His first field work in the Marathon region was on Permian marine strata that form a sequence about 7,000 feet thick on its northern side in the Glass Mountains. In this work he was greatly assisted by his brother and collaborator, Robert E. King, who at that time was investigating the Permian fossils, many of which were new to science. The Permian strata, they discovered, were quite differently arranged from those with which they had hitherto been familiar. Instead of an orderly sequence, whose component strata extended laterally for great distances, those of the Glass Mountains were a disorderly array of discontinuous bodies of carbonate rocks, shale, and sandstone, interspersed with lenses of gravelly conglomerate. As the work progressed, it became apparent that this array had been built up on the southern margin of a former marine basin. The conglomerate clasts had been eroded from highlands farther south, the carbonate bodies had formed as reefs or banks in well-aerated waters on the edge of the basin, and the shales and sandstones were the deposits of the basin floor. Nevertheless, at the close of their investigation of the Glass Mountains, many features of the rocks were still imperfectly interpreted; some of these features are little understood even today.

An opportunity to clarify the Permian stratigraphy of western Texas came later (1934), when King began work in the southern Guadalupe Mountains about 150 miles northwest of the Glass Mountains. There are about 4,000 feet of Permian strata in the southern Guadalupe, much less disturbed by later movements than in the Glass Mountains and much more boldly exposed on steep mountain slopes and canyon walls, permitting three-dimensional reconstructions. Many features of the Guadalupe Mountains had by that time been described by a succession of geologists, but they had been puzzled by a disorderly arrangement of the strata that was much like that in the Glass Mountains. Drilling for oil to the east had demonstrated that the Guadalupe Mountains, like the Glass Mountains, lay on the margin of the former large marine basin referred to, but in this case on its northwestern rather than its southern side.

During King's field work much attention was given to the Capitan Limestone, which stands in lofty white cliffs at the summit of the mountains. The Capitan had already been interpreted as a barrier-reef deposit by other geologists, and many observations were made by King on its details -- the marine organisms that built up the growing parts of its summit, the talus and slide debris on its basinward flank, and the manner in which the reef rocks intertongue toward the basin with very different deep-water deposits. Because of the steep mountainous relief, it was also possible to observe the structure of the strata beneath the Capitan and thus to determine its origin. During earlier parts of Permian time crustal movements downflexed the strata toward the basin, producing marked contrasts in depths of water, and reef deposits began to grow in shallower parts at the upper edge of the flexure. Once initiated by crustal forces, the reef became self-perpetuating, until the reef-building organisms were destroyed by an increase in the salinity of the waters of the basin; during the last part of Permian time the reef was buried by evaporite deposits.

Before working in the Guadalupe Mountains, however, King studied another aspect of sedimentary rocks and their structures -- an interaction between sedimentation and mountain building. The southern part of the Marathon region is formed of Paleozoic rocks older than Permian and had undergone several periods of mountain building before Permian time. Erosion of these mountains produced the clasts that formed the conglomerates in the Glass Mountains. Field work in 1929-31 revealed a sequence extending from the Cambrian to the Pennsylvanian, laid

down in a geosyncline. The older geosynclinal deposits (Cambrian, Ordovician, and Devonian) were laid down in deep water under quiet conditions -- thin deposits of shales, muddy limestones, and many siliceous beds (chert and novaculite). A dramatic change took place in Mississippian time, when clastic deposits (flysch) succeeded the novaculite and eventually filled the geosyncline to a thickness of 10,000 to 15,000 feet. Microscopic study of the flysch deposits showed that they were derived from heterogeneous sources -- from granites and schists of the basement and from older Paleozoic deposits -- and that the flysch was related to the initial orogenic pulses of the geosyncline, when tectonic ridges were raised and adjoining troughs were deeply depressed. High in the flysch sequence King discovered dramatic evidence of these early orogenic pulses -- beds of giant boulders composed of earlier geosynclinal deposits, shelf carbonates, and Precambrian basement. These beds were derived from sources that had been sharply uplifted and from which the boulders had broken off and slid into the deep troughs.

Field work also revealed the structures of the geosynclinal deposits -- a succession of folds and faults trending northeastward across the prevailing but much younger Cordilleran grain; these were a small-scale replica of the well-known "Appalachian structure" of the eastern states. The resemblance suggested, moreover, that these structures were a far-flung western prolongation of the Appalachian mountain system. Many details of the history of these structures could be deduced, beginning with low-angle thrusts that were later overwhelmed by folding and ending with the low-angle thrusts farthest northwest that had formed after the time of folding. These structural events could be correlated with the record of sedimentary strata that were being laid down in the region in the later part of the geosynclinal cycle.

In 1940-44 the wartime search for strategic minerals by the U.S. Geological Survey afforded King an opportunity to investigate the Appalachian Mountains in Virginia and Tennessee. The insights gained in the Marathon region made it possible to unravel and interpret the much larger and more massive folds and low-angle thrusts of that region.

Throughout these investigations of sedimentary rocks and their structures, he was aware of their broader significance and developed many regional syntheses, resulting in publications of a general nature -- on the Permian stratigraphy of the Southwest, on the Appalachian structure of the Southeast, and the like. Even broader in scope was Evolution of North America (1959), which evolved from a series of lectures. These larger concepts could also be represented on maps, so he made important contributions to the Tectonic Map of the United States (1944; 2nd ed. 1962) and later compiled the Tectonic Map of North America (1968).

King's early years were spent largely in Iowa, where he graduated from the State University (B.A., 1924). His father, Irving King, was a professor of psychology and education, and provided his three sons with an environment of scientific and scholarly inquiry. After his graduation King went to Texas with an oil company, and was assigned to the Permian Basin region of the western part of the state (later to become one great oil provinces of the nation). Then he continued his studies at Yale University (Ph.D., 1929). Most of his career from 1930 onward was spent as a geologist with the U.S. Geological Survey, although he taught at universities for short periods (Texas, 1925-27; Arizona, 1929-30; University of California at Los Angeles, 1954-56). In the autumn of 1965 he was a visiting lecturer at the University of Moscow. In 1965 he was awarded the Penrose Medal of the Geological Society of America and the Distinguished Service Medal of the U.S. Department of the Interior. He was elected to the American Academy of Arts and Sciences in 1966.

For background information see Geosyncline; Stratigraphy; Tectonic patterns in the McGraw-Hill Encyclopedia of Science and Technology.

San Angelo Standard-Times -- February, 1968

W T GEOLOGICAL PIONEER -- P. B. KING FILLED MAP BLANKS

By Jean Gillette

Science Writer

MIDLAND -- Dr. Philip B. King was a West Texas pioneer.

He did not establish any ranches or towns. He did not even find any oil wells or blaze any new roads.

Bachelor's degree in geology in hand, King came to Texas with a Marland Oil Co. scout party in 1924. The party was assigned to map Cretaceous outcrops on King Mountain in Upton County and surrounding country.

"Everyone was sure there was oil in this vast basin," King recalls, "after Big Lake came in and a little oil field near Colorado City at Westbrook."

"So they did the only thing they knew to do and sent out surface crews to map the Cretaceous. But the Permian underneath was entirely different."

The first West Texas production was from Permian age rock formations. In spite of this, the Marland crew managed to locate a structure thought favorable for oil entrapment on the flanks of King Mountain.

King recalled how one of the scout party members stood on the mountain, looking over the flats where McCamey stands today, and predicted there would be a big town and 1,000 oil wells.

Later, Marland and other firms brought in an oil field in the area which started the boom at McCamey.

But King grew restless.

"I was never cut out to be an oil geologist", he said. "I could see the mountains (Glass Mountains) where nobody had done much. They just drew me."

By this time King's brother, Robert, had completed work on his geological degree. Robert King wanted to come out and "do some paleontology."

Philip King, unmarried, and with three-fourths his year's pay saved, quit Marland and headed for the beckoning mountains.

"We just went over and started in."

The pair financed themselves out of their own pockets the first summer. Soon, others got interested in their work with one noted Eastern geology professor supplying the young geologists with \$300 out of his own pocket. The Bureau of Economic Geology added \$400 to the kitty.

Doing field work in the summers in the Glass Mountains and attending graduate lectures at Yale University during school term the King brothers completed doctoral degrees and a major treatise on the geology of the Glass Mountains.

The pair found some unexpected things in the mountain range that were to have profound influence on West Texas geology and Permian basin oil development.

At several points in the mountains they turned up "curious limestone slugs" which one of their advisors at Yale suggested might be ancient reefs.

"I didn't believe it," King said, "But nobody knew much about reefs. The more I looked at samples of the Alps in Europe, the more I wondered. But it was an outrageous idea. Then suddenly somebody else thought so too. Pretty soon about everybody believed it."

The reefs have played a significant role in Permian Basin oil development.

Later King was to work in the Guadalupe Mountains on the Texas-New Mexico border where the Permian reefs have their greatest exposure. He pointed out the reefs in the Glass Mountains were too confusing in their exposure.

Eventually, King ended up with the U.S. Geological Survey. His brother, who decided after looking at thousands of Permian brachiopods he did not want to be a paleontologist, went to work for a New York oil firm where he is still employed.

"At the time I went with the Survey, there were a lot who thought bright young geologists should be sent to new country to broaden their knowledge," King said, "But at this time they were just getting started on the geological map of Texas and needed somebody who knew something about West Texas."

King was sent back to his old stamping grounds.

"The Marathon folds were an irresistible attraction." They were also a blank spot on the Texas geologic map.

King recalled the pleasure of early day geologic work in West Texas. He had a Model A pickup which he drove in his Marathon Basin work.

"You could drive along a road that was barely a track. If you saw something off across the country you wanted to look at you just took off for it. If you came to a fence which in those days was never more than a few posts and a couple of stands of barbed wire, you pulled up a post and drove over the wire."

"Now I have to come around with this access problem -- all the locked gates and wolf-proof fences. The country was pretty empty and there weren't all these cow-stealing varmints around."

"If anyone saw you, they'd ask what you were doing. You told them, 'Looking at the rocks.' They nodded and would say, 'Okay, just close the gate.'"

King spent 15 years working in the summers doing field work in Texas and returning to Washington, D.C., to write up his results and draw maps.

"But I was always thinking about Texas. Then summer would come and I could get out in the sun and get some of the winter fat baked off."

By 1942 King had completed the major part of his work in Texas with the publication by the American Association of Petroleum Geologists of his paper on the Permian in West Texas and southeastern New Mexico.

His researches had carried him from the Glass Mountains to the nearby Marathon area, to Van Horn where he worked some with Peter T. Flawn now head of the Texas Bureau of Economic Geology, to the Guadalupe.

"For some time I had been feeling the urge to see something new. Along came World War II. The Survey got on a war footing even before the United States got into the war."

King was assigned to a group searching for strategic minerals and his new area was the Appalachians where he was to look for manganese.

"That was an awful change from desert country to forests. There was a different kind of weathering of the rocks, different outcrops."

Appalachian structures were not new to King. As he observes, he entered the study of the Appalachians through the back door on the far southwestern end. This was the Marathon folded belt which he had unraveled 10 years previously. He spent 14 years in the Appalachians.

More recently King has lived in California where he has taught at the University of California at Los Angeles, and has been involved in supervisory work for the Survey.

"I just try to be useful to people who have problems with their work," he said. "People are always asking me that my title is, thinking, I guess, that it ought to be something fancy, but I'm just a geologist."

Just a geologist who filled some important gaps and helped lay the foundations of geology in West Texas, who has completed and sent to the printer after five years of work a tectonic map of North America and who is at 64 starting to redraw the geologic map of the United States for the U.S. Geological Survey.

The present map available from the Survey is 30 years out of date.

King was in Midland last week to speak to the West Texas Geological Society on the "Tectonics of Eastern North America," and to visit with many of his friends among the Permian Basin geologists. Still looking for virgin country he and a Midland geologist head for Chihuahua over the weekend to look at some interesting country the latter found.

This is a pioneer.

PRAGUE -- AUGUST 18 TO AUGUST 24

To the reader.--This is my personal record of our eventful week at the 23rd International Geological Congress in Prague, Czechoslovakia. The narrative begins in Kosice at the eastern end of Slovakia at the close of our ten-day pre-Congress excursion, includes the first few peaceful days in Prague, then the Soviet takeover, and ends with my evacuation to Nuernberg, West Germany.

It is based on the records which I kept as the events progressed, but of necessity it cannot be a verbatim copy. First of all, I have had to decipher the nearly illegible scrawls in my notebooks, some put down under much duress. Then, too, I have had to edit and reorganize an otherwise incoherent account, shifting items which were out of context, adding explanatory words and phrases about things otherwise unintelligible, and beef up from memory some points not adequately covered. In a few places I have added in parentheses my retrospections and later information, if they materially affect the story.

