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EARTHQUAKES, MINERALS AND ME: WITH THE USGS, 1942-1995

by

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Oral History Interviews With
Stanley Scott[2]

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FOREWORD

This is one of a series of oral histories being prepared to record the development of earthquake engineering and seismic safety policy. The interview program was initiated by Stanley Scott, and is being conducted by him under the general auspices of the Regional Oral History Office of the Bancroft Library, University of California at Berkeley. Now retired after many years a research political scientist at the Institute of Governmental Studies, UC Berkeley, Scott has also had a major part in the development of earthquake hazard reduction policies, and served for 18 years on the California Seismic Safety Commission.

Oral history interviews are transcribed, revised and edited with the intent that they and the tape recordings be placed in the Bancroft Library for research and scholarly use. In the case of the Scott's earthquake engineering oral histories, the Earthquake Engineering Research Institute's interest led to the Henry J. Degenkolb and John A. Blume transcripts being published in Connections: The EERI Oral History Series. Others will follow in due course.

Meanwhile my oral history is being issued at this time as a USGS Open-File Report. While much of this material is based on recorded interviews between Wallace and Scott, some parts were composed by me on a word-processor, but using an interview format. This was merged with the interviews, and the result was extensively edited and reorganized, all with my participation and approval.

I hope my oral history will provide insight into:

1. The U.S. Geological Survey, and its traditions and mode of operations as observed during my tenure there.

2. The struggle to develop an earthquake-hazard-reduction program for the nation as well as the USGS.

3. Anecdotes about people and personalities that emphasize the important ways individuals influence the course of events.

This history basically covers my time at USGS for a period of 53 years--1942 to 1995. I retired in 1987 but continued for a few years as a part-time USGS employee. This was followed by an emeritus designation. While in emeritus status, I witnessed some of the drastic changes introduced by new USGS Director Gordon Eaton during part of 1994 and throughout 1995.

Special attention is given to the earthquake program from 1964, when the great Alaskan earthquake occurred, to 1977, when the National Earthquake Hazards Reduction Program (NEHRP) came into being. After my retirement from the USGS in July, 1987, I was no longer close to the national program as it continued evolving or to priority changes in the earthquake-hazard-reduction program of the U.S. Geological Survey.

In an oral history memoir, the interviewee focuses on events and developments observed personally. I of course witnessed only a small part of the total action going on at USGS, or even in the Menlo Park complex where I spent much of my career. I was not oblivious to the other things going on, but in this oral history I have tried to minimize second-hand reporting and hearsay information. I have tried to stay largely with what I myself observed.

Understandably some readers, particularly colleagues in USGS, may feel that I have neglected or underemphasized important activities or overlooked influential individuals and major turning points. They should understand that in large part this is due to the very nature of a personal oral history and the limitations of its principal source--the interviewee's own recollections. I acknowledge, however, that many omissions are intentional and made in the interest of brevity while retaining overall continuity and clarity.

I want to thank W. Porter Irwin, Hal T. Morris, George W. Walker, and Jerry P. Eaton, for their reviews of this document in an earlier form. My sister, Harriet E. Wallace, verified most of the early personal history. Robert D. Brown, Jr., and Nancy L. Blair read an early draft, encouraged me at a crucial time when I was disappointed with what I had done, and urged me to persevere. Thomas C. Hanks, Malcolm M. Clark, George Gryc, and Laurie D. Hodgen provided very helpful reviews. Hours of dedicated effort by Stanley Scott, interviewer, transcriber and editor, greatly improved the structure and organization. In the give and take of the interviews Scott's comments have added important information and insights.

Robert E. Wallace
Menlo Park, California, 1995

I EARLY YEARS, THROUGH GRADUATE SCHOOL

Family Background

Wallace: My name is Robert Earl Wallace, and I was born July 16, 1916 on Manhattan Island in New York. I like to tell people that cows and horses grazed on Manhattan Island in New York City near where I spent the first six years of my life. It was only a small meadow, but we did pass it on our walks from our apartment on northern Manhattan Island. Also there was an empty lot across from the apartment where rocks were sticking out--probably Manhattan schist. Older kids roasted potatoes in rocky crannies on this empty lot, and at age four or five I was impressed.

My parents, my mother in particular, would take us on picnics. We would walk over to the Dyckman Street Ferry, which we took across from Manhattan to New Jersey, and go picnicking on the west shore of the Hudson River, in the shadow of the Palisades of New Jersey. The Palisades are a geologic structure--the Palisades sill--as I found out decades later. That was exotic country for me. I am sure that was the start of my life-long love of exploration, nature and science.

I have a sister a year-and-a-half older--Harriet E. Wallace--who got interested in geology at Northwestern. She decided to major in it, mainly in economic geology and mineralogy. When we were teenagers, her interests led us to do some mineral collecting, zeolites and such, at Patterson, New Jersey, in the Palisade sill along the Hudson River, and exotic minerals at the famous site at Franklin Furnace, N.J. and elsewhere. That was my first taste of geology.

Harriet went on to work as an office geologist with a major chemical company in New York. She married another geologist, William C. Smith, who worked for many years with the U.S. Army Corps of Engineers, and with the

Geological Survey of Illinois. She eventually became head of the great geology library at the University of Illinois at Champaign-Urbana, where she was given a full professorship. During those years, she became a major force in starting the Geosciences Information Society and was elected its second President.

Scott: Say something more about your parents and about your grandparents.

Wallace: That is certainly part of the beginning. My father, Clarence Earl Wallace, was born near Van Wert, Ohio. He went off to the Garrett Biblical School at Northwestern University to become a minister. However after he graduated he had sufficient training to go back to Van Wert and teach physics in high school.

My mother, Harriet Wheeler, was born in Plano, Illinois, and grew up in the Oak Park area. She also went to Northwestern where she met my father. On my mothers side of the family, there were two aunts and a great uncle who went to Northwestern. My maternal grandmother, Clara Ryon, was born in California. In 1860 her parents decided to go west with the big westward movement. At the time, my great-grandmother was pregnant with my grandmother, who was born in Woodland, California, in August 20, 1860, just five days after the folks arrived there in a covered wagon. Her father, Hiram Ryon, practiced law there.

My grandmother used to say that her folks thought the three or four months of traveling across country in the covered wagon in 1860 was one of the most wonderful times of their lives. They just went along the trail, which by then was well marked, and traveled with other family members in four or five wagons. One uncle was only 1-1/2 years old. They had a wonderful trip for the most part, but at one point they ran across some Indians who wanted to buy the infant. The folks became worried then and joined a bigger wagon train for a while. So, you see, we have something of a family history connection with California from way back.

When my grandmother was seven, the family decided to move back to Illinois. They went by boat and crossed possibly through Nicaragua to the Gulf of Mexico, where they took lighters out to ocean-going vessels. Then they traveled by ship to New York, and back to Illinois by train. My grandmother later married John C. Wheeler, my maternal grandfather. I like to think about that early family history, and identify with it.

Scott: Yes, you have a long-standing family connection with California and the covered wagon era.

Wallace: Now back to my more immediate family background in Illinois and Ohio. As I said, several on my mother's side, including my mother, went to Northwestern.

My father was the only Wallace who went to Northwestern. My mother and father were really great parents. They had a sense of adventure. One of my mother's frequent comments was, "You can become anything you want to be." So my sister and I did. And, with my mother's encouragement, my father had the sense of adventure to go to New York and become an art teacher. My mother--born Harriet Wheeler had a lot of artistic talent. She had majored in biology at Northwestern, and we still have beautiful lab drawings she did of frogs, salamanders and such. Her mother, the Clara Ryon Wheeler whom I have already mentioned, also produced several beautiful pencil drawings of flowers.

Several of my father's early paintings suggest that he was not actually very artistic, but he had a lot of conviction and drive, and did become a very successful commercial artist and teacher. He wrote a book titled "Commercial Art" for McGraw-Hill which became a much-used high school textbook then was re-published for the military, which had a big publishing spree in the middle of World War II, distributing books to the armed forces. My father was also on the boards of directors of various art institutions and societies. For someone who was not basically artistic, he did very well.

My parents were both teachers. My father taught art in high schools in New York City, and my mother taught elementary school. After we children reached a certain age, in the seventh and eighth grade in Demarest, New Jersey, my mother actually became my teacher. The family had moved from Manhattan Island to New Jersey, first to the little town of Cresskill, and then to Demarest in 1932, which was my official home address until I finished graduate school.

Developing Interests: Nature, Rope Spinning, Radio, Minerals

Scott: Say a word or two more about some of those early-day experiences, and especially about some of your own developing interests? You mentioned picnicking with your mother and sister and collecting rocks.

Wallace: I remember walks with my mother, looking at bugs, flowers and the like. I found such things very exciting. My early life seemed to be dominated by a series of what I sometimes call "obsessions"--when I would be totally fascinated by something. This capacity for intense motivation has been one of the most precious things in my life.

Scott: With that kind of fascination with what you were doing, or what you wanted to do, you did not have to push yourself.

Wallace: No, I certainly did not, at least not for the subject of the "obsession" of the moment, although my other chores did suffer at times. I liked to go to bed at night thinking, "Tomorrow I will do this, and this, and this, and won't that be exciting?" As I have gotten older, I have tended to lose some of that desire or drive. Perhaps it is a matter of waning energy levels.

Scott: Say more about some of those obsessions. What were some of those major interests that fascinated you?

Wallace: When I was about age eight or nine, we had moved to northern New Jersey, across from New York City, and bird life captured my attention. There were all sorts of swamplands and deciduous forest--mini-wilderness--in New Jersey in those days. There were beautiful glacially-formed hills and the rocky Palisades. It was wonderful country for a kid. In the little towns of Cresskill and Demarest I could be out in the wilderness in minutes, looking at birds. I remember springtime so well--in May--being absolutely thrilled with the migration of the warblers. All the eastern deciduous forests have this marvelous annual event, the northward migration of the warblers. Many of the birds are in vivid colors at that time of the year, and myriad songs filled our yard and the surrounding woodlands.

I was fascinated by the birds, and we put up many feeders for them. I remember being so excited when the first birds visited the feeder that I used a whole pack of my father's expensive camera film. I did not get one good picture, but at the time I thought I was getting a rare photo every time I snapped. My father had built a darkroom in our home in Demarest, and before that he and I had developed pictures in a closet. I grew up with photography, mixing chemicals for the developer and hypo and so on. Photography has been a theme all through my life, but in a secondary way--mostly as a tool.

Then a year or so after getting interested in birds, I saw a book on trick and fancy roping, and rope-spinning a la Will Rogers. For a time all I could think about was spinning ropes. I wanted to be a cowboy, but could not have a horse, so that desire took another form: "Let's learn how to spin ropes." Using clothes-line rope, I taught myself by trial and error, with that one simple little book. My ability developed, and by high school, when the big rodeos came to Madison Square Garden, I was very excited.

I don't know how my father did it, but somehow he arranged for me to have an hour with the world's champion trick roper, Chet Byers. I demonstrated what I could do for this famous roper, and he gave me pointers. I got some of his special ropes, and--to brag--I got to be rather good at rope-spinning. While I was at Northwestern, I was invited to one of Chicago's fancy nightclubs as a floor show item.

Scott: By that time you would have been about high school age, I guess. Say something about high school and your interests there.

Wallace: I went to high school at Tenafly, New Jersey. (Tenafly is an old Dutch name, as is Cresskill.) The area was called the Northern Valley with a newspaper called The Northern Valley Tribune. In high school I had the usual college-prep courses, although I was always very heavy into science. I loved biology, zoology, and physics--you name the science and I loved it--but it was not the same with history or language.

My sister's interest in mineral collecting got me into geology before I finished high school. Through high school, however, I had thought of going into ornithology. With that in mind, in my senior year I went to the American Museum of Natural History in New York City, walked into the ornithology department, and asked if there was anybody I could talk to about becoming an ornithologist. I was admitted to the office of a Dr. Zimmerman. Dr. Zimmerman told me that, for earning a living, if I had any other interests, I had better pursue one of those, and consider ornithology a hobby. I said, "Yes sir."

Scott: I guess paying jobs in ornithology were not plentiful.

Wallace: You are right. While talking about hobbies, I cannot ignore ham radio. My father introduced me to radio-building--he had built several in the 1920s. I got my ham radio license at age 15. I had to go into New York City to take my written, theoretical test, the code test and so on. I had learned the code with a key made out of a hack-saw blade. Radio really became a passion. By the time I was getting out of high school I thought I would major in electronics and physics. At Northwestern, I took physics and enjoyed it. I took zoology and enjoyed that. These were both taken with the idea of following up on ornithology, or electronics and radio. Photographing birds became a passion, and that required invention and building remotely controlled electromagnetic shutter trips--winding the electromagnetic elements from scratch. With such gadgets I did get some good photos. My very first publication, in fact, was a bird photo published in a magazine for youngsters.

Northwestern: Choosing Geology as a Major

Wallace: I went to Northwestern in 1934, having taken one extra year of high school. My family could not afford to put my sister and me into college at the same time. Also, I was sort of immature, so they thought an extra year in high school would be good, and it was an excellent idea. I had skipped a grade somewhere along the way, and thus finished college at 22, which was about when others were completing college back then.

Wallace: Northwestern, our "family's" university, was a marvelous school. By the sophomore year, however, I could not make up my mind on a major subject. I knew something about minerals by then, and also I had a very good geology teacher at the beginning, Dr. C. H. Behre. I thought, "I'll procrastinate in deciding on a major--meanwhile I'll major in geology. That way I can go into physics through geophysics, or into biology through paleontology." I liked the out-of-doors--my favorite times were wandering the hills. The idea of working out-of-doors turned the trick in terms of majoring in geology, which also seemed to provide the possibility of a variety of kinds of careers in science.

The geology department at Northwestern had, and I understand still has, a sense of "family." The professors paid close attention to the students and took a personal interest in them. This was very good for me. I was not an extrovert, but was a loner type, and timid. I was out looking at birds, or into ham radio, or almost anything but socializing. So it was a fine atmosphere for me. Two geology professors, Charles Behre and Art Howland, were wonderful role models.

At Northwestern I got the basics in geology, structural geology, mineralogy, petrography, paleontology, and a course in weather and climate--a nicely rounded background. I did, however, get one very bad piece of advice there. I had a paleontologist as an adviser, and in my senior year we reviewed the courses I had taken. I had taken math through calculus, and my advisor told me I did not need to take more math to graduate. He discouraged me from taking more math, and that was a mistake. I should have followed up on math when it was easy for me and I liked it. Once one breaks the sequence in math, it is hard to recapture. So I never really progressed in math as I would have liked to.

In the summer of 1937, between my junior and senior year, I got a job with Al Hoagland, a consulting geologist in Val d'Or Quebec and a graduate of Northwestern. He needed a field assistant, and promised to pay my way up there and back, and to give me some experience. It was a summer when everything I touched was new, and, as you know, a youngster is like a sponge. I had never used a transit or a dipmeter before. I set the transit up and figured out from basic trigonometry what I should do. That experience was so very valuable.

In my senior year I applied to various graduate schools. When I started college, I had no idea that I would go on to graduate school, but I guess my professors began talking up things like that, so I applied at Michigan, Minnesota, and Caltech. It seems that there was a close association between the faculties at Northwestern and Caltech. For a period of ten or fifteen years they had traded students back and forth. A number of my friends started at Caltech and then went to Northwestern, and a number of us started at Northwestern and then went to Caltech.

Wallace: I did not know all this when I applied. A very fine fellowship was offered by Caltech first, although I was shooting for Minnesota. One day, my professor, Jack Stark, asked me if I had heard from Caltech, and I said, "Yes, I got an offer." "Did you reply and accept?" he asked. I said, "No." He said, "Well, for Christ's sake accept the offer!" I had heard how tough it was at Caltech, and I thought that probably I couldn't hack it there. But I said, "Yes sir, I'll accept."

To Caltech

Wallace: A few months later I was on my way to Caltech, on the Southern Pacific.

Scott: When did you go to California and to Caltech?

Wallace: I finished at Northwestern in 1938 and went on to Caltech for four years. At the end of the Southern Pacific train trip west, I landed at South Pasadena in what seemed to me to be absolute paradise. Palm trees were swaying, Scrub Jays calling, and the skies were beautifully clear. A high-school friend met me, and we stopped at a stand to have all the orange juice we could drink for ten cents. It was really superb.

I moved into Arms and Mudd, the brand-new buildings for the earth sciences on the Caltech campus. Dick Jahns was my next-door neighbor. In later years his name became well-known in California, and nationally, as Dean of the School of Earth Sciences at Stanford. I consider Dick just about the smartest person I ever knew. And what a constant flow of jokes, including bawdy and practical, he brought to every occasion. Caltech provided a superb experience.

After a couple of years I got a master's degree and took preliminary orals, an exam given to see whether one would be permitted to go ahead for a Ph.D, which was a separate program from the master's. Caltech did not make it a continuum as some schools do. Some friends, who I knew to be much smarter than I, had taken their preliminary orals ahead of me and failed. So when I came up for my orals a few weeks later, I thought, "This is the end of the road." Strangely, however, I passed.

I was pleased, of course, that I got through, and went on for the Ph.D. at Caltech. I started in 1938, and by the end of 1941 the nation was involved in World War II. But even before that the draft board was breathing down our necks. Ian Campbell, then a professor at Caltech, and later well-known here in California as head of the California Division of Mines and Geology, and a lot of other things, was a member of the draft board. I believe he helped protect some of the students at Caltech until they could get their Ph.Ds.

The Caltech Program and Some Memorable Teachers

Wallace: I thought Caltech had a wonderful program. They had an interesting policy in those days. For the Ph.D., students were required to write two theses in very different fields. The program was divided into different areas: vertebrate paleontology, invertebrate paleontology, structural geology, mineral deposits, geophysics, and petrography--something like that. So I chose structural geology and vertebrate paleontology.

A bunch of us chose vertebrate paleontology as a major or minor, I believe, primarily because in those days Chester Stock had research funds. These funds came from the Carnegie Institution of Washington; its president at the time, John C. Merriam, was a paleontologist. There was no National Science Foundation (NSF), there was no research funding from anywhere, except perhaps a little from oil companies. Now, when there are many sources of funds that students can tap into, it is hard to comprehend what a lean and difficult time it was back then.

Many students who were aimed at fields far from paleontology took degrees in vertebrate paleontology. For example, Paul Henshaw, who was going into economic geology and mineral resource work, and later became president of Homestake Mining Co., wrote his major Ph.D. thesis in vertebrate paleontology. Also two of my good friends, Bob Hoy and Art Drescher, who also became economic geologists, and worked for mining companies, did a lot of work in vertebrate paleontology.

Scott: Say something about your professors at Caltech.

Wallace: I think educational methods and teaching styles are so very, very important. I hear people objecting that universities are into research and do not do teaching.

That kind of comment just appalls me, because some of the best teaching techniques are in the research mode, where students participate in discovery, questioning, analyzing and thinking.

The whole thought process is so much more active and effective than just acting like a sponge trying to soak up what the professor says and then spewing it out on an exam. In my estimation the lecturing-and-regurgitation mode is scarcely teaching, but many consider it to be. Participation is critical--participatory education, so that the student is seeking answers with a professor,

Wallace: doubting things and doing independent thinking. That is the way to learn how to think, and how to study anything that comes along in life.

Scott: You are describing academia at its best, which can be very good indeed. It is the kind of effective teaching that should be emphasized and encouraged.

Wallace: Yes. In illustrating my own good experiences with teaching and research, I will compare and contrast the teaching styles of four people, Chester Stock, Ian Campbell, Horace Fraser, and John Peter Buwalda.

Chester Stock

Wallace: Chester Stock, who was very popular as a professor in vertebrate paleontology, specialized in a several-hour lab given twice a week, but I cannot remember that he lectured at all. He would wander into the lab, where he and we students would get to talking about anything from philosophy to vertebrate paleo. I remember that one of the most significant studies in my life was working in vertebrate paleontology with the little ankle bone called the cuboid. Over a period of weeks we compared the cuboid bones of the elephant, the shrew, the dire wolf, and human beings. Stock visited with us in the process--but did not lecture at us.

The cuboid stuck in my mind, because after going through that exercise, I never again could consider myself as anything but one of these critters. It proved to me that I definitely was related to them. It was such a simple little bone. All cuboids looked the same, whether of an elephant or a shrew or a human. To me that was a very important philosophical link with the rest of the animal world. Anyway, Chester Stock was certainly the epitome of participatory education--participating, questioning, and inventing new ideas.

Ian Campbell

Wallace: Ian Campbell was a very different person and had a very different teaching technique. He was people-oriented, and his style was to relate personally to individuals. He did that so well that many of his students named their sons Ian. One could not help but want to emulate this scholar--such a warm, capable person. He became president of such prestigious organizations as the Geologic Society of America, the Society of Economic Geologists, and the Mineralogical Society of America. I do not particularly remember the learning processes in his classes, although I took several under him, and then became a lab assistant for him in one of his classes.

Horace Fraser

Wallace: Next I'll mention Horace Fraser, a Canadian who taught ore mineralogy. He was a taskmaster, driving us to work hard, and to memorize chemical compositions of hundreds of minerals. Those are three very different styles of teaching, and all are effective. It was marvelous to have those three styles represented so clearly in my background by those three Caltech professors.

Scott: Those are three very different styles, but evidently they all worked quite well.

Wallace: They all worked--each filled in a niche between the others.

John Peter Buwalda: A Contrast

Wallace: Another teacher I shall describe was Dr. John Peter Buwalda, who became my Ph.D. thesis advisor--that is, the thesis on the San Andreas fault. Chester Stock was my adviser on the vertebrate paleontology thesis. Dr. Buwalda was revered but also feared, I believe. In class he never let the students know his thoughts about any geologic theory or situation. His teaching technique was to let students prepare reports on various subjects, then have them report their findings to the rest of the class. He remained the Sphinx. That technique has worked well for some professors, but we all wished that we could have some exchange with this famous geologist. I had very few conversations with him, even about progress on my thesis.

After the war, when I was offered the teaching position at Washington State College, I still did not have my degree in hand. Although I had completed everything and had passed my defense-of-thesis exam, Dr. Buwalda had procrastinated in taking the necessary papers to the administrative office. Not knowing how to handle this touchy situation, I asked Ian Campbell to intercede. In about 10 days I had my sheepskin in hand and was acceptable to Washington State College.

<u>Field Trips</u>

Scott: Before you leave Caltech, talk a little about some of the other things you experienced along the way. I know for example that you witnessed at least one of the famous debates between Bailey Willis of Stanford and Andrew Lawson of Berkeley.

A Trip to Stanford

Wallace: In the spring of 1939 a group of us drove up to Stanford for a meeting of the Cordilleran or western section of the Geological Society of America. It was wonderful to go up there in the spring when all the fruit trees were in bloom. But I guess the highlight was hearing Bailey Willis and Andy Lawson argue. It was traditional that they would disagree and have a shouting match at any of these meetings. They performed to our satisfaction, and it is something to remember when I read the multitudinous works of both those great scientists.

Scott: Yes, their disagreements are now legendary.

Two Fossil Finds

Wallace: Another memorable experience was finding some fossils, aided by Chester Stock. He had access to some funds from the Carnegie Institution of Washington, which could be used to support field work. It was the only such money in town. So when any of us wanted to have a weekend out in the hills, Stock could pay for our food and our gasoline, and we could use a department car.

With two other friends--Bob Hoy and Art Drescher, whom I have previously mentioned--I went on one trip to the Panoche Hills west of Fresno. We were looking for fossils of aquatic vertebrates, big reptiles, sea-serpent-type critters. We knew the formations where these creatures should be found, and walked the barren hills where those formations cropped out. Our search proved very successful. I think it was Art Drescher who made the first find--the nose of a Mosasaur sticking out of the ground. We hiked back over the hills to the truck to get shovels and equipment. Then while on the way back to the Mosasaur I saw three vertebrae (neck bones) of a Plesiosaur sticking out of the ground. They looked like a small log, protruding about eight or ten inches out of the bare, shaley ground. Here we had two major fossil finds of big creatures in that one weekend. What excitement!

We all had some previous experience in how to treat fossil finds, so we dug very carefully. You dig them out along the bedding, and finally get down with a toothbrush and small tools to clear details. We put burlap coated with plaster-of-paris over them to protect them, and ended up leaving both of them at the site for many months. Later we worked to excavate them and carry them back. Many others got involved in the later digging. Some of the digging went on in the summertime, and we had big cloth canopies over the site to protect the crew from the San Joaquin Valley sunshine.

It became a real "dig," and the Plesiosaur ended up on a wall at Caltech. It more or less covered a wall, 10 ft. by 15 ft. The skeleton was complete, except for the head. The bones I had found sticking up were neck bones. The head was gone, but all the rest was preserved, down through the tail, and including stomach material with bones of little critters the Plesiosaur had eaten. The Mosasaur turned out to be only the skull. It was perhaps four feet long and three feet wide, and had the typical big, double-hinged jaw. This was all very exciting. Good education!

Thesis Work on the San Andreas Fault

Wallace: By the time I completed my MS degree, I pretty well knew that my focus in geology would be in structural geology. Dr. Buwalda suggested that I try studying the San Andreas fault near Palmdale, California. I had had very little experience in real geologic mapping except in a class at Caltech and in Canada. I had missed a summer field camp at Northwestern University because of a bout with appendicitis. What stands out in my memory is that I really didn't know what I was doing, and I realize that is the status of many PhD efforts.

I remember sleeping under the stars in the desert along the San Andreas fault, listening to the coyotes howl, eating cold Campbell's beef stew out a can, and entertaining myself playing my violin. I think the violin prompted extra efforts by the coyotes. I had just barely enough money to buy the canned soup at the one small store in Palmdale. Oh, that I had had enough to buy empty land at that time as Palmdale is now a major urban area.

For decades I felt embarrassed by my thesis work because I did not think it was sufficiently extensive. The draft board was breathing down my neck and I had no money for typing or other help with the thesis. But in recent years I have decided it made no difference, it, plus my minor thesis in vertebrate paleontology, got me a degree. Recently a friend pointed out that I probably was first to talk about slip rates on faults, something that came so naturally I never thought it unusual and never bothered to research the history.

It dawned on me recently that the 75 miles of displacement that I had suggested in the thesis was by far the most anyone had suggested to that date. It was one of very few papers on the San Andreas fault at the time, especially including geologic field evidence and geologic mapping. When I think of the thousands of papers about the fault that have been published since, I really had some gall to write some things I did. But I now find nothing too awfully bad about the thesis, and, I have gotten over my decades of embarrassment.

I completed the paper for publication after the war while working for the USGS in Washington, D. C. My wife, Trudy, typed it for me and I worked over the map for publication. The published paper, I know was used in several university classes on structural geology. One professor told me he liked it because it related geomorphology to faulting and tectonics. (Wallace, R.E., "Structure of a Portion of the San Andreas Rift in Southern California," Geological Society of America Bulletin, v.60, 1949, p.781-806.)

II TO THE USGS AND ALASKA

Joining the Survey: The Assembled Exams

Scott: While you were still at Caltech, I believe you had some early contacts with the U.S. Geological Survey, which eventually led to your going to Alaska in 1942, interrupting work on the Ph.D. thesis. Say a few words about that, and the process of joining the Survey in 1942.

Wallace: Yes, I think this would be of some significance, not because of me, but because of the recruitment process USGS then followed. I think it was in the spring of 1939 that I first took the Survey's "assembled" or written exam. The assembled exam was given for those people who wanted to work for the Survey or be a geologist for any federal agency. The exam would usually be given at some conveniently located high school, and monitors from the government stood by. Usually a dozen or more students assembled to take the written exam. When I took it, the exam included multiple choice questions, and a geologic map to analyze. Earlier it had been a tradition to give essay-type questions. I have an old exam from the turn of the century in which applicants were expected to translate a page of German and French, as well.

Over time the essay-type questions became too cumbersome to use and grade, and the exam became multiple choice. Subjects were from all parts of geology, including chemistry, physics, mineralogy, paleontology. It was a very difficult exam, and it included the interpretation of a geologic map. The USGS produced a special colored map just for the exam, and the students were asked to interpret the map--to make statements about what sort of minerals the region could produce, and what the structure would be like, where water might be, etc. I always believed that the map interpretation was a very significant part of the exam.

Scott: The exam included a demonstration of ability to read a geologic map?

Wallace: Yes, not only ability to read it, but also to understand and interpret it, and to pick flaws in it. I thought this part was very valuable, but the whole exam was excellent. One had to get 70 to pass and be placed on the list. Those assembled exams were traditional in the U.S. Geological Survey for many decades, but they since have stopped that, and the unassembled process of grading has been used for decades. The unassembled exam involves tabulating credits for experience and education.

Scott: You mean the "unassembled" exam is basically a matter of tabulating credits?

Wallace: Yes, there is not much more to it than that. I later got involved in the committee to write the questions for the assembled exams, draw up the maps, and so on, which was an interesting exercise in itself.

A Job Offer and a Career Decision

Wallace: The assembled exam was not just for the Survey, although the Survey put it together. It was for any geologic job in the government and formed the basis for a government list of eligible people. As a result of the 1939 exam, in 1940 I received an offer to work for the Bureau of Mines. I mention that occasion, because that was one decision I made in my life that was very important. I had more or less drifted before that, moving from one place to the next with professors and others guiding me, but here was a definite job offer, and the real possibility of starting a career.

I had finished the master's degree and could have had that very nice job with the Bureau of Mines. I had nothing against the job, but finally decided that I wanted to get a Ph.D. That was a major decision for me. With war looming and the draft shaping up, it would have been easy to choose that nice job with the Bureau of Mines. I realized that by not taking it, I would have a very "iffy" several years or maybe many years--we had no idea how long the war would go on. Despite the uncertainties ahead, I decided to take the gamble and try for the Ph.D.

The Second Exam and the USGS Offer

Wallace: I think the next USGS exam was given in the fall of 1941, at a high school in Los Angeles. I had finished most of my work for the Ph.D. by then, and so found the exam rather easy. In the spring of 1942 I got the Survey's offer to work in Alaska, which led directly to my experiences on the Kuskokwim River in 1942, 1943 and 1944.

In May 1942 when I left Caltech I went to Seattle, Washington, to join Wallace Cady. We were assigned to Alaska to work on the strategic mineral programs aimed at finding and helping develop minerals needed for the war effort. Our target was quicksilver, or mercury. A very small quicksilver mine was being started on the Kuskokwim River, near the town of Sleetmute, and that was where we went.

Exploring the Kuskokwim Region

Wallace: We began by examining the local quicksilver deposit, and then enlarged the study to analyze the potential for more quicksilver in an enormous region referred to as the Central Kuskokwim region. Also in the field party in 1942 were Joe Hoare and Stan Johnson, plus our cook, Jacques Robertson, then a student. A year later George Gryc joined me. (In May 1993 George celebrated his 50th anniversary with USGS.)

The whole central Kuskokwim region seemed to have potential for cinnabar--the ore of quicksilver--but that was not known for sure. There were just a few scattered deposits of cinnabar, which gave us the first clues. This project became a major exploration. When we left Seattle it was during wartime and of course all the ships were loaded and overflowing with military supplies. We had to buy all of our food, packs, field clothes, boat parts and other things in Seattle, and haul them to Anchorage. The equipment was unloaded at the harbor at Seward then hauled to Anchorage by Alaska Railroad. Because of a shortage of help on the railroad in 1942, at Anchorage we actually had to rummage through dozens of freight cars to find our things, and had to unload them ourselves. We then chartered a small plane from Woodley Air to fly the supplies over the Alaska Range to the Central Kuskokwim region.