Aside from this, I have tried to keep it a true, complete, and factual account, free from embellishments, interpretations, and hearsay information. Because it is a complete account it includes matters which may not interest every reader -- notes on committee and commission meetings, and much personal piffle (how I felt, what I drank, the girls I hugged, and the like). If these bore the reader, pass on to the next page where something more interesting happens.

Finally, try not to be offended because I have omitted all the numerous accent marks on the Czech and German words!

Philip B. King, September 5, 1968

1 Sunday, August 18. Kosice to Prague

2 Up at 5:30 A.M. The time is all wrong in Kosice, as it is in the  
3 same time zone as Prague and Vienna; it has been daylight for hours. I  
4 decide to skip a shave and just pack. Bring my bags down in two trips.  
Everyone else is up, and we have our breakfast at 6:00. It is pouring  
rain, and it is good to know that this is not a field day.

5 The obnoxious French quartet of our field trip have disappeared  
6 and I hope we do not see them again. The other French quartet are still  
here, and must drive their car in the rain to Prague; wish I had known  
them better.

7 An airport bus arrives; we are off in the rain. A wait at the  
8 airport. This is a scheduled flight, and many locals are going too.  
The plane is a small job, about the size of a Convair, but we all get  
9 good seats. Thought maybe we would get some coffee en route, but the  
stewardess has disappeared; evidently she just put us onto the plane,  
10 then stayed in Kosice. Ride with Knut Bjørlykke, a really nice young  
Norwegian. Tells me his father and his grandfather were both geolo-  
11 gists, not to speak of his brother and his wife!

12 Cloud cover part of the way, clearing later; there is a profile  
of the High Tatras, and glimpses of Slovakia and Bohemia. I have no  
13 sense of position, although I try to follow the road map, but the young  
East German in the seat ahead (Karl-Bernhardt Jubitz) spots our posi-  
14 tions amazingly. Finally we are over Prague and I get glimpses of the  
city through the opposite windows. Flying time, 8:00 to 10:30 A.M.

15 A fine new airport, a great credit to the city; quite as shiny  
16 as Orly at Paris. I learn later it has only been operative for a month.  
Quite a contrast to the makeshift building where I landed in 1962.  
17 Congress representatives meet us, and take us to our own bus.

18 On the way to the city, I explain the local geology to my friends  
19 who have not been here -- the Cretaceous peneplain on which the airport  
stands, stretching far to the north with monadnocks in the distance,  
outcrops of basal Cretaceous sands, outcrops of turned up Paleozoic  
20 farther down the slope. Mr. Corps asks, "Is the Cretaceous thrust over  
the Paleozoic?" I say, "You've been brainwashed by the Carpathians,  
21 we are back into rational William Smith-style geology, like in England!"

22 The bus drops a big party at the Vetrnik Student Hostel west of  
the city, and a few more at the International Hotel; the International  
23 has lost its high mast with red star atop, and looks like any other  
big hotel now. The bus then doubles back to the Congress center at the  
24 Technical University, 6 blocks south, and drops the rest of us to fend  
for ourselves.

1 The Technical University is on the Square of the Revolution, which  
2 I remembered from 1962 as very bleak, with sad buildings and torn up  
3 earth. The University buildings are spanking new since then. They  
4 are severely functional, but with much glass and facings of stucco,  
5 and with large panels of purple mosaic. Huge signs and banners pro-  
6 claim the Congress. We go in and register, pick up pins, papers, and  
7 credentials. Everything and everyone seems gay with anticipation;  
8 cute Czech girls at the registration desk help me pin my badges on my  
9 coat.

10 Then I take my heavy bags out to the street and (contrary to dire  
11 predictions of everyone) find a taxi almost at once. To Pariz Hotel  
12 downtown, on U. Obecniho Domu in Stare Mesto (the old city), a side  
13 street off the Square of the Republic (a main intersection). The hotel  
14 lives up to expectations -- early Emperor Franz Josef I, mid-Nineteenth  
15 Century -- faded elegance. Later, the receptionist asked me what I  
16 thought of it and I said, "Well, it isn't modern," and she said "Yes,  
17 it is old-style," and I said "I love it for what it is."

18 My room is very sparse, with not enough storage space and only a  
19 wash basin; toilet and bath down the hall. But it has a high ceiling,  
20 big windows, two easy chairs, and it is four floors above the street  
21 (lobby, mezzanine, first, and second), hence above street lights and  
22 traffic noise. The staff is alert, water taps don't drip, maid service  
23 is excellent; my bag of laundry disappears the moment I put it out.  
24 Downstairs all is elegance. I have a mid-day meal in the dining room,  
25 served in style; waiters in tails and enormous dinner napkins.

26 I don't know whether to object, or just settle back and enjoy  
27 it, end up doing the latter. My only fear is that this will isolate  
28 me from my friends, and from people with whom I will do business, most  
29 of whom I fancy are at the International; I see few Congress people  
30 about, and don't know them. (The hotel filled up later).

31 I ride back to the Congress by the number 11 tram, very crowded.  
32 Wander about looking at map exhibits, which are only half put up. The  
33 display will certainly be the best of those at any Congress I have  
34 attended. Big tack-boards are put up the length of the three  
35 upper corridors of the building, with ample space for exhibits by all  
36 the countries. Our North America map has been hung in a prominent  
37 place, and looks as fine here as it did at home. I see Bill Johnston  
38 and Phil Guild of our delegation to the map conference, later Marçais  
39 and Frances Delaney. I hug Frances with fervor (wanted to do it for  
40 years). See lots of other people in my wanderings, Americans and  
41 others.

1 At 4:30, decide to go back to town. Tram is crowded again, a  
2 long time before I found a seat. Ask at the reception desk of the  
3 hotel where I could find an oculist to repair my white glasses (a lens  
has fallen out), and walked around until I located the place. Will go  
there in the morning.

4 I go down to the cafe to find a place where I can rest and drink.  
5 This seems to fit the local customs. Groups of old ladies occupy  
6 tables, drinking beer; men sit at tables with newspapers. I order  
7 Slivovits, three in all, and hold a table to myself for a long time.  
8 I catch up on this diary and my accounts, the first relaxing time I  
have had in a long while. Other people try to sit with me, but there  
are plenty of other tables so I shoo them away. When I am about ready  
to leave, two cute girls arrive and I let them move in.

9 A welcome letter from Helen at the desk, no others. Will write  
10 a letter home and call it a day. A chocolate bar in lieu of supper  
(the mid-day meal was more than enough). A most welcome bath, with  
vigorous help from the floor maid. And so to bed.

11 Prague looks much better than when I arrived in March, 1962.  
12 Leaves are on the trees, and the little squares and circles are bright  
13 with flowers; the bleak apartment buildings fade into the background.  
14 But it isn't just the difference in time of year; a real effort is  
15 being made to spruce up the city -- attractive new buildings, and much  
16 work refurbishing the old. The monstrous Stalin Statue is down. As  
17 in 1962, the vacant walls and boarded-up places are covered with posters,  
18 but now (as we also observed in Slovakia) the Communist inspirational  
stuff is all gone -- no indication we are in a Communist country. No  
posters about Sputnik and the Astronauts, no posters of massed peoples,  
men, women, and children, marching forward under the red banner. Most  
of the posters are about art exhibits, concerts, and far-out movies.  
A few advertise a geological exhibit at Beroun, with a big picture of  
a trilobite. A few advertise the castle at Karlstein. The increase  
in number of private automobiles is astonishing.

19 On the streets, many clean-cut Western European types, many cute  
20 girls in miniskirts, many hippies. But I also notice that there are  
21 more slobs among the populace than I had remembered from 1962, slovenly,  
22 big-boned, East Europeans like those who predominated in Slovakia.  
23  
24  
25  
26

1 Monday, August 19. Prague.

2 A long, complex day. When I awake it is raining hard, and so  
3 continues all morning. Wish I could go to the opening sessions (not  
4 at the University, but at the Park of Culture and Recreation in  
5 another part of the city). I heard later that high political figures  
6 made speeches, as well as geologists, so it would have been most  
7 enlightening. But there were many personal things to do.

8 In the city:

9 (1) Talk to the lady representative of the Congress at the hotel,  
10 who is advising all the Congress people staying there. Hardly useful,  
11 as she has the same little spiel for all of us (which she must have  
12 memorized in English) -- how to find places on the map, how to get to  
13 the Congress -- all of which I know already. For any real problems,  
14 she says I must go to the desks at the University.

15 (2) Get the lens put back in my white glasses at the oculists.  
16 Even with no language in common I am taken care of at once. When I  
17 offer to pay, they wave me away.

18 (3) To the National Bank, a block down the main street, to pur-  
19 chase Tuzex coupons to use at the import stores. Get \$50.00 worth,  
20 which is more than my immediate needs, but I plan to go to the Tuzex  
21 department store next week to buy films and gifts to take home.

22 (4) To Tuzex delicatessen to make purchases -- the same place  
23 near the Mozart theater in Stare Mesto where I went in 1962. This  
24 took a bit of doing in the pouring rain, as the place was farther off  
25 than I remembered, and I had to ask directions once. Returned to hotel  
26 with two bottles of Scotch and plenty of tobacco, all for about \$10.00.

27 At Technical University:

28 (5) Pay hotel bill in advance at Cedok travel agency desk; with  
29 meals included, this amounts to about \$71.00.

30 (6) Fix up transportation from here to Krakow for post-Congress  
31 field trip in Poland. The Congress circular had said this would be  
32 about \$17.00, and I have paid in only \$7.00, but they say everything  
33 is O.K. and give me a coupon for the trip. Heap cheap trip! But I  
34 must exchange the coupon for an actual rail ticket later, so the final  
35 reckoning is still ahead.

1 (7) To Polish room for advice on Poland. Hospitable welcome from  
2 Polish geologists and Orbis travel agency. They give each visitor a  
3 shot of Polish vodka, and tell us how welcome we will be in Poland.  
4 There is only one hitch in my plans; I learn that the plane out of  
Krakow for Copenhagen does not fly on Saturday, but on Sunday, so I  
will have to move my return to the States ahead a day. They tell me  
to see Czech Airlines about this.

5 (8) Long, long wait at Czech Airlines desk to have this change  
6 made. Only one girl is on duty, in airlines uniform, blonde, false  
7 eyelashes; speaks excellent Oxford English, and rapid-fire Czech. She  
8 is doing a marvelous job for everybody, in spite of handicaps, but it  
9 takes time. In line ahead of me are Lois Kent, John and Sally Hills,  
10 Snelgrove (the Newfoundlander), and others I know, so the time passes  
in visiting. My own problem is complicated because, by changing things  
a day all reservations have to be changed clear to San Francisco, in-  
cluding the hotel for my night in Copenhagen. She thinks she has it  
fixed, but says come back in a day or so to confirm it.

11 Had beer and lunch with Johnnie Hazzard at the buffet on the  
12 "first" floor; like me, he is alone as his wife elected to remain in  
13 California. Saw Fred Dunning of Britain and learned that our dear  
14 friend Mr. Trotter died of a heart attack this spring. Found Choubert  
and his girl friend putting up their map of Africa next to our North  
America map in the display of the World Map Commission. Together, the  
two maps make a stunning display. Saw others, including many of our  
Menlo group, and other Survey people.

15 My only scheduled event of the day was the meeting of the U.S.  
16 Delegates, at 1:30 P.M., presided over by Nolan and Thurston, on  
which I made a few notes:

17 The State Department advises us to use great care in commenting  
18 on the political situation, and to avoid black market money transac-  
19 tions (at least 3 locals have approached me on this already). Nolan  
20 says proposals have been made to inject political and social questions  
into the resolutions of the Congress, and we should fight against these.  
(I learn later a resolution has been fed in by the Reds to condemn us  
in Viet Nam.)

21 Proposed amendments to statutes of International Geological  
22 Congress and International Union of Geological Sciences, to clarify  
relations between the two bodies.

23 Proposals about activities of future Congresses, apparently  
24 mainly by the British:

25 (1) About abstracts.

1 (2) Problem of simultaneous translations during Council meetings  
2 and other important Congress sessions. Proposal for three languages,  
3 certainly English and French, one other depending on locale. But  
4 restricting to three will offend other national prides, and how can it  
be held down? Cost is excessive, and financing hard to get; UNESCO  
often has to bail out a meeting, from limited funds. We agree to stay  
out of this.

5 (3) As a substitute, make Esperanto the official language. Every-  
6 one hoots at this.