Scott: Say a word or two about the location, the geography, and the setting. Also, how did you get to the Kuskokwim region, and how did you get around while you were there?

Wallace: The Kuskokwim is the second great river in Alaska, after the Yukon. It heads in the Alaska Range west and northwest of Anchorage and flows southwesterly between that range and the Kuskokwim Mountains, in a general way paralleling the Yukon River to the north of it. It flows into Kuskokwim Bay, which opens on the Bering Sea. At the beginning of each season we generally flew to

Wallace: McGrath, Sleetmute, Crooked Creek or Aniak by small plane, landing either on the river with floats or on river bars with wheels. Those are all small camps or communities along the river, Sleetmute being roughly 200 airline miles upriver, and McGrath 100 miles or more still further upstream.

It was an amazing adventure, but it especially struck me as I felt transported back in time to Mark Twain on the Mississippi River. A big wood-burning, stern-wheeled paddle boat, the Wallace Langley, plied the Kuskokwim River between Bethel in the lower Kuskokwim delta and McGrath. The boat's route along the meandering river was approximately 600 miles, or twice the airline distance between the delta area and McGrath. The steamboat pushed a barge or two ahead of it, so that it could move the enormous load of fuel, food and all the yearly supplies needed by villages, fish camps, and fur-trading posts along the river.

We rode the Wallace Langley from time to time between Bethel and Sleetmute or Aniak. The boat had six or seven comfortable cabins aboard, and we ate at the mess hall with the captain. For change of scene we would climb out over the load on the barge and settle down on a sack or box to watch the river ahead. The crew took soundings in the many shallow reaches of the river in the "Mark Twain" tradition. In following the deep channels along the cut banks, the river flowed so fast that upstream progress could scarcely be detected.

When the boat stopped at a camp for wood fuel, we would get off and explore. Villages and camps were few and far between, and most had a dozen or fewer families, except for Bethel, Aniak and Sleetmute, which had larger populations. Bethel had a population of perhaps a little over 100, and Aniak and Sleetmute had smaller populations, perhaps about 50 each. When we arrived in McGrath in 1942, the town was bustling with construction crews of Morrison-Knudsen Corp., who were building an airfield. All told, McGrath's population was probably about 100. We were able to eat and bunk at the camp. During the next few years the McGrath field served as a stop on the military air-ferry route to the USSR.

Scott: Where did you start?

Wallace: In 1942 Wally Cady was the party chief. The field party included Joe Hoare, Stan Johnson, and me with Jacques Robertson serving as cook for the gang. We centered our effort around the Sleetmute quicksilver district, then in later years moved outward. George Gryc joined me in 1943. Wally Cady and Joe Hoare made extensive trips into the back country. George and I set up camps upstream and downstream from Crooked Creek, where Sam Parent ran the trading post. Most of the shacks that served as homes were downstream of the trading post. Their open lavatories were perched out over the river. River water also was their water supply. It was truly primitive.

Scott: Say a few words about how you worked, day to day.

Wallace: Much of our 1943 work was studying the outcrops of rock along the banks of the Kuskokwim River. For that we used a 20-foot flat-bottomed, poling boat equipped with a 35-horsepower Evinrude outboard motor or "kicker". That boat was also our main vehicle for all of our river travel. It could carry an enormous load; our entire camp equipment, tents, Yukon stove, axes, guns and plane table and explorer's alidade, large quantities of gasoline, and a several-months supply of food.

Scott: What is an explorer's alidade?

Wallace: An alidade is a surveying instrument including a telescope and vertical angle scales, and used in conjunction with a "plane table"--a flat board mounted on adjustable tripod legs--for topographic and geologic mapping. In the "explorer's" model the telescope is only a few inches high off the plane table in order to have an instrument of suitable dimensions for easy carrying.

We also used that boat to explore and study the geology along major tributaries such as the Oskawalik and Holokuk Rivers. Those rivers were shallow, and we ruined many shear pins when the propeller on the Evenrude struck rocks. We often had to resort to wading and lining (manually pulling) the boat up the river.

Back packing was the main means of travel to off-river sites such as the Horn and Russian Mountains. The pack frames we used were of wood and rather heavy and clumsy by today's standards of light, well-formed aluminum frames and light, strong synthetic fabrics. From our base camps, where we had fairly good-sized wall tents, we would take several-day side trips carrying packs and equipped with pup tents and K-rations (a military prepared packaged food).

Speaking of equipment and such, I want to add a note about radio communication. Approximately weekly mail served as our only contact with the outside world during the summer of 1942. Having been into ham radio since 1931, I knew what radio communication could do for us in our field work, so during the winter of 1942-1943, I got busy and built a very small one-tube crystal-controlled cw (code) transmitter and a two-tube receiver run off a "B" battery. John B. Mertie, an old-time Alaskan geologist, let me use his work bench in his home in Maryland to do the building, because I was rooming in a one-bedroom place, I believe with Bob Chapman. I got permission to use radio frequencies assigned to the Department of the Interior for native schools, and was assigned the call letters KWYD. The military's Alaskan Communications System agreed for me to check in daily on code with their station at Flat. With a length of wire tied to our tent pole and strung to some willows along the river, I had great success talking to Felix at Flat. He had an excellent "fist" on code, and I had always prided myself on mine. So our radio became the first successful one for the USGS in Alaska. I believe that John Mertie had tried unsuccessfully a few years before. Station KWYD served wonderfully for everything from getting medical advice to ordering "kicker" parts from Seattle.

In 1944 Ed Webber joined me. While passing through McGrath that year we found wrecked planes littering the airfield edges. We rescued belts of machine-gun ammunition that fitted our rifle--a 30-06 I believe. During that summer we often sat in front of our tent amusing ourselves by shooting tracer bullets so they would bounce or skip off the river surface and sail off into the twilight.

Ed once talked me into doing field work from about 7:00 pm to 7:00 am. The idea was that we could do without a sleeping bag by sleeping when the sun was up, and be cooler when hiking while the sun was down. We would eat lunch about midnight when it was a little too dark to do field work efficiently. What a terrible idea that turned out to be! We were so cold trying to sleep in our small pup tent, we lit our very small stove in the confined space, with difficulty coiled our legs around the stove, boiled some tea water and ate chocolate bars trying to get a little heat generated.

Ed was a wonderful, enthusiastic partner, a good petrologist and a fine linguist who learned the native Athapaskan language easily--something I tried to but failed to do--danced with verve at the native dances, and flew his own plane. I envied Ed's intensity for living, which few could match. I was surprised when, decades after he left the USGS, he ended his life by, I was told, diving his plane into the ground. I don't know what happened.

You asked earlier about how we got around, so I might say a word about flying with bush pilots. Each season we would fly to and from the Kuskokwim region with bush pilots, several of whom have become Alaska legends. There was Bob Reeve who picked us up at McGrath one year with his Ford Trimotor plane. He already had a load of cement in sacks, but agreed that if we could somehow lie or sit on the concrete, he would take us to Anchorage. Typical of such trips, we ended up spending many days at an emergency field (Farewell, I believe) west of the Alaska Range, waiting for the weather over the mountains to clear. On such occasions pocket books were in great demand.

Frank Barr was another pilot who hauled us and our gear many times in his old pot-bellied Pilgrim. He carried the weekly mail at least one summer. I wrote my first will while on the usually faithful Pilgrim when it developed engine trouble hundreds of miles from any air field. Trudy and I had just been married, and my first reaction, when I thought we were headed for a crash into the muskeg, was to grab a piece of paper and make a will for my new wife, even though I had nothing to leave her. After what seemed like ages, Frank tried a reserve magneto and the Pilgrim was soon under full power again.

Everyone who has worked in the wilds of Alaska has plane stories, bear stories, and disaster stories related to rivers and lakes. I won't start my own list here, and will resist the temptation to tell my story about challenging a bear while all alone and stark naked, except for my hat. I'll just report that I survived.

Scott: You were on the staff of the USGS throughout these Alaskan adventures, which were the summer phase of your main USGS project?

Wallace: Yes. The pattern was to do field work in the summer, and headquarter in Washington, D. C., where we curated specimens, did petrographic and chemical analyses of rocks, and wrote reports. It was felt that it took about two months in the office and lab for every month spent in the field.

Scott: Applying those proportions, it would have taken the rest of the year in Washington to back up each summer's work on site in Alaska.

Wallace: Yes. Those were about the right proportions for that type of venture and exploration.

Pioneering Exploration: A Last Opportunity

Wallace: The Alaskan field work was true exploration. In 1942, after we landed at Sleetmute and settled into camp at Ossie Willis' cabin, we started out on a traverse to the south, in the direction of Barometer Mountain. There were no maps, and aerial photos covering that particular region had not yet been taken. Only when you are without maps can you really appreciate what maps mean. That was about the last time anywhere in the United States one could set out into the wilds without any idea what was over the next ridge. We knew that major streams were coming out of the mountains in the distance, but we had no idea as to their drainage patterns. I cherish the memory of that--it was like being with Lewis and Clark when they were trying to guess what would be the best route west. Where would the easiest pass be found? Where could we get through with our poling boats?

Scott: In time, did you get base maps to help you get around in the region?

Wallace: Finally, but we had a long way to go to get adequate base maps, and spent weeks and weeks in the winter of 1942-1943 in Washington, D.C., making crude planimetric (regular) maps from these distorted oblique aerial photos called "trimetrigon" photos. This was reconnaissance-type photography in which shots were taken off the two sides of the plane, plus a vertical shot of the area directly below. While this was a great boon, for much of the area covered many streams and hills could be seen only far out near the horizon in oblique side-view photos.

As bad as the results were, for all the years of our work they were the only base maps we had on which to record and analyze geology. Indeed, our final geologic map used these maps as base. We had a simple optical instrument we used to rectify the oblique photos, but accuracy was extremely poor at the far horizons. In our exploration and mapping, we gave names to unnamed valleys and peaks. Near the end of the project we submitted these to the Board on Geographic Names for formal recognition and acceptance.

In 1942 a group of topographers headed by Toivo Ranta joined us at Willis' cabin, then headed out to establish control points for the maps that began to be developed from aerial photos. Before we had the trimet photos, however, and before we made our crude maps from aerial photos, we just set out over the hills and found out what was there.

Our primary task, of course, was to figure out the geology. We collected rock samples and recorded the dips and strikes of sedimentary beds. We identified the trends, offsets and timing of faulting and folding, and the association of minerals with igneous rocks and structure. Such a study is always a difficult, fascinating, jigsaw puzzle.

The Successful Search for Quicksilver

Wallace: With base maps in hand, we started on the quicksilver exploration, mapping and analyzing the basic geology, the distribution of intrusive rocks, sedimentary basins, and structures. We covered an enormous area over a period of five years, and produced a fairly good geologic map of an area of about 8,000 square miles, an area of about the size of New Jersey. Many of the far reaches were explored by Wallace Cady and Joseph Hoare, and they compiled the first regional geologic map of an area approximately the size of Oregon. It was true exploration.

Scott: You conducted a basic geological survey of the region, but the real or immediate motivation behind it was to find cinnabar deposits, or areas with promise of such deposits?

Wallace: Yes. Find the setting, and then figure out how to focus on the most likely local areas. There were a couple of prospectors who were very competent, aggressive explorers. Russ Schaefer was one. God, how he covered ground with enormous packs on his back! There were small widely scattered cinnabar deposits throughout the region. We used that distribution to develop theories of how and why the deposits formed where they did. When we arrived in 1942 the Red Devil mine was just a little hole in the ground. We mapped it in great detail, and helped the prospectors develop it. The area became one of the biggest quicksilver sources for the war effort. This was a very direct and rapid response to our investigation. Burr Webber of the U.S. Bureau of Mines was part of the team and worked with us to advise on mining techniques.

For the most part, the prospectors were inexperienced in mining and ore processing. They did not know how to go about starting from scratch to develop a mine, or how a retort for quicksilver worked. They had no idea of the chemistry involved. For example, Russ Schaefer built a small retort out of an oil drum propped up on rocks so that a fire could be built beneath the drum. On one end of the drum he built a door, and at the other end he ran a short line of drain tile as a condenser where the mercury was supposed to precipitate after the cinnabar (mercury sulfide) had been heated. He fired up, and for a few hours everything worked fine, but the drain-tile pipe soon clogged up with black stuff. I pointed out to Russ that in some way he must use up the sulfur--so as to free the mercury from the sulfur--and allow the mercury to run into his collecting puddle.

In big retorts air is often used for this, but in Russ' small-scale operation, air just carried the mercury fumes out to poison the environment, including us. Mercury has very toxic effects on humans. Next we decided to try iron, obtained from tin cans in the camp dump. When mixed with the cinnabar, the iron did indeed take up the sulfur, and Russ began to get a good recovery of mercury, which he shipped and sold to brokerage centers. The black stuff that had clogged his condenser was metacinnabar. The residue of combined sulfur and iron clearly was a form of marcasite, and sparkled like the fools gold it was. Simple chemistry did work.

Our efforts were very successful, and the Kuskokwim region produced a lot of quicksilver for the war effort. We ended up producing a USGS "professional paper" about the Central Kuskokwim Region, its geology and potential for quicksilver and other minerals. (Cady, W.M., Wallace, R.E., Hoare, J M., Webber, E.J., The Central Kuskokwim River Region, Alaska, U. S. Geological Survey, Professional Paper, n.286, 1955.)

Other stories go along with that Professional Paper, including the fact that it was the last USGS professional paper that included an extensive bird list, a plant list, a fish list, and so on. In the early decades of USGS exploration of untouched areas in the West, while the main focus was on rocks, minerals and geology, the format of USGS Professional Papers commonly included something about wildlife and vegetation. Although by 1942 the USGS had mostly dropped that format for the lower forty-eight states, we had started our exploration with that mode of publication in mind, as it was truly unexplored country.

Moreover, inasmuch as I was an avid birder, we prepared a long list of birds, with some truly new ornithologic findings. Dr. Frederick C. Lincoln, of the U.S. Fish and Wildlife Service, reviewed and checked my list. The director of the Survey at that time was Tom Nolan, and being an avid birder himself, he let our professional paper become the last one to include bird and plant lists.

Other Projects

Exploring the North Slope

Wallace: Another project came along in January or February 1945.

Scott: So the work along the Kuskokwim came to an end in the fall of 1944 or thereabouts, and then you started on something else pretty soon. You might say a word or two about the transition from one project to another.

Wallace: There is not a lot to say about the transition. We were just ordered by a Branch Chief to take on another piece of work for the war effort that the USGS had contracted or accepted to do. The Survey was, of course, involved in a whole set of different activities related to the war effort, one of which was to evaluate the petroleum potential of the North Slope of Alaska, where the Prudhoe Bay field eventually was found. This was the beginning of a major exploration, although the Survey had also done some early work there in the 1920s. The U.S. Navy wanted to look at the North Slope for its potential for oil and thus involved the USGS. The area was already part of a Naval Petroleum Reserve, and the Navy put up money for the geologic exploration in 1945.

In February 1945 I was sent up to Fairbanks to find an office and warehouse space. The USGS was going to be shipping all sorts of material--float boats, food, and you name it--for several expeditions along parallel rivers draining the north slope of the Brooks Range, from the divide north to the Arctic Ocean. We were to map structure, find promising structures and stratigraphic situations for petroleum and gas, and in general to assess the petroleum potential of the reserve. My partner of 1943 on the Kuskokwim, George Gryc, was to play a leadership role in that effort for many decades. Off I went to Fairbanks in February of 1945, and, since that interrupted my social life in Washington, I asked my "social life" (my future wife Trudy) to join me. A month later Trudy came to Fairbanks and we were married the next day. (Incidentally, we returned to Fairbanks for the first time in 50 years to celebrate our golden wedding anniversary.)

Naturally that was very exciting. We had so many Alaskan adventures that I can't begin to tell you about them all. I was in and out of Fairbanks, a real hub of activity in early 1945. Russians were ferrying airplanes along the Alcan route, and then took off from Fairbanks for the Soviet Union. We would run into the Russians in Fairbanks buying shoes in the stores, and all sorts of things that were in short supply in Russia. A Russian group had a house diagonally across from where Trudy and I lived. Although they kept much to themselves, we would see them around town every day, buying things.

There were a lot of other U.S. government women employees, and wives of service people, so a government women's dorm was maintained in Fairbanks. When I would go out in the field, Trudy would move into the government women's dormitory. She worked for the Reconstruction Finance Corporation in Fairbanks under the direction of Larry Doheny, a character to remember. Then when I was back in town, we would move into a hotel, or get a small apartment. It was a very hectic year, but what great fun.

Uranium Exploration

Wallace: In the spring of 1945, a few months before the first atomic bomb was exploded in New Mexico, a USGS friend, Bob Coats, came to our hotel in Fairbanks and asked me to come to his room. He said, "We have a new project for you that is very hush-hush. It has to do with uranium." I already knew a considerable amount about the uses of uranium, although Bob was not aware of that. By 1938 a lot of material had been published about the potential of atomic energy and the atomic bomb. I had even given a couple of talks about it myself.

I remember a talk I gave in Washington, D.C., about the atom bomb and the horror of it. It is a wonder that some agency didn't haul me into the pokey. So the subject was not totally new to me when Bob came by in 1945. The upshot was that I went off looking for uranium, sampling "heavy minerals" in placer deposits. Particles of uranium-bearing minerals settle in sluice boxes and gold pans along with other heavy minerals such as gold.

Scott: Were you looking at areas previously mined for something else, or in new areas? How did you go about looking for uranium?

Wallace: We looked for uranium the way you would look for gold. We would pan streams, and go to old mines where we could often find old sluice boxes, and thus could easily collect the heavy mineral particles concentrated there. My collections were all shipped back to Washington and analyzed for radioactivity. For this project I went once again to the Kuskokwim River region, where I already knew a lot about the geology. I also went far beyond the area we had studied for quicksilver, to some gold placer regions.

On one expedition, Trudy joined me at McGrath and we flew to a little trading post called Medfra, at the head of the Kuskokwim River, where a Frenchman had a small trading post. Our small plane snagged a tree in landing, but, fortunately, that did not cause us to crash. His French wife was an ex-prostitute and proud of it. The trading base consisted of a dozen or so natives. The economy of a trading post was based on selling canned goods and staples to the Indians at a high price, in trade for furs at low prices. The post did fairly well.

En route to meet me at McGrath, Trudy had to help the pilot deliver a side of beef to a mining camp by opening the door of the plane and shoving the package out to drop to the camp. Such deliveries continue today.

It was all a great adventure. Trudy and I hiked about twelve miles up into the mountains above Medfra, where there was a gold placer deposit that I wanted to test for uranium. The mine was temporarily closed, and the mine owners had told us just to make ourselves at home and to use their supplies such as they were. You might call that one of our honeymoons.

The first evening after starting a fire in the cook stove, we were surprised by a knock on the door. We thought we were far from anyone. It was Mr. Richardson, a prospector who lived a couple of miles down the hill beyond the mine cabin in which we stayed. He said he was surprised to see smoke when he approached the cabin. He climbed up the hill daily to listen to the news on the camp's radio. He said that he had missed the whole of World War I, because he did not learn about it until it was over.

We were invited down for dinner a few days later, and were treated to strawberry wine, fresh strawberries for dessert, and a trip around his clearing which was covered with strawberry plants. He said he planted strawberries throughout the region just to see if they would grow, truly a Johnnie Appleseed of the strawberry world. But Mr. Richardson himself was allergic to strawberries!

We hiked in and were scheduled to be back to Medfra in about six days. On our return, when we got within about two miles of Medfra and got on the log road--more or less of a road, made of logs laid on the muskeg--ahead in the distance we saw this great big old truck loaded with the whole town, perhaps a dozen people. They had decided we were a little late so drove the town's one and only truck out to the end of the log road a few miles distance to meet us. What a wonderful greeting.

Permafrost Study for the Military

Wallace: In August 1945, another project came into being. I mentioned that the U.S. Air Force and the Soviets were ferrying planes over the Alaskan route to Russia. They were having a lot of trouble with their airfield runways collapsing and buildings falling apart because of permafrost thawing. So the USGS started a program to study permafrost under the auspices of the Military Intelligence Division of the Office of Chief of Engineers, U.S. Army.

I was put in charge of the program, and Bob Black, Max Elias and I took over from Si Muller--Dr. S.W. Muller--a professor of paleontology at Stanford. Born in Russia, he was fluent in Russian, so the Air Force had gotten him to translate all the information in Russian about permafrost, and he had prepared a fine manual. But he had done about all he could in the translation program, so the Air Force turned the program over to USGS to continue, and to engage in new geologic and field studies to evaluate and understand the problems at individual airfields.

We spent several weeks on a briefing trip with Si, going from Nome to Edmonton and back, stopping at each of the airfields to see the permafrost problems. We decided to examine three fields in more detail, Northway, Point

Spenser, and Galena. Following that selection of sites, Trudy and I went out to Northway. Northway airfield was near the Canadian border, a little way off the Alcan highway, where permafrost had given them a terrible time. Although it had been a rather major field, by the time we got in there in October 1945, the Air Force was closing it down.

So Trudy's and my second honeymoon was at Northway, Alaska, in the fall of 1945. My assistant, John Zydick, and I worked outside even when it was 25 degrees below zero, Fahrenheit. The Air Force provided us with a truck and a Chevy sedan staff car. We drove from Fairbanks down to Northway on the Alcan Highway. Unfortunately, John, who had just joined the Survey as a part-time employee, had never learned to drive. His learning experience was driving this big truck on the Alcan Highway in the middle of winter.

Scott: He learned to drive under rather extreme conditions!

Wallace: Yes. Just a few miles out of Fairbanks, the first hill we went down, he spun out of control, around and around and around. Fortunately there was no traffic and he spun down the middle of the highway, and not off into the ditch. But he learned fast and we got to Northway safely. We lived in a little prefab house called a "Stout House." I think the designer was a Mr. Stout.

The temperature dropped to 50 below while we were there, and for one week it did not get above 35 below. For storing our truck and car nightly, we had full access to the airfield hangar, which was still being kept heated. The big hangar doors opened so easily Trudy could slide them with just a slight push, and we would drive our one little staff car in there to keep it warm--one lonely little sedan in this gigantic hangar.

Despite the cold, we managed to get a lot done on the permafrost project. It was an interesting time. Bob Black went to Point Spenser out west of Nome, and Max Elias went to Galena, on the Yukon in central Alaska. These were other big air fields that were having serious problems with permafrost. We all returned to Washington, D.C. in December of 1945. We wrote several papers, and I believe we were very helpful to the military in analyzing permafrost problems.

III TEACHING AND FIELD WORK: A FIVE-YEAR INTERLUDE

To Washington State and Coeur d'Alene

Wallace: With the war over, I began to think about my long-term interest in teaching, so I left the Survey to teach in Pullman, Washington, at Washington State College (now University). But while on the permafrost story, I will continue one more step. While at Pullman, I was visited by a Colonel Orr of the Air Force, who wanted me to head up a snow, ice and permafrost investigation group that they were starting. I understand it later became a big operation in the Air Force.

Scott: The Air Force asked you to move into a long-term permafrost and cold environment program?

Wallace: Yes. Also it involved a grade or two raise from what the Survey was paying. I said, "Yes, that might be interesting." I signed all the papers, but then never heard another word. More than a year later, I received an inquiry from another Air Force person. He told me that Colonel Orr had been killed in a parachuting accident, and said that in sorting through Orr's papers, they had found my application, which obviously had never been acted upon. Was I still interested? By then, I said, "No." That was one of those quirks of fate--a decision made for me. I would have gone with the Air Force, but because of an Air Force colonel's accident in practicing parachute jumps, my future life had been changed.

Scott: Save for that Colonel's fatal accident, you probably would have embarked on a career with the Air Force?

Wallace: Yes. So I taught at Pullman for five years, from the Fall of 1946 through the Spring of 1951.

Scott: Had you especially sought employment at Washington State?

Wallace: I applied for a post at Pullman and also at Redlands, I believe. Redlands, as a strongly religious school, did not like my religious credentials, i.e. my lack of any church affiliation. Washington State hired me as an assistant professor at a salary of $3,000 for a nine-month school year.

Scott: When you went in, was it with the thought of doing a stint of teaching, and then coming back to the U.S. Geological Survey later? Or did you intend to give teaching a try, and decide later after you had a chance to see how it went?

Wallace: It was more the latter, to give teaching a try. Inasmuch as I was from a family of teachers, my father, mother, and an aunt had been teachers, I knew that teaching was a fine, rewarding career. Both my father and mother got so much out of teaching, my mother especially, that I decided, "I'd better give it a try." But I believe I went into it thinking, "This is my life--I'm going into teaching."

Scott: What subjects did you teach at Washington State?

Wallace: I considered myself primarily a structural geologist with major interests in mineralogy and petrology. So I taught classes in structural geology and beginning mineralogy. But the department had a course in crystallography on the books and I agreed to teach that, although I had only elementary courses in crystallography myself.

I had to do considerable study and decided to use the class as an exercise in viewing things in 3-D, as well as a simple adjunct to mineralogy as the department wanted it. That approach also helped students in structural geology as well, inasmuch as for most of structural analyses, one must have a keen sense for picturing three dimensions. We used the stereographic projection for displaying crystal faces and internal structure. I also emphasized 3-D drawing in perspective, orthographic construction, etc. I must admit I benefitted as much as the students--assuming of course that they did benefit.

Scott: Probably a lot of teachers find teaching a powerful learning exercise for themselves. What other courses did you give?

Wallace: We had only five or six full-time staff at any one time, and about a half dozen teaching assistants out of about fifteen graduate students--a nice small family. As a result I took on several other teaching tasks, partly for my own amusement. They included physiography of the United States, structure of the Western US, and, the most difficult, vertebrate paleontology. I even had the gall to teach a class in geologic report preparation. (From grade school on, I have never been able to spell well, even after an all-out effort in graduate school. I can see no rationality to spelling.)

Scott: Teaching such a variety of subjects must have been quite a load.

Wallace: It was an enormous teaching load in comparison to what professors at the larger schools carry, today at least. Several of the students expressed great interest in vertebrate paleontology, and the department had a fine vertebrate collection, so I was conscripted to take on the class. I had done a thesis in vertebrate paleontology, but that was of limited breadth compared to what should be in a course. It was a lot of fun even though a lot of work. The students and I learned together.

Scott: You pointed out earlier that the best teaching is not just lecturing, but a combined effort by teacher and student, working together.

Wallace: Yes. The class and I had one really great success, working together. I once said glibly, "Let's go find a mammoth tooth." The next weekend out we went to find a mammoth in the Palouse. We followed some of the search techniques I learned under Chester Stock, and amazingly, by early afternoon we found a mammoth tooth! Excitement reigned.

We had good students there at Pullman. With small classes one can't help getting well acquainted. Several became very successful with careers in the USGS, ...including Don Peterson, George Becraft, Bob Schuster, Willard Puffett, and Tom Cheney, who later went with private industry in South America. Others went on to teach and into industry. Very gratifying.

Scott: But you did give this up and return to the USGS?

Wallace: Yes. After five years of teaching I realized that my love of exploration and of research was too strong to deny, and I had also found the teaching load very trying. Moreover while I was at Pullman I had never really disconnected from the Survey. During my second, third and fourth year there, I was with USGS every summer doing field work in the Coeur d'Alene mining district of northern Idaho. Also while at Pullman, we developed strong friendships with the USGS group headquartered in Spokane, Washington. So we had a very close association with the Survey all through those years. By 1951 I knew I wanted to go back to the Survey, and good friends arranged for me to return full-time.

Summer Work in Idaho

Scott: Talk a little about that summer work. Was it related to mining?

Wallace: Yes, the Coeur d'Alene mining district has produced several billions of dollars worth of lead, zinc, and silver. Warren Hobbs and Allan Griggs were the co-leaders of the USGS project, many others worked directly on it, and still others carried out specialty studies in geochemistry, metamorphism and ore minerals.

Scott: It must have been a major USGS effort.

Wallace: It was, and it stretched over many years. While I was at Pullman, I worked as what was called a WAE employee--the acronym meaning "when actually employed." It has been a very common practice for the USGS to use university faculty members as part-time employees. It helps the USGS and also keeps academic people abreast of new field findings, giving them material for professional writing and teaching.

Scott: When did you work on the Coeur d'Alene project?

Wallace: I first joined the project in 1948, but even after moving to Menlo Park in 1956 I was still involved with the major report. Meanwhile in 1952, along with John Hosterman, I had extended our project into Mineral County Montana. We completed a report on that work the next year, and it was published as a USGS Bulletin.

The major report appeared as a USGS Professional Paper, co-authored by Hobbs, Griggs, Wallace and Art Campbell. Vern Fryklund also did a separate Professional Paper. Preparation of our report stretched into 1956-1957, well after I had returned to the USGS full time. Allan Griggs became the spearhead for the final report and carried out the main work that brought it to completion. Somewhat strangely perhaps, although I had been only part time on the project field work, I ended up writing almost all of section on structural geology, which amounted to most of the pages in the report.

Scott: Would you care to comment on the considerable length of time it took to get the report completed and published?

Wallace: The difficulties of field studies, learning about the geology, the rock types, the significance of structural relations of rocks, identifying and defining faults and folds. Writing such a report is always a research-type of exercise on the forefront of knowledge. With all the intellectual effort required, the report preparation is no mean task. It is difficult to describe all of the complex steps, especially when precise, colored geologic maps are to be a major product. Problems included obtaining good base maps, printed on stable materials on which to draft the geology.

The stability is needed so that registry will be possible of perhaps 50 or more colors representing different rock types in final printing. Maps of the underground workings of mines must be alined from one working level to another. On this project we used base maps printed on excellent rag paper permanently mounted on metal sheeting. We did hand-drafting from field sheets, mine maps, and the like--it was the only way to compile the data and present geometric interpretations. Newer techniques are becoming available, but maps are still a complex problem in editing and publication.

Wallace: The complexities and agonies of bringing major reports to completion could be a subject for an in-depth statement. You would have to talk about the psychology of authors, the diversion of authors to other momentarily more important priorities, the ebb and flow of editorial staff, and the frequent lack of capabilities at the printing and publishing end, whether within USGS or in outside contract companies. Such problems have often been encountered in publishing colored geologic maps, which are inherently complex. Some geologic maps depict a hundred or more rock types and structures, and precise color rendition of those is very difficult. With the new computer methods now coming into being, several processes seem promising, but nothing is standardized or universally suitable yet.

Exploring the Osburn Fault

Wallace: The geologic theme of my life has been structural geology and faulting, so I was fascinated by the great Osburn fault, which dominates the Coeur d'Alene district and greatly influences the distribution of ore bodies. In many ways the Osburn fault is like the San Andreas fault on which I was weaned, except that the Osburn fault has only 25-28 km of slip instead of the hundreds of kilometers on the San Andreas fault. Nevertheless, the Osburn is a fine analogy of the San Andreas. Because of the mine shafts and tunnels that cut the Osburn fault, we were able to explore this great fault in 3-D to depths of a mile or more. We could feel it, see it, sample it, even taste it, as is rarely possible with other great faults.