7 (4) Publication of Congress proceedings by time of Congress.  
8 This has been done at Copenhagen and Prague. The Mexican proceedings  
are still only partly out, and whether we will ever see the New Delhi  
proceedings is doubtful.

9 (5) Establishment of a Sub-Commission for Cenozoic of south-  
10 eastern Europe. No comment.

11 (6) Establishment of a Commission for the study of the Dinarides.  
12 Nolan comments aptly that there are too many Commissions now, which  
confuse things by calling mini-Congresses of their own between the  
main Congresses. They all require financing, and there is not enough  
money to go around.

13 (7) A more formal means of setting up National Geological  
14 Committees. The U.S. and other major countries already have such  
committees; many other countries do not.

15 The next session of the delegates will be at the same time on  
16 Thursday, when reports will be made on the activities of Committees  
and Commissions by their American participants.

17 The next Congress will almost certainly be in Montreal, Canada.  
18 Canada is the only country which has issued an invitation.

19 Back to hotel at 5:00 P.M., very weary, but managed to get a  
20 seat early on the tram. Had a good drink of Scotch, the first real  
booze in a week, and sorted out my thoughts. Down to dinner at 7:00.  
21 I was disdained in the restaurant, which is too small, and was very  
full. Sent over to the larger cafe, where all the local slobs eat.  
22 The waiters try to put me at a table with two women in dark clothing,  
whom I assume are some of the local beer-guzzling ladies, but I walk  
23 indignantly to a table of my own.

1 But I had not looked closely enough and had made a mistake. The  
2 ladies come over and invite me back to their table. Turns out that  
3 they are two American biddies who are attending the Congress, who are  
4 very kind and friendly. It is hard to explain to them just why I  
5 acted as I did. They are Mrs. Helen Biren of Brooklyn College, New  
6 York, and her companion and old pupil, Mrs. Helen Wolfe of Santa  
7 Barbara, California.

8 So we have a jolly dinner after all. Presently, their young  
9 Czech boyfriend arrives, who is showing them around -- an instructor  
10 in geology at Charles University.

11 Service is terrible tonight, especially toward the end. A long  
12 wait for dessert, a still longer wait for the bill. It is all too  
13 reminiscent of the 2-hour dinner service in the Soviet Union. How I  
14 hate it!

15 When leaving, I find that Augusto Gansser and his wife are dining  
16 at another table, and are staying at the hotel. It is good to know  
17 that there will be friends here after all. So far, all the Congress  
18 people I have seen here are nonentities.

19 I heard amusing news today. Bill Johnston was staying at the  
20 student hostel, but they moved a bunch of Russians into his corridor  
21 who made the wash room and toilets so filthy that he had to move to a  
22 hotel!

23 Tomorrow, my own business begins, with meetings of the World Map  
24 Commission at the International Hotel, starting at 9:00 A.M.

25 And so to bed.

1 Tuesday, August 20.

2 Up at 6:45 A.M. A sunny, cool morning. Breakfast. My laundry  
3 is delivered by the maid. Off to meetings at 8:00.

4 At the tram stop, a young Czech advises me to take the #30 car  
5 instead of the #11, which is always crowded. The #30 car goes along  
6 the river to Mala Strana, then up the hill, arriving at Revolution  
7 Square like the other one. When we alight, I gratefully shake his  
8 hand. Then he informs me that, alas, #30 will run for only a day or  
9 so more, after which it will be discontinued because of repairs on the  
10 line. Walk to the International Hotel for my Commission meetings.

11 Convene at 9:30. Big meeting hall on lobby floor, sometimes  
12 used as a theater; room never much more than half full. We will have  
13 simultaneous translation, receiving it through little portable radios.  
14 Old home week. See most of the familiar faces of people who attend  
15 these meetings Bogdanoff, Egiazarov, Tillman, Michot, Choubert, the  
16 jolly Romanian and his girlish wife (his pin shows he is Head Delegate  
17 from Romania), Goguel, Von Gaertner, Lafitte (the crazy Frenchman with  
18 his metallogenic maps), Nalivkin (to my surprise, as he is very old),  
19 many others. Sit with John Rodgers.

20 At head table -- Guild, Kinney, Gourdanoff, Lamego, Nalivkin,  
21 Marcais, Delaney, Von Gaertner, Roy, Mostofi, Richter, Choubert.

22 Marcais and Delaney make various opening remarks, including some  
23 on resignations and replacements to the personnel of the Commission.

24 Richmond reports on an international map of the Quaternary, being  
25 prepared by INQUA.

26 Reports by convenors of different continents for World Atlas:  
27 Kinney, North America. Roy, Asia and the Far East. Lamego, South  
28 America. Australia, Fisher. Von Gaertner, Europe. Gourdanoff, Soviet  
29 Union. Mostofi, Near East. Antarctic, Adie (in the field, report  
30 given by Delaney). Choubert reports on the sheets of the World Atlas  
31 which have been completed and are now on display.

32 Walter of UNESCO talks; he is a thorn in the side of Bill  
33 Johnston. Among other things, says he hopes the differences with his  
34 North American colleagues can be resolved in a friendly manner.

35 Coffee break, 10:30 to 11:00.

1 On reconvening, the Volcanological Map of the World is discussed  
2 by Professor Kuno. A project of the International Association of  
3 Volcanology. Volcanoes are plotted -- red for those active in  
4 "historic" time, black for non-active, which are restricted to those  
which still preserve their morphological forms. Compositions of  
volcanic fields are indicated, including andesite, tholeite, and  
alkali basalt.

5 The Metamorphic Map of the World is presented by H. J. Zwart of  
6 Aarhus University. Metamorphism has orogenic and tectonic implications.  
7 Metamorphic facies are shown, represented by a series of mappable units.  
8 Ages of metamorphism are indicated by letters, representing orogenic  
events such as Alpine, Variscan, etc. Still in an experimental stage,  
but extensive compilations have already been made in the Baltic Shield,  
eastern United States, and Africa.

9 Bogdanoff reports on the Sub-Commission for the Tectonic Map of  
10 the World. Another Tectonic Map of Europe is in progress; this is to  
11 be entirely new, not just a reprint or a correction of the map of 1964.  
12 Representation of oceanic areas on the continental maps and the world  
13 map is under study, and was discussed at the recent Oceanographic  
Congress in Moscow. Much material has been received for the Tectonic  
Map of the World, and plotting and correlation of the materials is  
under way in Moscow.

14 Guild reports on Metallogenic Maps. Progress is being made on  
15 continental maps, which is the first phase. The next phase will be a  
world summary.

16 Richter reports on Hydrogeologic Maps and discusses progress on  
a hydrogeologic map of Europe.

17 Announcements by Marcais; asks for an inventory of the maps  
18 which are being exhibited here.

19 Kinney follows this up with a request for a list of available  
20 continental maps, where they can be purchased, and their cost. A list  
of printed continental maps is not enough, for they are often out of  
print, or otherwise difficult to trace and to obtain. Asks that the  
Commission undertake this responsibility.

21 Adjourn at 12:00.

22 People:

23 Bogdanoff is still the great big lovable Russian bear. Gave me  
24 a great hug, to which I did not adequately respond, and presented me  
25 with gifts -- a pint of Georgian cognac, a book on ceramics, one on  
the ballet, an amber necklace, two little animal figurines.

1 Nalivkin, a surprise and a delight to see him. Thought I would  
2 never see him again when I said goodbye to him in Leningrad in 1965.  
3 If he was born in 1885 (as I recall) he would be 83. He looks as fine  
4 and rosy and distinguished as ever, although someone said he had been  
5 in bad health. I'd like to get his picture this time, having missed  
6 it on all previous occasions, but my flash has gone bad on me; finally  
7 took a poor one at the street car terminal outside, after the meeting.

8 Hills of Australia, we ride the tram back to the University after  
9 the meeting. I had received the impression from the other Australians  
10 that he was a sort of stuffed shirt, but my impressions were quite  
11 otherwise. He seems to be a pleasant, decent sort -- not the big  
12 operator I had expected.

13 Zapletal of Brno, whom I met in Prague in 1962. I remembered  
14 him as a sharp chap in his middle years, full of incisive questions,  
15 looking much like Bob Laurence. Doug Kinney and I found him at the  
16 University, looking at the Tectonic Map of North America. I was  
17 shocked; he looked old and haggard and was using a cane -- must have  
18 had a stroke. Pathetically, in broken English, he says to me, "You  
19 are young!" (which isn't so, of course). I present him with our little  
20 yellow book on the map, and inscribe it with real feeling, "To my good  
21 friend."

22 A hectic and long afternoon. From the International Hotel I  
23 ride back to the University on the tram, and have lunch at the buffet.  
24 Long wait in line for food and hard to find a seat at a table. Once  
25 seated, I can relax, catch my breath, and say hello to passers-by.

26 My briefcase is bulging at the seams from papers and gifts, so  
27 I decide to take them back to the hotel and do some needed business in  
28 town. Arriving at the hotel, my little maid brings the second packet  
29 of laundry, so now I am all caught up again. I give her a package of  
30 chewing gum.

31 Then, a walk across Stare Mesto to Mala Strana. The sun was  
32 good in the morning, now it is temperamental and the light waxes and  
33 wanes; not so good for photography. I will take pictures anyway, as  
34 I may not get to these places again. My roll of films in Stare Mesto  
35 was ruined in 1962, so I must rephotograph things I saw then.

36 The old town square -- John Hus Memorial -- Tyn Church -- Old  
37 Town Hall and its famous clock (they are rebuilding the wing lost in  
38 World War II out of the grass plot I saw in 1962). Here, and in Mala  
39 Strana, all the wonderful old sights, but somehow they don't have the  
40 impact this time. Is it that there is no novelty of discovery now?  
41 Am I jaded? Anyway, I take pictures.

1 Hordes of people are sightseeing or just wandering -- may be some  
2 walking tour groups. There is very little wheeled traffic in the  
narrow streets, so everyone disdains the sidewalks.

3 The Charles Bridge -- a shock. Major repairs and innovations are  
4 in progress -- scaffolds, piles of lumber and pipe. Maybe it was about  
5 to fall in after all these centuries, but I hope they don't hurt the  
6 dear thing. No traffic on the bridge as a result, so people are walking  
any old way. The bridge has become a hangout for hippies -- quite as  
unkempt as the U.S. variety -- playing guitars, exhibiting bad paint-  
ings, or just relaxing.

7 My main objective is the Czech Geological Survey office on Mala  
8 Strana Square, where I was told I could exchange the receipt for my  
9 railroad ticket to Krakow. I find Catherine Campbell in the reception  
10 room with another problem -- trying to find Professor Boucek and give  
11 him a present. Also one of our Menlo Park Water Resources men and his  
12 family (Fred Kunkel). Incomprehension of my problem by the Czechs,  
13 some of it just square-headedness. One man says, "I do not understand  
the trend of our conversation," which sounds exactly like it came out  
of a phrase book. Finally, the right person is found, who tells me  
that the tickets will not be ready until next week and that they can  
be obtained at the University a few days before we leave for Poland.  
Otherwise, everyone is very kind, and they give me candy and orange  
juice.

14 Anyway, the trip is not wasted, as I always enjoy a visit to  
15 Mala Strana. Once there, it is worth a courtesy call at the American  
Embassy, where I fill in a card with my name, address, and other  
16 information.

17 I have to go back to the University to attend a 4:30 meeting of  
Tuzo Wilson's Upper Mantle Group. I saw him on the street this morn-  
18 ing and he handed me the invitation on a slip of paper. The #7 tram  
from Mala Strana to the International Hotel is a real Toonerville  
19 Trolley; all the other lines have modern cars. So we inch along, and  
I am finally back at Revolution Square. On the way over to the  
University, see Jim Gilluly who hasn't heard about the meeting -- says  
20 he'll have to skip it; will attend later ones.

21 Tuzo's outfit is some kind of a study group on tectonics, under  
the main Upper Mantle Committee. I had thought it was confined to  
22 North Americans, but it consists of about 24 people from all over the  
world, of whom only about a dozen are present. Seems to be a small,  
23 struggling outfit, trying to find itself, so I am glad I made the effort  
to come, as a gesture of support to Tuzo. Dietz, Belousov, Khain,  
24 Goguel, Gansser, Laubscher (another Swiss), Kuno, some others.

1 First, routine business -- approval of agenda, presentation of  
report by rapporteur (Tuzo), time and place of next meeting, etc.