For many years, I tried to organize a field trip of geophysicists and geologists who had no concept of what a real fault is like. Many theoreticians, especially, need that kind of exposure to the real world. To many, a fault is just a simple plane as seen on a computer screen, rather than the complex mess of irregular fractures and broken rock seen in a mine. It would help many of them if they could personally pluck out some plastic gouge and mould it in their hands, find water pouring into a tunnel where they least expect it, or find a branch fault onto which most of the displacement had shifted.

Wallace: Examining things underground is not, however, as simple as it might seem. The walls are commonly covered by dust from blasting and drilling of the mine face, so washing is often needed. Rocks don't look just right under a carbide light, or even a regular electric mine light, and careful chipping with a hammer is standard practice to examine fresh surfaces. For good identification the chips or samples may require the preparation of thin sections and analysis under a petrographic microscope.

Then, too, when first starting to work underground, one's initial apprehension is not easily dispelled. Moreover, anyone underground in a mine needs to be on the lookout for hazards such as "widow makers"--large blocks that could fall on someone passing underneath--or abandoned shafts that must be edged around to keep from falling, perhaps a thousand feet. I grew to love underground work, but taking a group of inexperienced people underground for only a day or two is seldom enough to give a usually lab-bound theoretician the insight needed.

Scott: The underground environment is intimidating at first, but with experience you learn to deal with it.

Wallace: Yes, including "bad air," which is another underground hazard. "Bad air" means air that has little or no oxygen, which has usually been taken up by rotting mine timbers, or minerals that combine with oxygen readily to form oxides ("rusting"). On two occasions I nearly "got it" by entering bad air, but with experience, and using candles, matches, etc., as indicators of air quality, we got to know just about where to expect bad air.

Under conditions of poor or nonexistent air circulation, we knew that on entering a tunnel, crosscut or drift, we would find bad air beyond rotted timber that extended for more than a few tens of feet along the tunnel. Unfortunately, carbide lights are not helpful as they use acetylene, which burns brightly in air bad enough to make me feel faint--air in which a candle or match would not burn. While concentrating on fascinating geological puzzles, it is easy to forget about such mundane things as oxygen content of air.

To recapitulate, the teaching at Washington State College was from 1946 to 1951, but summers from '48 to '51 were devoted to work with the Geological Survey.

Meshing Theory and Observation

Scott: Would you say a little more about the problem of meshing theory and real-world observation? You have been alluding to some of the difficulties of doing that.

Wallace: Yes, it is inherently difficult. I have devoted a lot of effort to trying to bring different disciplines together. Geologists and geophysicists really need each other's insights, but often do not see eye-to-eye. The geologist may be more comfortable with the complexities of the real world. On the other hand, those who wish to quantify things and deal with them mathematically, usually must simplify questions in order to treat them mathematically.

Many examples can be cited in which known physical or chemical principles seem to rule out an interpretation of field data, but later field observations demonstrate that the interpretation was correct, and what was ruled out theoretically, indeed did happen in nature. So, although it may seem facetious, I like to say: "If it happened, it must be possible." Plate tectonics and large-scale gravitional sliding are two things now accepted that at one time seemed improbable, if not impossible.

Scott: Most people have at least some idea of plate tectonics, but I am not familiar with the concept of "large-scale gravitational sliding".

Wallace: Well, most people have some idea of landsliding, as along road cuts, but the same process can happen at much larger scales. In Tadzikistan just south of Garm, Igor Nersesov treated me to a helicopter ride over the Peter the First Range. The whole north flank of the Peter the First Range apparently is sliding to the north along beds of gypsum and similar materials. At the range crest, at the head of the slide, is an area of "pull apart".

A similar gravity-driven block, the Heart Mountain detachment, can be found in the Absarokee Mountains east of Yellowstone National Park in Wyoming. There Bill Pierce described a block of rocks a hundred kilometers long, fifty kilometers wide and one to two thousand meters thick. The block has slid on slopes of only a degree or so and moved eastward tens of kilometers. Many geologists see reasons to interpret subcontinent-size blocks as having moved great distances by gravitational forces. Dr. Rein W. Van Bemmelen of the University of Utrech and others early championed this concept.

The earth sciences have certainly become more quantitative, and properly so--but the real world is extremely complex. I coined the phrase, "synthesis of multiple suggestions" to characterize how geologists think.
Geologists seem to be comfortable working with a mixed range of facts,

Wallace: Geologists seem to be comfortable working with a mixed range of facts, inferences, and fuzzy suggestions in developing a working hypothesis. Such a synthesized model can be much stronger than many of its individual elements, and is amenable to testing. Interpretations can be greatly improved by synthesizing ideas derived by different disciplines. Where different disciplines work side-by-side, as they do at USGS, great things can and do occur. I continue to emphasize that geologists and geophysicists need each other desperately.

Geologists and geophysicists are not the only groups who find it difficult to understand and appreciate each other. Jointly they have an even more difficult time getting through to the rest of the world--especially to those who set and administer policy, and to the lay public. The problem is generally referred to as one of "communications," using a term that itself is so overused and ambiguous as to be almost meaningless. But the point is that disciplines and groups come at problems so very differently.

IV FULL-TIME WITH USGS AGAIN

Wallace: In 1951 I rejoined the Survey full-time, and we moved to Spokane, Washington, to continue working in the Coeur d'Alene district. In 1952 I spent a summer with an extension of that project in Montana. The big Osburn fault extends on east through Mineral County, Montana, where we felt there should be a potential for big ore deposits.

Trudy and I camped at the Broken Heart Ranch which was just being built not far from Haugan. It wasn't fancy, but rustic and pleasant. In August Trudy returned to Spokane, where our son, Alan was born. He eventually became a geologist in his own right, earned a Ph.D. at Oregon State University, and after several years with industry has now worked for the USGS for many years. Anyway we had a very happy time in Spokane, when the USGS suddenly decided that I should go back to Washington, D.C., to headquarters. Trudy and I had to sell our home in Spokane, which we had owned for only a few years, and lost our entire equity because of a depressed real estate market.

As a further comment on our finances I might mention that I returned to USGS in 1951 at a salary of $6,400 per year. That was more than double my munificent 1942 starting salary of $3,100 per year, back in the days when an expenditure of as much as $20 required family planning. But all these older figures seem small compared to 1995 levels, don't they?

My Washington Tour of Duty

Wallace: My Washington tour of duty ran for two-and-a-half years, from 1953 to 1955. At headquarters, I worked in what they called the program office of the Geologic Division, and my assignment was to help Harold Bannerman, the assistant chief geologist for programs. We arrived in Washington the week

Eisenhower was being inaugurated as President, in January 1953. One of the first weeks I was there, I wandered over to Congress and, by a fluke, got to hear Joe McCarthy's congressional hearings, and witnessed Senator McCarthy quizzing the military generals. What a disgusting and frightening period in US history that was--shades of all the evil that egomaniacs and demigods bring to the world!

The Washington assignment was interesting in many ways. I got acquainted with the inner workings of the U.S. Geological Survey, the directors, and other divisions. When I joined the Survey in 1942, Mendenhall was director, and then during the War Bill Wrather became director.

At the end of the headquarters tour of duty, we spent the winter of 1956-'57 in Spokane followed by the summer in Oregon. In Oregon I worked on the state geologic map project. I produced a reconnaissance geologic map in the Izee region, which had been a blank previously. We moved on to Menlo Park, California, late in the summer of 1956.

The Rotation System and Scientist-Administrators

Scott: So you had already been thinking about getting out of Washington anyway?

Wallace: Yes. That was one nice thing about the Survey. All through those years--but not so much any more--many administrative assignments were temporary. The Survey has practiced a rotation policy for most science-supervisory jobs. After a "tour" one returned to a scientific or technical role. I had gone to headquarters in 1952 for a two-and-a-half to three-year "tour of duty."

The strength of the rotation system is that it helps assure that the management of science is done by people who understand science and research. Most agencies, however, seem to have their main career ladder in administration, with grades related to number of people managed. Although there was an administrative ladder, the USGS also set grades according to the scientific contributions of individuals.

Administrators lacking a science background themselves seldom seem to understand what makes scientists and researchers tick. Many seem to think that one could "order Mr. Einstein to develop the theory of relativity by next Tuesday because we need an atomic bomb by Thursday". Not so! Furthermore, it is hard to find a straight business administrator who understands the long-term flow of new ideas from research into application and then into the creation of jobs and an improved standard of living for all.

Wallace: Rotation meant that administrators would also be people who were close to the field, close to the research, and really knew the geologic problems, but not necessarily familiar with management techniques. In a way, you might say that they were excellent scientists but not necessarily good administrators. That, of course, depended on their native talent. Today the policy is to give potential future administrators management training. While I doubt that such training changes personalities very much--and the basic personalities are the most important element--some management techniques can be passed along that way.

Over the decades rotation seemed to me to make a much more dynamic research and real-world-oriented organization, with clear missions to be solved. Nowadays one often hears the term "inside the Beltway." Without rotation of personnel, attitudes tend to change once a person has been inside the "Beltway"--or into administration--for a time. I think it is tragic to see that change take place. The USGS had a precious thing preserved over the many decades to have an emphasis on research. From my own experience, I detected that my outlook changed during even a few-year tour in Washington, D. C. I hated myself when I began to think of the field people as problems.

During the century and more the Survey has been in existence, it has chosen the research approach to solving problems. You cannot deal with some of the problems of application without the fundamental research, and that research needs to be kept coming along. Fundamental research is needed to make real progress, although it also has to be linked with day-to-day application. We all believe in what has come to be known as "outreach" and "technology transfer." But you need the basic research in order to have valuable information to transfer. Society's "information bank account" is constantly being drawn down, and fundamental research is essential to replenish and build up the account for tomorrow's needs.

Scott: Do you feel that research is still the principal goal of the USGS?

Wallace: I'll express my own opinion first. Good impartial data gathered and interpreted in a research framework is necessary in serving the national interests well. From its very beginning, an important characteristic of the USGS has been its enduring tradition of and dedication to excellence and integrity. Ignorance certainly does not assist in problem solving, and solutions to even the most immediate problems, such as concerns about the economy or jobs, require fundamental data and knowledge about our land and earth resources. The necessary data and knowledge are not readily available but require long-term efforts. For the long haul, we must continue to try to understand the earth and its limits better. Ultimately the survival of humankind is at stake.

Wallace: The USGS has had the unique responsibility and capability to consider both long-term, broadly significant national problems, and more specific national needs of the moment. It is mission-driven by Congress, and advises other agencies of the government as well. The USGS has tended to seek solutions to national problems by first understanding fundamentals, rather than only treating problems in a superficial way with short-term and "band-aid" responses. Each fundamental innovation or breakthrough in research is followed by a wide range of important applications, which could not have been conceived of beforehand. The USGS has always emphasized translating research breakthroughs into practical applications.

Scott: That says a lot about the USGS. Do you wish to add anything more?

Wallace: The USGS, as a federal agency, has the responsibility to provide earth-science information to guide federal policies and actions, and to assist the general population in many ways. Included in its missions are a broad range of concerns including the purity and availability of water; mineral resources; the topography of the land for the needs of industry, highway building, and every-day use; and the hazards of earthquakes, landslides and volcanic eruptions.

To my mind, research has had, and should have a major emphasis within the USGS program. Remember, however, that the research always has been driven by the legally assigned missions of the agency. Practical solutions are impossible without the fundamental understanding provided through research! And quick solutions for immediate problems will always require some fixes already on the shelf.

The term "outreach" has become a popular buzzword in the government. It refers to the need to exert special efforts toward putting research findings into use. I am all for that, as are my friends who administer the USGS. One difficulty, however, is that with declining budgets each unit of effort shifted to another program, for example to public relations or "outreach", means the original programs decrease. Maintaining a good balance is difficult. Personally, I would like to see the USGS continue emphasizing the development of new concepts to serve society--through research. Even with research emphasized, I am confident the Survey will never lose sight of the goal of service to society and the national needs.

Menlo Park and the "Field-Center" Concept

Scott: You had gotten to the point where you had returned to California, to Menlo Park. Menlo Park was then a relatively new but very important USGS field-center. Previously you described doing your field work in the summer--as in Alaska--and then going to the USGS Washington headquarters to write up your findings. But with the creation of a field-center like Menlo Park, the sample analysis and write-up for field work in this USGS region could be done here.

Wallace: With the creation of a field-center like Menlo Park, the sample analysis and writeup for field work is this USGS Region could be done here.

cott: Scott: I presume that was at least part of the rationale for creating such centers. Would you discuss the concept a little more at this point? Talk some about the move to establish regional centers, and the postwar shift from having only the single base in Washington, D.C.

Wallace: Before about 1950, Washington was the main "headquarters," but then the "field-center concept" was born, and there are now three main centers. One center, of course, was in Washington, D.C. (later moved to Reston, Virginia). The other two centers are in Denver and Menlo Park. The idea was that each USGS center could have excellent library and laboratory facilities, all sorts of high-tech instruments, and extensive rock and fossil reference collections. People of many disciplines could interact as a group. Although we each did much of our day to day work alone, we needed, and benefitted from, interdisciplinary contacts and consultations.

The growth of the centers was based on a carefully thought out field-center policy. In the 1980s and 1990s, smaller offices and centers have become popular, in part because of the higher-cost housing at Menlo Park. It became difficult to recruit bright youngsters, because they just could not afford the cost of housing and services in the Menlo Park area. The argument that we should work closer to our "customers" has been a strong one, but there is also a counter-argument that by doing so we become too provincial, and lose sight of the national character and priorities of our missions. Under the mineral resources program, for example, smaller offices closer to the mining regions and companies are currently in favor so that offices in Spokane, Washington, Reno, Nevada, and Tucson, Arizona, are staffed with from 5 to 30 people.

An Innovative Center

Wallace: After the group came together in Menlo Park about 1956, for some reason the creative sparks started to fly and things began to happen in a way that I have never experienced elsewhere. So Menlo Park turned out to be very innovative, and many major programs were born here, such as those in marine geology, earthquake studies (including prediction), volcanic studies, astrogeology, and geothermal energy. Up to early 1995, there were roughly a thousand people here at Menlo Park, enough for a strong, versatile center.

Headquarters administrators have told us, however, that Menlo Park was a pain for them--I guess we tended to be rather independent and to a large degree nonconformists. Good friends at other centers, who, I believe, envy Menlo Park, have referred to the "prima donnas" of Menlo Park. Truly, there has been an electricity, an excitement, a creativity at Menlo Park that is hard to find many other places. I believe the Menlo Park spirit did infect the group in Denver and elsewhere.

I recognize a lesson in the innovative success of Menlo Park, but cannot spell out the exact formula. The birth of each new program to some extent can be traced to one or a few individuals who provided an initial spark. For example, Parke Snavely provided inspiration and continuing drive to create the marine program. Gene Shoemaker took on and succeeded at an even bigger task--shifting NASA's whole lunar program objective from simply getting people to the Moon and back, to focus on lunar scientific exploration. As that developed, the USGS astrogeology program came into being. Gene felt that the USGS was a better scientific base than if he moved to NASA.

Scott: At the centers, the Survey people had the organization and the environment helping them, presumably making it possible to operate more effectively.

Wallace: There has to be the right environment. In some way, however, as Pres Cloud said, in the USGS innovative approaches always "well up" from the ranks. There is something about innovative people that cannot be denied. Regardless of the program, certain elements seem essential, including an environment amenable to innovation, and the freedom for individuals to take on new ideas and see them through. I think the Survey had an excellent environment here in Menlo Park. I cannot imagine a better one than existed in Menlo Park through the late 1950s and early 1960s.

Interdisciplinary Communication

Wallace: As I said earlier, the field-center set-up helped my long-term goal of getting geologists and geophysicists to work together. For example, in buildings 7 and 8 on the Menlo Park campus, there are many superb geologists, and geophysicists. We see each other, we have lunch together, we talk, we debate and argue, have seminars together. A fundamental of merging disciplines is to get people to work together; then to know and respect each other through day-to-day contact.

Scott: Certainly having such a variety of specialists working together promotes innovation and cross fertilization of ideas.

Wallace: So with the field-center concept that Tom Nolan and others developed, the center here at Menlo Park was born, and a whole variety of people started working together. This helped so much to bridge gaps and promote multidisciplinary approach that can be awkward when people are housed too far apart. Distance is a always a barrier, even when another building is only 100 yards away. The seemingly chance events that brought creative individual scientists together into such a great group in Menlo Park cannot be documented, but, thank goodness, it did happen.

Earth-Orbiting Satellites and Space Exploration

Wallace: Before resuming my chronology by taking up the Nevada geology mapping story, I want to digress for a moment to describe significant developments that were occuring on quite another front. A year after we moved to California, in October 1957, Sputnik was launched by the Soviet Union. That event must be considered a major threshold in human affairs, and with it civilization was transformed forever. The first night Sputnik crossed over the San Francisco Bay area, I watched it and was awed, and the next night I captured in on timed photos.

Earlier, I have told of being captured by obsessions, and this was a moment when my psyche turned flip-flops. Night after night I watched Sputnik, and was puzzled by changes in its path, its fluctuations in brightness, and many other things. My curiosity lead to all sorts of calculations, readings, and association with others who were involved in space activities. A new world opened up for me and a lot of others.

Scott: I recall my own fascination with Sputnik. I even got up early one morning at about 4:00 a.m. just to watch it go over, way up there shining in the sunlight. But please proceed. What sorts of things did you do with regard to satellites?

Wallace: I got acquainted with scientists at Stanford Research Institute and Lockheed space center. In the first weeks, volunteers were the main trackers of Sputnik, and I was told that on one night, when clouds obscured other sites, I got the only fix on Sputnik. Remember that there were few computers in those days, and orbital mechanics were the domain of only a few astronomers and mathematicians. I knew nothing except the simplest physics. After months, even years, of recording and photographing satellites, however, I finally had a fair idea of the orbital mechanics. I was even bold enough to write a paper, and fortunately the British Interplanetary Society accepted it for publication. (Wallace, R.E., "Graphic Solution of Some Earth Satellite Problems by use of the Stereographic Net," Journal of the British Interplanetary Society, v.17, 1958, pp.120-123.)

An important connection for me was with the Photo-Track group, headed by Norton Goodwin, a lawyer in Washington, D.C., who was executive director of the American Society of Photographic Scientists and Engineers. Eventually, Goodwin arranged with the Smithsonian Institution to provide a small group of volunteer photographers with ephemerides of the passage times of satellites for particular locations.

This group of photographers included a mathematician from the University of Kansas, a scientist from the Dupont Corporation, and half a dozen others. We worked out ways to intersect the photographic track of the satellite at specific times according to the National Bureau of Science time signal, WWV. I designed and built a camera and interruption system with a synchronous motor. This system permitted me to determine within about 1/10 second the time of the passage of a satellite. On the photographic film a background of stars permitted location of the satellite. If I remember correctly, when four or five of us combined our records from across the US, we could determine the position of the Echo satellite to within about 200 meters.

My life-long hobby of ham radio also came into play. A group of amateur astronomers on the West Coast, who were also hams, created the "Astronet." We met nightly on the radio to track satellites and make lunar observations, especially of anomalous lights on the moon, while the times and concurrences of observations were recorded on tape for later analysis. The net continues to this day, but most of the original members have died. At times the group took up earthquake reporting, which was helpful to me personally.

Scott: You must have taken great satisfaction in all of this. Did this activity have anything to do with USGS missions?

Wallace: Not at first--it was all evening hobby activity. But Gene Shoemaker of the USGS ramrodded what came to be an Astrogeology Program, financed by the National Aeronautic and Space Agency (NASA). Early on, I commiserated with Gene frequently, and eventually made a decision not to formally join the USGS astrogeology effort, but to continue with the field-geology studies. Space exploration will always be dear to my heart, however, and for a while I became deeply involved with remote sensing of geology and especially faults, a byproduct of the space program. Gene went on, and, in my opinion, he personally influenced the transition of the whole space program toward science exploration.

President Kennedy had set the goal for men to reach the moon and return to earth in a ten-year period. Gene had a major role, if not the principal influence, in inserting a scientific goal into the original goal of NASA. "Why go to the moon and ignore the chance to discover what the moon is made of, how it formed and how it might be used as a platform for farther explorations?" This was the rationale that Gene successfully sold. The USGS did end up having a major role in lunar and planetary exploration, and Gene and his wife Carolyn made major scientific breakthroughs by personally embarking on studies and tracking of asteroids and comets as part of the story of the solar system. Of course, the moon had been bombarded by asteroids, and the Shoemakers research was really an extension of Gene's early lunar work.

Gene's USGS career characterized the USGS approach throughout much of its history, in which the "upwelling of nonadministrative" leadership carried the USGS into major new fields which proved to be of national significance. For his contributions, Gene was awarded the Presidential Medal of Science. It was Gene and his wife, plus a colleague, David H. Levy, who identified Comet Shoemaker-Levy 9. That comet crashed into Jupiter, the first fragment hitting on July 16, 1994, followed by a series of other fragments. These were spectacular planetary events. Dark spots larger than the earth were produced on the face of Jupiter. A slightly different path could have carried the comet into collision with the Earth, obliterating all life.

Scott: Or most life, and then maybe a new evolutionary sequence could unfold, as it did after the dinosaurs met their end. I am glad you brought out this side of your interests, as well as the USGS involvement in the space program. Many people are not really aware of that.

V GEOLOGIC MAPPING IN NEVADA

Scott: The account of your Sputnik interest and satellite observations fits right into the period you were discussing, the mid-1950s. That would have been sparked shortly after you had returned to California and embarked on the Nevada geologic mapping work.

Wallace: In 1956, Charles Anderson, who at the time was in charge of the mineral resources program, decided that I should work in Nevada next. I was unhappy about that because I had a really significant project staked out as an extension of the effort in the area of Coeur d'Alene, Idaho. But at the time, Anderson was in charge of the mineral resources program of the Survey, and as such was my boss. He had received his Ph.D. at the University of California at Berkeley, and taught there for several years. While at USGS he become noted for his geologic research, was elected to the National Academy of Sciences, and served as president of several national professional societies, including the Society of Economic Geologists. He served as Chief Geologist of the Survey, and was a strong research leader, as well as practical and familiar with industry.

My Idaho project had been given preliminary approval, but in the end never came to be. I planned to investigate what we call the Lewis and Clark Line (named by the famous economic geologist Paul Billingsley), across Washington, Idaho and Montana. The Osburn fault is a major element of the "line." The proposed project had important implications for both continental tectonics and mineral resources. But instead of pursuing that, I went to work on a geologic map for Nevada.

My Nevada mapping started in 1956. The program Anderson assigned me to in Nevada was being conducted cooperatively with the Nevada Bureau of Mines and Geology. The bureau's director was Vernon Scheid, a long-time friend from Pullman, Washington, days. He had been at the University of Idaho as professor and head of the Geology Department, and director of the Idaho Bureau of Mines and Geology.

The Goal: A Geologic Map of Nevada

Scott: Would you discuss the purposes of the Nevada project?

Wallace: The goal of the program was to prepare a geologic map of Nevada, for which there was only a very general, grossly incomplete and inadequate map at the time. Such state geologic maps assist the exploration for minerals and fuel resources, development of highways, and many industrial projects by private industry, and they form a data base and interpretation of the geologic structure of the region.

Scott: That sounds like a major undertaking. How did you go about it?

Wallace: The plan was to start from scratch by mapping geology at a scale of one-mile-to-one-inch (1:62,500 actually) of selected quadrangles. Unfortunately, there were very few existing topographic maps at that scale available for use as base maps on which to plot and analyze geologic findings. Topographic and geologic programs within the USGS had to be coordinated in order to proceed with the geologic work. Thanks to the USGS Topographic Division (now the National Mapping Division), we did get new topographic maps on schedule. Now, in 1995, it seems hard to remember the time when only a few topographic maps were available in Nevada.

Scott: Regarding the scale of 1:62,500, when you multiply 12 BY 5,280, you get 63,360. So there is a slight discrepancy.

Wallace: Yes, the so-called mile:inch map is actually 1:62,500 rather than 1:63,360. For decades the 1:63,360 scale was used and was stated on the maps, but then it was changed. I don't know what year the change to 1:62,500 occurred, but believe it was after I joined the USGS. The reason was that 1:62,500 is an even fraction of 1:125,000, 1:250,000 and 1:500,000, all scales that were becoming more and mored used by map makers.

In the summer of 1956 I started field studies and geologic mapping of the Humboldt Range in north central Nevada, with colleagues Porter Irwin and Norm Silberling. We started in the Buffalo Mountain quadrangle, which straddles the southern end of the Humboldt Range near Lovelock. (A quadrangle is a map 15 minutes of latitude and longitude on a side.)

Mines in the range had produced silver, gold, quicksilver, iron and some rather rare nonmetallic minerals such as dumortierite, which had been used in the manufacture of spark plugs. Classic localities for a hundred or more species of fossil ammonites were found in the range, and were of special interest to Silberling and to paleontologists around the world. The detailed geologic studies of the Humboldt Range were to serve as an anchor for a reconnaissance study of Pershing County, and preparation of a geologic map for the county. Our geologic studies eventually were to lead to geologic maps of each county in Nevada each published separately. These would then be merged--and, indeed, were merged--into a generalized geologic map of the state.

Reassignment and the Work Pattern

Wallace: In 1956 the idea had been that I would be responsible for the synthesis of the state geologic map, but other assignments came along to prevent this. Years later the state geologic map responsibility fell to Jack Stewart, who produced a superb geologic map of Nevada. Jack developed many other important syntheses and scientific findings based on information gathered in producing the map.

Scott: Explain a little about your reassignment.

Wallace: In 1960 I was reassigned to serve as Chief of the Branch of Regional Geology in the Southwest States, which included responsibilities for Nevada, Utah, and Arizona. Following common practice, I rotated out of that administrative assignment in 1964. With the work in the Humboldt Range and the wider ranging responsibilities with the Branch, I became deeply involved with and excited about the geology of the Basin and Range province as a whole. I still am. I noted earlier how one usually served as a branch chief for four or five years. I followed the pattern, and after that assignment, fate took me back to studying the San Andreas fault again.

Scott: Were you headquartered here in Menlo Park when working on the Nevada geologic mapping project in Nevada, and after you were made branch chief?

Wallace: Yes, we all were headquartered here in Menlo Park, California. The USGS was just opening up its headquarters here, and in 1956 I moved into a brand-new building. Following the pattern I have already described, the field work was done in Nevada, where we would spend three or four months in the field, and then bring all our rock samples back to Menlo Park to do microscopic work and chemical analyses, and do our report writing. As you recall, when we worked in Alaska, we would go to Washington, D.C. to do the analyses and writing. That had been the operational pattern since the USGS was founded in 1879.

Geologic Mapping and Investigation

Wallace: There was a great deal to do. It took a lot of original field examination, analysis and plotting of geologic relations on a map. That is why I have come to resist using the term "geologic mapping"--most people interpret "mapping" as a very routine operation. In fact, if geologic mapping is done properly, the process is a fundamental scientific effort and a demanding intellectual exercise.

Scott: What term do you prefer to use instead of "geologic mapping"?

Wallace: I like to say, "geologic analysis of a region." A geologic map can be described as presenting in graphic form a four-dimensional (including time, of course) interpretation of a part of the earth's crust, and a history of how it was created. It is far more complicated than a topographic map, on which rather straightforward things such as roads, buildings and elevations are depicted.

I maintain that almost every rock outcrop looked at is, in a sense, a research project, and the synthesis of all the outcrop data is a major research undertaking. Multiple working hypotheses are employed at a given time, and trying to weave them together is a demanding intellectual exercise. Since the mid-1960s, for example, interpreting the meaning of an outcrop or a mapped area in the context of plate tectonics demands the best intellectual capabilities one has to offer. Any major new geologic concept that becomes generally accepted then tends to make older geologic maps obsolete.

For example, after the plate tectonic revolution of the late 1960s, one might look at a rock outcrop and say, "This rock type cannot have been created anywhere near that rock type across the valley, so a great conveyor belt of the plate tectonics system must have dragged it here to Nevada." Indeed, the geologic investigations--"mapping"--themselves account for major changes in ideas. The hundreds of kilometers of displacement along the San Andreas fault came from geologic mapping, as did the concept of microplates and terranes.

The Basic Geology of Nevada

Scott: I can see that would be very demanding work, but also very rewarding. What were some of the geologic findings in Nevada that stand out in your mind?

Wallace: In Nevada, slabs of rocks many kilometers thick and hundreds of kilometers on a side have been moved eastward 100 km or more, to be telescoped and shoved together along great thrust faults. In 1956, when I first went to Nevada, the concept of such large-scale regional thrust faults was just coming into being, and plate tectonics had not yet been invented, or at least had not matured to a point where the majority of geologists accepted the idea.

In actuality, the concept of continental drift had been strongly argued for decades, and in 1958 S.W. Carey of Tasmania put together an impressive symposium volume on continental drift. (Carey, S.W. (Convenor), Continental Drift: A Symposium, University of Tasmania Symposium Series Proceedings, 1958.) Many of the concepts presented there are now accepted and merged under the name "plate tectonics". But the missing element in 1958 was proof of seafloor spreading, which gradually received major acceptability in the mid-to-late 1960s.

Some geologists doubted that the large thrusts were connected, or that these were relatively large regional phenomena. They continued to argue that thrust faults were rather local cases of smaller slabs or rock having slid off local mountain blocks. We were trying to verify or disprove the reality of such huge thrusts. The great Roberts Mountains thrust and the Sevier orogenic belt were of Paleozoic and Mesozoic origin respectively. Structures like that had characterized the crust of the earth tens of millions of years before the great block faults started to break up the crust to produce what we now call the Basin and Range structure.

Block-fault movement is such that where one mountain-sized block moved downward several kilometers, and the adjacent block moved relatively upward. Upward movement of a block led to formation of a mountain range, and downward motion produced a basin. All of this vertical action in Tertiary time, starting some 40 million or so years ago, was superimposed on the much earlier great thrust structures. The combination produced an extremely complicated geometric and geologic puzzle.

As the Nevada work proceeded, people began to realize that not all of the slab movements were due to thrusts from the rear or from depth. Some slabs 10 or 20 kilometers thick and a hundred km on a side seem to have slid eastward. That is still a puzzle--why did they slide toward the interior of the continent. Perhaps similar blocks also slide toward the ocean basin. That question is one I would like to answer.

Such were the problems. The possibilities, or working hypotheses, had to be kept in mind when considering every outcrop. The synthesis of the data, in part expressed and illustrated on the geologic map, produced the rationale for one interpretation or another. These were exciting times, prompting discussions and arguments whenever two or more geologists got together.

Scott: This background sets the stage very well for describing the kind of investigations you actually did in the field.

Administration and Field Work: A Survey Tradition

Wallace: I mentioned above my personal field activity in the Humboldt Range. In addition, being in charge of the branch, I also took time and made trips through Nevada, Utah and Arizona and visited our staff that was working in each of these areas.

Scott: They were doing similar things to what you were doing in the Humboldt Range?

Wallace: Yes. I would visit them and we would have field discussions. We all knew about this big picture that we were trying to evolve, and we had wonderful arguments and debates.

Wallace: In summer months, some of the people, such as Tom Nolan, who was then director of the USGS, worked out of Eureka, Nevada. He was marvelous in that by his own activities he demonstrated the Survey's interest in major geologic problems. In the old days, back to John Wesley Powell, Clarence King and others, they all got out in the field. Nolan was the last director really to do that while he was director. Each summer he ran the Survey by telephone from Eureka, Nevada.

Another who was out there was James Gilluly, who, without doubt, is seen as one of the two or three greatest geologists to work with the USGS. He was attracted to Nevada because of intriguing and difficult problems to be worked out, and a lot of his fame came from his interpretations there. Art Baker, associate director under Tom Nolan, was working in Utah on the big Charleston thrust. And, strangely enough, Nolan, Baker and Gilluly were listed as members of the branch I managed.

Scott: So despite their senior administrative responsibilities and titles, they were also working within a USGS branch doing geologic mapping?

Wallace: Yes. Of course, I really had no administrative control over them. I was really a servant to them. I like to think that the best operation of the Survey is in having the branch chiefs and office chiefs work as the servant of the research staff. Over the years that relationship was promoted by rotation: your field assistant today may be your boss tomorrow. Branch chiefing seldom lasted more than four years.

Scott: Yes, you mentioned rotation before, in connection with your tour of duty in Washington and your return to California. I see how that was very much a pattern in the USGS.

How Field Work Was Done

Wallace: Our daily activity in the late 1950s started by driving our government jeeps from Lovelock--later we stayed at Unionville--out to the work site. We would drive to the base of a mountain someplace and start climbing up a ridge. Each day, I would go up one ridge and my partner would go up another, climbing from an elevation of about 4,000 feet up to about 8,000 or 9,000 feet. We worked out a strategy so that we could effectively sample all the rock types and distribution of structures in the Humboldt Range.

We mapped about one-half a square mile a day, or maybe a full square mile. Typically a quadrangle like that would take from three to four years. During that time we would hike it, record the rocks, gather the samples, analyze them chemically, check the fossils as to age, species and so forth, draw the structural diagrams, and then write it up. We really put together a four-dimensional history of the block covered.

Scott: It was a pretty thorough investigation of the territory.

Wallace: It is thorough for its purposes, but, of course, there are various levels of detail in mapping. For "mine mapping," what we did would be considered only the broadest-brush investigation and wholly unacceptable.

Scott: It must have taken a lot of effort to cover that kind of area with that kind of care daily, especially considering the terrain. That was a lot of hiking!

Wallace: Yes, it was day-to-day hiking, looking, and collecting. When we say "fieldwork", we mean it. You are out in the field and in the mountains, hiking. The crest of the Humboldt Range is about 9,000 or 10,000 feet in elevation, and the base of the valleys is usually about 4,000 to 5,000 feet. So it was very rigorous work. We all loved it. You got to know the plants, soils, rocks, and geography intimately--better than almost anybody else, and all features related one to the other. I found it to be very satisfying. USGS is not, however, doing as much of that kind of geologic investigation anymore.

Funding for geologic mapping has decreased dramatically, for several reasons. For one thing, most areas in the U.S. now have some sort of a geologic map, inadequate as they may be. As a result, the original charge to the USGS in 1879 to make a geologic map of the country has almost been forgotten. Most of our earth science colleagues in geophysics and geochemistry, furthermore, have little concept of what a geologic map is or how it can be employed in interpreting the earth.

At present, mineral and energy resources are not perceived as being as critical as they were, for example, during World War II. At least the federal government is not seen as having a major responsibility. But, mark my word, with a limited planet and a burgeoning population, this can only be a temporary situation.

Scott: I heartily agree. You don't have to be an earth scientist to look at trend lines and realize that some crunches lie ahead.

Thrust Faults: A Four-Dimensional Picture

Wallace: I noted that our pattern of operation was usually four months in the field in Nevada, and the rest of the year here in Menlo Park, writing, and getting chemical analyses and paleontological analyses and the like. The end result was a four-dimensional picture--the three dimensions of space, plus the geologic time dimension. In the 1950s Nevada was geologically unknown in many ways and in many of its regions. The idea of great thrust faults was being developed, and then the plate tectonic revolution was changing the way we all looked at the rocks and structures we saw. It was an exciting time and place to be doing that kind of investigation.

The earth's crust is not shaped quickly. I mentioned thrust-faulting periods hundreds of millions of years ago. The crust was fractured early, and those fractures are still there, forming the framework for the more recent faults that control the earthquake story. I have given papers on the "pre-fractured" nature of western North America and its relation to modern earthquake activity. The crust was fractured from early-on. From 200 million years ago on there were great sutures or ruptures that break again and again. Those ruptures set the stage for the later activity, although the stresses and strains change dramatically.

The thrust faulting is usually thought of as a compressional phase--although there is some question about that. For the last 30 to 40 million years, the Basin and Range province activity has been extensional--maybe as much as 100 percent extension. This is particularly true of about the last 14 or 15 million years up to the present--the period of block faulting that characterizes the Basin and Range.

I have long maintained that the Basin and Range story and that of the San Andreas fault are closely linked. Others are beginning to come around to that. In a recent seminar by Tom Brocher at the Survey, and Ben Page at Stanford, they spoke about the structures along the San Andreas fault being related to thrusting from the Basin and Range region. I like their ideas, but I believe that there is more to the story than simple "thrusting".

Plate Tectonics: A Transition in Thinking

Scott: The acceptance of plate tectonics must have helped a lot with the new interpretations you have been talking about. As I understand it, before plate tectonics was understood, interpretations assumed that rather static conditions prevailed over very long periods of time.

Wallace: Yes, one school of thought saw things as basically static. But in the 1940s, being weaned on the San Andreas fault, I was indoctrinated by personally finding movements of tens of kilometers. In fact, my thesis mentioned possibly 70 miles (112 km) of displacement. That was extreme for the time, however, and I put it in the thesis on the basis of very fragmentary evidence.

A few years later in 1953 Mason Hill and Tom Dibblee came out with their paper projecting even greater displacement--hundreds of kilometers. Such ideas were considered pretty drastic, of course, and a great many people were not accepting it. Then came plate tectonics. In my mind plate tectonics was easy to accept after I had concluded that great faults could have hundreds of kilometers of slip. (Hill, M.L., and Dibblee, T.W., Jr., "San Andreas, Garlock, and Big Pine Faults, California: A Study of Character, History, and Tectonic Significance of Their Displacements," Geological Society of America Bulletin, v.64, n.4, 1953, pp.443-458.)

In the Soviet Union, some of the great geologists there were still thinking vertical tectonics. In their view, blocks could come up, shed sediment and fill up a basin, but they did not accept the idea of these great lateral movements. Moreover, when I was back at Northwestern, I had been taught that great thrust faults were impossible because rocks were not strong enough. But at Caltech my life in graduate school geology related to great earth movements.

There has been a extremely exciting transition in fundamental physical ideas about how the earth works. For example, when I was at Caltech up to 1942, the term "granitization" had never been mentioned in any of my classes. People like G. E. Goodspeed at the University of Washington, however, had seen that granite-like rocks were produced by metamorphic changes in sediment. No original molten mass was involved. What a controversy that caused!

Scott: The idea of long-distance movement along faults, and granitization, were new ideas broached at about that time?

Wallace: Yes. Those ideas were new in about the early 1940s and 1950s. There were tremendous transitions in thinking, and they are still going on, right now. It is one of the fascinating things about the field. I do get irritated when I see some very simplistic statements about how the earth works, and many of our earthquake hazard-reduction methods still rest on antiquated concepts of that. Nevertheless, we try to push ahead so that what is known is assimilated in the practical or applied end.

Paleoseismology: A New Approach

Scott: I understand that in recent years studies of faults have focussed on identifying prehistoric earthquakes and estimating slip rates.

Wallace: That is so. For me that meant that I turned to looking at faults in a different light than I had been doing in mineral programs and even differently than when looking at regional tectonics, although the new views do contribute in major ways to concepts of regional tectonics. Field techniques for these studies were very different than for the traditional geologic mapping that I had spent my life doing. I had to invent new ways to do the job, as did others.

Scott: Would you explain that a bit more?

Wallace: Identification of prehistoric earthquakes followed two general lines, one geomorphic, for example, analysis of ancient fault scarps generated during earthquakes, the other by the analysis of offset of sedimentary layers revealed in trenches dug for the purpose. Combinations of the two methods permitted long-term slip rates on faults to be determined. Short-term slip rates are determined by geodetic methods which can be meshed with the long-term rates. In any event, these kinds of studies not only have great theoretical value, but also can have some practical application in estimating the likelihood and size of earthquakes for engineering and planning purposes.

Scott: That is an interesting point. Would you say a little more about the practical applications?

Wallace: The dating of prehistoric earthquakes has shown, for example, that in the Basin and Range province major earthquakes along any one fault occur only at intervals of thousands of years. In contrast, along the San Andreas fault system the interval is measured in hundreds of years. Such differences can not be even recognized by instrumental records only a few decades long. The instrumental records, or even the longer historical records, do not bracket even a single cycle of the great earthquakes in the U.S. How can we possibly study long-term patterns with such a short sample?

As for slip rates, the idea is that elastic or brittle crust must be bent and warped (strained) before it breaks to produce an earthquake. Just as a rubber band must be stretched before it breaks. Slip rates measure that stretching, warping or bending. For a fault to break and create displacements of 5 meters, which could produce a magnitude 8 earthquake, the ground (crust) must first be bent 5 m. Geologic mapping of offset rock units can document the rate of offset over millions of years, and that slip rate can then be translated into how long it would take for a 5 m. bend to be produced, thus how long between great earthquakes.

Scott: Please describe how this has been used practically, in terms of public policy.

Wallace: I'll give two examples. Data on recurrence and slip rates were the fundamental data base that made possible the 1990 estimate of probability of large earthquakes in the San Francisco Bay Region, one of the first detailed analyses of a forecast (synonymous with long-term prediction). This analysis of probability underlies many plans, such as justifying the retrofitting of the Golden Gate Bridge, and adopting new values in building codes.
(Working Group on California Earthquake Probabilities, Probabilities of Large Earthquakes in the San Francisco Bay Region, U.S. Geological Survey Circular 1053, 1990.)

Second, the recognition of fault slip and the demonstrated ability to identify and map seismically active faults led to the enactment of California's Alquist-Priolo Act of 1973 which pertains to controlling building across active faults. That Act was actually the first legislative act based on fault slip that I know of.

Scott: Historic and instrumental records alone do not give a good idea of how often and where earthquakes will happen do they? Say a word or two about their inadequacies.

Wallace: While such data is of course extremely important, it is only a partial and incomplete record. The periods of time covered by instrumental records are not sufficiently long. Also, while historic information in some parts of China and a few other places in the world goes back pretty far, the incompleteness of such records emphasizes the inadequacy of such historical records and the need for paleoseismology.

Scott: Is that especially true for California?

Wallace: In California, our instrumental seismic records do not bracket even one cycle of great earthquakes, which are not frequent events along a given portion of most major faults. In contrast, the stratigraphic record along the San Andreas fault is fairly clear over a few thousand years. In Nevada we can clearly recognize fault-scarp evidence of earthquakes going back to 100,000 years. Dating of scarps that can be used to document earthquakes over the past 10,000 years is getting more and more precise. In Nevada we found evidence of dozens of individual big earthquakes having occurred in prehistoric time.

Through our cooperative programs with the Japanese, Chinese and Russians, interest in paleoseismology has spread around the world. The late 1970s saw the first trenches dug for paleoseismologic purposes in Japan and China.

Scott: Where did the term "paleoseismology" originate?

Wallace: Having worked in Nevada, I knew about the big faults where earthquakes had occurred in historic times--1915, 1932, and 1954. I decided that if I was going to work on the earthquake program as a geologist, I needed to get geology into the program more effectively. I knew I had something to contribute, but about 1965 I was not sure what would be a good course of action.

I picture these thoughts as leading to what we now call "paleoseismology." Against the strongly expressed advice of some of my colleagues, I started pushing the use of that term. Others did not favor introducing new terms, although that term had been used once or twice before. John Adams of Canada was one of the first to use it.

Scott: Did your plan work out?

Wallace: For one thing, I think use of the term contributed to one long-term goal, that is, seeing that geologists and geophysicists got together more effectively. Seismology was devoted mostly to instrumentation, yet, as I said earlier, the seismic record was far too short to determine the long-term pattern of big earthquakes along the San Andreas fault. Paleoseismic techniques could, and, indeed, have filled that need. Furthermore, the term "paleoseismology" legitimized the use of geologic techniques in the broad field of seismology.

Estimating the Age of Scarps

Scott: You have mentioned Nevada several times in discussing paleoseismic work yourself. Is that where you got started?

Prehistoric Fault Scarps in Nevada

Wallace: Yes, I gravitated to Nevada because of my previous work there, and found that I could do paleoseismic analyses there rather easily. I could see not only the 1915 earthquake scarp, but also others along the same line that were more subdued. These were older prehistoric scarps for which there was no instrumental record. The 1915 scarp was superimposed on earlier scarps representing at least two earlier earthquakes. In an area of several 2-degree sheets of the topographic series, I could find evidence of a dozen or more large prehistoric earthquakes for which, of course, there were neither historical nor instrumental records.

Observations of the amount of degradation of the scarps permitted ballpark guesses of how old they were. It worked out that, even working alone, I could quickly measure the profile of a scarp. And from a comparison to the shoreline of glacial Lake Lahontan I could get an idea of how old the scarp might be. The last high stand of Lake Lahontan is known to be about 12,000 years old, and the wave-cut cliff produced at that time has since eroded to a certain slope angle and form. Using that slope and form as a calibration base then permits an estimation of the age of a fault scarp.

I tried several techniques of measuring scarp profiles and finally found that simply laying a fiberglass stadia rod on the scarp in a succession of end-to-end positions, measuring the slope of the rod at each position with an Abney level, gave excellent, reproducible results. It took me a couple of years to latch on to this method, but early-on, no fiberglass stadia rods were on the market. At first I was also not sure just which measurement would be the most valuable.

Scott: What did you find?

Wallace: Very soon I concluded that thousands of years separated the big earthquakes on individual fault segments. Also, the long-standing puzzle about repetition of displacement on narrow strands of a fault was immediately answered. There had been repeated movement on narrow fault strands in Nevada, just as I had found along the San Andreas fault at Wallace Creek.

Wallace: One regional question I tried to answer was whether in the older record earthquakes had been migrating toward or away from the historic 1915 break, which resulted in a 7.6 earthquake. I wanted to see whether or not I could determine any long-term geographic pattern of strain release. Perhaps that would be a forecasting tool.

Scarp Degradation

Scott: You mentioned using evidence of scarp degradation to estimate the age of prehistoric earthquake. Say a few words about the factors causing degradation.

Wallace: The main agents of degradation are water, wind, and the action of biologic agents such as lichens and mosses. In Nevada I found that the most important agent seemed to be the freeze-thaw cycle--the freezing and thawing of the moisture in the soil. In Nevada approximately 130 times a year the ground goes through a freeze and thaw cycle. Those cycles make the soil swell and collapse and move downhill. Farther north the ground stays frozen longer, and farther south it does not freeze at all.

Scott: I take it that at first the scarp profiling gave only a rather crude approximation of a scarp's age? Did the method progress on from that?

Wallace: Yes. I teamed up with Tom Hanks and we wrote a paper employing a diffusion model to quantitatively describe profiles, and to get a more precise age calculation. Diffusion is a basic process in chemistry and physics, and determines the rate at which particles or concentrations move. Tom found that this could all be put into a formula of down-slope movement of material. My joint study with Tom Hanks started one day as we sat on a scarp along the San Andreas fault. Tom said: "Is that really a fault scarp? It looks like an error function to me." That floored me, but indeed the shape of an "error function curve" does nicely approach the profile of many fault scarps.

Scott: Would you briefly explain "error function curve" so non-geology readers can understand its relation to fault scarp profiles?

Wallace: A simple description of an "error function" for the nontechnical person is difficult. In graphic form, an error function appears as a curve that has a somewhat flatttened and subdued "S" shape which relates directly to the diffusion process. Thus, the error function curve is a convenient means of quantifying the shape of a fault scarp that has been subdued over many centuries by downslope movement (diffusion) of sand and rock particles. From that we can quantitatively deduce the age of the scarp and its related earthquakes.

Wallace: We could apply some of the same formula to areas in China that had about the same climate as Nevada. I began comparing scarps in Nevada with those in Montana, China, Japan, and elsewhere to assess some of the variables related to climate.

Inasmuch as we have been talking primarily about the Nevada story here, I would prefer to develop the more general story of paleoseismology later, when we talk about the development of the USGS earthquake program. Many others played more important roles than I in the evolution of paleoseismology, and I want to mention them at an appropriate time.

VI FORMULATING A NATIONAL EARTHQUAKE PROGRAM: A TRAIL OF DOCUMENTS

Scott: Would you discuss the development of policies and programs aimed at reducing the disastrous effects of earthquakes? You participated in many of those developments, and I would appreciate your giving your recollections of some of the things you consider most relevant.

Wallace: Yes. I think it is important to record how some of those developments came about. Also, some of the things I can talk about represent important general tendencies, even generic lessons. We should not lose sight of those. I see younger members of the USGS staff who seem to think that by some magical process earthquake issues and geologic hazards were always on national agendas. They seem to think that the justification for their work has always been clear and well-understood. Nothing could be further from the truth.

In describing the process as I saw it, I would first like to review a trail of selected activities and documents that led to the Earthquake Hazard Reduction Act of 1977. It took some thirteen years of struggle, debate, and in-fighting to formulate and enact a National Earthquake Hazards Reduction Program (NEHRP). These battles--and they were battles--took large investments in manpower, time and funds, which were diverted from other programs in anticipation of future authorizations and augmented funding for earthquake programs. The process, of course, is never-ending, and the balance of program elements changes and evolves. But that major step of getting the 1977 Act in place was a big victory.

A Pre-1964 Effort

Scott: Most of us agree that the great Alaskan earthquake of 1964 started the modern era of earthquake awareness in the United States, and later other serious earthquakes emphasized the danger to life and the seriousness of economic consequences.

Wallace: Yes. In the early 1960s, before the Alaskan earthquake, several of us here at USGS discussed new big programs we should begin to push. One idea was a complete study of the San Andreas fault. We knew the fault had had big displacements, events that created earthquakes, and, thus, major public policy issues were involved. So in 1961 or 1962, several of us started pushing that idea. We proposed about a $2 million program with geology, geophysics, and allied topics, and sent it off to headquarters in Washington, D. C. for consideration. It got nowhere.

In seeking funds, the management at USGS headquarters had to make judgments on what could be "sold" to the Department of the Interior, and to Congress. They decided that our proposal would not sell, or perhaps they just did not want to put it high on the priority list. Also, we probably did not present it properly. But we made a noble effort back in 1961 and 1962. Parke Snavely, Jr., and I did most of the work on the proposal, and Earl Brabb was also involved. But we got nowhere.

Scott: But even before 1964 there had been some significant actions regarding earthquake hazards.

Wallace: Oh, yes indeed, there were spectacular accomplishments from the late 1880s on. Much of that early history is nicely summarized in publication that you and Bob Olson prepared. (Scott, Stanley and Olson, Robert. A., (ed.) California's Earthquake Safety Policy: A Twentieth Anniversary Retrospective, 1969 - 1989, Earthquake Engineering Research Center, University of California, Berkeley, 1993.)

Within the USGS, for example, G. K. Gilbert in 1884 wrote a paper which was published in Science titled "A Theory of the Earthquakes in the Great Basin, With a Practical Application." Also in a 1909 issue of Science, Gilbert published a paper titled "Earthquake Forecasting," the first reference I know of that has "forecasting" or "prediction" in the title. An early USGS publication pertaining to earthquakes was by C.E. Dutton. It appeared in 1889, reporting his findings on the Charleston earthquake of 1886.

Scott: In your view, was USGS pretty well-positioned to take a lead role in the more recent developments?

Wallace: By tradition, yes. But in fact, as late as the 1950s only a handful of investigators were active, and only meager funds were available for seismologic, geologic and engineering studies.

The Alaskan Earthquake: The Start of the Modern Programs

Wallace: The great Alaskan earthquake on Good Friday 1964 was a major turning point and a trigger for new programs. To my thinking, and most people's thinking, I believe, the Alaskan earthquake was the beginning of, and stimulus for, our whole modern earthquake program. That earthquake showed what great seismic events could do very close to home.

Scott: That earthquake did generate a lot of concern. Did you get involved in the Alaskan earthquake studies?

Wallace: I did not go to Alaska. That year I was trying to get out of branch chiefing--I was not cut out for administration. The Alaskan earthquake happened in March, but I was still deeply involved in Nevada, Utah and Arizona, and could not even get away to go to Alaska. I probably should have just gone, but I could not get away because of my other responsibilities.

Although I did not go to Alaska, I was involved in a lot of discussion about what should be done as a result of the Alaskan earthquake. Dozens of other USGS people did participate, however, and a wealth of information came out of the Survey's efforts. The USGS produced twenty-eight volumes in the USGS Professional Paper series, plus numerous others in the USGS Circular series and outside journals. I shall fully reference only two as examples. (Hansen, W.R., Eckel, E.B., Schaem, W.E., Lyle, R.E., George, Warren, and Chance Genie, The Alaskan Earthquake March 27, 1964: Field Investigations and Reconstruction Effort, U.S. Geological Survey Professional Paper 541, 1966. Plafker, George, "Tectonic Deformation Associated with the 1964 Alaska Earthquake," Science, v.148, n.3678, 1965, pp.1675-1687.)

Scott: Could you give us an overview of some of the more important other reports that came out, in addition to the USGS reports?

Wallace: Two types of reports began to emerge. The first concerned reporting on the earthquake itself, the second were reports by committees established to recommend future federal responses to the earthquake hazard. One of the first big reports on the earthquake itself came from The National Academy of Science, which established a committee to do that, chaired by Konrad B. Krauskopf of Stanford. A 596-page report was prepared and published based on the work of 48 contributors, including several from the USGS.

The U.S. Coast and Geodetic Survey, an agency under the Environmental Science Services Administration in the Department of Commerce also put out a three-volume report on the earthquake. This great federal effort to investigate the Alaska earthquake led to follow-up proposals for federal actions. But then it took years of long and arduous debate over what elements should be included and what the funding levels should be. As I noted, eventually the National Earthquake Hazards Reduction Act of 1977 was enacted.

Press Report: Earthquake Prediction

Scott: Review some of these other reports and recommendations, and if possible, say something about what you saw as the effects of each.

Wallace: In September 1965 an ad hoc panel on earthquake prediction chaired by Frank Press issued a report, which was prepared for the Office of Science and Technology. Press was a prominent seismologist at the Massachusetts Institute of Technology, as well as one of the most skillful scientist-politicians to come along. Press later served as Special Assistant to the President for Science and Technology in 1979-1981, during the Carter Administration, and as President of the National Academy of Sciences (1981-1993). After retiring from the presidency of the National Academy of Sciences, Press moved to the Carnegie Institute of Washington, D.C.

The Press report was slanted toward earthquake prediction, and used the term in its title. (Ad Hoc Panel on Earthquake Prediction, Press, Frank, (ch.), Earthquake Prediction: A Proposal for a Ten-Year Program of Research, Administrative Report to the Office of Science and Technology, Washington, D. C., September, 1965.) But many people did not think earthquake prediction was a reasonable goal. Charles Richter, after whom the Richter Scale was named, characterized as "charlatans" those who claimed that prediction was plausible.

Moreover the "ten years" in the title led to a misperception by some, especially the media, that scientists were promising to predict earthquakes routinely within ten years. That misunderstanding has plagued us ever since 1965. We heard the recurring comment: "Well, you said you would be predicting earthquakes in 10 years." To my knowledge, no responsible scientist ever said that in print. Admittedly, however, there were periods of great optimism and euphoria, when many scientists thought prediction breakthroughs were just around the corner.

Scott: The "ten years" probably referred to the proposed research program. That is, the program was to continue for ten years.

Wallace: Yes, that is correct. The Frank Press report anticipated a lot of the principles, such as the "gap theory" of earthquake succession, with maps of some areas, showing recent earthquakes, and gaps where others might be expected in the next couple of decades. The gap theory became very popular and still is a reasonable idea.

Scott: My impression is that the gap theory is seen as a good deal more than a reasonable idea. I have heard Bruce Bolt and some others refer to it very favorably, even quite recently.

Wallace: Yes. The Press report had diagrams showing how we should test strain at depth using deep wells, but only one deep well has ever been drilled. It was located at Cajon Pass, and was stopped long before completion. Now people are again looking at the idea of deep-hole drilling along the San Andreas fault. The Press report also proposed a full array of instruments to measure crustal changes, including seismometers, magnetometers, and strain meters.

That gives a pretty good picture of the Press report, an exceptionally good report that was very influential. A lot of what it recommended has come to pass.

Prediction Defined: A Third Line of Defense

Scott: I have long been bemused by the prediction discussion and debate. Some of the controversy hinged around the question of how quickly prediction would be achieved, and what degree of precision might be hoped for in location and timing of predicted events. Another issue was whether prediction would be possible generally, or only on certain special faults.

There seemed to be a lot of differences in interpretation of what prediction is, as well as its "validity" as a major research goal. Maybe you could start by defining what you see as constituting an earthquake prediction?

Wallace: In 1976 a panel of the National Academy of Sciences, chaired by Clarence Allen, defined earthquake prediction as a statement of magnitude range, geographic area, and time interval of occurrence. A confidence level was to be given for each prediction. (National Academy of Sciences, Panel on Earthquake Prediction, Allen, C.R., ch., Predicting Earthquakes: A Scientific and Technical

Evaluation--With Implications for Society: National Academy Press, 1976.)

In the early 1980s, three of us, Karen McNally, Jim Davis and I, who were serving on an advisory panel to the Southern California Earthquake Preparedness Project (SCEPP), found that planners needed more specific statements of the time element to help design hazard mitigation plans. At the time, the presumption, incorrectly, had grown that routine predictions would be forthcoming at any time, and, naturally public officials wanted to have response plans ready on the shelf.

We three prepared a report for SCEPP which defined more precisely what short, intermediate and long-term predictions were and how they all differed from the more common general statements such as "an earthquake is coming", or that "California is earthquake country." Planners made good use of it. Los Angeles, for example, designed rather different mitigation response plans for short-term predictions than were to be taken for long-term predictions. (Wallace, R.E., Davis, J.F., and McNally, K.C., "Terms for Expressing Earthquake Potential, Prediction and Probability," Bulletin of the Seismological Society of America, v.74, n.5, 1984, pp.1819-1825.)

I would like to review those controversies, but it is pretty complicated. There have always been strong and quite honest differences of opinion about where emphasis should be placed in the earthquake hazard reduction program. Over the years I have talked about three lines of defense against earthquakes.

Scott: What do you mean by "three lines of defense"?

Wallace: The first line of defense is earthquake engineering, that is, building earthquake-resistant structures. Over and over it is said, "The collapse of buildings is what kills." The Long Beach earthquake of 1933 triggered many innovations in earthquake engineering.

The second line of defense--which is just now beginning to be used effectively--relates to the prudent use of land--not building on active faults or areas susceptible to landsliding or earth movement. California's 1972 Alquist-Priolo Special-Study Act requires special examinations of areas near active faults before major structures are put up. Such an act was conceivable only after the USGS had produced several rather detailed strip maps showing locations of the most-recently active strands of the San Andreas fault.

Furthermore, some of us were finding evidence that movement on a given strand of the fault tended to occur again and again in the same place. Consequently, avoidance of those strands seemed imperative. Previously, until about 1960, we had no proof that movement on one narrow strand of the fault was more likely to repeat, rather than shift to other parts of a mile-wide band of highly sheared rock found along the fault zone. Of course, we all knew that it took repeated movements to create the broad fault zone itself. In 1967, however, I published a note reporting repeated movement on some strands along the San Andreas fault in the Carrizo Plain of central California. Since then, thousands of examples have been found.

The third line of defense should be prediction, but short-term and intermediate-term prediction are still in the research phase, and still to be realized, but long-term prediction (forecasting) is with us already. Many structures around the world do not conform even to minimum standards of engineering practice. Prediction, when achieved, could help us know when to evacuate collapse-hazard structures for short periods of time. In less-developed countries many communities cannot afford sophisticated engineering methods nor do they have suitable natural materials nearby with which to build resistant structures. That is a story in itself.

A Variety of Prediction Techniques

Wallace: Stan, you referred earlier to some skepticism about prediction--and, indeed, prediction is not here yet, except in this broad way of probabilistic forecasting. For example, the USGS estimates a probability of about 67 percent that a magnitude 7 earthquake will occur within 30 years on one of the faults in the San Francisco Bay area. We can also do pretty much the same thing in southern California, where the chance is about 80 percent in 30 years, I believe, but we cannot yet do it most places in the world. We are starting a whole new process here in California. It is exciting. (Working Group on California Earthquake Probabilities, Probabilities of Large Earthquakes in the San Francisco Bay Region, U.S. Geological Survey Circular 1053, 1990.)

We must, however, be wary of those who say, "The less data you have, the more you need probability." The weakest part of the probabilistic approach is the tendency to give a definite numerical answer, despite the enormous unknowns. We must continue to try to understand the fundamentals of how the earth works, and not settle for quick answers that may be misleading or meaningless.

I believe that for a long time, we won't be able to predict the hour, day and minute, although there were times when some of us thought that sort of prediction was just around the corner. In earthquake prediction there have been periods of excitement that; "By golly, we might be at the threshold of a technique for making short-term predictions."

Scott: Discuss some of the techniques that have seemed promising.

Wallace: In the late 60s, for example, the Soviets told us about Vp/Vs, a ratio of the velocities of the "P" wave and the "S" wave. They had beautiful diagrams showing how this ratio changes before a big earthquake. In 1969 Karl Steinbrugge led a group to the Soviet Union, and I was privileged to go along. I photographed these diagrams that were on the walls of Soviet labs, and we were really excited--"Here we are!"

The U.S. picked up the technique and followed it. Some people thought, in retrospect, that the 1971 San Fernando earthquake could have been predicted by this technique. US scientists developed theories about why this ratio change should happen. So there was euphoria in the prediction world for a couple of years after 1970. But it sort of fell apart. It did not prove out, although I think one small earthquake was reasonably predicted in the Adirondack Mountain region by the Lamont group using this technique.

About then the Chinese were predicting earthquakes by observations of animal behavior. They made a strong case that they had predicted earthquakes by combining animal behavior and what they called macroscopic events; observations of changes in water wells, gas wells that had exploded and started burning, and the like.

Scott: Those seem like rather different things--predictions based on large-scale events and predictions based on animal behavior.

Wallace: They are, but they were all woven into a mish-mash in the Chinese literature. In 1978 Ta-Liang ("Leon") Teng and I were with a delegation to China headed by George Housner. Leon and I represented earth science on an otherwise engineering-oriented mission. At one point, Leon and I left the engineering discussions and interviewed the Chinese about their prediction program, focussing on animal behavior and their so-called macroscopic precursors. Having been born in China, Leon is fluent in the language, including several dialects, so the interviews were especially productive, and Leon and I prepared a report on the subject. (Wallace, R.E., and Teng, Ta-Liang, 1980, "Prediction of the Sungpan-Pingwu Earthquakes," Bulletin of the Seismological Society of America, v.70, n.4, August 1976, pp. 1199-1233.)

Scott: Do you have any other examples of prediction?

Wallace: One of the most exciting and significant was the prediction by the Chinese of the great Haicheng earthquake of 1975. A special group headed by Barry Raleigh of the USGS went over from here to try to learn how the prediction had been accomplished. Apparently that prediction had involved a wholly different technique. There had been many, many foreshocks in the few days before the quake.

Scott: That is the earthquake where they warned people in advance to go outside.