2  
3 Then, discussion of future program. Tuzo has listed about half  
4 a dozen topics as possible subjects for study -- crypto-explosion  
structures, model experiments, vertical uplifts, diapirs, strike-slip  
5 faults, sea-floor spreading. Much discussion. Belousov as usual  
6 tries to dominate the meeting, but he does make at least one good  
point at the start -- everything proposed is just data-gathering and  
7 cataloguing; something more fundamental should be done. My own re-  
8 action when I read the proposals beforehand was "What does any of this  
9 have to do with the Upper Mantle? The things listed are just momentary  
geological hobbies of Tuzo." This seems to be the general reaction,  
and many of the proposals are tossed out by the group. Also, many of  
the more worthy topics are already being covered by other committees  
and commissions. So the meeting ends inconclusively, with plans for  
further discussion on Thursday.

10 Back to the hotel by tram. Ivan Barnes is on the loading plat-  
11 form also, and is taking #11 to a hotel considerably beyond my own.  
This tram arrives, full to the doors as usual, but a nearly empty #30  
12 comes up just behind, so I take this instead and have a comfortable  
seat to the city.

13 Gansser had invited me to dine with them this evening and says  
14 he will call me when they are ready. I soak up Scotch for an hour or  
so, catching up on notes, and he calls at 7:30. A delightful,  
15 sociable meal; toward the end we are joined by Mr. and Mrs. Bally (the  
Swiss geologist who is with the Shell in Houston). My only regret was  
16 that there was too much talk on geology; it was hard to keep the ladies  
in the conversation.

17 Gansser is irritated by the slow service; I had thought this was  
18 a purely American disease, and it is good to know that Europeans don't  
like it either. For a long while I don't mind myself, as the company  
19 is delightful. But I lose my cool at the long, final wait for the  
check. It is late, most of the diners have left, and there have been  
no waiters in sight for ages. I finally resort to my ultimate strategy,  
20 and just walk out. This brings results immediately! We are surrounded  
by the help, who are very attentive and apologetic; made me feel  
21 guilty at losing my temper. We are free at last at 10:00.

22 The evening is over, so I go to bed.  
23  
24

Wednesday, August 21. Prague.

11:00 A.M. -- I have come to my room to rest, have a drink of Bogdanoff's cognac, and write some of this down. The room is quiet and peaceful, after all that has happened outside (and is still happening).

Awakened at 4:00 A.M. by an unholy racket in the main street half a block away. Interminable honking of auto horns, an awful roar, the sound of many voices. This is not ordinary, and I suspect a coup of some kind, but I convince myself this is unlikely and have a restless sleep until 7:00. Going down to breakfast I find it is all too true; the Russians and their satellites have moved in, tanks and troops are all over the city. I first get the news when most of the people in the cafe go to the window to look at something down the street. I ask the girl at the next table what it is about; she tells me with tears in her eyes they are looking at Polish tanks. (She was wrong on only one thing; the tanks were not Polish.)

(Later, I heard an explanation for the mystery of the auto horns. Czechs in their private cars were racing out of the city, ahead of the tanks, maybe to the border, paying no attention to traffic signals, relying on their horns to get the right of way.)

Presently the Ganssers join me, then the Ballys. Gansser and I are determined to make our way up to the Congress, anyhow, but this is not to be. No trams are running and all the river bridges are blocked. Besides, word has come from the American and other embassies for all outsiders to stay off the streets, and to keep to their hotels. Another good piece of advice from somewhere, have your passport with you at all times. Another, sillier piece of advice -- the power may go off at any minute, stay out of lifts and trams.

Since we are all marooned here, I get a better idea of who the Congress people are at the hotel -- Americans, British, French, Swiss, etc. The American delegation is bigger than I expected. Most of them were with an A.G.I. tour group, and arrived piecemeal Monday and yesterday; some got in late last night and never saw Prague when it was normal, and never got to the University to register. Many A.A.P.G. people, including Ira Cram and Lew Weeks; many Survey people from Washington and Denver, including Winonah Bergquist and her husband.

With no prospects ahead, we linger long over breakfast, not worrying about the slow service any more. Afterwards, we try to sit in the lounge off the lobby, but it is small and there are not enough chairs. Togetherness helps; no one has the urge to sit alone in his room. Finally, most of us venture out, first to the hotel steps, then to the corner of the main street, on the Square of the Republic.

Clearly, we are in an exposed situation. A block down the street is the Czech National Bank (where I went yesterday for my Tuzex coupons), which was one of the first things siezed by the invaders, **Across** the square is a big building which I am told is Czech army headquarters. Something might happen at any minute, given an overt act.

5 Tanks roar up and down, seemingly without plan (except maybe to overawe the populace), there is a distant sound of firing. Screaming ambulances race by. In a strange situation like this, it is hard to sort out what is going on, to whom all this military hardware belongs to. **Is** any of it Czech and will there be a battle? Identifications on the vehicles are not obvious -- no hammers and sickles, no Russian lettering. Then I am told that the Czechs have barricaded themselves in the Army building across the way and have refused admittance. Sometimes tanks surround the place, all guns pointed at it, and I expect a bombardment. But nothing happens. Curiously, some of the windows of **the** building are opened, and men inside look at what is going on, with their elbows on the sills.

10 Thousands of **people** are walking -- where, I can't imagine. Women are weeping, men are grim. One weeping woman is making a scene of herself by crying in a loud voice, but she looks like an alcoholic; the rest have more to commend themselves. Everyone is carrying transistor radios, listening to the news. People who can speak English stop and talk to us, tell us some of the news, and their outspoken opinions. One man says, "I fought with the Russians in the War; I have been a Communist 25 years; this is the end!" Another, "The Gestapo is here!" Another, "Communism is dead in Czechoslovakia!" The Czechs are not fooled by propaganda blather; the analogy with Hitler's takeover in 1939 is all too obvious.

16 Private cars race in and out among the tanks; the traffic lights are still working and the cars (as well as the tanks, apparently) respect them. Youths on motor scooters roar by, waving Czech flags. 17 Then great truck loads of youths come by (too many of them hippie types), waving flags, holding up posters, shouting slogans, which are easily translatable -- "Russians go home!" "Czechoslovakia, neutrality!" "Dubcek, Svoboda!" "Breznev, Hitler!" Scooters and trucks weave in and out among the tanks, each side ignoring the other. A commandeered civilian truck pulls up and disgorges troops, is immediately taken over by youths, who fill it up and drive off.

Many of our silly Americans are taking pictures like mad -- of youths, of tanks, of anything dramatic that is going on. I am tempted to go back and get my own camera, but don't in the end (this was the end of my picture taking during the trip). In retrospect, maybe I should have done so, as it was quite possible during the confusion of this first day. But will any of us ever get any of our films out of here -- political or geological?

1        Rumors -- Romania is occupied too, maybe Yugoslavia. Dubcek is  
2 a prisoner; Dubcek is meeting with the presidium. Kosygin (the faintly  
3 liberal Soviet) has been deposed and the Soviet Union has been taken  
4 over by a military dictatorship. Most of the Soviet soldiers are  
5 Mongols from Siberia, who have never been in Europe before. All rail-  
6 roads, airports, border points, and cables are blocked. The Soviets  
7 told their troops they were coming in because West Germany had already  
8 invaded (a big lie, Hitler style). The Congress is over; the Congress  
9 will resume tomorrow; the Congress will resume this afternoon.

10        Weary, I return to the hotel. To my amazement, I find Bogdanoff  
11 in the lobby, talking to the nice elderly French professor and his  
12 wife. I squeeze his hand and say, "My friend!" He looks emotionally  
13 upset and says, "Very sad, but it will all be straightened out in a  
14 few hours!" (wishful thinking!). He goes out of the door and dis-  
15 appears down the street. Where did he come from and where did he go?  
16 I had a feeling he had come to say farewell, but he was much in evidence  
17 the next two days.

18        Noon.--The ancient bells toll the hour of twelve, oblivious of  
19 the turmoils and follies below.

20        In my room, I am left a prey to my thoughts. What is Helen  
21 thinking? Will the Congress continue? Will we be evacuated? I wish  
22 there could be a meeting of the American delegation -- but how? Now  
23 I am glad that yesterday I so casually filled out a card at the Embassy;  
24 I'm sure few other Americans took the trouble. All told, I'm not sorry  
25 I am in a downtown hotel where I can see what is going on. Think of  
26 being stuck away out at the Student Hostel, or even in the International,  
27 not a witness to anything, panicked by every rumor!

28        I descend for lunch, and join forces with my two American biddies.  
29 The cafe is closed and we have a try at the smaller and fancier  
30 restaurant. It is crowded, but we get a table next to a window, and  
31 are presently joined by the Grossers and the Ballys. I have borscht,  
32 beer, and pastry; the girls something equally light. An interminable  
33 lunch, an interminable wait for the check, but it doesn't matter now.

34        Part way through lunch, twenty-five tanks roll down our little  
35 side street, at eye-level with our plate glass window. Do they know  
36 where they are going? The streets ahead get very narrow and crooked.  
37 The twenty-fifth tank conks out right in front of our window and has to  
38 be repaired; that ends the show of force on our street. Some of the  
39 tanks have bashed in front ends, as though they ran over obstacles in  
40 their way (a tank can evidently crumple up just like an auto). Most of  
41 them have their men's knapsacks lashed to them, and one of them incon-  
42 gruously has an ordinary traveler's suitcase! One of them has a red  
43 flag with hammer and sickle on the rear -- the first identification of

1 anything as Soviet that I have seen. But on the flag the Czechs have  
2 smeared a big swastika!

3 After lunch, we walk up to the main street intersection again.  
4 The tanks have played havoc with the paving and tram rails, have  
5 smashed the loading platforms. Even if the Congress resumes, tram  
6 service is no more. Tanks and personnel carriers are in front of the  
7 Army building, and are surrounded by local boys and girls; some girls  
8 have climbed onto the hoods of the machines. We are informed this is  
9 no ordinary "fraternization"; the local boys and girls are telling the  
10 Russian boys what the situation really is, and what they think about  
11 it.

12 A group of youths parade by with Czech flags, and with an equal  
13 number of black flags, and go past the massed troops. A still bigger  
14 group marches in from another direction, singing the national anthem,  
15 with massed Czech flags in front. Someone is getting a broadcast  
16 from West Germany -- very clear and simply spoken, if I knew German.  
17 Part is translated for us, but it says nothing we don't know already.

18 More of the same, more of the same. I go back to sit in the  
19 little lounge off the lobby. A Canadian wife tries to get a bridge  
20 game started but the response is lackluster, and all the ladies just  
21 continue to sit.

22 Finally, it is 5:00 P.M. and time to return to the room and write  
23 some more of this down. Half an hour later, a strange incident. I  
24 go to the window to watch a knot of men up the street, waving their  
25 arms and shouting. Then, just below my window two youths give a  
26 vicious uppercut to a third and knock him to the pavement. They drag  
27 him to a doorway, drop him with a trail of blood behind him on the  
28 sidewalk, then disappear. A knot of bystanders gathers around the  
29 man, then drifts away. Presently, the injured man walks away on the  
30 arms of two girls, smoking a cigarette! Where does this fit in, if  
31 at all? Was it a private quarrel, or political?

32 After writing this, I must have fallen asleep, as I come to with  
33 a start at 7:15. I had told my two biddies I would eat with them at  
34 6:30, but I have long missed them. Descending, I find service at a  
35 minimum. The big cafe is dark, only the restaurant is open, and this  
36 is jammed. No one stands on ceremony now; I wander around and find the  
37 Ballys at a table with an empty chair, the other diner is a Tunisian  
38 who soon leaves. The Ganssers arrive, pull up chairs, and join us.  
39 Finally, the waiters accept my order, but there are no luxuries now --  
40 meat course, beer, coffee -- no soup, no ice cream, no pastry. An-  
41 other long wait for the bill, but we have learned patience.

1        During dinner, Gansser calls Ken Hsu at another hotel. Hsu,  
2        Trumpy, and some of the other Swiss have their cars here, and are  
3        thinking of making a convoy and heading for the border tomorrow.  
4        Rather chancy, as the rumor is that the Russians have confiscated all  
5        the petrol and have sealed the border points. One would have to make  
6        it to the border with whatever petrol was in his tank.

7        While we are eating, tanks and other vehicles come down our  
8        street again and park solid in the little triangle behind our hotel.  
9        There is a rumor that this has something to do with a meeting of  
10       Russian bigwigs in another hotel a few doors down the street. I have  
11       a queasy feeling that the boys in the tanks are going to be billeted  
12       in our hotel, but nothing happens. Where do they sleep? Do they blow  
13       up air mattresses and sleep on the streets beside their tanks?