Wallace: Yes, they claimed they saved more than 100,000 lives with that prediction and warning. The people got out of their houses into the open spaces. (Haicheng Earthquake Study Delegation, Barry Raleigh, ch., "Prediction of the Haicheng Earthquake," EOS, Transactions of the American Geophysical Union, v. 58, n. 5, 1978, pp.236-272.)

Probabilistic vs Deterministic Approaches

Scott: The literature on earthquake prediction often refers to the "probabilistic approach" and the "deterministic approach." You also see that discussed in earthquake engineering generally, as well as in many other fields. Would you digress a moment to discuss how you see those approaches?

Wallace: Yes. During the past several years a push has been made to develop quantitative means of expressing the future likelihood of strong ground motions and other hazards related to earthquakes. A major driving force has been the needs of the nuclear power industry to develop plants that were safe, and could be demonstrated to be safe. Unless the general public, public administrators, and politicians could be satisfied that nuclear plants could be made acceptably safe from earthquakes, nuclear power would be a questionable source for the ever-increasing demand for power. Back in the era when many saw nuclear power as the wave of the future, a lot of effort was put into geologic and seismic studies, and into methods of analyzing risk.

Both deterministic and probabilistic methods of seismic hazard analysis try to quantify the earthquake hazard so as to satisfy a variety of needs, ranging from figuring the kinds and amounts of reinforcing used in concrete, to estimating the relative value of different regional planning methods and emergency response techniques. Without such values, designers, decisionmakers and others concerned with reducing the hazards have no common starting point.

In 1988 the National Research Council of the National Academy of Sciences issued a key publication on the topic of seismic hazard analysis. Keiiti

Aki served as chairman of the panel of ten, including me, to review the state of the science. (National Research Council, Panel on Seismic Hazard Analysis, Aki, Keiiti, Chairman, Probabilistic Seismic Hazard Analysis, National Academy Press, 1988)

This review recognized five types of seismic hazard analyses, including "deterministic seismic hazard analysis" (DSHA), single- to multiple-model "probabilistic seismic hazard analyses" (PSHA), and hybrid procedures. Tom Hanks and Allin Cornell presented the idea that PSHA and DSHA have far more in common than they have differences. The main difference is that PSHA considers time. (Hanks, T. C., and Cornell, C.A., "Probabilistic Seismic Hazard Analysis: A Beginner's Guide," Proceedings of the Fifth Symposium on Current Issues Related to Nuclear Power Plant Structures, Equipment and Piping, Center for Nuclear Power Structures, Equipment and Piping, North Carolina University, 1994, pp. I/1-1-I/2-17.)

Advocates of the probabilistic approach champion its use in engineering and planning, whereas the deterministic advocates decry the probabilistic approach. I believe that I was invited to be on the Aki panel because, as a geologist, I was considered able to help balance what many saw as a band-wagon charge to standardize on a probabilistic approach.

After the Aki report was published, I was sorely criticized by a good friend and staunch deterministic advocate, Ellis Krinitzsky, who insisted that I had sold out to the probability advocates. (Krinitzsky, E.L., 1993, "The Hazard of Using Probabilistic Seismic Hazard Analysis," Civil Engineering, November 1993, p.1-52.) In fact, however, I embraced neither the PSHA nor the DSHA approach entirely, but believe that a combination will most likely serve best. I do advocate the need for much more effort on learning more precisely how the earth works, so that the data, rather than the mathematical process, will control the outcome. The mathematical process, in principle, quantifies the exercise, but too often tends to obfuscate it.

My first exposure to PSHS had been several years before the Aki committee met, and it horrified me. I saw probability used to create numbers, where basic data on earthquake process was scant or lacking. A mathematical friend of mine actually said, "The less data you have, the more you need probability." In my opinion such a philosophy must be terribly wrong. The process could be another example of what I call "implementing ignorance"--that is, give the client a number, even though based on incomplete or inaccurate basic data.

Scott: Yes. While your math friend was right about having to rely more on probability when you lack adequate information, there is a serious down-side. Relying more on probability than on good information can give some exceedingly questionable results.

Wallace: Yes. But of course many similar types of inadequacies go along with the deterministic approach, which is generally molded into a final report by using "engineering and geologic judgement." Here again, the lack of data on how earthquakes actually operate may overwhelm the whole process. In short, both the probabilistic and deterministic approaches have to operate "out in the blue" to the extent that they lack good data and a firm understanding of earthquake processes.

Fortunately, the wealth of data has grown enormously in the past several decades, and the geologist who has followed this development may well have the best intuitive grasp of the hazards. On the other hand you probably will not find two geologists--or two seismologist--in full agreement. So where does this all leave the decision-maker, who probably has little personal background in the subject?

Housner Report: Earthquake Engineers Respond

Wallace: The Frank Press committee report on earthquake prediction, which I discussed earlier, apparently made many engineers furious. The engineering community did not consider it the right approach for a national program to counter the effects of great earthquakes. The National Academy of Engineering, through a group of engineers chaired by George Housner, developed a committee on earthquake engineering research just to prepare an alternate agenda. This was under the Division of Engineering, National Research Council. Before this committee was created, I believe they had no formal group considering earthquake engineering, but I may be wrong on that point.

The Committee on Earthquake Engineering Research, Division of Engineering, National Research Council, National Academy of Engineering, with George Housner as chairman, prepared a report to the National Science Foundation. Their excellent report developed the engineering viewpoint on what should be done to reduce earthquake hazards. The report represented a major difference in emphasis between (1) earthquake prediction, seismology and earth science, on one hand, and (2) earthquake engineering, on the other. (National Research Council, National Academy of Engineering, Committee on Earthquake Engineering Research, George Housner, (ch.), Earthquake Engineering Research, National Academy of Sciences, 1969.)

These two documents--the Press report and the Housner report--were the beginning of well over a decade of consensus-developing reports and discussions--they might be called battles. These two documents really started the process of identifying what the nation should do to reduce the hazards of earthquakes. While both reports were very important contributions, they did not in themselves develop consensus at all. Quite the opposite--they sparked controversy. But they both brought out very important elements that had to be part of any comprehensive program.

Scott: Yes, these were two key documents treating the idea of a national program. What about the others that followed?

Wallace: Indeed there were others--these two were just the beginning.

Pecora Report: A Ten-Year Program

Wallace: Just before the 1969 Housner committee report came out, a proposal for a ten-year national earthquake hazards program hit the streets. It was put together by a federal interagency working group for earthquake research, of the Federal Council for Science and Technology. William Pecora, director of the U.S. Geological Survey, was chairman of that interagency committee, which was made up of government people, with none from the outside. (Federal Council for Science and Technology, Ad Hoc Interagency Working Group for Earthquake Research, Pecora, W.T., ch., Proposal for a Ten-Year National Earthquake Hazards Program: A Partnership of Science and the Community, Prepared for the Office of Science and Technology and the Federal Council for Science and Technology, 1968/1969.)

That report emphasized primarily earth sciences, which left large sectors out in the cold. In addition I will say that they made the mistake of starting to talk about money too soon. The report proposed budgets for a variety of rather detailed studies. Admittedly, at some point that had to be done, and their doing so helped shape the later plans. But the narrow focus and premature budgets alienated many serious players.

1970 Steinbrugge Report

Wallace: About the time these three reports were produced, a fourth panel was created under the Executive Office of the President, Office of Science and Technology, and was chaired by Karl Steinbrugge. Others on the panel were Clarence Allen, Henry Degenkolb, Richard Jahns, Nathan Newmark, Jack Schoop, Robert Shea, James Stearns and James Wilson. This committee produced the Report of the Task Force on Earthquake Hazard Reduction. I think an earlier version was issued in 1968 or 1969, but the copy I have here is dated August 1970. (Task Force on Earthquake Hazard Reduction, Steinbrugge, K.V., (ch.), Earthquake Hazard Reduction, Executive Office of the President, Office of Science and Technology, September 1970.)

This task force benefitted from all the earlier reports on the elements that should be included, but used an entirely different tack, and a very smart one. They started out, for example, with "engineered earthquake resistance for new governmental facilities," through "local seismic networks", "basic research in seismology", and so on; A-1 through C-8. They did the smart thing of classifying these according to long-term (C), intermediate-term (B), and short-term (A) payoffs. Everybody--each of the competing groups--had something in there ranked "high priority," which helped make everyone feel good. What a very skillful move that was! And the report did not mention one word about money. This is my interpretation of how it worked. Everybody's pet project was mentioned, and everybody had a high priority in short intermediate or long-term payoff. They all had something.

The report had a very simple format with a series of recommendations, given as a short underlined item for each recommendation. I will open it up to Recommendation No. A-9: "It is a federal responsibility to provide a realistic total plan for earthquake disaster response, and the plan must involve state and local governments where the hazard warrants." There is another page or so of elaboration, but that, as a policy statement of a priority--of something important to do--really rang a bell.

I give the Steinbrugge report credit for beginning to pull a consensus together. It is short, whereas the others are all rather long and ponderous. With that, people started talking the same language and thinking in more nearly parallel ways.

Scott: In your view, was it the deliberate intent of Steinbrugge and the others involved to do precisely that--to side-step controversy over dollars and promote consensus on set a of recommendations that could move ahead?

Wallace: Yes, absolutely. They were very much aware of all the dissension that had accompanied the other reports. I understood from Karl that it was Dick Jahns who came up with this format for the 1970 Office of Science and Technology report, as a way to avoid the conflicts of money priorities that Bill Pecora had fallen into. I think it is a real lesson for any groups that are trying to get consensus. Don't speak of money, too soon, and speak of priorities in some general way so that everybody is included.

Scott: Do you know any of the background of the formation of those various groups, and especially of the Steinbrugge group?

Wallace: No, I know very little. I see that you are listed as an advisor to the Steinbrugge group--do you know of the background?

Scott: I never really knew much of the background, but sometime after the group was already established he called me up one day and asked if I would help out. I do, however, recall some discussions with Karl, and attending one meeting of the group in Washington. You are quite right about Karl trying to avoid discussions of money and budgets. While the goal was to include something that each group considered valuable, he and the others also very much wanted an end that would truly be effective in reducing hazards. He wasn't just trying to throw everybody a sop--he was trying to build a workable program that would be accepted and that would make some progress in earthquake safety.

For a decade or more, Karl seemed to be participating in high level, federal executive and policy roles--the Executive Office of the President, the National Science Foundation, with the President's Science Advisor, etc. Karl was a leader--he was president of the Seismological Society of America, and of the Earthquake Engineering Research Institute along about then. He played a leadership role in many ways.

Later on in the 1970s, Karl and Frank Press were two advisors to the Office of Management and Budget (OMB) on earthquake programs. As I understand it from Karl, he sort of fought against the emphasis on earthquake prediction idea at first, but finally about 1973 or 1974 decided that prediction was here to stay.

OMB was looking into consolidating the earthquake programs in one agency--they were worried about duplication. Karl told me that he had a drinking-buddy friendship with Coast and Geodetic Survey people such as Bill Cloud. He was more for having the whole earthquake program go to the Coast and Geodetic Survey, which then was part of NOAA (National Oceanic and Atmospheric Administration). In the end, however, he joined Frank Press in recommending to OMB that the earthquake program come to the USGS, which it did in 1973. That is an activity that I should recount at some point.

Allen Report: Earthquake Prediction Evaluated

Scott: With all the controversy about earthquake prediction, how was the idea of prediction itself evaluated?

Wallace: Karl Steinbrugge's 1970 report, activities of the Committee on Seismology, NAS, and the continuing controversy about prediction, prompted establishment of a panel to evaluate prediction. Clarence Allen chaired the panel on earthquake prediction of the Committee on Seismology of the National Research Council, National Academy of Sciences. (Committee on Seismology, National Research Council, Allen, C.R., (ch.), Predicting Earthquakes: A Scientific and Technical Evaluation--With Implications for Society, National Academy of Sciences, 1976.)

The panel addressed the question: Is prediction a credible goal for scientific research, or are the people who talk prediction, indeed, "charlatans," as Charles Richter suggested? In brief, the report concluded that prediction is not only a credible area of study, but, in fact, very important. The report did not get published right away. They had a series of meetings, and then it took time to get the contributor's papers in and edited, I suppose. It is dated 1976.

I remember Clarence making a preliminary report to the Committee on Seismology, probably in 1972 or 1973, indicating that the panel would say that prediction is a credible area of research. A representative of Federal Emergency Management Agency (FEMA) was there. It may have been Ugo Morelli, who has had a lot of influence over many years, in a consistent, low-key way. Anyway the comment was made, "If you are finding prediction credible and recommending that it should be pushed as an area of research, this has such important public policy implications that we should start a parallel review of the public policy implications of earthquake prediction." There was immediate agreement on that.

Earthquake Prediction and Public Policy: The Turner Report

Scott: As I recall it, there was a good deal of concern that a prediction could cause panic, and thus be a disaster in its own right.

Wallace: Yes, that was the essence of concern about even pursuing prediction, so it was important to look at that problem in an objective and rational way. The National Research Council selected Ralph Turner, sociology professor at UCLA, and a committee got under way. I think FEMA was the principal sponsor. I was a liaison to the study and followed it with great interest.

As an aside, in my opinion, FEMA itself was a disaster area for so many years. And before FEMA, it had been like that with civil defense, emergency services, etc. Those efforts themselves tended to be a series of disasters. At one time I had the horror stories about them all in mind, but thank goodness I have forgotten the details. There was the big concern about nuclear attacks.

Scott: My own impression at the time was that much of the nuclear preparedness was quite unrealistic. I suspect the priority given to nuclear attack diverted attention from realistic preparation for the more conventional disasters that occur all the time.

Wallace: I agree. Anyway, President Carter tried to improve things by dividing up the activity differently. He also tried to put more preparedness into what they did, rather than just responding to disasters after the fact. Also in 1975 Ralph Turner's group issued Earthquake Prediction and Public Policy, even before Clarence Allen's panel report came out in July of 1976. Essentially the Turner report said that prediction is difficult, but if done right, it can have enormous benefits. I certainly agree that public policy and response strategies should be developed simultaneously with predictive capability. (National Research Council, Panel on the Public Policy Implications of Earthquake Prediction, Turner, R.H., (ch.), Earthquake Prediction and Public Policy, National Academy of Sciences, 1975.)

Newmark-Stever Report

Scott: You talked earlier about how tricky it was to address questions of fund allocation. Say something about how these money matters were resolved.

Wallace: In September 1976, the National Science Foundation and the U.S. Geological Survey produced a report on NSF and USGS program options that came to be called the Newmark-Stever report (often referred to as the Stever-Newmark report). Guy Stever, President Gerald Ford's science adviser (1974-1977), and head of the National Science Foundation, along with Nate Newmark, a renowned engineer at the University of Illinois, oversaw the preparation of this report. Newmark served as chairman of the Science Adviser's Advisory Group on Earthquake Prediction and Hazard Mitigation. Thirty or more people attended a planning session. Stever attended the meeting, which meant that the issue of earthquake hazard reduction automatically reached the Presidential level, and thus gave the report a lot of leverage. (National Science Foundation, Research Applications Directorate (RANN), and United States Geological Survey, Earthquake Prediction and Hazard Mitigation Options for USGS and NSF Programs, 1976.)

The panel decided that it was time to get busy with actual programs to do something, and so made recommendations on fund allocations. The report recommended money for the earthquake process, global seismology, engineering, and on and on. Meanwhile a good deal of consensus had been developed. USGS had taken over the old Coast and Geodetic Survey (NOAA) earthquake program. The Earthquake Engineering Research Institute (EERI) was going well, and was influential. By now, everybody felt fairly comfortable about dividing into a national program. But the consensus-building had taken time--it was now twelve years after the 1964 Alaska earthquake had created the concern, and eleven years after the Press report had made the first recommendations for a national program.

Scott: The Newmark-Stever report was the last big planning effort before the 1977 Act was passed. Why don't you discuss NEHRP next?

Wallace: Yes. It has been fascinating to me to reflect on all the pushing and pulling, and thousands and thousands of hours of effort, that it took to develop some sort of a working consensus. It was a slow, slow, difficult process.

The 1977 National Earthquake Hazards Reduction Program (NEHRP)

Wallace: I referred to "developing consensus," which is really a polite way of saying that agreement was reached on a "power structure" and a "money distribution plan." Many thought the wrong elements were emphasized, but that was the way the politics of personalities and power of the time worked it out.

Scott: Do you mean that many who more or less went along with the consensus nevertheless really thought that the Newmark-Stever report, and the NEHRP program, emphasized the wrong elements?

Wallace: Yes. There was a battle between the engineers and earth scientists for dominance in the program, and within the earth sciences there was a battle between seismological strategies and geologic strategies. The sociologists and a few public policy people had worked within a National Academy of Sciences committee on socioeconomic effects of prediction, but they had no significant place within the program, at least until much later.

Moreover, from the moment that the Press report championed "prediction," that idea seemed to dominate. Even the sociologists hung their program proposals on prediction. The seismological community, strong proponents of prediction, dominated within the earth sciences in terms of budget distributions. An important factor, I believe, was that Congress, at least some members, were turned on by the idea of earthquake prediction--a hightech approach in a hightech era. In my opinion the whole earthquake program might not have gotten off the ground without the excitement generated by the idea of prediction.

At long last, in 1977 the national act was passed--the National Earthquake Hazards Reduction Act of 1977, Public Law 95-124. Congress essentially said, "We have passed this act defining goals for earthquake hazard reduction, now we must have an implementation plan." Responsibility for preparing the implementation plan was assigned to the Office of Science and Technology Policy, which was headed by Frank Press, who was also by then, science adviser to President Carter.

Scott: Regarding the consensus-building, I guess what we did was achieve enough of a consensus to get an act passed. But the battles continued after NEHRP was in place, and I believe the expectations of some of those who went along with the consensus were not realized. Also I believe the engineers saw themselves as having a rather limited in-put into the actual process of implementing and administering NEHRP, and allocating the money. They saw USGS and NSF as administering NEHRP, and allocating the money. They saw USGS and NSF as dominating the spending, and they--the engineers--felt left out. Those are my impressions, admittedly gained more from the engineering side.

Wallace: On that point, let me digress for a moment to emphasize and characterize again the Press and Housner reports. The Press report championed the topic of earthquake prediction but made no attempt to push earthquake engineering even though George Housner was on the panel. The Housner report emphasized engineering to the exclusion of earth science, except for soils engineering, but it seems to be presented as an appropriate total program. Efforts to agree on a national program, thus, started with a standoff of two major concepts divided along discipline lines. One might argue that this standoff stalled progress toward a National program.

In contrast the Pecora and Steinbrugge reports clearly were developed with the idea of bringing many disciplines together in a National program. Later the Newmark-Stever report also considered a broad range of disciplines to tackle earthquake problems. In my estimation, the Newmark-Stever report was very effective. But there never has been a way to divide up money so as to please everyone, nor did the contest end with the passage of the National Earthquake Hazard Redction.

For each of the reports it is illuminating to note the membership of panels and advisory committees. The advice received from any committee or advisory panel very naturally turns on the backgrounds, disciplines and institutional attachments of members, and particularly who prepares the final report.

Anyway, Karl Steinbrugge was selected to head the effort to put the implementation plan together. He assembled a working group and invited me to participate. He wanted me to move to Washington, D.C., for six months, but I ended up commuting--I would be back there for a couple of weeks, then return here for a couple of weeks. I worked very closely with Karl and Ugo Morelli. Others on the Working Group included Bill Anderson, Charles Culver, Henry Hyatt, James Lefter, and Don Nichols.

Scott: That must have taken a lot of effort.

Wallace: Yes, it did. I was really impressed by how Karl approached it. When we got back there to Washington, he said; "First of all, let's make a spread sheet." Along the top of the sheet we listed all the different disciplines and expertise needed. Along the side, we listed all the different regions of the U.S. We filled out the resulting matrix with names of people and organizations we needed to contact, people who should be involved.

Scott: You wanted to involve all the appropriate disciplines and also to ensure geographic coverage of all the main regions?

Wallace: We wanted disciplinary and organizational involvement, and geographic coverage--the things that work in grass-roots politics. It was really an exercise in the grass-roots politics of earthquake hazard reduction. In addition to the members of the working group members, there was a steering group, including Phil Smith, deputy to Frank Press, Bob Hamilton, who headed the USGS earthquake program, and Charles Thiel of FEMA. Rob Wesson of USGS was also author of important documents.

In addition there was an advisory group consisting of Clarence Allen, Henry Degenkolb, Charles Fritz, Paul Jennings, Karl Kisslinger, Henry Lagorio, George Mader, Nate Newmark, Bob Rigney, Nafi Toksoz, Ralph Turner and Bob Whitman. All of them had some important input into the effort. Phil Smith, for example, part of the President's science advisory staff, has a superb intellect, is skillful as a politician, and has an intense interest in the earthquake program. In the top levels of government the project was highly visible and enjoyed instant communication. It got the kind of attention that bigger programs often do not receive. No wonder it was successful!

Scott: There must have been regular feedback to the President's inner staff, and maybe to Carter himself?

Wallace: Yes, I believe so. First the report discussed more general matters, such as background and policies, public and private financial institutions, earthquake hazard reduction through construction programs, and things like that. Then the report presented a set of 37 issues. I will give some examples here: Issue no. 1, "Lack of contingency response planning," Issue no. 13, "Fire following earthquake," No. 14, "Lifelines," No. 17, "Risk map development," No. 20, "Critical facilities," No. 23, "Lack of earthquake hazard reduction criteria in federal grant programs," No. 36, "Emergency health services," No. 37, "International cooperation", and on and on. We tried to cover everything. The new program was supposed to deal with each of these issues, which were phrased in terms to facilitate this. (Office of Science and Technology Policy, Executive Office of the President, Working Group on Earthquake Hazards Reduction, Steinbrugge, K.V. (ch.), Earthquake Hazards Reduction: Issues for an Implementation Plan, 1978.)

Scott: There were all kinds of debates in the course of trying to formulate a national plan, and you were in a ringside seat at the crucial stage of agreeing on the implementation plan. Do you want to say anything more about the process and the often conflicting interests involved?

Wallace: Yes, throughout the preparation of all the documents, and especially in the NEHRP phase, ideas on how to prepare for disasters covered a range of approaches. For one thing, they seemed to divide between (1) pre-disaster efforts at prevention and mitigation, and (2) planning for faster and more effective post-disaster response. There were and still are debates about the roles of the federal vs. state governments, and the public sector vs. the private sector.

Other important matters include the roles individual citizens can play, how the public can be kept informed, and the ways education can help. Each major idea has its protagonists, who fight for recognition and especially for financial support.

Guidelines for Handling Prediction

Scott: I believe there were some parts that you especially worked on.

Wallace: I was involved in things said about prediction--for example, I told Karl that I thought we should have a section about ethics in earthquake prediction. After the 1971 San Fernando earthquake, policies regarding the issuance of statements that might be interpreted as earthquake predictions became more of an issue. Question were raised about how such institutions as Caltech, U.C. Berkeley, or USGS, should handle the problem.

Should individual scientists issue predictions, or should all predictions be issued through institutions? We knew that if a major prediction were even contemplated, word would always get out. We also knew that a prediction of a major earthquake, if picked up by the media, would have a big impact and cause major public reactions. So, what were the ethics of making a prediction? How could prediction be made beneficial? Institutions also began to wonder about legal liabilities. The Turner report of 1975 had considered many of these problems, but by 1978 other concerns had surfaced.

About that time, the USGS formed a National Earthquake Prediction Evaluation Council, and California developed its own evaluation council. There was much concern about how to deal with, neutralize or counteract soothsayer-type predictions that lacked scientific merit. A few scientists--or at least people possessing some scientific credentials--clearly had elements of fortune-telling in their predictions. People were making predictions based on all sorts of wild ideas. I urged that we have a section on the ethics of prediction, and Karl said, "I wouldn't touch that myself with a ten-foot pole, but if you want to write it up, go ahead." I did write a very brief recommendation to professional societies that they develop guidelines.

Not long afterward, the Seismological Society of America took up the issue, in response to this report. SSA publishes many scientific papers which could be seen as containing predictions. What was their responsibility to the public, and legally what was their liability to lawsuits? They wrote up a policy for SSA.

Scott: Those were early efforts to deal with a very difficult subject that is still with us. A lot of progress has been made, but problems still come up regularly over the issuance of predictions or forecasts.

Wallace: They certainly do--people are making predictions around the world. At one point the Soviets predicted big earthquakes for southern California. Keilis Borok, an impressive scientist and mathematician, member of the Russian Academy (formerly Soviet Academy) of Sciences, and with an institute of his own, made such a prediction, which led to a review by the National Earthquake Prediction Evaluation Council (NEPEC) and by the California council. They did not validate the predictions in question.

One might question these negative evaluations, however, because the predictions seemed to be fulfilled by the big Landers earthquake, which occurred east of Los Angeles in 1992, and the Northridge earthquake of 1994 in western Los Angeles basin! The Russian (Soviet) methodology is still being discussed here. Jack Healy of the USGS worked with them to find out exactly what they do mathematically, and to suggest how the U.S. might proceed with their prediction method. Jack's efforts got little support, however, and have currently ceased because of his retirement. Also our Russian colleagues have fallen on very hard times.

Scott: You found dealing with predictions, forecasts and other such projections a complicated, tricky business.

Wallace: Yes, and I hope this discussion brings out some of the complexities, but I can't cover them all. How does one cope with the uncertainties of thought and concepts that are bound to surround an emerging science? How should California engineers translate a Russian prediction into what ought to be done about constructing buildings in southern California? Say the prediction shows up in the newspapers, and is discussed by the seismologic community at various meetings. What does one do about it?

The federal and state councils do provide for evaluations, but with prediction in its infancy, a new type of prediction or forecast has to mature as an idea. A scientist gets an idea, which may prove correct and useful, but maybe not. First, a new idea needs to be tested and evaluated. If it begins to prove out, an institutional and application phase must follow. That is true of prediction, and really of all technological research. In any event, I did write up a section on it in the Issues volume. I will also discuss it more when I take up various USGS projects, especially the regional planning effort that went on in the San Francisco Bay Area.

NEHRP Implementation Report, 1978

Scott: What was the next step in developing the NEHRP program?

Wallace: In 1978 President Jimmy Carter transmitted to Congress a 30-page statement about the program, summarizing the things that needed to be done. (Executive Office of the President, The National Earthquake Hazards Reduction Program, June 22, 1978.) This was the document that finally set the National Earthquake Hazards Reduction Program into action. At that point, we were really under way as a program. That is fairly recent history, although by now it has had numerous reviews. How will it be modified? I am sure it will not look exactly the same, except in its overall concepts.

Scott: Yes, quite a few years have gone by since the 1977 Act was passed, and there have been several critical reviews that call for changes in the program, and especially in its management.

Wallace: True. On the other hand, the next big changes will probably come as a result of the next big earthquake, whenever that is. The 7.3 Landers earthquake occurred in 1992--the world's biggest in that calendar year. It hit in a relatively remote, unpopulated area. An earthquake that size would have had a huge impact if it had occurred in the Los Angeles metropolitan area. Or if we had an extension of the 1989 Loma Prieta earthquake nearer San Francisco, the damage would be double or triple what it was in 1989. Events like that would set major changes in motion.

The 1994 Northridge earthquake occurred after I wrote the previous paragraph. As perhaps the most costly earthquake disaster in US history, it will certainly have an impact on NEHRP programs. This is a perfect example of the influence that special earthquakes exert on policy. Early data suggest that strong-motion seismology and hazard delineation of finely defined areas should carry higher priorities. Furthermore, the engineering community seems already to view strong ground motion somewhat differently, inasmuch as unusually high values of acceleration were recorded and records were promptly and widely distributed. The performance of welded steel is also being reviewed.

Similarly, the earthquake of 1995 in Kobe, Japan, I am sure, just two weeks after it happened, will stimulate additional attention among engineers on the role of unconsolidated sediments in influencing both ground shaking as well as liquefaction and ground failure.

Scott: Yes, that has been the history of earthquakes--the big policy changes result from major damaging urban earthquakes.

Wallace: I made up a graph once to show this to audiences. Each earthquake is followed by a spike of interest and activity, then comes sort of a typical decay curve; then another spike at the time of another earthquake, and again it tapers off. So the intense concern immediately after a damaging earthquake falls off sharply a little later, nevertheless the baseline of interest and awareness has gradually risen. That has been true here and elsewhere in the world.

When I started my Ph.D. work on the San Andreas fault in 1940, the fault was relatively unknown. At least, its name certainly was not a household word. Now almost anywhere in the world that I travel, I find that people have heard of the San Andreas fault. Both here and elsewhere, earthquakes are much better understood now than they were a half-century ago. While it seems like a slow process, cumulatively major progress has been made. Each decade has seen significant advances in understanding.

The Importance of NEHRP Funding

Scott: The passage and funding of the 1977 NEHRP Act (also called the Cranston Act) resulted in a big boost in financing for earthquake programs generally, but especially for USGS. Would you say a few words about that?

Wallace: Certainly the Newmark-Stever report had the greatest effect on funding for the whole NEHRP activity. Indeed, it was the final step needed to make NEHRP feasible. Newmark-Stever suggested A, B, and C levels of funding. The A level--the lowest level--was essentially the one chosen, and it worked out to about $50 million total for the whole program. The figures that had been suggested for the higher levels of funding were of the order of $100 million. The funds budgeted in a direct appropriation continued to be in the ballpark of $50 million for quite a few years.

In addition to the $50 million in NEHRP funds in the form of a direct appropriation, and there were special funds from other sources. So when the NEHRP program started and for quite a while afterward funding actually went on at the $60 or $70 million level. That was for everything, including earthquake prediction, earthquake engineering, and social/economic issues.

Something like $30 million was allotted for the USGS program, with about half of that for prediction, and about one-fourth for non-USGS research (e.g., university and private industry research). There was about $20 million for earthquake engineering, although it was more in some years. So if I talk about a $70 million funding level in those years, that figure includes monies transferred from other agencies for special studies. In summary, however, earthquakes became a much bigger program after passage of the 1977 act. For a while I would say the program was fairly well-funded, although since NEHRP's early days inflation has eaten into the program.

Scott: The NEHRP program was really a major new development for the earthquake field and for USGS.

Wallace: Yes. Through the years the USGS program had been driven by the original authorization creating the USGS in 1879, and subsequent legislation and budget authorizations. A lot of the early earthquake work by the USGS was accomplished under general budgets know as "Surveys, Investigations and Research, (SIR)." But the new NEHRP earthquake program represented a separate and major funding initiative, which at present is coordinated by the Federal Emergency Management Agency.

Within the Geologic Division the NEHRP budgeting has given very strong emphasis to the earthquake program. The U.S. Geological Survey is divided into three divisions, (1) National Mapping, (2) Water Resources, and (3) Geologic. Earthquake work comprises about one-quarter of the geologic division's entire program, which represents a remarkable change. I noted earlier that back in 1962 the Survey's directorate did not consider an earthquake program was salable at the $1 or $2 million level. Yet in 1977 it was salable at a $30 to $35 million level.

Scott: To what do you attribute the big change? I'd guess it was partly all the work that led up to NEHRP, and also it was partly due to a series of earthquakes.

Wallace: Yes, a series of well-chanced earthquakes, plus a lot of tender loving care. In the earthquake business, you need to keep up-to-date plans on the shelf or in your back pocket, ready for a time when things happen without warning. Then, when the opportunity strikes and interest is high, "We just happen to have a plan that meets the need."