14       After dinner, our restless group proposes a "stroll" outside; a  
15       little knot of us gather at the doorway in front of the hotel. The  
16       streets are dead and empty; the traffic signal at the main intersection  
17       is on the "flash." It is 10:00 P.M. There may be a curfew; I don't  
18       know. At least the hotel is enforcing its own, and the porters lock  
19       the door on us. I knock to get in, and the rest sheepishly follow.  
20       And so ends the day.

21       Just before going to bed, much noise outside -- some like vehicle  
22       backfires, more distant ones certainly gunfire. Ambulances wail. We  
23       may have an uneasy night.

1 Thursday, August 22. Prague.

2 I slept later than planned, and woke at 7:45 A.M. Coming down,  
3 I find that the rest of the Congress people have nearly finished their  
4 breakfasts. We get word that the Congress will continue, and that a  
5 bus is leaving for the University at once. Gulping my food, I race  
6 upstairs to get the things I will need, but when I return the bus has  
gone. Then I learn that another bus will leave from the Hotel Central  
around the corner at 9:20, so those of us who were left behind go  
around there to wait. This bus fills up also, and off we go, arriving  
at the University at 10:00.

7 The saturation of the city with tanks and troops is overwhelming;  
8 they are parked everywhere. The big park off Obrancu Miru where the  
9 Stalin statue used to be seems to be a main bivouac, and is full of  
them. At least a dozen are near the University in the Square of the  
Revolution.

10 I see Bill Johnston at the entrance and learn that there will be  
11 an informal meeting of all the Americans very soon in Room 334, so I  
go up. The door is locked, and there is a great mob of us in the  
hall -- several hundred, at least, including wives and dependants.  
12 After a wait, someone finds the key and we go in. Fortunately, the  
room is bigger than I remembered, and there are only a few standees.  
13 Nolan is not there, said to be "at the Embassy," so Jim Gilluly takes  
over.

14 A rather inconclusive meeting. General talk, many proposals,  
15 some wise, some foolish. Jim asks for a general consensus that we will  
all stay to the end, as a gesture of solidarity to the Czechs -- or  
16 until the Embassy orders us to leave. Someone proposes that there be  
a central information desk at the University for Americans, or at least  
17 a special bulletin board, so that we will not have to get everything  
by rumor. This is enthusiastically seconded. There will be another  
18 meeting at 1:30 (which had been planned far in advance, before all this  
happened) with the hope that Nolan can get there and give us an  
19 official report.

20 With this meeting adjourned, I decide to go over to the Inter-  
national Hotel, to see if the Map Commission is functioning, walking  
there with John Rodgers. We find the meeting in session, and we have  
lost much of what has happened during the morning. Miscellaneous  
21 reports are being read, which I have not bothered to record.

1       Then, a discussion of future plans. It is decided that, instead  
2 of our hitherto leisurely morning meetings, everything will be crowded  
3 into this week, with morning and afternoon sessions today and tomorrow,  
4 maybe Saturday morning.

5       The meeting adjourns about 11:45. Since the buffet at the  
6 University is crowded at best, even on normal days, I decide to try  
7 the one at the hotel. I get there just in time; the Czechs are hold-  
8 ing a general strike of everything from 12:00 to 1:00 P.M., and I  
9 barely manage to get beer and sandwiches. I share a crowded table with  
10 a succession of Americans, ending with Lew Weeks and his wife, who are  
11 very friendly; we walk back to the hotel together.

12       An American movie company is staying at the International Hotel,  
13 making something about the Remagen Bridge, but they have decided to  
14 clear out, at great loss, and are forming a convoy of autos to drive  
15 to the border today.

16       At the front entrance to the University there has been, since  
17 morning, a big poster, signed by the Czechoslovak Communist Party,  
18 which is so worthy of recording that both the Weeks' and I decide to  
19 copy it; my copy follows:

(Poster at entrance to Technical University, August 22, 1968; copied by P. B. King)

TO ALL PEOPLE OF THE CZECHOSLOVAK SOCIALIST REPUBLIC

Yesterday the 20th August about 23 hr. in the evening the troops of the Soviet Union, Polish People's Republic, the German Democratic Republic, the Hungarian People's Republic, and the Bulgarian People's Republic crossed the borders of the Czechoslovak Socialist Republic. It happened without the knowledge of the President of the Republic, the Chairman of the National Assembly, the Prime Minister, and the First Secretary of the Communist Party of Czechoslovakia, and these institutions.

At that moment the Presidium of the Central Committee of the Communist Party were in session and were discussing the arrangements of the 14th Session of the Communist Party Congress. The Presidium of the Central Committee of the Communist Party asks all the people of our republic to keep calm and not to obstruct the advance of the troops. Therefore, even our army, police, and people's militia did not get an order to defend the country.

The Presidium of the Central Committee of the Communist Party considers this act to be not only against all the rules of the relationships between socialist states, but also a negation of the basic norms on international law.

All leading functionaries of the state, the Communist Party, and the National Front remain in their posts to which they were elected as the representatives of the people and the members of their organizations, according to the laws and other rules which are in operation in the Czechoslovak Socialist Republic.

The session of the National Assembly and of the Government of the Republic is being called at once by the leading representatives according to the Constitution, and a plenary session of the Central Committee of the Communist Party is being called in order to discuss the situation which has arisen.

THE PRESIDUM OF THE CENTRAL COMMITTEE OF THE COMMUNIST PARTY

1 While I am doing so, I look up and find a lens pointed directly  
2 at me. I ignored it, thinking it was some Jap or Hindu, but when the  
3 lens came down it was a white man, who quickly walked off. This was  
4 too much to take; I followed and collared him, "What nationality are  
5 you, mister?" "A...Russ...ian." "What in the Hell are you taking my  
6 picture for?" "I...just...wanted...to...take...home...a...record...  
7 of...how...people...were...reacting." "You s.o.b. I damn well don't  
8 like this! If you are taking that anywhere, you are taking it home to  
9 the Kremlin!" I wish I had hit him in the face and smashed his camera,  
10 but I let the matter drop there. He looked positively scared.

11 (When I told people about this later, several reported that they  
12 had seen other Russians taking pictures of us in similar situations,  
13 presumably "to take home." What motivates these bastards, anyway?  
14 Certainly they give no sign of affinity or sympathy, either to us or  
15 the Czechs!)

16 After that, another meeting of the Americans at 1:30, nearly as  
17 well attended as the one in the morning. Nolan is still absent, so  
18 Gilluly takes over again. There has been some news from the Embassy;  
19 it is working on a deal to get us and other Americans out to Austria  
20 by sending up 30 buses from Vienna, but this is still uncertain as the  
21 Austrians are reluctant to send their buses into enemy territory, where  
22 they might be confiscated.

23 Hollis Hedberg reports that, at a morning meeting of the Council  
24 it was voted 39 to 4 to end the Congress officially at 9:00 A.M.  
25 Saturday morning; also, that all post-Congress excursions have been  
26 canceled. (Goodbye, Poland!).

27 Someone proposes that the Americans at each hotel appoint a  
28 monitor or foreman to look after their interests and keep in touch  
29 with the Embassy; this should insure getting accurate news and having  
30 an orderly evacuation; also prevent every Tom, Dick, and Harry from  
31 panicking and phoning the Embassy on his own, which wouldn't help much.  
32 The monitor will also decide the order of evacuation -- women and  
33 children first, etc.

34 Gilluly says, "This is something constructive we can do right now."  
35 So, with help, he writes the names of all the Prague hotels on the  
36 blackboard, and the monitors for each are appointed then and there.  
37 The one for the Pariz is Richard Boebel and his alternate Ira Cram,  
38 both A.A.P.G.-ers who arrived late with the A.G.I. tour group.

1       Adjourning at 2:30, I manage to get back to the Map Commission  
2 meeting at the International at 2:45. I had told Frances Delaney that  
3 I very much wished to give my report that afternoon, but that I would  
4 probably be late getting there.

5       When I arrive, reports are being presented on the progress  
6 of the Tectonic Map of South America. Almeida, the general chairman  
7 from Brazil is talking in French. The simultaneous translation is not  
8 yet set up, so one of the women translators is at the podium with him,  
9 rendering each paragraph into English.

10       I note that all the pieces of the South America tectonic map are  
11 up on a tack board, not fitted together, and evidently still not  
12 coordinated -- Brazil and adjacent countries, Argentina and Chile  
13 separate, northwestern South America, centering around Venezuela.

14       Then Senora Bellizia reports on northwestern South America. She  
15 tries it first in English, but decides she can do it better in Spanish.  
16 She is a fine, vigorous woman, but she has no sense of proportion in  
17 these circumstances. It is long, long, long -- every little thing on  
18 her part of the map is mentioned, and the reasons for the representa-  
19 tion adopted is explained -- most of it meaningless to the audience.

20       While she is talking, Bill Johnston moves in beside me and urges  
21 me to say something flattering and encouraging to the South Americans,  
22 so when she finishes I jump up and do so -- the substance being that  
23 I was enormously impressed and pleased to see all the data for the map  
24 assembled so quickly, when it was only a dream last October.

25       But there is to be one more speaker, Ozario of Brazil, who dis-  
26 cusses the problems of the Precambrian shield. He, at least, knows  
27 what he is doing; he says enough, but not too much.

28       Then it is my turn. I had sent a two-page report to Marçais in  
29 May, which has been mimeographed and is available to participants. I  
30 go over this orally, with a few embellishments, and make it very  
31 brief.

32       Choubert follows on Africa, and is sensibly brief also, but  
33 much discussion follows.

1 Then Bogdanoff, who goes over the general problems and progress  
2 of the Sub-Commission on Tectonic Maps. He discusses the new Tectonic  
3 Map of Europe, the parts of which that are now compiled being on the  
4 wall for the first time. The areas covered are all in northern Europe,  
5 but include the part in Greenland (which I am pleased to see looks  
6 much like my own rendering on the North America map). Then the map  
7 of the World, also on the wall, with only North America, Africa, and  
8 the Indian Ocean colored in; also, the map of the Arctic, hand-colored  
9 for me by Trudy Edmonston.

10 Just before I was going to give my talk, Doug Kinney called me  
11 into the hall to meet a Japanese (Dr. Koichiro Ichikawa, Department  
12 of Geosciences, Osaka City University, 459 Sugimoto-cho, Sumiyoshi-ku,  
13 Osaka), who is taking part in preparing a tectonic map of Japan, and  
14 wants to talk to me. He says he wants my advice on the Pacific Mobile  
15 Zone. I am appalled; this is an imposition, so I say "Later maybe, I  
16 am about to give my talk to the Commission."

17 During Bogdanoff's oration (most of which was already familiar  
18 to me), I suddenly remembered this unpleasant chore, so signaled the  
19 Japanese to follow me outside. We go into the little anteroom that is  
20 serving as an office for the Map Commission. He tells me about the  
21 new tectonic map of Japan on a scale of 1:2,000,000; it was probably  
22 on display here, but I have not seen it. Then he shows me a draft on  
23 1:10,000,000 of their contribution to the Tectonic Map of the World.  
24 It is very cute and neat, in the Japanese manner, and I can see nothing  
25 wrong with it.

26 But -- he says, there are problems, because they cannot fit  
27 their representation to the master legend worked out in Moscow. I  
28 say, sure there are problems; we had the same trouble in North America.  
29 He shows me a xerox copy of a letter the Japanese wrote to Bogdanoff about  
30 their problems. I pocket it, saying that I will read it later. Ah  
31 no! you cannot have it; it is my only copy; I want you to read and  
32 comment on it right here!

33 I am stuck in an impossible situation; wearily, I try to read.  
34 Just then my troubles are dramatically solved. At 4:45 the help at  
35 the International Hotel throw all the switches in the meeting rooms  
36 and we are left in inky blackness. We stumble back into the main  
37 meeting room, which is almost dark, too. The whole meeting has to be  
38 suddenly adjourned. I tell my Japanese to write me sometime.

1 (This was the last I saw of the Commission meetings; I doubt very  
2 much that anything more was done. Except for displaying our North  
3 America map, none of the things I had come to the Congress for had  
4 been accomplished. Reading our reports before the Commission was a  
5 routine matter, which had to be done. But what was really needed was  
6 an opportunity to discuss leisurely, in open meeting, the general  
7 problems of tectonic map specifications and tectonic map legends --  
8 for both continental maps and the World map. But none of us had any  
9 spirit for this kind of thing after the events of the last few days.)

10 Doug Kinney and I debate what to do with our U.S. map exhibits,  
11 especially maps recently published or those still in press (like the  
12 North America map). We are determined that they shall not fall into  
13 the hands of the Russians, or even into the hands of the stinking  
14 French. I propose that they be taken to the Embassy, with instructions  
15 to destroy them. Doug says he will burn the maps himself. I don't  
16 see how he can do this, especially not at the Park Hotel where he is  
17 staying (but evidently he did so, from his later report).

18 The #11 trams have been coming up to Podbaba Circle in front of  
19 the hotel, which lends a hope that one can ride to town on them --  
20 or at least to the near side of the river bridge. So I climb on one  
21 and try it. The conductress vainly wishes to advise me of something,  
22 but she knows no more English than I do Czech. Finally, a light dawns  
23 and I get out my city map. She then shows me that #11 doesn't go to  
24 town any more; it just goes up to the Square of the Revolution at the  
25 University, then turns west into the suburbs. I save my feet by riding  
26 to the Square, at least -- the last use that I was able to make of my  
27 streetcar pass.

28 At the square, I find a great crowd of Congress people waiting  
29 for a bus that is supposed to take them to town at 5:00 P.M. Charlie  
30 Drake of Lamont, and P. J. Hart of the National Academy come along;  
31 they were going to walk to town, but I persuade them to wait with me  
32 for a while. The crowd grows denser. Still no sign of the bus; even  
33 if it arrives, it will be a rough trip. So we strike out after all.

34 (While I am waiting for the bus, I see Almeida, the Brazilian,  
35 who is general coordinator for the Tectonic Map of South America. He  
36 asks the usual question, "When can I get a copy of the North America  
37 map?" I parry with the answer Doug Kinney and I agreed on, "We are  
38 not distributing any proof copies, everyone will have to wait till  
39 the end of the year when the edition is printed." But it becomes  
40 clear from our conversation that this is a special case; the South  
41 Americans really need a copy of the map now, as a guide in making  
42 their own map. I must do something about this when I get home --  
43 and I did).

4 I am pleased to discover that Drake and Hale have no idea how to go, so there is no argument over my navigation. With my knowledge of the city, and with the aid of the map, I had picked out the most feasible route this morning. But the prospects are not encouraging; there is a light drizzle, and in my haste to get off this morning I had left my raincoat at the hotel.

8 We first go southeast from the square along Dejvicka Street, which the bus had followed this morning. This is solidly lined with big apartment buildings which have shops on the street floor. This morning, great queues of people were lined up outside the groceries; this was a common sight in 1962, but I hadn't seen it before on this trip; must be panic buying. The groceries are still open, but most of the people have gone; probably the shelves have been stripped bare. In the center of the street is a petrol station; this morning and now there is a long line of private cars waiting to be served. When the line moves up, each driver pushes his vehicle forward -- probably to save gas starting and stopping.

10 I get to the hotel at 5:40, after a stiff 40-minute hike, out of breath and in a sea of perspiration, dinner is scheduled from 6:00 to 8:00 and people are lining up already, but I just can't eat now. I go to the room instead, strip off my sopping clothes and hang them up to dry, rest, drink, and catch up on this diary.

14 The front door of the hotel was locked when I arrived, and the doorman is admitting only those he recognizes. No casual slob can wander in for a beer, nor any Russian either.

16 I go down to dinner at 7:00, feeling better. Find a place again at a table with the Ganssers; also the **McConnells** (from the U.K.), and the nice older French couple (whose name I was never able to remember). Soup, meat dish, and beer -- sparse, but quite adequate.

18 After dinner, I look for the meeting of the American group that was scheduled for 8:00 P.M. A great lot of Americans are milling around in the lobby and the sitting room, and all sorts of wild rumors are flying, but no kind of a meeting is being held. Boebel, our monitor, is just talking to buddies in a corner. I had sized him up as an effective, executive type, but he just isn't taking hold. Single-handed, I force him to call some kind of a **meeting** to order, so we can get our story straight.

He announces that their A.G.I. tour director (a little Frenchman! is working on a deal to get buses to take the tour group out to Vienna\*, any other American is welcome to come also. Have your bags packed; the bus might come at 7:30 tomorrow, later that day, or maybe days later.

I say that I do want to go to Vienna, but tomorrow is impossible for me; I definitely have more business at the Congress; any later day will be fine. Take the women, children, and the decrepit out first, I'm willing to stay by the ship to the last -- but don't forget me!

With this I leave and go to bed early, bone weary.

~~Prague~~, August 23.

A beautiful, cool, sunny day (for what little good that does, in these times). I get up promptly this morning. Breakfast with the Ganssers and the **McConnells**. They, like Jim **Gilluly**, are determined to stick it out to the end, but are moving up to the International this morning, "to be closer to what is going on." The International is reputed to have plenty of rooms, now that the movie company has left. The logic of the move is not irrefutable; we cannot possibly stay in Prague any more than a few days longer, and the Pariz is closer to railroad and bus terminals. I decide to stick it out here, so they give me all their meal coupons, on which I could eat for weeks -- a useless gift, as I will leave soon too, and meal service is getting sparser and sparser here.

The other Americans tell me that the A.G.I. tour director has actually found a bus, which will leave this morning for Frankfurt at 10:00. They say I am foolish not to join them, but I am not much interested. I have no friends in Germany, and would have no plans there; if I went to Vienna I could profitably spend some more time in Austria. And I keep remembering **Gilluly's** and Gansser's determination to stay until the end of the Congress.

Since I might be marooned here for a while, I decide to lay in more supplies, so walk down to the Tuzex store again. In these back streets all is quiet -- no tanks, no soldiers. Many, many Czechs walking, more queuing up at the food stores. Arriving at the Tuzex store I find a queue also, and it is not open. Fortunately, there is not a long wait; the doors open at 8:30. People troop into the store, form queues again; a few people in the line speak a little English. There are large sales of coffee and other delicacies. I get **more** pipe tobacco, cigars, chocolate bars, a bottle of Scotch, which I take back to the hotel. On the way out I see again a kind, sad, English-speaking Czech lady who was in the line behind me, and give her one of my now useless and unused 50-crown Tuzex coupons. She is grateful and tearful, and I wish her good **luck** (which seems fatuous in retrospect).

At the hotel, there are mountains of baggage in the lobby, and the American group heading for Frankfurt are standing around; I say goodbye to some of them. Our hotel monitor and his alternate are in the group, so our well-laid evacuation plans have evaporated; I am on my own. I should have given one of the **Americans** the text of a message to cable home to Helen, but in the confusion it slipped my mind.

1       Then, I set out to walk to the meeting -- a worse trip than the  
2 other way yesterday, despite the good weather, as it is all uphill. I  
3 am exhausted when I get there, and in another sea of perspiration. I  
4 cross over Svermuv Most (bridge); tanks and soldiers are on the west  
5 side, civilians are using the eastern walkway. A tug is towing a coal  
6 barge up the river, looking very peaceful and pretty -- a big Czech  
7 flag on the tug, geraniums in planters above the barge cabin. The  
8 city is very beautiful today, and I wish that I could have used my  
9 camera for a few farewell pictures,

10       At the next bridge (**Svatopluka** Most), before I ascend the hill,  
11 soldiers with helmets and guns have stopped a truck loaded with rolls  
12 of newsprint, and are making the driver rub off the slogans on the  
13 sides -- the first time I have seen that; they are beginning to rough  
14 up the civilians. Hitherto, the troops have completely ignored the  
15 people.

16       At the University, I look around for friends, talk to some fel-  
17 low Americans, say goodbyes to other nationals. The Russians are over-  
18 whelmingly in evidence (the bastards), All of them have taken off  
19 (or have been ordered to take off) their nameplates, which is funny,  
20 because now we can spot them every time.

21       I look around for an American "desk" or an American "bulletin  
22 board," but there obviously isn't any; another well-laid plan that  
23 didn't work out. However, there is a placard in the main lobby stating  
24 that Cedok (the Czech travel agency) will run a fleet of buses to the  
25 Austrian frontier, starting tomorrow morning, perhaps on succeeding  
26 days. This looks interesting, as the timing is good, and it is the  
27 way I want to go. Bob Neuman, Bill Berry, and Earle McBride show up  
28 -- just then, and are interested also, so maybe a group of us can go. I  
29 go to the Cedok desk at the University, but they tell me I will have  
30 to buy my ticket at their downtown 'office; will do this in the after-  
31 noon.

32       I have one important chore yet to do -- to get my Congress publi-  
33 cations and put them in the mail. I had put this off because of the  
34 time-wasting queues at the publication office and the post office  
35 earlier in the week. A hitch; I had managed to leave in my hotel the  
36 receipt which entitled me to receive the publications, and the girls  
37 won't give them to me, despite my pleas; they send me to the registra-  
38 tion office. Here, I marvel at the orderliness of the Czechs, under  
39 terrible pressure; the girls there find my records with no trouble,  
40 indicating my payments, and issue me a duplicate receipt. Then I go  
41 back for the publications, neatly wrapped in two bundles ready for  
42 mailing, and turn them in at the post office with no trouble. There  
43 are mountains of packages there, which are being loaded into trucks to  
44 take away, as fast as they accumulate. Looks like there will be a good  
45 chance of receiving them at home despite the Russians all around us.

1 I am too worn out to go on to the International Hotel to see if  
2 the Map Commission is meeting, but I do see Marcais in the corridor,  
which suggests that it is not. Goodbye Map Commission!

3 We may have another meeting of the Americans at 1:00 or 1:30;  
4 thought we had agreed on such a meeting yesterday, but no one seems to/  
know today. I'll wait for that, hoping to get more information, before  
5 my long, hard walk back to the city.

6 I get a big glass of beer, and find an empty table where I can  
consider the situation and catch up on this diary. While I am writing,  
7 Egaziarov suddenly appears and tries to talk to me., This seemed ill-  
timed and unwelcome, and I gave him a black look. He wants to talk  
8 about his previously proposed exchange of articles between Soviets and  
Americans on the Cordilleras of North America and Asia. I finally  
9 have to tell him, "I just don't feel like it now; maybe we can corres-  
pond about it under better circumstances." He says, "This is a terrible  
10 is a situation, you and your buddies made it!" It would have fulfilled  
a pent-up urge, but on the other hand, maybe he is more affected and  
11 dismayed by all this than one could guess; I hope a lot of the Russian  
geologists are!

12 One of the Czechs gives me a black band to put on my name tag;  
everyone has them on now except the Russians. Saw dear Professor  
Boucek for the first time in the lobby, and it was a sad greeting. He  
14 asked me to take a book out with me to mail to Henry Faul. I hesitate  
at first, because of my already heavy luggage, but realized I would be  
15 a churl not to; it turned out not to be a very big book, after all,  
and it was neatly wrapped and addressed for mailing. About the same  
16 time, one of the women helpers gives me a letter addressed to her son  
in Hamburg, and asks me to mail it when I get outside; which I am  
17 happy to do.

18 A general strike at noon again, this one much more effective  
than the one of the day before. Young people are circulating everywhere,  
19 distributing fliers in English and other languages, signed by this  
technical group and that, -denouncing the invasion. (I-brought out with  
20 me fliers signed by "The employees of the Geological Survey of  
Czechoslovakia," "The scientists of the Czechoslovak Academy of Sciences,"  
21 "Electrotechnical School of the Czech Technical University," "On  
behalf of the engineers and technicians of Czechoslovakia").

22 The beautiful, wonderful map exhibits are going down and dis-  
23 appearing; most of the tack boards are empty now. Alas, I never had a  
real opportunity to study them and record them; I had planned this as  
24 a major project, for most of my afternoons during the last half of the  
Congress.

I go to the appointed meeting room about 1:00, but the corridor is deserted and the door is shut; I confiscate a chair and sit down to wait in the hall. No one shows up for a long, long time -- finally Dick Armstrong and his wife. We find the door unlocked and move in. For want of anything better to do, we write on the blackboard the ways and means we have learned on how to get out of **the** country, thus:

(1) Express train to Vienna at 10:00 P.M. tonight, from Station Praha Vrsovice in southeast part of city.

(2) Cedok buses to Austrian frontier at 9:00 A.M. tomorrow, from, Florenc Bus Station, downtown.

(3) Swiss buses to Vienna at 4:00 P.M. today, from the University. Open to Swiss nationals, holders of Swiss Airlines tickets, any others if there is room for them.

(4) Train to Nuernberg at 10:50 A.M. tomorrow, from Station Praha Smichov in southwest part of city.