VII DEVELOPMENT OF THE USGS EARTHQUAKE PROGRAM

Scott: You have talked about NEHRP and its big effect on USGS and its earthquake program. Would you now discuss the development of that program?

Wallace: Yes. This is a good place to tell about the evolution of the earthquake program within the USGS. That would include, of course, both the things supported by NEHRP, and those initiated and financed otherwise. It is a fairly complicated business. I set the stage by starting with the 1964 Alaskan earthquake, but many things preceded that event, and there was more than one line of action going on. It was more like the channels of a braided stream, first one channel then another carrying more water, always shifting, sometimes a dam or a sudden flood altering the whole pattern.

USGS Organization and Administrative Terminology

Scott: Before you start discussing the earthquake program, let me ask about the organizational structure of USGS. You just now mentioned the three divisions into which USGS is divided--National Mapping, Water Resources, and Geologic. Also I have heard the terms "office" and "branch" referred to, but have never understood how they all related organizationally.

Wallace: I do not want to get deeply into organizational structure. For purposes of this oral history it is probably sufficient to refer to the three divisions, and to note that among them is the Geologic Division headed by a Chief Geologist. As to why the USGS switched from having Assistant Chief Geologists with Branches under them to "Offices"--I'd guess it was a change that made management feel good. Branches also changed in size and character. A history of all these changes would no doubt be of interest to some readers, but it would involve a lot of administrative detail that I don't see as belonging in this history.

In any event, I think the personality characteristics of participants far outweighed in importance the administrative structure within which they worked. For example, George Plafker, to whom I give the highest marks as a leader in earthquake matters, has always been administratively in the Alaskan Branch of the Geologic Division, but he has worked all over the world on earthquakes, and the Chief of the Alaskan Branch exerts almost no control over George. The "upwelling of nonadministrative leadership," to use Preston Cloud's phrase, characterized the USGS for 100 years and made its administration an "improbable bureaucracy," as he called it, because it was so different from the typical perception of a bureaucracy. (Cloud, Preston, 1980, "The Improbable Bureaucracy: The United States Geological Survey, 1879-1979," Proceedings of the American Philosophical Society, v.124, n.3, June 1980, pp.155-167.

Scott: Those are some really interesting observations about administrative behavior that I hope you will talk about more when you take a retrospective look at your entire USGS experience. But for now, USGS titles and organizational nomenclature need to be outlined, to help readers who may be unfamiliar with the terminology. Could you give a very brief description of the structure of the USGS so that readers will understand what Branch Chief, Division Chief, and so on stand for?

Wallace: The administrative levels of the USGS include the Director of the USGS at the top, who oversees and speaks for 9,000-10,000 staff members. Under that level are three Divisions, including the Geologic Division, Water Resources Division, and National Mapping Division (formerly named the Topographic Division). Each has a Chief, e.g. a Chief Geologist. For many decades the next level consisted of Branches headed by Branch Chiefs. Before 1960 there were 10 branches with membership ranging from 50 to over 500. In 1960 branches were increased to about 30, so that membership of each could be dropped to between 50 and 100, a number with which a Branch Chief could have personal contact.

Almost all my comments in this oral history pertain to the Geologic Division. For many years the parts of the Geologic Division program were administered by an Assistant Chief Geologist, who divided up money for the various branches. In 1973 when the earthquake program began to grow, an intermediate "Office" level was inserted between the Chief Geologist and Branches. Parallel to this pyramid structure, representatives in the three regions were titled Regional Geologists, or Assistant Chief Geologist for such and such a region, but Regional Geologists had no control over money.

In 1995 the names were being changed to such things as "Team Leader" for what may be somewhat parallel to a former "Branch." The stated goal is to produce a "seamless" operation. Many names have changed at the branch level as administrators try to adjust to changing times. Over the years many experiments have been tried, including establishing "Program Coordinators." who then share authority for the distribution of money with Branch Chiefs, in a "matrix management" scheme. In my opinion, there never has been a perfect solution to managing a complex program in which research specialists are expected to contribute to a variety of programs, while maintaining a steady course in his/her own area of research. I have lived and operated under several administrative setups.

Scott: This brief description helps, although I see how it easily gets complicated.

<u>The VELA Uniform Program of ARPA</u>

Wallace: From a seismological point of view, by far the most important effort had been the study and research aimed at the detection of underground nuclear explosions. This effort was to find ways to monitor the Soviet underground nuclear tests from afar, and to make nuclear test-ban treaties meaningful - even possible. Seismological techniques seemed to have great promise, but the seismographs then in place were too few, too primitive, and poorly placed for the task. There were not very many competent seismologists available then, and some called seismology "a cottage industry."

Recognizing this state of affairs, the Department of Defense, through its Advanced Research Projects Agency (ARPA) and a program known as VELA Uniform, turned on the money faucet for seismology in the mid to late 1950s. Within a very few years more seismologists had been trained, new instruments were designed and deployed, and theory had been developed to do the assigned task. To make a long story short, the program was a huge success, and underground explosions could be detected routinely.

Scott: How did the USGS respond to this clear earth-science need?

Wallace: Under the leadership of Lou Pakiser, s USGS group at the Denver center participated actively in the VELA program. Their specific project was to develop information about the structure of the earth's crust. All of the seismic signals that came from Soviet underground explosions had to travel to distant points where they could be detected, and the interpretation of the signals depended heavily on knowing the structure of the earth's crust.

With success in hand, VELA projects began to be phased out, and by the early 1960s, the USGS group decided to get involved in natural seismic events--earthquakes. Along came the 1964 Alaskan earthquake, and during 1965 and 1966 the USGS Crustal Studies group moved to the Menlo Park center in order to study natural earthquakes.

The National Center for Earthquake Research

Scott: Would you say a word or two more about that move, which seems to have been a very important one?

Wallace: The Crustal Studies group--headed by Lou Pakiser--essentially took the initiative itself to move to Menlo. The move was not initiated by the Chief Geologist, but of course had his approval. It might be cited as an example of the "upwelling of non-administrative initiative," because the idea did not come from on high. While it did create problems, it in fact drove the agency to seek a better administrative structure. Remember, this was well before the concepts of what a national program should look like came into being. The consolidation of program elements then spread across seven or eight branches. While such seeming chaos may be anathema to some with the "pyramid-military-administrative" mentality, I see it as one of the strengths of the USGS in the way it facilitates the applications of talents of creative individuals to important missions.

A Gathering of Geologists and Geophysicists

Scott: I presume the move to Menlo Park tended to concentrate earthquake efforts there?

Wallace: That move certainly added a lot. The Crustal Studies group from Denver, together with the Menlo Park engineering and structural geologists, most of whom were at that moment working on the Alaskan earthquake, made up an impressive capability within the USGS. We could see that the USGS could expand an already impressive record and mount a major multi-disciplinary, long-term study of earthquakes.

Wallace: Sometime after the Crustal Studies group came to Menlo Park, we started to use the name "National Center for Earthquake Research." That was a move, in part, to recognize the unity of the overall USGS earthquake effort, and to identify the many, many products we were beginning to turn out here in Menlo Park. The name served well for a number of years and was printed on all reports and maps concerning earthquakes.

For several years everyone had seen that some sort of high-level Division organization was needed to manage the earthquake activities of many Branches. In 1973 an "Office of Earthquakes Studies" was established, Bob Hamilton was selected to head the new office, and the name National Center for Earthquake Research was abandoned.

Some Major Players

Wallace: I will name just a few of the players. Among the geologists were Art Grantz, Parke Snavely, George Gates, George Plafker, M. G.(Doc) Bonilla, Wally Hanson, Ed Eckel and Dave McCulloch, and among the geophysicists from Denver were Jerry Eaton, Dave Hill, Jack Healy, Barry Raleigh, and the Branch Chief, Lou Pakiser. What a powerhouse that collection represented!

I might nominate here a Hero Number 1 of all the USGS investigators who worked on the Alaskan earthquake. That is George Plafker, who first figured out what really caused the Alaskan earthquake. Using an amazing array of geologic information from decades of Alaskan Branch studies, George demonstrated that a gigantic thrust fault produced the earthquakes observed. He followed with some beautiful analyses of marine terraces to establish how often such great earthquakes had happened in prehistoric times.

Scott: The Plafker story certainly bears out one of your themes--the importance of individuals. Who were some of the major players in Washington?

Wallace: By then many people at USGS headquarters in Washington recognized the potential earthquake responsibilities of the USGS. Wayne Hall and Dallas Peck were two of these who championed the earthquake program. Wayne Hall and Lou Pakiser had worked on the Pecora committee report of 1968, and Lou Pakiser had been a member of the Press committee of 1965. Operating out of the Office of the Director, Jim Balsley and Jim Devine played important roles later.

Soon after the move from Denver, Lou Pakiser, as Chief of an operating branch, the Branch of Crustal Studies, took strong initiatives to create a program. He organized an advisory committee of non-USGS people for his

Branch, and began to develop concrete ideas on how to move ahead. The program ideas that came out of these crustal-studies deliberations focussed on seismology and geophysics. The capabilities and interests of the dozens of geologists who had been so productive in studying the Alaskan earthquake were almost totally ignored. Indeed, I felt that Lou had identified geologists as "the enemy," and for several years this standoff prevailed. Of course, the geophysical-seismological camp and geologic camp were competing for funds and control of future plans. I can understand that Lou did not want any of his VELA money to be siphoned off for geologic work. There were other disciplinary conflicts--some leading members of the engineering community considered the USGS earthquake program to be anti-engineering.

Scott: To what extent were these conflicts resolved, and how did you try?

Wallace: As I suggest, conflict within USGS was only one facet of a bigger contest among all those interested in developing a National earthquake program. I have told about the string of reports and struggle that went on for 13 years after the 1964 Alaskan earthquake until the 1977 passage of the National Earthquake Hazards Reduction Act (NEHRP).

To answer your question more directly, an important one-word answer might be "patience." Perhaps it is healthy to have some continuing tensions. As time went by, different groups were merged or mingled, breakthroughs were accomplished all around, new people came aboard, others left the program, moved away, retired, and gradually a sense of community grew within the USGS program. But consensus remains fragile, particularly in light of the plethora of projects and programs whose proponents are asking for support.

Scott: You have mentioned a few of the USGS people who were influential. Were there others who played a part?

Wallace: In 1969 George Gates, as Regional Geologist in Menlo Park, was designated by Washington headquarters as coordinator of the earthquake program in Menlo Park. Art Grantz, as Chief of the Branch of Pacific Coast Geology was intimate with the geologic thrust of the Alaskan investigation. Art had strong convictions about what the emphasis should be in a USGS earthquake program. At the time, Art was a major force in developing the mapping of the active strands of the San Andreas fault, an effort directed toward defining earthquake hazards and learning the geologic basis for minimizing hazards. That program led directly to making California's Alquist-Priolo Act possible.

George Gates was closely associated with the engineering community, and during those years he served on the Engineering Criteria Review Board (ECRB) of the Bay Conservation and Development Commission (BCDC), which

I will discuss a little later. Perhaps the main concern of ECRB was seismic safety. Cooperation with the state of California, its Division of Mines and Geology, and the emerging Seismic Safety Commission was always high on Gates' agenda.

At some point Jerry Eaton became Chief of the Branch of Seismology and he was very influential in shaping the program. He championed the development and led the way for a microearthquake study of the San Andreas fault. To my mind that net and project has produced some of the most important advances in understanding the geometry of the fault and the time-sequence of strain release through earthquakes. The so-called CAL-NET comprised seismometers and other instruments used to record, transmit and analyze data. Knowing really very little about seismology myself, I turned again and again to Jerry for guidance in the subject.

Scott: Did all these people work well together?

Wallace: The strong personalities of Pakiser and Grantz, and their very different ideas of what should be emphasized in a USGS earthquake program inevitably led to harsh words and strong feelings.

Scott: That must have made for difficult times.

Wallace: It surely was a difficult time. The rationale that "only seismology equals earthquakes," seemed to rule the day in the USGS. Geology took a seat far to the rear despite the fact that it was geological data that dominated the contributions in the 28-volume USGS series on the Alaskan earthquake. And it was through the interpretation of geologic mapping that the enormous slip on the San Andreas fault had been defined. Art Grantz gave up the fight, and I wondered whether or not I could stand these internecine battles. Fortunately, those very strong animosities have long since disappeared and mutual respect seems to rule the day.

Earthquake Control

Scott: What other things were happening about then?

Wallace: Early-on, the Nixon administration started a program--called something like "New Scientific and Technologic Initiatives for Economic Development". Out of a hundred or more ideas, seven or eight items survived a winnowing process, and earthquake prediction came out a winner. Jack Healy, Barry Raleigh and some others went back to Washington, D.C. for a congressional hearing. After their experiment at Rangely, Colorado, where they literally turned earthquakes on and off in the oil field there, by controlling fluid pressures, they started talking about the possibility of turning earthquakes on and off intentionally. This translated into the concept of earthquake control, which was a dramatic possibility to talk about. As I understood it, that idea caught the curiosity of a congressional staffer. I am convinced, earthquake prediction and related items was included in the economic development program because of that congressional aid's excitement. As a result the earthquake program got about $7 million, whereas we (USGS) had been getting about $1 million a year. It was a big jump, but in some ways it was kind of a fluke.

Wallace: In many ways earthquake control made real sense. Say there is some region in the country--maybe Nevada, where the recurrence of earthquakes may be in thousands of years--and where you know there are earthquakes waiting to happen. You might trigger a bunch of earthquakes which, because of the state of strain, are ready to go off in the next 100 years--or even 1,000 years. Once the earthquakes are set off, you get rid of the accumulated strain and release much of it. Then, whatever installation you want to put there, be it a nuclear reactor or waste depository, will have a much lower possibility of being hit by a damaging earthquake during its lifetime. That is the kind of rationale you might go through in making use of earthquake control.

But, of course, in no way were we then or are we now prepared to trigger earthquakes on the San Andreas fault, or to try to let the strain seep off gradually. We just don't know enough about how the whole system works, and the possible consequences of earthquake surprises would be completely unacceptable. No way--I cannot see that coming, certainly not in my lifetime, or for a century or more.

Paleoseismology: A Significant Part of the Program

Scott: You talked about the origins of paleoseismology before, and I understand that it now has an important role in the USGS earthquake efforts. What is the story on how it gained acceptance?

Wallace: Yes, I noted earlier the Nevada paleoseismology studies that had really begun serendipitously with the work on a geologic map of Nevada. Then in the 1960s I began looking for a way to justify geology's role in the earthquake program, based on evidence of prehistoric earthquakes preserved on fault scarps in Nevada. Also between 1965 and 1968 field examinations along the San Andreas revealed the first clear evidence of repeated displacements, each presumably occurring at the time of a great earthquake on the fault.

Scott: When would you say paleoseismology actually began? Were there still earlier forerunners predating your own involvement?

Wallace: I credit G.K. Gilbert with first using geologic evidence for making a statement about prehistoric earthquakes, which he did a full century ago. In his 1884 paper Gilbert notes a small cliff running along most of the base of the Wasatch Mountains of Utah, and says, "This little cliff is, in geologic parlance, a 'fault scarp,' and the earth fracture which has permitted the mountain to be uplifted is a 'fault.'" The small fault scarp of which Gilbert speaks represents one "fossil earthquake," one spasm of uplift of the mountain, and that spasm occurred a relatively few thousand years ago. (Gilbert, G.K., "A Theory of the Earthquakes of the Great Basin, With Practical Application," American Journal of Science, v. 27, n. 157, 1884, pp.49-53.)

Gilbert even used the evidence of scarps to make a prediction: "From Warm Springs to Emigration Canyon fault scarps have not been found, and the rational explanation of their absence is that a very long time has elapsed since their last renewal. In this period the earth strain has been slowly increasing, and some day it will overcome the friction, lift the mountains a few feet. and re-enact on a more fearful scale the catastrophe of Owens Valley."

The term "paleoseismology" was not used in Gilbert's time, of course, nor was it yet in use even decades later when Charles Richter noted in New Zealand that "Everywhere in the principal active areas of both islands, are scarplets of the right height and extent to have originated in single seismic events, without indication of accumulation or repetition, as if the locus of fracture were constantly shifting." Surely this was an important paleoseismological observation. (Richter, C.G., Elementary Seismology, 1958.)

Scott: Are there other early examples of using the idea of paleoseismology, if not the term itself?

Wallace: After the Alaskan earthquake of 1964, George Plafker not only found marine platforms that had been raised above sea level during the earthquake, but also many prehistoric marine terraces which showed, without a doubt, that similar uplift events had taken place in the past. He and Meyer Rubin (Plafker and Rubin, 1978) dated some of these events and demonstrated that they had occurred at intervals of from 500 to 1400 years--a major paleoseismic finding. (Plafker, George, and Rubin, Meyer, "Uplift History of Earthquake History as Deduced from Marine Terraces on Middleton Island, Alaska," In Isacks, B. L., and Plafker, George, (co-organizers), Proceedings of Conference VI, Methodology For Identifying Seismic and Soon-to-break Gaps, U.S. Geological Survey Open-file Report 78-943, 1978, pp. 687-722.)

Scott: Hasn't trenching become one of the principal means of doing paleoseismology?

Wallace: Indeed it has. The idea is that in just the right circumstances slip on faults cuts and offsets sedimentary layers of silt, sand or gravel which can be dated by carbon fourteen or other means and thus the time of offset can be determined. Of course, the offset is assumed to occur at the time of a prehistoric earthquake. Trenching is a powerful method of investigation, and today is the most commonly used paleoseismological technique. Around the world I'll guess that thousands of trenches have been excavated to find paleoseismologic data. Such data has helped determine slip rates on faults, and the slip rates are then translated into estimates of the earthquake frequency--truly a predictive exercise.

Scott: When were the first trenches dug to look for paleoseismologic data?

Wallace: In 1968, Jay Smith of Converse, Davis and Associates may have been the first to gather paleoseismologic data by trenching. Also in 1968, after the Borrego Mountain, California, earthquake, Malcolm Clark and Art Grantz certainly were among the first to get important paleoseismologic results from trenching. (Clark, M.M., Grantz, Arthur, and Rubin, Meyer, "Holocene Activity of the Coyote Creek Fault as Recorded in Sediments of Lake Cahuilla," in The Borrego Mountain earthquake of April 3, 1968, U.S. Geological Survey Professional Paper 787, 1972, pp. 112-130.) M.G. Bonilla (1973) and H.E. Malde (1971) were also engaging in similar studies at about the same time. (Malde, H.E., Geologic Investigations of Faulting Near the National Reactor Testing Station, U.S. Geological Survey Open-file Report, 1971; (Bonilla, M. G., "Trench Exposure Across Surface Fault Rupture Associated With San Fernando Earthquake," in National Oceanic and Atmospheric Administration, San Fernando, California Earthquake, of February 9, 1971, Geological and Geophysical Studies, v. 3, 1973, pp. 173-182.)

Scott: Some paleoseismologic studies had a profound effect on planning efforts in the Los Angeles area if I remember correctly. Would you comment on those efforts?

Wallace: Yes. In the late 1970s, when Kerry Sieh was working toward a Ph.D. under Dick Jahns at Stanford, he latched onto the thesis topic of analyzing young faulting along the San Andreas fault in central and southern California. I was pleased to participate as a reviewer at his defense of his thesis. He discovered a very significant site at Pallett Creek on the north side of the San Gabriel Mountains where he used the trenching technique very effectively. By meticulous and detailed analysis, he identified a series of prehistoric offsets along the San Andreas fault. He determined that the offsets had been formed at intervals averaging between 140 and 150 years, presumably when earthquakes had occurred.

The fact that the most recent great earthquake in that area was in 1857, led to the ominous conclusion that a quiet cycle of about 150 years was probably almost over, and that another great earthquake was imminent. This caused much concern among public officials and the public in the Los Angeles region. Kerry's very careful work, and his ability to communicate the significance of a complex scientific story to the lay public, had a profound effect and helped increase manyfold the region's and the nation's awareness of the value of earthquake-hazard mitigation. Since then, Sieh has continued to be a leader.

Scott: Once it had been established pretty securely, I guess the idea of using paleoseismology spread pretty rapidly.

Wallace: Oh my, yes. By the late 1970s paleoseismology trenches were being excavated routinely in Japan, and in the early 1980s the practice also became widespread in China. I reviewed some of this history in a paper presented at the first international conference on paleoseismology organized in 1987 by Anthony Crone and Eleanor Omdahl. (Wallace, R.E., "A Perspective of Paleoseismology", Directions in Paleoseismology, Proceedings of Conference XXXIX, U.S. Geological Survey Open-file Report 87-673, 1987, pp.7-16.)

So by 1987 paleoseismology was recognized as meriting that kind of attention. Following that, in 1994 a second major international conference on paleoseismology was organized by Dave Schwartz and Carol Prentice of the USGS and Bob Yeats of Oregon State University. In 1995 at least two other conferences or special sessions are planned in Germany and Italy. Paleoseismology clearly has come of age.

The Bay Area Project: An Example of Successful Outreach

Scott: You planned to talk more about promoting the introduction of new ideas into everyday practice? I have always thought of the remarkable USGS Bay Area planning project as an extremely effective program of outreach and bridging between researchers, practitioners and public policy people.

A Little Background

Wallace: Government uses buzz words like "technology transfer" in answering the question: How do we translate what the scientist develops into immediate application? I take something of a market approach. To use an analogy, if Ford produces an Edsel, it won't sell, if it produces a Mustang, it will sell. If a scientist produces a useful theory or concept, it will be used. If they don't, it won't be used. We all try to have our ideas and products be useful and used, and try to put these ideas in useful forms. Our egos feed on such successes.

Scott: On the other hand, much research in its original form needs to be translated or interpreted for use by practitioners, and researchers may need help with that. Also, the successes can also help feed budgets, especially when they involve things seen as useful to society. So everybody should benefit from effective outreach.

Wallace: Yes, "outreach" is another currently popular buzz word, and the Bay Area project is an excellent example. It was in 1970 that we started the San Francisco Bay Region Environmental and Resources Planning Study, a cooperative effort with the Department of Housing and Urban Development. The purpose was to develop some of the fundamental earth-science principles and data sets that would make regional planning possible and effective, then to develop bridges to the planning community. That in turn would promote wider use of earth-science knowledge and ideas in regional planning. The Bay Area project also had a strong earthquake-hazard-mitigation component.

Scott: Give a little background and history of the Bay Area project.

Wallace: In the late 1960s two federal agencies in Washington, USGS and HUD--the Department of Housing and Urban Development--were considering a joint effort to aid urban planning by helping with its earth-science aspects. Meanwhile here in Menlo Park, ideas for such a study had grown out of experience with a Geologic Hazards Committee in Portola Valley, California.

Wallace: Portola Valley is a small residential town of about 3,000-4,000 population with a strong commitment to maintaining its rural atmosphere and setting. It is located right on the fault--in fact, the "Valley" part of the town's name comes from a valley that follows the San Andreas fault. Several of us geologists from the USGS, as well as geologists and geophysicists from Stanford University, and private consultants, reside in Portola Valley. Naturally, we earth scientists were very much concerned about the earthquake hazards that faced the town. Dwight Crowder had organized us and had been the inspiration for the town's planning committee.

Scott: Yes, others have mentioned Dwight Crowder and the Portola Valley effort that he got started. So the USGS-HUD interest in Washington, and these local activities in Portola Valley, came together in a most constructive way?

Wallace: Yes, they did.

Program Initiated in 1969

Wallace: Art Grantz and George Gates, both of whom held administrative posts at the time, took the local ideas back to Washington. To abbreviate the story, the cooperative USGS-HUD program came into being by the end of 1969. George Gates became the first Director. Jim Balsley, as USGS Assistant Director, carried the ball in Washington, and Director Bill Pecora championed and aided the project, from the early discussions on.

When George Gates retired in early 1970, Bill Pecora asked me to take on the direction of the study. Almost simultaneously, I was also assigned the post of Assistant Chief Geologist (soon to be renamed Regional Geologist), Western Region, for the Geologic Division.

As one who hated administration, that was a sorry time for me. For the San Francisco Bay Area study, we put together a plan that involved all three operating Divisions of the USGS: Geologic, Topographic, and Water Resources. Something like $4 million to $5 million was spent on the study. As a result of the study, close ties were developed between the scientific-technical people, and the planners and other "decision-makers" around the nation.

At the outset, my counterpart in HUD thought the project would be immediately able to draw on the 300,000 volumes in the USGS, Menlo Park library for help in urban planning. True, there was a lot of data in the library, but not accessible to local planners and policymakers in a usable form. We also lacked some of the fundamental information they needed. So we ended up having to do a lot of new fundamental studies, basic research and interpretation, and then developing practical steps for application.

New report formats had to be invented. We created three series of publications:

1. A basic data series.

2. An interpretive series.

3. A series directed specifically toward application, intended for the planners and others who would really use the material. Preparing that was a complicated process that had many steps.

After I directed the Bay Area Study for a couple of years, Bob Brown took over from me and did a magnificent job until its completion around 1976. The project demonstrated how important earth-science knowledge was in the planning of urban and regional areas. City and county governments developed and adopted guidelines based on the work of the study, which produced more than 100 publications, providing a wealth of information for regional planning and urban policymaking.

For example, San Mateo County adopted ordinances governing density of housing on steep slopes and landslide-prone areas. Most communities in the San Francisco Bay Area that were along the San Andreas fault hired consulting geologists, prepared fault-hazard maps, and created ordinances to minimize earthquake hazards. Prudent use of the land became the word of the day.

Scott: Many people may need to be reminded of that Bay Area Study effort and its significance. It had a much broader influence than just on earthquake preparedness and was widely used both in the Bay Area and nationally.

Wallace: Yes, that study won several awards in the planning arena. Also there were important spinoffs from the effort.

Bill Kockelman's Role: "Bridging"

The late Bill Kockelman played an essential role that I want to mention. Bill had previously been New Mexico State Planner and came to USGS to work as a planner on the Bay Area Study. Later he carried his talents over into the earthquake program. He developed a rapport with a community of information users that was wholly different from the usual contacts of our scientific and technical people.

After he had applied his "bridging" efforts to the earthquake hazard reduction for a decade, the planning community came to think of Bill as "Mr. U.S. Geological Survey." He had an intense interest in this important work and a remarkable ability for effective bridging that few can equal. For his expertise as a planner, he was invited to serve on the California Seismic Safety Commission. Later he received the Department of the Interior's Meritorious Service Award for his work.

Scott: I knew Bill Kockelman well, enjoyed working with him, and served on the

Seismic Safety Commission with him. As part of the USGS outreach effort, he and Bob Brown wrote a very useful guidebook--a USGS professional paper--to help Bay Area policymakers and planners use geologic knowledge more effectively. (Brown, Robert D., Jr., and William J. Kockelman, Geologic Principles for Prudent Land Use: A Decisionmaker's Guide for the San Francisco Bay Region, USGS Professional Paper 946, 1983.)

Then I got them to do a streamlined version for the Public Affairs Report (bulletin of the UC Berkeley Institute of Governmental Studies). Their article was published in 1985 and widely distributed in the Bay Area and beyond. (Brown, Robert D., Jr., and William J. Kockelman, "Geology for Decisionmakers: Protecting Life, Property and Resources," Public Affairs Report, v. 26, no. 1, February 1985.)

Wallace: During the Bay Area study we often talked about "bridging" as a separate discipline. In fact, as I mentioned before, we treated it in the 1978 Steinbrugge report Issues for an Implementation Plan. You find some scientists who by personality and certain natural talents are effective in reaching across to planners and policy-makers. I think of George Mader as a planner who was also able to reach across and link with seismologists, geologists and engineers. Another person who could reach across was Bob Brown here in USGS, who followed me as director of the Bay Area study.

Summing Up

Wallace: To sum up, the Bay Area project illustrates the marvelous interactions among different disciplines that developed within the USGS. This gave it a power that many mainly scientific research organizations lack. You also mentioned the broader significance of the approach of the Bay Area Study. I should make it clear that the Bay Area Study was conceived as one of several demonstration studies (and not primarily earthquake), another was conducted in the East (Pennsylvania I believe) and a third in Arizona using very different designs and different techniques. Seattle is now getting still another approach, and other experiments are under way, but I don't know the details. The volcano program has similar hazard reduction experiments--witness its work on the Philippine volcano, Mt. Pinatubo, which erupted in 1991.

The whole idea was for the federal government to test approaches and show the way, after which state and local governments would pick up the long-term implementation. While it seldom happens that way quite as fully as one might wish, unquestionably the Bay Area is far ahead of the rest of the world, the USGS-HUD program must have had a lasting influence.

Scott: The Bay Area Study made excellent use of the Portola Valley experience as something of a model that other communities might learn from in adapting its policies to geologic hazards. I am sure it has had a significant continuing influence in the Bay and more widely in California. I greatly fear that now the budget crunch and force reductions will make it difficult for USGS to continue doing innovative things in the future. My view of how our federal system can work best is not prevailing these days.

Wallace: Yes, and we also get into the philosophic problem of downsizing all government, so popular today.

A Personal Note on Portola Valley

Wallace: As a long-time Portola Valley resident, I include this personal note on my own experience, which also illustrates the difference between voluntary risk acceptance and involuntary risk acceptance. The Portola Valley elementary school, one of three in town at the time, was situated directly over the San Andreas fault. I began to make strong statements at school board meetings, advocating that the school be abandoned. With this as my main policy positon, I even tried--unsuccessfully--for a vacancy on the board. When I was interviewed by the board of education in applying to fill the vacancy, I also spoke out for higher teachers' salaries. But they did not like me very much.

Regarding the school on the fault, as a father, I took the position that I personally was willing to accept the risk for my son to go to this school, because, during the few years he would be attending the school, the probability of a killer earthquake was very low, particularly for the relatively few hours he would be in the most vulnerable room. I thought his chances of being injured in a school-bus crash on the town's winding roads far exceeded the risk of dying at the school in an earthquake.

On the other hand, as a potential school-board official, I would need to consider the hundreds of students who would be exposed to the risk while attending school there, over many years. I found the idea totally unacceptable that, during the decades the school would be used, some students would probably be killed because we required them to attend a school situated on the San Andreas fault.

After a decade or more of my pushing this idea, this school was abandoned. But I must add that its use was changed at a time when the town's school-age population was declining. With other schools taking up the slack, Portola Valley school was turned into the Town Center, a use that drastically lowered its occupancy level. Many other elements of the Portola Valley experience could also illuminate practical earthquake hazard implementation methods, which is why it became an important pilot project in the Bay Area Study.

The Influence of Individual Earthquakes: San Fernando, 1971

Scott: Earlier you commented on the influence that individual earthquakes have on earthquake studies, public policy, and the directions taken by earthquake preparedness efforts. Would you say more on that, with particular reference to the San Fernando earthquake, which was very influential in California?

Wallace: The San Fernando earthquake happened on February 9, 1971, just a few days after we all had met for the Earthquake Engineering Research Institute's annual meeting near the Los Angeles airport. Everyone immediately became involved, and were swamped with things to do. I flew down from Menlo Park in a light plane with Jack Healy and we entered the Los Angeles basin flying along the San Gabriel fault, thinking that we might as well be looking for earthquake things as soon as possible. It was also a fine opportunity for taking some early air photos with my beloved Roloflex.

Scott: San Fernando had a major impact on programs and mitigation efforts, followed by other earthquakes, such as Coalinga, Loma Prieta, and Northridge, each with their individual and unique impacts.