Bob Neuman, Bill Berry, and two or three others show up; that is all. Mrs. Armstrong says she went by the Embassy this morning, and they seemed to be in more of a sea of confusion about getting the Americans out than we are ourselves. She thinks the other embassies have done much better with their nationals. Bob Neuman is quite bitter, says the whole American situation has dissolved, not only with our Embassy but with the leaders of our Geological Congress contingent; **is** sure that all of them are now out of the country, leaving us to hold the bag; from now on it is every man for himself. He and Bill Berry, who are staying at the Student Hostel, decide to try the Swiss Airlines deal, which would be quite unfeasible for me from my downtown hotel, so I will lose them as company.

There is one more chance; somebody says perhaps the missing Americans are at the closing assembly of the Congress, which is being held at the big auditorium a few steps down the hall, so I go to it. Both the main floor and the balcony are jammed with people; the aisles and the rears of both floors are black with standees. I push my way in and look around for a while. Certainly there are no Americans at the head table, and from what I can see over the heads of the standees no Americans in the audience; I did see Charlie Behre among the standees, but he was as vague and confused as I was.

A long, long succession of speeches by one national representative after another, mostly in French and German, one in thick British English. There seems to be no end to it, and with the Russian tanks outside, it seems the height of futility. In my present mood, I couldn't care less, and leave in disgust. Wonder if they will present the Spendiaroff Prize, and what young Czech would be willing to accept it? (But, in retrospect, there was real point and need for all this. This has been a disaster for the poor Czechs, who have worked for 4 years to prepare for what would have been one of the best Congresses ever held. They needed all the praise that could be lavished on them.)

(Bob Neuman's surmise about the American leaders was pretty much, true; nearly all of them had left that morning by train for Germany. But I guess there were **extenuating** circumstances; all of them had their wives with them; most of them were at downtown hotels much more dangerously exposed than my own; no doubt there was strong pressure from the Embassy to get them out of there.)

I had been calm and relaxed so far; now I begin to get a **queasy** feeling; I must not panic. I dread the long, hard walk back to town, but it is easier downhill. The weather has gone bad again, to suit my mood, and it begins to rain when I reach the city.

As I walk along the south embankment of the river between Svatoopluka and Svernov bridges, an enormous convoy of tanks is roaring out of the vehicular tunnel from the north, and across **the** latter bridge into the city. What now? The sound of massed tanks roaring into a city is something I will never forget, and something I hope I will never hear again.

I go direct to the Cedok office in the city, a little beyond my hotel. Three or four Finns from the Congress are ahead of me, getting tickets to go north. No, there won't be any Vienna buses tomorrow, after all; it was decided to send everybody to Vienna on the train at 10:00 P.M. tonight. It leave: from the Main Railroad Station downtown, which sounds ominous, but I get a ticket anyway, which requires an endless amount of paper work by the staff. While I am doing so, Earle McBride shows up to do the same thing for himself and Earl Ingerson. Maybe we can get together on the train, or at least in Vienna.

I return to the hotel. The staff at **the** porter's desk tell me that all the guests are being moved **down** to lower floors, the upper floors being abandoned (or turned over to the Russians??). But I say I am leaving tonight for Vienna, so they relent and let me go up and pack. I ask them to order a taxi for me **at 8:30**.

1 In the box with my key is a note -- "10:00 A.M. WC are leaving  
2 by bus for the German border. Hope you are all right and get home  
3 safely. Winonah Bergquist." Dear, dear Winonah! I never much cared  
4 for her during the 20 years we have known each other on the Survey, but  
5 she was the only one of our Americans who had the thoughtfulness to  
6 worry about me. It takes an emergency like this to bring out the real  
7 character in people!

8 It is only a little past 4:00, so I have plenty of time. I rest  
9 for a while and have some Scotch; the bottle is low, and I might as  
10 well clean up most of it rather than pack it. Then I pack leisurely,  
11 considering every item, where it will go, what to abandon. I throw  
12 away large accumulations of paper -- advertising, copies of Czech  
13 journals, mimeographed sheets from the meetings. I debate over  
14 Bogdanoff's gifts; the trinkets are easy to carry, and even the little  
15 book on ceramics doesn't take up much room, but I toss the book on  
16 ballet into the waste basket. I have never been a ballet enthusiast,  
17 and in my present mood it is nauseating.

18 Once packed, I descend early with some of my lighter bags, hoping  
19 to get some food, but to my dismay both dining rooms are dark, and the  
20 lights are nearly out in the lobby. Still more ominous, all the  
21 familiar staff have disappeared, leaving only two men I never saw before.  
22 I must have been somewhat unsteady, which was unwise in the situation;  
23 maybe I had had too much booze, but also I had had no real food since  
24 morning, I was weary to the bone, as well as emotionally jolted.

25 I say I am taking the train to Vienna, please bring my bags down  
26 and get my taxi. They say, "Not possible, Russian tanks and machine  
27 guns all about!" I am taken back to my room with what baggage I had  
28 carried down, and they say, "One moment, please!" This is the good  
29 old East European brushoff, and I recognize it for what it is. I sit  
30 on the edge of the bed to pull myself together; the "moment" becomes  
31 half an hour.

32 I descend again to get some more positive results. This time, one  
33 of the women of the staff is at the porter's desk and I get real help.  
34 She says, authoritatively, "It is impossible for you to go to Vienna  
35 /tonight. Your Embassy has been notified you are here and will help you  
36 tomorrow, not tonight. All you can do now is go back to your room and  
37 go to bed!"

38 Thus ends a bad day.

1 Saturday, August 24. Prague to Nuernberg

2 A quiet night, but an uneasy one. To add to my bigger miseries,  
3 my usual European cold or flu is setting in -- happily, two weeks later  
4 than usual. What a wonderful joke on the stupid little viruses! They  
5 thought they were going to ruin the last half of my travels, just as  
6 /they always do, but I'm fooling them by going home now!

7 Descending in the morning, I find the lower floor of the hotel  
8 nearly deserted, but the breakfast room is open. Breakfast is minimal --  
9 no orange juice, no egg, just rolls, jam, and coffee. Perhaps it is  
10 just as well, as it is hard to eat.

11 Back to the lobby, I go to the reception desk for information  
12 and find the nice blonde receptionist on duty again. (I had not seen  
13 her for several days, and there was a rumor that she had fled to Austria).  
14 She has written out a message for me -- a train to West Germany will  
15 leave Smichov Station in the southwest part of the city at 10:50; my  
16 Embassy says, "take it!" I start to demur that I was hoping to get to  
17 Vienna, but obviously this is no time for quibbling.

18 But how? All the taxis have disappeared. The receptionist sug-  
19 gests a tram; says a hotel porter can take me to it, and that it goes  
20 somewhere near the station. I object that I have a mountain of baggage,  
21 which would be impossible to transport by tram or on foot. She gets  
22 the Embassy on the phone for me, to seek further advice, but of the  
23 several people I talk to, all must be Czechs and none of them speak  
24 clear English. I get the vague idea that somebody may come for me.  
25 The receptionist quizzes me, what did I find out? She emphasizes that  
26 this is very, very serious and important, and there must be no misunder-  
27 standings. Finally, she calls the Embassy herself, talks to them in  
28 rapid-fire Czech, and gets it straight. Yes, a driver will come for  
29 me at 9:30, so "sit down and wait."

30 (There were complaints about the Embassy among the Americans,  
31 maybe deserved; some told me in Germany that the Embassy told them to  
32 'abandon their baggage and flee with light hand bags. I, at least, did  
33 get substantial help for both myself and my baggage. But I am inclined  
34 to think that it was not so much due to the Embassy as to the alert  
35 Czech staff at the hotel, who hounded the Embassy in my behalf until  
36 they had to do something! What wonderful, kind people!)

The only other Congress attendants left at the hotel are a Ceylonese geologist, his wife, and his boy -- P. W. Vitanage, Department of Geology, University of Ceylon, Peradeniya, Ceylon. He is a pleasant, eager fellow. We talk about the situation here, then he begins to tell me about the paper he was scheduled to present at the Congress, on the tectonics and morphology of Ceylon. This would be of interest to me at any other time, but right now I am hardly in the mood for it, and wish he would stop. I fight a terrible desire to vomit, and finally control it.

Just then, the receptionist brings over another Czech woman, distinguished, **well'educated**, fluent in English, who gives me a letter to mail outside to a man friend in California. She also asks me to telephone him when I get home; for this, I would need to know her name, but she says, "Just tell him it was Doctor Eva." She begins to weep, and I put my arm around her to try to comfort her.

The driver arrives on schedule, in a diplomatic station wagon. Alas, he speaks no English, French, or German, only Czech, so we have communication problems. He makes another downtown stop at the Palace Hotel, and I hope I will have some American company, but nobody is there -- then off toward the station. Getting through the city is hairy. In one narrow street a tank blocks the way, but he drives around it on the sidewalk. Many streets are torn up or blocked, probably because of the disturbances of the last few days. He tries to cross the river by the bridge next to the Opera House, but the approaches are barricaded. This necessitates a lot of back-tracking and detouring, but the next bridge farther up is still open, and once across the river the station is in view ahead.

I had feared the driver might dump me in the street outside the station, leaving me to fend for myself, but he carries through to the end. He parks, locks, carries my bags for me through the station, asking directions, and **gets** to the right platform and right train. He helps me search through the train for a vacant place, and we finally end up in the first class car at the end. After helping me to stow my bags he motions for me to follow him outside. I am not quite sure what to do next; I reach for my purse to give him **money**, but he waves it away. So far as I can see, all he wants to do is shake hands and wish me farewell, which I do with fervor. Hop!: that was enough! Wonderful, brave guy:

In our car I find **Hollis** Hedberg and his family (at least one leader stayed to the end), Marie Segrist, many others. The train will not leave for nearly an hour, lots of evacuees are still not here, so there is still room to shuffle around and find places. I end up **sitting** by Marie in a half-compartment with three seats.

1        Mario has never been my most favored girl-friend, but (as in the  
2 case of Winonah) one never knows what an emergency will do. She is  
3 very kind and sweet to me, insists on helping me like I am an old man  
4 in need of assistance. I guess I looked it; I was exhausted and ill  
from all I had been through, and I had my Swiss cane in my hand, which  
I was determined to take back to America (quite a stage effect).

5        Our companion in the third seat was an odd one -- a distinguished  
6 lady of German ancestry, living in London, who read English magazines  
7 all day. Eventually, we found out that her son had married a Czech  
8 girl and that they were living on the outskirts of Prague, where she  
had been visiting them. He and his wife are stuck here because they  
are caring for her mother, who is very ancient and cannot be moved.  
The son had driven her in this morning and has seen her off from the  
platform; looked like a fine guy. What will happen to him?

9        We sit and we sit; I am not at all confident that we will go at  
10 all. Contrary to rumors, the railroads are not blocked; many local  
11 trains are leaving the station in various directions. The German lady  
12 reports that a helicopter is hovering over the station area, dropping  
13 leaflets that tell the natives the Russian version of the story! How  
14 futile! What Czech would possibly pick one up, let alone believe it:  
The train fills up; all the compartment seats are taken, baggage and  
standees fill up the corridors.

15        Saw Earle McBride on the platform, waiting for the train to start;  
16 he and Earl Ingerson didn't make the Vienna train either last night.  
17 Even if I had made the train, I would not have had their company, after all.

18        Now that I am safely on the train, I face another gnawing worry.  
19 Everyone else has bought a ticket to Cheb, the border town; my driver  
20 didn't help to buy me one too. I fear bureaucracy, being put off the  
21 train, etc. I consider handing the conductor my ticket to Vienna.  
22 There is some talk of one of the other Americans going back to the  
23 station and getting me a ticket, but nobody does, and everyone assures  
24 me there will be no trouble. (Despite my fears, no conductor ever came  
25 through to collect the tickets; if he had attempted it, he would have  
had his work cut out for him for the whole day, with this great horde  
of passengers).

26        There are conflicting rumors as to what the train is going to do --  
27 that it will dump us at the border, whence we will have to carry our  
28 bags to a German train -- that it is a through train going direct to  
29 Paris. Some of the cars are certainly Czech, but ours is a standard  
30 international European car, with French advertising, and pictures of  
31 French vacation spots. (Actually, everyone was wrong; the train did  
32 go through, but to Frankfurt, not to Paris.)

1 Promptly at 10:50 the train leaves after all, picking up speed for  
2 awhile, then slowing down ominously. But the slowdowns are just for  
3 suburban villages, where there are many grade crossings. (We went  
4 right through; I learned later that the same train yesterday stopped  
mysteriously many times for long periods, including the 1-hour general  
strike at noon,) The roadbed is very bumpy, and we are badly jostled  
when we pick up any speed.