Wallace: Yes indeed, and in speaking of earthquakes that happened during this period, I must not forget the Parkfield earthquake of June, 1966. Although not large or damaging, it set into motion what turned out to be the very first formal earthquake prediction, validated by both the National and State earthquake prediction councils. That was an important development, both scientifically and from a disaster-management point of view.

Also, don't let me forget to say a little about another happening with major impact on the program, the identification of the so-called "Palmdale Bulge" by Bob Castle. That led to a "first" in hazard notification in 1976 by USGS Director Vince McKelvey, who had just been delegated responsibility via FEMA and the Department of Interior to issue warnings and predictions. He took great pleasure in this "first," and flew to California to notify the Governor Jerry Brown personally about the strange Palmdale Bulge, which was believed possibly to presage a big earthquake in southern California.

After San Fernando: Rebuilding the Earthquake Program

Wallace: Following a few days investigating the San Fernando earthquake, I met Jerry Eaton at the Burbank airport when we were both on our way back to the Bay Area. At the time, and despite the USGS's marvelous response to the San Fernando earthquake, in many ways our earthquake program was in disarray. Some key figures were gone: Lou Pakiser had moved back to Denver, and George Gates had retired. In visiting while waiting for the plane, Jerry and I seemed to say simultaneously, "It is up to you and me to get the USGS earthquake program on track again."

The task was not easy for two people who disliked administrative roles. But many things were brewing at the time. The U.S. Office of Management and Budget (OMB) was beginning to say things about "duplication." The old U.S. Coast and Geodetic Survey, which had an earthquake studies role for decades, was now in NOAA, which had a powerful personality as its head--Director Bob White.

I think the first thing Jerry and I did was to recommend through USGS Director Bill Pecora that an advisory committee be reestablished. Frank Press agreed to serve as chairman, which was very fortuitous because by the spring of 1973 OMB was prepared to consolidate the USGS and NOAA earthquake programs into one.

Merger of USGS and NOAA

Scott: What was the makeup of the advisory panel, and how did it proceed?

Wallace: The advisory panel--which was a successor to the one Lou Pakiser had established several years earlier--consisted of non-USGS people from universities, state agencies and private practice. I made a point of expanding the discipline representation, including an earthquake engineer, Karl Steinbrugge. Such panels provide a broad perspective from outside the organization, which is invaluable. Any organization commonly is blind to its own faults. Unfortunately this and other such panels became the victims a presidential directive to reduce "agencies of the federal government," even though it is questionable that a panel of 10 or 12 should be classed as an agency.

At the June 1973 meeting of the advisory panel, Frank Press admonished the USGS to prepare a National Earthquake Program. There was some at least mild resistance, because many felt that we could not speak for NOAA, the Bureau of Standards and other agencies. Frank essentially scolded us, saying; "If USGS cannot define a national program, who can and will?"

At first I was not aware that Frank Press and Karl Steinbrugge were advising OMB on the merger of USGS and NOAA earthquake programs, but during the meeting that became apparent. It was much later that I learned that Karl was for the earthquake program to go to NOAA. He had good friends there, such as Bill Cloud, Don Tocher and Fritz Matthiesen, all of whom were strong, competent players in the earthquake business.

Scott: I suppose both the USGS and NOAA groups were concerned about how to approach the merger and what the impact on each agency would be.

Wallace: I should say so! Unfortunately, I seemed to get more and more involved and was put on the spot to prepare the total plan by drawing on the myriad programs which Art Grantz, Jerry Eaton, Parke Snavely, I, and many others had written over the years.

Scott: Give us some insight from your point of view on how things developed.

Wallace: On the Fourth of July, 1973, several of us of the USGS spent the holiday in Washington, D.C., getting out copies of all the USGS papers about earthquakes that had ever been published. I was duly impressed as the piles on a table in the Chief Geologist's office grew and grew until they toppled over. What an impressive record we amassed; going back to G.K. Gilbert in 1883 and Dutton's report on the 1886 Charleston, S.C., earthquake! We were sure that over at NOAA a similar panic exercise was going on.

I had abundant material for the program statement, but until August I had not made much progress in integrating it. Inasmuch as Frank Press had "ordered" it completed by the time of the next advisory panel meeting in September, time was running out. I was scheduled to attend a meeting of the International Geological Union in Montreal in late August. As things turned out, my bed in the hotel in Montreal became the work place over which I spread all of the various pieces of the report-to-be. Program plans that Art Grantz had prepared were especially helpful. I heard almost none of the fine papers presented at the meeting.

Scott: But you did get a national plan outlined?

Wallace: Yes, a plan of sorts. It was at least a basis for a national program. As soon as

I had a draft, I turned it over to the USGS Directorate. The document was published with my name as author, even though I merely compiled materials prepared by many others. More or less concurrently with the September meeting of the advisory panel, the report was transmitted by the USGS directorate to OMB.

What an explosion that caused at NOAA. Bill Hess, head of the NOAA earthquake program, phoned me. I don't remember being so loudly scolded ever before or since. I had to hold the phone at some distance from my ear. He said that we were supposed to develop a joint, cooperative program, and that I had lied and cheated in sending the "national plan" to OMB unilaterally. Of course, others had made that decision, but I had a part, of course.

Frank Press or Karl Steinbrugge would have to report on the details of action at OMB. Suffice it to say, OMB decided to consolidate the earthquake programs of NOAA with that of USGS. In 1974 the NOAA group moved from their San Francisco office to Menlo Park.

Scott: That merger was a major change in a long history of earthquake studies, wasn't it?

Wallace: Yes, it was traumatic for the folks in NOAA, and many of them were very unhappy. The USGS bent over backward to make the merger as positive a move as possible, but it was quite difficult. There is always the question of whether competing programs are wasteful "duplication," useful "redundancies" or very valuable from a straight "competitive" point of view.

With the merger, the management of the USGS earthquake program began to take better shape. Bob Hamilton, who had been with the Crustal Studies Branch in Menlo Park had moved to Washington, D.C., to be a deputy in the office of the Chief Geologist, with duties to take care of earthquake program matters. About the time the NOAA group came in, the USGS earthquake program was raised to the status of an Office, Bob being designated as office Chief by Chief Geologist, Dick Sheldon, who headed the Geologic Division.

I was delighted by the earthquake program's added stature. I had finished my tour as Regional Geologist, and a temporary role in dividing up earthquake monies, and could tactfully refuse other administrative assignments. I looked forward to years of research ahead. That was achieved in large part, except for one assignment.

The Office of Earthquake Studies

Scott: I believe that for many years you had the title of Chief Scientist, Office of Earthquake Studies. Later, "...Volcanoes and Engineering" was added to the office's name.

Wallace: Yes, that title represented a strange turn of events, and did not carry much meaning. Chief Geologist Dick Sheldon, and Bob Hamilton, by then Chief, Office of Earthquake Studies, were concerned about a still-active schism between the geophysics group--the former Branch of Crustal Studies--and the dozens of geologists in different branches who felt that geology was being given short shrift. With the appointment of seismologist Bob Hamilton as Chief of the new office, geologists felt that they had lost out again--although actually Bob also qualified as a geologist.

Jointly, Dick and Bob proposed that I take on a title and some responsibility to formalize the more-or-less informal lead role I had played in the few months following the San Fernando earthquake. They suggested the title, "Chief Scientist, Office of Earthquake Studies." The idea was that inasmuch as I was a geologist, and it was the geologic group who felt disenfranchised, this would make them feel better represented. I responded with a strong; "No way!" I didn't want anything that smacked of administration.

Under pressure, however, I accepted their idea, on the condition that I would just do my thing and have no real administrative responsibility. The ambiguous assignment had its pluses and minuses. To outsiders, it seemed that some one person did represent the Office in Menlo Park, which was the largest earthquake group. I had no administrative authority, which internally was well understood, so I could not and did not try to take certain steps. The only influence I could exert was by selling an idea, or still better, getting individuals to invent the same or a similar idea themselves. Every three or four years I resigned the title: Bob Hamilton, Rob Wesson, and John Filson each in turn rotated out as Chief. But the Chief Scientist title stuck until I retired in 1987.

<u>Other Parts of the Earthquake Program</u>

Scott: You have covered important facets of the USGS earthquake program, but there was a lot more going on, I believe. Could you briefly sketch out some of the other major efforts?

Wallace: Yes, I will give those things some attention here, but first I want to emphasize that I never intended to make this oral history memoir into a full history of the USGS earthquake program. Instead I have planned all along to deal largely with my own personal research, field work, writings, and travels. I was, of course exposed to almost all parts of the program at one time or another, but often the involvement was spotty and fragmented. So I would rather not tackle some topics to which I do not feel able to do justice. Interested readers might look at an excellent summary that Tom Hanks did of the program up to 1985. (Hanks, Thomas C., The National Earthquake Hazard Reduction Program--Scientific Status, U.S. Geological Survey Bulletin 1659, 1985.)

Putting Prediction Studies in Context

Scott: In doing that, you might put some of the projects related to prediction into context. Prediction figured in the public mind and was often treated in the media like some kind of "Gee Whiz" high-tech spectacular. In reality, a lot of solid and fundamental scientific work and thought underlay the prediction effort. You might talk about that as you round out your treatment of the USGS earthquake program.

Wallace: Prediction has already figured in my "Trail of Documents" discussion. First off, I want to emphasize that "prediction" refers to more than just the prediction of the day, hour, minute and size of an earthquake. The prediction of the effects of earthquakes and their distribution is equally important for hazard-reduction measures. (See: Holzer, Thomas L., "Predicting Earthquake Effects--Learning from Northridge and Loma Prieta," Science, v. 265, August 26, 1994, pp.1182-1183.)

"Almost everything bends before it breaks" is the simple concept that underlay several of the USGS prediction projects. I often used a stick or a thin piece of cedar roofing shingle to illustrate this point for lay audiences. I would bend the piece of wood until it almost broke. Then as I continued bending it I would ask members of the audience to call out when they thought the stick was about to break. Someone in the audience was nearly always right on the mark. Then I would tell the group that before the stick broke, I could hear little crackles and pops as wood strands and fibers gave way. I compared the crackles to earthquake foreshocks, and the break itself to the main earthquake. Furthermore, the vibration I felt when the stick broke were like the ground shaking that goes with an earthquake. I pointed out that we would be better able to predict the stick's breaking point if we knew more precisely how strong the stick was, how its fibers were arranged, and how hard I was pressing.

Scott: You used the breaking-stick analogy to demonstrate the range of geologic phenomena that must be studied to get a better idea of when earthquakes may occur?

Wallace: Yes, and the analogy suggests some major parts of the prediction program. How can we measure bending (straining) of the Earth's crust before the big break? How can small shocks noted before a major earthquake be identified as foreshocks of the big event and not just the usual background of seismic activity? How can we predict the strength of the earthquake shaking?

Scott: Those are straightforward questions, but are probably very hard to answer.

Wallace: I should say so. In 1960, who knew how to measure the bending (strainig) of the earth's crust over hundreds or thousands of kilometers, or even over much smaller distances, in both two- and three-dimension, and in a suitable time frame. Just how do you set about identifying small shocks as foreshocks?

Each idea for an experiment to address one of these problems is bound to have branches and subtopics. Furthermore, the simple model of an earth that behaves elastically is complicated by the presence of water almost everywhere. Water makes rocks less brittle and more ductile. Similarly, the greater pressures and heat prevailing at depths in the crust make rocks behave in a more ductile manner.

Measuring Crustal Bending and Warping (Straining)

Scott: How is the bending of the earth's crust measured?

Wallace: Methods first used for measuring bending or warping (straining) near the San Andreas fault were rather straightforward. They included simple surveying and levelling, creep meters, and tilt meters. The USGS began testing and refining these techniques from the 1960's on, using alignment arrays, creepmeters, and tilt meters. Straight lines of monuments were placed in a fence-row pattern across the San Andreas fault and resurveyed repeatedly for any deviation from the original alignment. This might detect slight precursory movements.

So-called "creepmeters" were made of invar steel wires, perhaps 100 meters long and suspended in buried, protecting pipes. The wires were anchored diagonally across the fault; one end was attached to a sensitive measuring device. If even very small movements on the fault occurred, the amount of extension or shortening was recorded. Bob Burford and Sandra

Schulz (later to become Mrs. Burford) spent many years recording changes in the earth using these techniques. The Parkfield prediction area was an important target.

Sensitive "tilt meters" were placed in patterns to detect very slight disturbances of the ground surface. Tilt meters are nothing but a levelling device, commonly a bubble device electronically attached to recording devices, which can measure very minute changes in the slope of the ground.

As simple as they were, each of these methods required the invention and construction of basic new instruments, followed by designing ways to protect the devices from temperature and weather changes. In the shallow vaults built to protect the recording devices at the end of a creep-meter, Sandy Schulz would often find a family of black-widow spiders climbing around the recorder, or a pool of rain water covering the instruments. Many variations in instrumentation were tried to determine which were the best, and of course cost was always a factor. How could it be done at lower cost and with less maintenance? Improvisation was a daily affair.

Much interesting data was obtained, and some of it seemed to hold promise for prediction. Tectonic creep--the slow slip sometimes observed along faults--would proceed at a constant rate for a time. Then, not uncommonly just before an earthquake, the rate of creep would slow down, halt, or speed up. But there was always a big background question. Could these near-surface changes really give a good clue as to what was happening at the depths where earthquakes nucleate?

Measuring Long Surface Lines

Wallace: When the USGS program began to take shape in the 1960s, there were no entirely satisfactory methods for measuring longer lines on the earth's surface. Following antiquated and unsuitable surveying methods at first, the invention and development of the laser provided a new and potentially powerful approach. Taking each laser measurement was complicated, however, because the determined distance varied according to air temperature and humidity along the line of sight. For a time, light airplanes were flown along the line being measured, in order to make the atmospheric measurements while the lengths were being determined. Later, to counteract the correction problem, the USGS had two-color lasers designed and built. The two-color instruments could automatically correct for variations in air temperature and humidity along the lines of sight, and more frequent line measurements became practical.

A very successful two-color unit was deployed at Parkfield for the prediction experiment there. The unit has performed very well over the years. But the cost of building and operating custom-made units proved far too great for our budgets. Consequently, and sad to say, similar laser units have never been used for measurements along other reaches of the fault.

Following the launch of Sputnik in 1957, earth-orbiting satellites gave science a way to view large areas of the crust simultaneously. Now the Global Positioning System (GPS), developed by the U.S. Air Force, uses signals from several orbiting satellites in a ranging mode to give positions routinely. The potentials of this technique are now being realized in the earthquake prediction program.

I can remember in the 1970s hearing presentations to the Committee on Seismology, National Academy of Science, on the precision of space techniques. Early on, investigators were pleased to be able to make measurements accurate to within hundreds of meters, then to meters and then to centimeters. Now aircraft or hikers can use light-weight GPS units to determine precisely where they are anywhere in the world. Automation of the process permits aircraft to be guided to a landing automatically, without human intervention.

Jim Savage and associates, especially his colleague Mike Lisowski, have been especially successful in using the long-line techniques, including trilateration networks, laser ranging and GPS, to track crustal changes in the western U.S. and Alaska. One of their papers recounts a long-term study in Nevada and illustrates some of the techniques used and types of findings. (Savage, J.C., Lisowski, M., Svarc, J.L., and Gross, W.K., "Strain Accumulation Across the Central Nevada Seismic Zone, 1973-1994," Journal of Geophysical Research, v.100, n.B10, 1995, pp. 20, 275-20, 269.) Efforts to monitor the strain across the Long Valley caldera and Mammoth Mountain on the east side of the Sierra Nevada have been extremely important in trying to keep abreast of the possibility of major earthquakes and volcanic eruptions there.

Dave Hill has been the principal investigator of the Long Valley caldera. The potential for both a major eruption and a major earthquake has caused local consternation, inasmuch as the area is a major ski resort with as many as 50,000 visitors during winter weekends. So the USGS has attempted to maintain an on-going evaluation of the hazardous situation there.

While pursuing his specialty, which is seismology, Dave has been kept busy watching a wide range of changes, e.g. emission of excessive amounts of carbon dioxide, constantly providing the public with up-to-date information, and doing his own scientific research. (Hill, David P., "Earthquakes and Carbon Dioxide Beneath Mammoth Mountain, California." Seismological Research Letters, v.67, n.1, 1996, pp.8-15.)

The Palmdale Bulge

Scott: I remember that the so-called Palmdale Bulge in southern California attracted a great deal of attention for several years. It was also called the Southern California Uplift, particularly by those who did not like the Palmdale area singled out. There was a considerable debate about the Bulge, during which even its very existence was seriously questioned. The Bulge issue was in a way part of the earthquake prediction discussion. Can you say a word about that?

Wallace: Yes. The phenomenon was identified by some of the geodetic techniques also used for prediction. The major finding that called attention to the bulge was based on leveling records gathered over many decades by the National Geodetic Survey (NGS) and others. Bob Castle (USGS) in 1974 began studying and reviewing these records, obtained largely from NGS.

Regional leveling had been done much as it is done in local surveys such as those done in laying out foundations for homes. Using a telescopic level to view a rod placed at a point some distance away, the difference in elevation between two points is determined. When the leveling must be extended over miles or even hundreds of miles, however, very stringent procedures are essential to prevent the accumulation of small routine or systematic errors that could otherwise significantly distort the results.

Many level lines had been surveyed across southern California by both the NGS an others working on railroads, highways and pipelines. It was these records that Bob Castle reviewed, and to make a long story short, he found that an enormous elliptically-shaped area had risen episodically by as much as 0.45 meters during the previous several decades. The area covered much of the San Gabriel and San Bernardino Mountains, and beyond to the west and east, extending in width from the front of the ranges on the south to well out in the Mojave Desert to the north.

Scott: I believe it was called the Palmdale Bulge because it sort of centered on Palmdale, or the highest uplift was noted as near Palmdale. Is that correct?

Wallace: Yes, the uplift became known as the "Palmdale Bulge" because the town of Palmdale on the north side of the San Gabriel Mountains was in the area of greatest uplift. This pattern of uplift was not particularly surprising to me, however, or to many other geologists either, because the geomorphology of the Transverse Ranges clearly indicates a pattern of uplift over the past million years or more, with rapid uplift in most recent geologic time.

Nevertheless, such clear instrumental evidence of modern uplift was alarming. Among other concerns, the area defined by the uplift lay astride the San Andreas fault, and across a part of the fault whose 1857 rupture produced one of California's great historic earthquakes. Elsewhere I have told how Kerry Sieh had estimated that the recurrence interval of great earthquakes on that fault segment approximated the elapsed time since the 1857 earthquake.

Was another earthquake the size of 1857 about to occur? Considering what I described earlier about the crust of the earth bending before breaking, perhaps here was a mammoth-sized precursor of an impending great earthquake in southern California.

Scott: The phenomenon did have ominous overtones. Because the implications seemed very serious, I can see why USGS Director Vince McKelvey took Bob Castle's analysis very seriously, and made a trip to California in 1976 to alert and brief the Governor personally. The Bulge issue riveted the attention of the new Seismic Safety Commission, all the more so since Commissioner Bob Rigney was the administrative officer of San Bernardino County, whose territory ran near Palmdale. With so much interest, I presume that the data and its interpretation got some pretty thorough independent scrutiny by others, to minimize the chance of some mistake having crept in?

Wallace: Yes indeed, they were checked by many people and by several independent techniques. Perhaps the most significant challenge came from David Jackson, a geophysicist at the University of California at Los Angeles. Jackson's analyses led him to believe that systematic errors in the original surveying had lead to the misinterpretation of an uplift. The error concerned the accuracy of the level rods and improper adjustments for weather factors and refraction along the lines of sight between the rods and the leveling instrument.

As a result, many other tests were run, additional level lines were measured, and other techniques were tried, in order to get independent confirmation or denial of the Palmdale Bulge's existence. Among these, several studies in the changes of gravity across the Bulge area strongly supported the reality of an uplift. While the Palmdale Bulge survived these close scrutinies, in the final analysis the height of the uplift may prove to have been less than originally estimated.

The Bulge was first recognized not long after the 1971 San Fernando earthquake, and since then numerous earthquakes of moderate size have occurred along the southern flank of the Bulge, including such damaging earthquakes as Landers (1992), and Northridge (1994). Clearly the southern California region has been unusually active in the past few decades, and earthquakes must be considered part of the overall pattern yet to be understood. I wish we had a way to judge the significance of these earthquakes, because they could be part of a pattern leading up to a great, disastrous earthquake. The final chapter on this is yet to be written.

Seismological Studies and the Parkfield Experiment

Scott: You mention seismology, Dave Hill's specialty. Presumably the USGS had many such projects going. Could you say something about them, particularly as related to earthquake studies?

Wallace: The projects range from global seismology to detailed local studies, from the study of extremely large earthquakes to microearthquakes, and from foreshocks to the changes in normal patterns of seismicity. Within each area of study exciting possibilities continually show up in the search for the Holy Grail of prediction. Generally, however, very few of even the most exciting possibilities have had sufficient funding for adequate follow up. The same holds true in the search for better ways of defining related potential hazards, regardless of when the big event might occur.

Scott: A mostly seismology project that was exceptional in attracting a great deal of attention and getting quite a lot of funding was the Parkfield study. Maybe you could start off with the Parkfield study. It was a very big thing for several years. I even recall the Seismic Safety Commission taking a day-long field trip to Parkfield for briefings on the whole program.

Wallace: Let me begin with a little background. Many in the USGS earthquake group that came together after the 1964 Alaskan earthquake were excited about the idea of earthquake prediction. It would require scientific investigations at the cutting edge of science, and the potential societal benefits seemed to be enormous. As noted earlier, Frank Press, Science Adviser to President Carter chaired a panel that wrote the first major proposal in 1965 for a national program to reduce the hazards of earthquakes. That proposal outlined a ten year program for learning how to predict earthquakes.

The stage thus was set when a moderate-sized (M 5.5) earthquake struck the hamlet of Parkfield along the San Andreas fault in 1966 in a remote area of central California. The main earthquake hit at 9:26 p.m. on the evening of June 27th. A group of seismologists and geologists at Caltech, including Clarence Allen, left Pasadena for Parkfield soon after they had an epicentral location determined for the earthquake.

The next morning a USGS group, including Lou Pakiser, Doc (Manuel) Bonilla, and me, set out for Parkfield. Where we crossed the general trace of the San Andreas fault zone near Cholame, we stopped the car, got out and looked for ground fractures that might have occurred during the earthquake. "Eureka," there they were. The main fracture offset the white line in the middle of Highway 46 by about 5 cm, and the apparent movement--east side to the southeast--was what we would expect along the strike-slip San Andreas fault.

Scott: I guess that was the beginning of the interest in the Parkfield region?

Wallace: Yes it was, and some exciting things began to show up immediately. We found Clarence Allen lying out in the shade of a big live oak tree, after an all-night stint of driving and field study using headlights and flashlights. "Did you see the offset in the white line on Highway 46?", he asked. "Yes we did", I said taking out my sketch of the offset. "It was about 5 cm.", I reported, or nearly 2 inches. "It was?" Clarence replied incredulously. "I measured only about 1 inch". We soon realized that we had encountered a new, previously unreported phenomenon--now known as post-earthquake "creep" or "slip." That was very exciting.

Scott: I can imagine. I know about Karl Steinbrugge's discovery of tectonic creep at the winery near Hollister. So at Parkfield you were getting first-hand exposure to a new form of creep?

Wallace: Yes. It soon led to the notion that creep might speed up before an earthquake, and thus serve as a good precursor. Later that same day we heard about a water pipe line that broke where it crossed the San Andreas fault near the Wilson ranch, and that the break had occurred about nine hours before the June 27 earthquake. The break in the pipe showed the characteristic movement on the San Andreas fault--east side to the southeast. Did this truly represent pre-earthquake fault slip?

A few days later I examined the site carefully, looking for other possible causes of the pipe break, such as being bumped or shifted by moving farm machinery. After questioning local ranchers and making careful examinations, I had found no explanation for the break other than pre-earthquake fault movement.

A local rancher, Herbert Durham, took me to a spot on Turkey Flat road where the 1966 fault break had offset the road and recounted how the fault also broke there in 1934. He told me of the difficulty he had back then getting his team of horses to cross the break.

Clarence Allen reported that on June 16, 1966, some eleven days before the earthquake, he had taken a group of Japanese scientists along this part of the fault. Clear ground fractures could be seen even then. Dr. Keichi Kasahara, of Japan, had taken photographs. With D.B. Slemmons, University of Nevada, as intermediary, Dr. Kasahara kindly provided me with a copy of the photographs for use in our paper on the earthquake. Here again was a strong suggestion of slip and cracking before the earthquake. At that time, however, we did not have a good idea of the extent of fault creep in the absence of an earthquake.

Two years later, in 1968, Bob Brown and I reported in print that a creeping section of the fault extended from Cholame northwest to San Juan Bautista, but that south of Cholame the San Andreas fault seemed to be "locked," and no slip had been observed since the earthquake of 1857. We reached this conclusion by a study of fence lines. Northwest of Cholame, fences were clearly offset where they crossed the fault, but fences south of the latitude of Cholame were not offset. Our paper was the first to state the distribution and rates of slip along this creeping section and to note the sharp change to no slip to the southeast. Later, more precise measurements confirmed our crude first determinations. (Brown, R.D.Jr. and Wallace, R.E., "Current and Historic Fault Movement Along the San Andreas Fault Between Paicines and Camp Dix, California," Proceedings of Conference on Geologic Problems of San Andreas Fault System; Stanford University Publications, Geological Sciences, v. XI., 1968, pp. 22-41.)

In those days I became very optimistic that we would find many more things happening before earthquakes on which we could base predictions. I could not and still do not believe that a gigantic event involving perhaps hundreds or thousands of cubic kilometers of the earth's crust could sneak up on us without some warning. Could an elephant creep into our camp without us at least hearing some slight noise, if we had the proper detection system?

Scott: I can see why your 1966 experiences made you optimistic about prediction. But I believe only considerably later was any effort made to predict a repeat of the 1966 earthquake.

Wallace: That is right. In May 1984 Bill Bakun (USGS) and Tom McEvilly (University of California, Berkeley) noted in print that the 1966 earthquake had been preceded by almost identical events in 1922 and 1934. The detailed lines on the seismographic records of the three earthquakes could be laid exactly one over another. In their joint paper Bakun and McEvilly suggested that the next such "characteristic" earthquakes might occur between 1983 and 1993. (Bakun, W.H., and McEvilly, T.V., "Recurrence Model and Parkfield, California, Earthquakes," Journal of Geophysical Research, v. 89, no. B5, 1984, pp. 3051-3058.)

This idea was amplified by Bill Bakun and Al Lindh in two papers published in 1985, although written in 1984, and they added the evidence that after 1857 a series of five very similar earthquakes had occurred, spaced about 22 years apart. On that basis they suggested a ten-year time for the next Parkfield earthquake. In the Terra paper the prediction is stated as "within a ten-year time frame window centered on 1987-1988." (Bakun, W.H., and Lindh, A.G., "The Parkfield, California, Prediction Experiment," Earthquake Prediction Research, 3, 1985, pp. 285-304, published by Terra Scientific Publishing Co., Tokyo, Japan.) (Bakun, W.H. and Lindh, A.G., "The Parkfield, California, Earthquake Prediction Experiment," Science, v. 229, pp. 619-624.)

In early 1984 the emerging Parkfield prediction matter was reported at a meeting of the National Earthquake Prediction Evaluation Council (NEPEC). But just before the Parkfield discussion, and by sheer coincidence, I had presented a proposed set of definitions of terms to apply to predictions. Jim Davis (California Division of Mines and Geology) and Karen McNally (University of California at Santa Cruz) and I had been designated as a committee of the Southern California Earthquake Preparedness Project (SCEPP) to prepare definitions.

The definitions had been requested to serve as a guide for southern California jurisdictions trying to design response measures to predictions. A misconception had grown that scientists were prepared at any moment to produce a valid prediction of a disastrous earthquake in southern California. That idea prompted SCEPP to contract with several communities, such as the City and County of Los Angeles, to prepare response plans.

Scott: All of this reminds me of the widespread excitement that prediction had generated back then. The idea had taken hold, seemingly all around the world.

Wallace: The national council having been presented with an apparent prediction of a Parkfield earthquake at the same meeting as a new set of prediction definitions was approved, made it almost inevitable that Parkfield should be designated as a formal "prediction." Soon after, the California Earthquake Prediction Evaluation Council also formally accepted the Parkfield prediction as reasonable.

Within the earthquake hazard reduction program, an experiment to "trap" a potentially damaging earthquake had been discussed ever since the Press report had made such proposals. Parkfield seemed to be a excellent site for the "trapping" experiment.

Scott: So prompted by the Parkfield "prediction," the scientific community began marshalling resources for a "trapping" experiment there?

Wallace: Yes. In addition, those who were administratively responsible for disaster response measures--especially at the state level--were also activated. The network of seismometers was gradually enlarged and improved, a project to monitor water wells was started, more tilt meters, magnetometers, strong-motion recorders, and geodetic nets were put in place, and a time-lapse photo net was set up. Signals from each net were telemetered to Menlo Park, where they could be compared and analyzed. In addition, a local headquarters was established near Parkfield in an old farm house next to the fault.

I should interject here that although the "characteristic" M5/5-6 earthquake held the focus of the Parkfield prediction, a series of interpretations arose that suggested the next earthquake might be a magnitude 7. Furthermore, many considered it not unreasonable that the successor to the great 1857 earthquake might start in the Parkfield reach of the fault, as it may have in 1857, possibly starting with a Parkfield-type earthquake as a point of nucleation. Concern ran high for several years, and still persists in a more moderate form. Meanwhile there has been no new hard evidence.

In connection with the Parkfield prediction, the California Office of Emergency Services began designing and testing procedures for coordinating local jurisdictions and the state in handling communications and prediction announcements, and in developing responses. New terminology had to be developed, radio frequencies assigned, and chains of command and delegations of authority had to be devised. This was all considered essential for an earthquake prediction to have beneficial societal consequences.

The USGS management in Reston, Virginia, had to ease its stringent policy of requiring official headquarters clearance of important announcements or news stories. The old procedures would not work, given the short time for developing a prediction from seismic data analysis and formulating an announcement. While that might seem like a relatively simple matter to rectify, it actually took several years to achieve a satisfactory delegation of authority to smaller offices such as Menlo Park, where predictions were most likely to be made.

Bill Bakun and many others worked long and hard on specific criteria and definitions, to designate predictions of different levels as indicated by signals from the different instrumental systems. Finally the USGS headquarters came to trust the staff at Menlo Park with the important duty of issuing predictions! In fact that delegation may have been one of the most important administrative accomplishments to come out of the Parkfield prediction experiment.

Scott: As one who has studied government in action, I can understand the extraordinary difficulty of achieving such delegation, particularly when the decisions on sensitive, high-visibility matters. USGS deserves some real credit for what it was able to do. I also think many other scientific and public policy agencies learned a lot from Parkfield.

Wallace: Yes, a lot of good was done. Unfortunately, however, the predicted Parkfield earthquake simply did not happen within the time window where it had been expected. It still has not occurred, even as we are concluding work on this oral history.

The predicted earthquake had been assigned a probability of about 10 percent per year, with an estimated cumulative probability of well over 90 percent within 30 years. Although it missed the time-window, most scientists involved with Parkfield felt that a characteristic earthquake of about 5.5-6 magnitude would still occur, following the pattern of nucleating in the same small volume of rock along the fault where the 1966 event began.