5 I think we went up the Vlatava River for quite a while, then west  
6 through Beroun, as much of the first part of the journey looked very  
familiar from 1962. Anyway, we were in the stratified Paleozoic rocks  
of the Barrandian Syncline for a long, long time. (The geologic map  
shows that these rocks extend along the rail line nearly to Pilsen.)  
Afterwards, we see crystallines, and later maybe Carboniferous.

7 The beautiful, sad countryside of Bohemia rolls past the windows.  
8 Alas! poor Czechoslovakia! There are forests, low mountains, fields  
9 with hay neatly stacked, villages, little industrial towns. Although  
10 the route is generally westward, the line is very twisty, following  
11 the valleys and the contours. We saw one village with its onion-domed  
12 church for nearly half an hour, from almost every direction, as we  
circled around it, apparently ascending a long slow grade.

13 No Russians are in evidence; I expected that the countryside  
would be swarming with them. Apparently they are all in Prague, over-  
14 awing the capitol, leaving its surroundings to "wither on the vine."  
15 Slogans are everywhere -- on freight cars, stations, walls; flags are  
at half mast. Someone points out that all the red stars have been  
16 ripped off the front ends of the locomotives, and the Czech emblem sub-  
stituted (a circle, divided three ways into red, white, and blue). All  
17 this was done in the last few days, for the stars were still there during  
our field trip in Slovakia. In fact, it is easy to see from the re-  
18 painting where the stars once had been. A very efficient operation,  
for what good it will do; at least the Russians will have a big job  
putting them all back!

19 Someone tells us that by 5:00 P.M. yesterday the Technical  
University was deserted of geologists, Czech and otherwise, and that  
Russian troops were taking over the building, ripping down posters as  
they moved in.

20 Someone said yesterday that the situation was stabilizing and that  
'life was getting back to normal. Someone else said that when the  
Russians got all the foreigners out of the country, they would really  
'set to work roughing up the Czechs. From what I saw yesterday, there  
21 is every reason to believe the latter, alas!

A fat American Jewish lady, a real New York Bronx type, wanders moaning through the train, looking for sympathy. Will the airlines honor her tickets and get her new reservations? (Of course they will! Why worry so'!) Also, her baggage, which she left in the Prague Airport and was trying to retrieve. The Embassy told her to stop whining, save her skin, and get out of the country fast! I doubt if she will ever see her silly baggage again; reports are that the Russians have made a shambles of the airport!

A long stop at Pilsen, where the locals hand the passengers free bottles of beer through the windows. Will we proceed? Another stop at Marianbad, where there is a tremendous thunder and hail storm; at first I feared that the claps of thunder were gunfire. I am glad I am inside, leaving this wet Europe, instead of being outside on another field trip.

There is no formal food on the train. The son and daughter-in-law of the German-English lady had filled her traveling bag with ham sandwiches, buns, and little cakes, which she shares with me and Marie; I have my two Swiss chocolate bars besides, and offer them around in return. But I am pretty much repelled by the sight of food, and find it hard to eat anything.

Finally, we roll into Cheb, the last town in Czechoslovakia. Still no Russians, border formalities are entirely in the hands of the Czech officials. A long wait, but not an ominous one; it takes much time to clear everyone on this crowded train. First, passports are inspected and visas taken up. Then, we turn in our money statements. Then, a walking "bank"; a woman who exchanges our Czech money for German marks. Bags are not opened or even looked at; I had feared confiscation of films and notebooks. At last, an official comes through, saying rather sadly, "Auf wiedersehn!", and we are free.

A short run to the frontier, marked by a high barbed-wire fence, and a heavy gate on the adjacent highway. Here, we see the first Russians after leaving Prague; I thought they would be there in force, but there is only a single tank. Czech officials are checking cars in and out on the highway without interference from the soldiers. We roll through the border without stopping, and are in West Germany at last. The country looks better already. Maybe it is just that the sun is out again after the rain, but the German landscape does look different. The farm houses are more spruced up, brighter colored, many with Tudor-style half-timbered ends. The sad, drab little villages of Bohemia and Slovakia are behind us.

Quite a run to the first town in Germany (Schirnding). Long wait at the station, but not as long as in Cheb. Back to civilization; a man with a caterer's cart on the platform is selling beer, coca-cola, sandwiches, candy bars, etc., which helps pass the time. Passport inspection is perfunctory; they just check off the nationalities, never look inside.

We are O.K., but the German-English lady tells us that a mysterious group are not. A man who said he was a West German had stored a huge suitcase in our compartment, which he now takes down so he can open it in the corridor. I was indignant at this foolishness; tell him he can save himself all that trouble by opening it right here on my seat; I'm willing to stand while he does it. But he refuses. Our friend tells us his own papers are in order, but that those of the three people traveling with him are not; they are Czech refugees without visas. All of them are asked to get off to complete the formalities, and take the next train; evidently they will not have too bad a time of it.

Sale of tickets by the German conductor. Our German-English lady elects to buy one to Frankfurt, as she is going on to London next day. The train will not reach there until late tonight, but she predicts that it will be easier to get a hotel room there, as everyone else will get off at Nuernberg, and jam up the facilities. However, we Americans decided to end our trip at Nuernberg; it will be long after 7:00 when we get there, we are all bushed, and need a good night's sleep as soon as we can find lodgings. Complicated financial arrangements about tickets; Marie and I have a small, assorted amount of German money between us, and the conductor ends up selling us a double ticket! We have fun about our sudden and unexpected liason.

The roadbed improves vastly in Germany; the train picks up real speed, but we are no longer tossed about. Beautiful country in the setting sun. Limestone topography -- cliffs, towers, pinnacles, isolated rocks -- an extension of the Jurassic of the Schwabian Jura, which I saw farther west in Baaria in 1960.

It is well after dark when we roll into Nuernberg. Hardly had the train stopped at the station than a German oaf rushes into our compartment saying, "Are these seats taken?" and won't leave. I say, "Dammit, they are still taken until we can get our baggage out!" I had to hit him in several places with Marie's baggage and mine, not to speak of my wonderful Swiss cane to make enough room to leave -- all "accidental" on my part, but I made no apologies. Once on the platform, I say good-bye to Marie with heartfelt thanks, and give her a well-deserved kiss.

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When we were out of Czechoslovakia I'm sure we all felt our big problem was solved, leaving us to worry about our little problems, such as where we were going to sleep tonight. Me, anyway. It didn't seem to worry competent big wheels of the Hedberg type; they had snagged porters and carts and were disappearing down the station platform, as though they knew exactly where they were going.

I put my bags down on the platform, wondering what to do next, but my little problem was dramatically solved for me. A loud-speaker is booming out in German and English, "Attention please! All refugees from Czechoslovakia in need of assistance please go to the nearest U.S.: Military Policeman! Achtung! etc. etc." (The American Flag to the rescue! Hooray for the U.S. Army, whose presence in Germany most of us had thought unnecessary!).

I don't need to go for assistance, help comes to me. A civilian helper takes me and my bags to a Lieutenant Colonel in fatigue uniform, who says, "Relax, just wait a bit here and you will be taken care of!" I am happy to do so. While waiting, I am amused to note that all the confident big wheels have come back with their tails between their legs, and have ended where I am.

We are escorted to a loading place outside the station, where buses will pick us up. They are slow in arriving, so Army sedans are pressed into service too, and I am put into one -- another bonus today for looking so old and having a cane! My fellow-passenger is a ringer, a little Texas high school girl from near Amarillo who had been spending the summer on a farm in southern Bavaria. She had wandered into Nuernberg that evening for a few days of sightseeing and decided she would get some free hospitality also.

I see the Armstrongs later, and they marveled at the efficiency of this operation, and its contrast with the flabbiness of the official American help at the other end, in Prague. Still, our Embassy in Prague did get me to time train, so I can't complain. (Also, as I learned still later, all was not as rosy in Germany as it appeared; this operation was only set up today. Yesterday, nobody but nobody met the evacuees, either here or in Frankfurt, and some of them slept in the stations all night).

Anyway, it all seems marvelous just now, and we are very grateful. We are taken to the U.S. compound in Furt, at the edge of Nuernberg. We receive coffee, sandwiches, cigarettes, later soap and towels; then we are processed:

(1) We register, giving names, addresses, passport numbers. The girl who takes care of me is a service wife who grew up in Menlo Park, of all places!

(2) Then, the Red Cross, where we can send messages home. My message to Helen -- "Safe and well in West Germany; home soon; notify Survey people." This, and the other people's messages, will be telephoned out from Washington tomorrow morning. My message seems short and curt, and I realize there is little love in it, but I am too tired and confused to embellish it; at least it will be wonderful news to her.

(3) Military intelligence (our only obligation in return for all this); I am happy to tell them all I can, and feel it is my duty as a citizen to do so. My interviewer says they have had little information about what has been going on in Prague after the first day of the takeover.

(4) Billeting (in the Army barracks). I insist on a room to myself; I snore badly at best, and my snores should be resounding tonight, since I am so weary. Besides, I want privacy so that I can sit up late and write all this down before it escapes me. Accommodations are minimal, like a college dormitory -- double tiered beds, a table and chair, little other furniture, wash room and toilets across the hall, What the Hell, it's home, back under the U.S. Flag! The Russian tanks seem far, far away.

What I will do next I must decide in the morning -- probably move to a German hotel downtown, see the airlines as soon as possible, and arrange for a rescheduled flight home. I learn that it is not necessary to go to Frankfurt for a plane (as some have assumed); there are connecting flights out of here to Frankfurt via Lufthansa, after which I can get an American flight the rest of the way. It is now fruitless to make my way to Vienna; it would mean many hundreds of kilometers back-tracking. I have had a healthy, rewarding four weeks in Europe so my European trip has not been wasted. Now, I am far from well, and had better get on home and get cured,

To bed at last, after 1:00 A.M. I sleep the sleep of the Gods on an American mattress and under American blankets. Farewell to hard, narrow Czechoslovakian beds and their covering of down comforters!

The First Book of the New Tectonics

Commonly called "Genesis"

In the beginning God created the mid-ocean ridge, The rest of the earth was without form, and void and darkness was upon the face of the ~~ridge~~ and the Spirit of God was moving over the face of it.

And God said, "Let there be volcanoes," and there were volcanoes, And God saw that the volcanoes were good. And there was evening and there was morning, one day.

And God said, "Let the outpouring from these volcanoes be called lava, and let the lava congeal to form a rock." And God called the rock **basalt**. And God said, "Let the basalt spread laterally in each direction from the crest of the ridge," And God saw that it was good. And there was evening and there was morning, a second day,

And God said, "Let there be great masses of granitic rock which form on top of the basalt." And God called these masses continents. And God said, "Let there be island arcs to stand off the continents, at which points a great slab of basalt is to be jammed downwards and ~~partially~~ destroyed by **vulcanism**." And God saw that it was good, And there was evening and there was morning, a third day.

And God said, "~~Let~~ there be waters on the face of the earth to fill in the gaps between the continents and to cover over the basalt." And God called these waters the oceans, and the ridge from which the ~~ocean~~ floor is created the mid-ocean ridge, and the parts of the ~~contin~~ ~~ents~~ submerged by the oceans the continental shelf and slope. And God saw that they were good, And there was evening and there was morning,

a fourth day.

1 And God said, "Let there form on the continent pairs of **troughs**  
2 **Which** will become filled with sediments." The pair he called an or~~tho-~~  
3 **geosyncline**, the shallower of the two a miogeosyncline, and the **deeper**  
4 ~~one~~ a eugeosyncline. And God said, "Let the sediments that collect in  
5 the miogeosyncline be well sorted and let them form **quartzite**, or **pure**  
6 **quartz** sandstone, and let those sediments which collect in the **eugeo-**  
7 **syncline** be poorly sorted and let them form graywacke, or dirty **sand-**  
8 **stone**. And let first the **eugeosyncline** and later the miogeosyncline be  
9 **uplifted** to form **mountain ranges**. And God saw that all these things  
10 ~~were~~ good. And there was evening and there was morning, a fifth day.

11 And God said, "Let us make geologists in our image after our **like-**  
12 **ness**, that they may study the earth, map it, draw structure section~~s~~,  
13 write geological histories, publish geosynclinal theories (and **refute**  
14 **them**)?" And God saw that these geologists were good. And there was **eve-**  
15 **ning** and there was morning, a sixth day.

16 Thus the heavens and the earth were finished and all the host of  
17 **them**. And on the seventh day God finished his work which he had **done**,  
18 and he rested on the seventh day from all the work which he had **done**.  
19 So God blessed the seventh day and ordered all geologists to **observ**  
20 **t**, because on it God rested from all his work which he had done in  
21 creation.

22 Andrew King

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