Despite the Parkfield earthquake's failure to arrive on time, many things were learned about earthquake processes that occur before, during and after moderate-sized events. Many smaller earthquakes were captured on the dense array of instruments deployed at Parkfield, and hundreds of technical papers have been written reporting a wealth of scientific findings.

Scott: In short, the Parkfield project produced an enormous amount of data and substantially furthered our understanding of the earthquake process itself?

Wallace: No question about that! The failure to predict the specific time of the earthquake soon led to growing pressure to close down the prediction experiment and shift the funds to other projects. In response the USGS convened a special working group (made up of non-USGS scientists and emergency-management experts) of the National Earthquake Prediction Evaluation Council to evaluate the results of the experiment and to advise on what should be done: close down the experiment or continue at some level?

The working group reported in 1995, and its recommendations included the following:

"The USGS should recognize and provide support for the Experiment as a scientific experiment in the broader integrated context of an actual public policy activity." (p. 11)

"Parkfield remains the best identified locale to trap an earthquake." (p. 13)

"....the Experiment should be viewed with a long-term perspective. The Experiment should not stagnate: rather it should continue to evolve." (p.13)

(National Earthquake Prediction Evaluation Council Working Group, B.H. Hager (ch.), Earthquake Research at Parkfield, California, for 1993 and Beyond, Report of the NEPEC Working Group to Evaluate the Parkfield Earthquake Prediction Experiment, U.S. Geological Survey Circular 1116, p.1-14, 1995.)

Scott: The working group clearly concluded that the Parkfield prediction experiment had been very worthwhile.

Global Seismology

Wallace: Let's now shift from the localized Parkfield project to other earthquake studies, starting with global seismology. For many years Waverly Person in the USGS office in Golden, Colorado, has been responsible for reporting earthquakes on the global network. In a worldwide cooperative effort, Waverly draws on the records from a myriad institutions and universities around the world, as well as managing USGS networks.

To serve the cooperative organizations well, more than a decade ago Waverly's group was one of the very first to distribute vast quantities of data on CD-ROMs. First the preliminary determination of epicenters of earthquakes were released even before the majority of investigators had computers. This first experiment in data distribution has been expanded in almost every other program of the USGS: geologic map data, water resource data, and topographic data in digital form are now available on CD-ROMs.

Microearthquake Seismology

Wallace: Microearthquake seismology has become one of the most valuable and productive parts of the USGS earthquake program, and its history is well described by Jerry Eaton. (Eaton, J.P., 1996, Microearthquake Seismology in USGS Volcano and Earthquake Hazards Studies: 1953-1995, U.S. Geological Survey Open-file report 96-54, 1996.) It is hard to overstate the importance of the microearthquake seismology program's accomplishments.

I cannot overemphasize the microearthquake nets in contributing to understanding fault behavior along the San Andreas system. The nets have given us our first four-dimensional picture of earthquake occurrences along the faults (time being the fourth dimension). Microearthquake data clearly define the base of the brittle zone that produces earthquakes, as well as irregularities in the base, and laterally across the fault. The timing of extensions and migration of seismicity are readily seen, as are gaps in seismicity. The clustering of aftershocks observed suggests signifcant physicial processes.

I have already mentioned Jerry Eaton's development of microearthquake nets, which he did first in Hawaii, and then along the San Andreas fault in central California. Jerry made many of the early instruments out of begged and borrowed components. But as the usefulness of mapping seismicity in time and space became apparent, the net in central California was expanded to southern California.

Wallace: Automation in analyzing data became a necessity, and real-time processing (RTP) by computer evolved through efforts of Sam Stewart, Rex Allen, and many others. Most small earthquakes are now located automatically, and magnitudes assigned, in a matter of seconds. An office of the USGS was established in Pasadena in southern California adjacent to the Caltech offices to facilitate close cooperation with that major center for seismologic research. Tom Heaton and Lucy Jones became major players there.

Tom Heaton initiated and has led the way to what is truly a very short-term prediction technique--a prediction measured in seconds to tens of seconds. It depends on having one or more seismometers very close to an epicenter. As soon as the start of earthquake movement is detected, an electronic alarm is transmitted to outlying areas. By arriving many seconds before the damaging earthquake shaking arrives, the warning signal can automatically trigger a variety of emergency measures--such as around nuclear power plants or along rail or transport routes. Or automatic alerts can be flashed by radio and TV.

Scott: Say a little more about the deployment of microearthquake nets.

Wallace: Microearthquake nets became essential for the study of other regions. After the great earthquake of 1964 in Anchorage, the seismically active areas of Alaska could not be ignored. Bob Page and John Lahr have lead the investigations there in recent years. Microearthquake nets and investigations were also established in Nevada, Oregon, Washington, the mid-continent, and along the eastern seaboard. Most of these studies were carried out cooperatively with Universities. Finding funds for expansion and maintenance of the regional networks became a major perennial problem. Many other federal and state agencies participated.

Temporary, portable nets are deployed immediately after almost every major earthquake. At first, the seismic signals were scribed mechanically and directly as the swinging arm of the seismograph scratched its movements onto the smoked drums. As technology progressed the signals were recorded on photographic film. More recently, the signals are sent in digital form to be analyzed and stored by various computer techniques.

Strong-motion Seismology

Scott: Those are valuable observations on some of the management and budgetary problems of program operation. Could you now say more about some of the other elements of the seismology effort? What about the strong-motion studies that engineers view as so important to their needs?

Wallace: Yes, I want to stress the importance of strong ground-motion studies. To study strong motion requires a very different approach than regional or global seismicity, different instrumentation and different deployment. Ordinary seismographs are driven beyond their limits by strong ground motion and thus fail to record significant details of motion. From very crude instrumentation, we have progressed to where earthquakes of very wide dynamic range (from very weak to very strong shaking) can be fully recorded. The advent of digital recording underlies this fundamental progress.

In recent years, the strong signals can be dissected, and each new generation of computers permits analyses of more and more complex signals. As a geologist fascinated with faults, I have been excited by the work of seismologists such as Ralph Archuleta (now at the University of California, Santa Barbara), Paul Spudich, and Roger Borcherdt. (Editor's Note: Roger Borcherdt received EERI's Outstanding Paper Award at the Annual Meeting in Los Angeles, February 1996. The award was given for his 1994 Earthquake Spectra paper on a methodology for estimating site-dependent response spectra.)

Analyses of strong-motion records have permitted them and others to show exactly where rupture begins on a fault plane, then how a fracture spreads in a microsecond time frame up and down and along the length of the fault plane. I can see such information eventually letting us identify the weak and strong places on a fault and thus permitting us to focus attention in just the right places to look for predictive signals. Large lobes of directed energy commonly radiate from a rupture in a process that still is not fully understood. Strong-motion studies are essential to a better grasp of what is going on.

Scott: Yes, and we have been making a lot of progress in collecting good strong motion information, through instrument installations in buildings, and so-called "free-field" installations away from buildings. A Los Angeles ordinance requiring such instrumentation was a major breakthrough, and later a state-wide program was set up, administered by the Division of Mines and Geology, which eventually took over the Los Angeles program. The installations are financed from a very small surcharge collected through fees for building permits. I think the program is generally viewed as one of California's great success stories in obtaining information that is essential to effective earthquake engineering.

Wallace: Yes, good estimates of probable strong motion are crucial for earthquake engineers, because it is the effect of strong ground motion on buildings that causes failure and collapse. The study of both the source of the motion in the earth and the response of buildings to that motion stand out as of the highest priority in assisting engineers to design earthquake-resistant buildings.

For decades Ted Algermissen and his colleagues worked at preparing and improving maps of the United States showing estimates of the strong ground motion to be expected in each region. Engineers have used these maps to set basic parameters for design and construction. In the field of strong-ground-motion seismology, Bill Joyner and Dave Boore have taken their fundamental studies and, working with engineers, have helped to keep building codes abreast of the state of the science.

Roger Borcherdt and several other investigators found ways to estimate local variations in strong ground motion, and have prepared detailed maps (microzonation maps) to depict these local variations. Addressing the microzonation problem after the Loma Prieta earthquake, Jack Evernden developed a powerful algorithm which, with the help of computers, permitted him to successfully predict in considerable detail the strong motion likely to be generated by any earthquake.

Selecting Priorities in a Complex Program

Scott: It sounds enormously complex.

Wallace: Vast amounts of data have been accumulated, creating great opportunities for new scientific breakthroughs. But the potential is largely untapped, as available scientific manpower has seldom equalled the opportunities begging to be addressed.

Other parts of the earthquake hazard reduction program always were in competition with the seismology program. I remember scientists complaining again and again that seismic nets were using so much of the budget, that "My more important project cannot be funded or staffed." But how can we possibly understand earthquakes if we don't know where and how big they are. Without seismic records myriad cause-and-effect relations can never be established.

Even as I have been writing this, in brown-bag luncheon discussions at the tables outside USGS Building 8, W.P. (Porter) Irwin repeatedly asserts, "If even a small part of the funding had been devoted to geologic mapping, we would now be way ahead in our understanding of the earthquake processes, and, furthermore, we would have an invaluable legacy for a variety of other uses." Earl Brabb angrily adds that, "The earthquake program would not continue support for the landslide program that I labored so hard to develop under the Bay Area study--even though landslides triggered by earthquakes are a major hazard."

At the same brown-bag lunches, Jack Healy holds to his conviction, "If we had just put our money into drilling deep holes in the regions where earthquakes form--2 to 10 km down--and then instrumented them properly, we would by now have earthquake prediction in hand." These three brilliant and productive scientists now are in emeritus status, but a wealth of diverse ideas still swirl among the fully-active scientific staff.

Scott: I presume USGS staff members generate intriguing and potentially valuable ideas all the time. It must be hard for managers, who have to select what to support, and what not to support, particularly when funds are limited.

Wallace: Yes, that is a perennial problem. As the USGS rotates managers, you can be sure that the personal ideas and prejudices of the manager of the moment play strongly into the decisions made, especially the distribution of money. Unfortunately, the manager's judgments about projects and directions are never neutral, in so far as scientific staff is concerned. That is just human nature. So it could put things in a rut if a single manager's preferences regarding approaches and personnel were to dominate for a extended period.

Determining Deep Crustal Structures

Scott: The geologic study of earthquakes not only deals with phenomena at or near the surface, such as visible fault breaks or shallow earthquakes but also has to consider things that go on much deeper down. Would you say something about that kind of work?

Wallace: The crust of the earth is not a single, simple block of rock, but is complexly layered and divided into a multitude of blocks and blobs of different rock materials. At places where the crust has been upwarped and eroded deeply, we can see this diversity at the surface.

It takes "remote sensing" techniques, however, to determine the arrangement of things at depth. Deep holes can be drilled, as in the exploration for petroleum, but generally for the very deep crustal exploration, the numbers and depths of drill holes needed far exceed realistic funding possibilities.

Geophysical techniques include mapping the patterns of rock density by measuring gravity and mapping patterns of magnetic differences. Each rock and group of rocks differ in their density and magnetic attraction. Analysis of these patterns can disclose large regional patterns, sharp boundaries between rock types, perhaps earthquake-related faults can be recognized, and the general configuration of the structures deciphered. Deep-sounding techniques are essential to the study of deep earthquake faults and to understanding how deep forces cause the crust to bend.

Bob Jachens and Andy Griscom have focussed on gravity techniques, and have found places where the upper mantle (the material below the crust) comes to the surface, or close to it. Izzy Zietz also comes to mind for being loudly vocal during his career in cajoling, and insisting, and selling the idea that a magnetic map of the U.S. must be financed and completed, regardless of what happened to other programs. Thanks to him, as well as many others, a magnetic map of the U.S. does exist, and major invisible structures can be deciphered from it.

Seismic methods also are invaluable in exploring the deep crustal and upper mantle parts of the earth. Many seismic refraction and reflection teams have worked since before the earthquake program began. I have already told of how in the late 1950s the group headed by Lou Pakiser, and headquartered in

Denver, worked to help solve crustal problems for the underground nuclear-testing and detection programs. Helping national defense needs in the battle with the Soviet Union was no small accomplishment.

The Pakiser group moved to Menlo Park in the mid-1960s and formed the core of the geophysical group studying natural earthquakes. Lou, himself, and many members of the Crustal Studies team were leaders in the international seismology community, and played a key role in assuring that the USGS became an indispensable agency in the National Earthquake Hazards Reduction Program.

More recently, Walter Mooney and Gary Fuis have lead the way in the study of the deep crust. Using seismic reflection and refraction techniques, they have explored deep "cross sections" along many long lines or transects across Alaska, the margins of the United States bordering the Pacific, the San Andreas fault, the Sierra Nevada as well as many other major faults, mountain ranges and basins within the continent. Without these geophysical views of the deep structures within the earth, earthquake prediction and many other needs of the nation would never be possible.

In recent years, however, the program has shrunk because of an inflation-driven decrease in usable funds and personnel available. The RIF ("reduction in force") imposed on the USGS in 1995 has severely limited most programs. The RIF and the reorganization that accompanied it also prompted a decline in staff morale. As Jerry Eaton observed to me recently, "Without the USGS's strong tradition for innovation and encouragement for individual scientists to follow their own instincts, an attitude which has withered recently, I would not have come to the USGS to work."

Volcanos

Scott: For many years the office to which you were attached has been called the Office of Earthquakes, Volcanos and Engineering. Would you say a little about the "volcanos and engineering" portions of the program?

Wallace: Yes, but I will only say a word, because at this stage I cannot really do them justice. For further information, I recommend to readers the bimonthly USGS publication Earthquakes and Volcanos, which includes both technical and non-technical articles.

The volcano program has many notable accomplishments, one of which I will mention here--the successful forecasts of Mount Pinatubo's 1991 eruption in in the Philippines. Those forecasts came about after investigations by a group of USGS volcanologists and seismologists headed by Dave Harlow. They had an office close to the mountain, and stuck it out there even into the major phases of the eruption when volcanic ash and blocks were falling all around them.

On the basis of their work and advice, the U.S. Air Force was able to conduct a timely evacuation of Clark Air Base, saving many lives of military personnel, as well as planes and equipment worth hundreds of millions of dollars. In addition probably thousands of the lives of the local population were saved. It was a remarkable accomplishment for science, and for USGS.

The Computer Revolution

Wallace: The USGS followed, adapted to, and has made use of the explosion of the computer era. Early on scientists in the earthquake program took to computers to solve many geophysical problems. Computers became essential tools of research and investigation. I think of Willie Lee and Pete Ward as leading the USGS earthquake program's venture into the computer world.

Scott: You use a computer quite a bit yourself, don't you?

Wallace: I would be totally lost without a computer, although I have a lot still to learn. Learning about and coping with computers seems to become a full-time job. And I find that my joy in pondering and inventing new concepts about earth-science questions has suffered.

Scott: Yes, in addition to all you can do with them, there are some real down-sides to working with personal computers. You have to be careful how you spend time on them.

Wallace: We all use them. Every scientist in the earthquake program either has a personal computer on his desk, or can tap into the larger computers maintained by the USGS's Information Systems Division. During the 1980s, funding to tool up and join the computer age added problems to an already severely stretched budget.

The earthquake program has suffered the same stresses most computer users face. The rate of technological change is phenomenal. What basic computer techniques deserve large expenditures as the industry grows and is transformed? Costly individual units seem to become obsolete even while they are being warmed up.

Scott: I think most organizations and businesses have faced the dilemma of keeping up with the fast-changing field, but avoiding unwise expenditures.

VIII EARTHQUAKE EFFORTS OF OTHER INSTITUTIONS

Scott: You have covered the USGS earthquake program well, in my estimation. Could you now say something about the principal other organizations and individuals who were involved in earthquake-related programs? I realize that it is a pretty complicated subject.

Wallace: Yes, it is a big subject, and what I can contribute will have to be very selective and incomplete. I will also have to apologize in advance for leaving out many of the people and institutions that had major influences. Nor can I begin to cover the constant and chameleon-like changes, with the flow of people and activities through programs like these.

I can identify periods of five to ten years in which the same names appear on one advisory panel after another. Then ten years later, the whole cadre of names would have changed, along with the priorities that topped their agendas. Some institutions would have disappeared and new ones created. Individuals retire or change jobs--some much-needed individuals are inconsiderate enough to die. The fact of constant change is always with us.

State Agencies

Wallace: California state institutions have played enormous roles in the evolution of earthquake programs, and this is a complex story I can only touch on. Organizations such as the California Division of Mines and Geology (CDMG), the California Seismic Safety Commission, and the California Office of Emergency Services were extremely important. The Nevada Bureau of Mines and Geology was also deeply and effectively involved, as were other state surveys.

Wallace: CDMG began to shift its priorities to do more on earthquakes. Wesley Bruer, who was CDMG Chief at the time of the 1971 San Fernando earthquake, began to emphasize hazards and other programs over minerals, and this track was followed by his successors Jim Slosson and Jim Davis. For a few years in the late 1980's, state politics placed Joe Ziony, who was well steeped in geologic hazards after a career with the USGS, in effective control over the CDMG. After Jim resumed his responsibilities at CDMG, Joe retired.

Scott: Yes, that business of the attempted demotion of Jim Davis and his later reinstatement was complicated and traumatic. I respect your preference to say nothing more about it.

Wallace: California Office of Emergency Services Director Dick Andrews, an academic and historian by background, but an extremely effective leader, had grown in stature in the earthquake arena as head of the Southern California Earthquake Preparedness Project, and later as Executive Director of the Seismic Safety Commission. Subsequently, he moved to the OES to the post he holds now.

Scott: As you know, over the years I have been especially interested in the Seismic Safety Commission, having been a member for 18 years. Do you have any observations about its history or role?

Wallace: I did discuss the idea of such a Commission with Karl Steinbrugge when he was first thinking about it, but my own information is very incomplete. Your paper with Bob Olson is one of the best documents about the SSC. The reference should be given here for a more complete and accurate account than I could possibly give. (Scott, Stanley and Olson, Robert, (eds.), California's Earthquake Safety Policy: A Twentieth Anniversary Retrospective, 1969-1989, Earthquake Engineering Research Center, University of California, Berkeley, December 1993.)

BCDC's Engineering Criteria Review Board

Scott: You had some very direct experience with one state-regional agency that has always interested me, ever since the days when I was a consultant to the original San Francisco Bay Conservation and Development Commission (BCDC). Would you discuss that?

Wallace: I can describe the BCDC organization on which I served--the Engineering Criteria Review Board (ECRB). I had the honor of chairing the board for about 10 years, and continued well after retirement from the USGS. Several other present and former members are included in your Oral History project.

Wallace: The main focus of the ECRB was to look at seismic safety of structures built on the margins of the Bay. I do believe that it has had a major influence in interjecting state-of-the-art thoughts into the general engineering design and construction habits of the area.

Unlike building codes, which tend to require minimum values of earthquake resistance in construction, the ECRB follows a "question, answer, review and approval" technique. Some of us have talked about describing this technique formally, and perhaps giving it a formal name, because it is a very powerful approach, flexible and useful for many situations. It encompasses engineering lore long before that lore has matured to a point that it can or should be codified.

Scott: I have always considered the board to be one of BCDC's really valuable innovations. Would you say a little more about some things the board did?

Wallace: I'll report just one action, related to BCDC's legal responsibility to approve building on or around San Francisco Bay, as well as major changes in such structures. The 1989 Loma Prieta earthquake caused a section of the Bay Bridge to collapse, severing the main direct link between San Francisco and the East Bay, and creating huge transportation problems. Timing was such, of course, that Caltrans (the California Department of Transportation) had to do the repair work promptly, and could scarcely wait for BCDC's formal approval of work on the structure, which came under the BCDC regulatory power just noted.

The ECRB soon got involved, and the board visited the repair job while it was in progress. We were all impressed by the promptness of the Caltrans work. Both at the site, as well as later, however, we voiced great concern when we were told that the supporting flange from which the deck slab had slipped and collapsed was to be widened by only about three inches. We thought that was too little for the long haul.

There was no chance that the all-volunteer membership of ECRB could follow the day-to-day investigation and repair planning. Fortunately Caltrans had a contract for Prof. A. Astaneh-asl of the University of California, Berkeley to conduct an investigation and to do what research and planning was necessary to retrofit the eastern part of the Bay Bridge. While the ECRB was pleased that Caltrans would carry out an in-depth study, the board unanimously felt obligated to issue a warning to members of the full BCDC Commission.

Scott: That sounds like a serious matter. What did the ECRB do, and what was the response from BCDC?

Wallace: We certainly felt it was serious, so on November 15, 1989 the board issued a strongly worded statement.

Scott: Would you quote the board's warning?

Wallace: Yes, here it is:

> The design criteria of the present bridge, including those used in retrofitting in the 1970's, are inadequate for the long term. These criteria cannot be considered appropriate for earthquakes of equal or larger magnitude than that of October 17, 1989 that are likely to be generated on nearer sections of both the San Andreas and Hayward faults, such as those of 1906 and 1886. During such earthquakes failures involving more serious threats to life safety and impact on the continuing function of the bridge would be likely.

Scott: That was forthright. What did BCDC do, and was there any other response?

Wallace: The Commission asked for a report on our findings, and I as chairman made an oral presentation to the full BCDC. Our report made headlines in the local press for one day, and since then has been totally forgotten. I shudder at the thought that some day I might have cause to say, "We told you so in 1989." I am afraid of what will happen when the Bay Bridge is hit by another quake, perhaps closer than Loma Prieta and with stronger shaking.

Scott: From what I have been seeing in the newspapers, it now appears that in due course there will be a major retrofitting of the Bay Bridge, if they can agree on how to finance it.

Universities and Private Consultants

Wallace: The universities and private consultants are other very important participants about which I have said little or nothing. Both are essential to effective earthquake-hazard reduction, and they engage in a spectrum of activities ranging from research to application.

In engineering I should note specifically the University of California, Berkeley; Caltech, Pasadena; the University of Illinois, Champaign; and MIT, Cambridge. But many others also made significant contributions. The University of New York at Buffalo emerged as a force when the National Science Foundation chose it as the site of the National Center for Earthquake

Engineering Research (NCEER). As I understand it, New York state came through with matching money and California did not, thus tilting the support decision of NSF to Buffalo.

Scott: Admittedly, the interested California groups were slow getting their act together, but also there seemed to be almost a stacked-deck in NSF that was pro-New York. The whole thing was very painful for Californians, especially academic researchers who lost significant sources of support.

Wallace: Regarding private-sector leadership, structural engineers Henry Degenkolb and John Blume, as well as the firms associated with their names, provided remarkable leadership in earthquake engineering practice, and many other fine firms have also become eminent in the field, especially in the past decade or so.

Scott: Yes, Henry and John were remarkable engineers, and of course many other practicing engineers played very important parts over the years. Working especially through the Structural Engineers Association of California (SEAOC), the Applied Technology Council (ATC) and similar groups, the structural engineers were instrumental in developing and promoting codes for seismic design.

Wallace: Yes, they did. Woodward-Clyde Consultants was a power-house among earth science consultants for several years, under the guidance of Lloyd Cluff. After he left, however, others in the organization decided to de-emphasize earth science, so that capability took a nose-dive. Several of the members of the organization then banded together to create Geomatrix Consultants. The large geotechnical engineering firm Dames and Moore also played an important role in the use of earth science expertise.

I mentioned the Earthquake Engineering Research Institute (EERI) earlier, and its evolution from a small elite group of earthquake experts with a closed membership into a large and influential national organization. The analogous growth of the Seismological Society of America and the explosion of seismological publications reflect the overall concern and especially the increased funding for earthquake studies under NEHRP. There has been a similar increase in the number of earthquake-related geological papers given at the regional and national meetings of the Geological Society of America.

Unquestionably, the membership growth of both EERI and SSA resulted directly from money being available to employ earth scientists and earthquake engineers. Research money from NEHRP, along with the public demand for safer new and retrofitted structures, have created the dynamic and vital earthquake-hazard reduction enterprise we see thriving in the 1990s. Thirty years ago one could not have envisioned the large attendance that gathered at the joint Pasadena meeting of the SSA and the EERI in Pasadena.

As for geology, geophysics, and seismology, the Universities have been prime movers. Their role in the evolution of the earthquake program deserves a full account, but I am not going to attempt to do so here.

Other Earthquake-Related Activities: Selected Bibliography

Scott: Are there other developments of this nature that you would like to touch on briefly?

Wallace: So many things were going on in parallel that I have been very selective in trying to simplify the history of the flow of actions. I have more or less phased out my account in the 1980s, although the pot kept boiling, and still is (1995). To give some sense of the scope, diversity and continuity of action, and the many institutions that have contributed, I shall simply list a few publications not mentioned previously, with a note after each. The titles of the reports speak for themselves in suggesting their role in building ideas about earthquake-hazard reduction.

1. Olson, Robert A., and Wallace, Mildred M., Geologic Hazards and Public Policy: Conference Proceedings, May 27-28, 1969, Office of Emergency Preparedness Region Seven, Santa Rosa, California, 1969.

> This conference helped alert FEMA's predecessor, the federal Office of Emergency Preparedness (OEP), to the importance of earthquake concerns.

2. Joint Committee on Seismic Safety, Meeting the Earthquake Challenge: Final Report to the Legislature, Submitted Pursuant to Senate Concurrent Resolution 128 (1969), January 1974.

> This report combines the findings of five advisory groups. Karl V. Steinbrugge was Chairman of the combined advisory groups, and others chaired individual groups as follows: 1) Advisory Group on Engineering Considerations and Earthquake Sciences; Gordon B. Oakeshott, Chairman. 2) Advisory Group on Disaster Preparedness; Robert A. Olson, Chairman. 3) Advisory Group on Postearthquake Recovery and Redevelopment; Will H. Perry, Jr., Chairman. 3) Advisory Group on Land Use Planning; George G. Mader, Chairman. 4) Advisory Group on Governmental Organization and Performance; Marcella Jacobson,

Chairman. The breadth of state concern and action in California was one of the most important driving forces in the whole program.

3. National Land Agency (Japan), Large-scale Earthquake Countermeasures Act--Law No.73 (1978.6.7), National (Japan) Land Policy Series, no.6-2, 1978.

This document is included to note that the concern for earthquake hazard reduction was growing world wide. It parallels many of the concerns and actions taken in the National Earthquake Hazard Reduction Act in the United States. The US kept in close touch with Japan in developing science, engineering and governmental response to earthquake hazards.

4. McKelvey, Vincent E., Earthquake Prediction--Opportunity to Avert Disaster, Conference on Earthquake Warning and Response, Held in San Francisco, California, on November 7, 1975, U.S. Geological Survey Circular 729, 1975,

This document came a decade after the 1965 Press report on earthquake prediction. It was an important step in that it emphasized prediction through contributions from the City of San Francisco, the National Science Foundation, the State of California, the U.S. Department of the Interior and the University of California at Los Angeles, Department of Sociology. Simply getting papers presented by a variety of people from organizations having different agendas than the USGS tended to give broader recognition and emphasis to prediction.

5. Steinbrugge, Karl V., Earthquake Hazard in the San Francisco Bay Area: A Continuing Problem in Public Policy, Institute of Governmental Studies, University of California, Berkeley, 1968. (Monograph also available in IGS Franklin K. Lane compilation, The San Francisco Bay Area: Its Problems and Future, vol. 3, 1972.)

The Steinbrugge monograph was one of many Franklin K. Lane monographs published by the Institute of Governmental Studies to inform the Bay Area public and leadership on major regional problems. It was followed early in 1969 by an article by Stanley Scott in the UC Institute's Public Affairs Report, suggesting a regional approach to seismic hazards through formation of a nine-county earthquake commission resembling the Bay Conservation and Development Commission. The Steinbrugge monograph and the Scott article caught the attention of Senator Alfred E. Alquist and this led to activation of the Joint Legislative Committee on Seismic Safety.

6. Scott, Stanley, Policies for Seismic Safety: Elements of a State Governmental Program, Institute of Governmental Studies, University of California, Berkeley, 1979.

Universities were active in helping government to respond to the earthquake problems, not only in science and engineering but also on administrative issues. This monograph generalized on California's experience and suggested approaches other states might use.

Wallace: 7. Federal Emergency Management Agency, An Assessment of the Consequences and Preparation for a Catastrophic California Earthquake: Findings and Actions Taken, Prepared by Federal Emergency Agency from Analyses Carried out by the National Security Council ad hoc Committee on Assessment of Consequences and Preparations for a Major California Earthquake, 1980.

In this document it was estimated that the cost of certain earthquakes could be as high as $69 billion, and deaths as many as 23,000. These figures added fuel to the importance of earthquake-hazard reduction, and were used for years in justifying investments in program(s), both in the governmental and private sector.

8. Mileti, Dennis S., Hutton, Janice R., and Sorensen, John H., Earthquake Prediction Response and Options for Public Policy, Institute of Behavioral Science, University of Colorado, Environment and Man Monograph, no.34, 1981.

9. The Institute of Behavioral Sciences, University of Colorado has examined a broad range of natural hazards. Its bimonthly publication Natural Hazards Observer has helped demonstrate the commonality among different natural hazards.

10. National Research Council, Panel on Data Problems in Seismology, Gilbert, Freeman (ch.), Effective Use of Earthquake Data, National Academy Press, 1983.

This is only one of a continuing series of panel reports of the National Research Council, National Academy of Sciences, which greatly influenced the progress of the earthquake hazard reduction program.

Scott: Reviewing this list of publications gives you a feel for the breadth of earthquake-hazard reduction--geographically, institutionally, and in terms of disciplines. It is a highly interdisciplinary field.

Wallace: I hope those points come through clearly in this history. I think another cardinal lesson is the power of individuals to make things happen, through their personalities, dedication, and, of course, just plain hard work. In my opinion, the three people who influenced the earthquake hazard reduction program most profoundly were Frank Press, Karl Steinbrugge, and George Housner.

IX STORIES AND ADVENTURES (I): MEMORABLE TRIPS TO TURKEY AND THE MIDDLE EAST

Wallace: I would like to recount a few tales of geologic travels, some having to do with earthquakes, and title this section, "Stories and Adventures..." It could be subtitled, "Join the USGS and See the World." Of course, anybody who travels accumulates stories, but visiting earthquakes and earthquake country around the world takes the traveller off the regular tourist routes and into exciting and unusual situations. While sometimes a bit risky, these expeditions offer wonderful opportunities for exploration off the beaten path.

Scott: Earthquake scientists and engineers do get involved in postearthquake investigations in many countries, and of course the USGS also works in a wide range of geologic fields. Your accounts of your travels in the line of duty would be a valuable addition to this oral history.

Wallace: I won't describe every trip, or any one trip thoroughly, but instead will relate some experiences that were adventurous, gave me insight into history, or highlighted vignettes of humanity.

Assignment in Turkey (1964)

Wallace: I have already described our explorations in Alaska Territory, which were at the very beginning of my career. I did not, however, get an assignment that took me off the North American continent until I was 48 years old. That seems so unlike more recent decades, when it seems that even lots of teenagers have travelled extensively around the world. But for me my first assignment to Turkey in 1964 was an exceptional event, especially as I was accompanied by my wife, Trudy, and eleven-year old son, Alan. The assignment lasted two months.

Wallace: Through the State Department's Agency for International Development (AID), the USGS was to provide advice to the Turkish government and a review of its geological mapping program. The Turkish equivalent of the USGS, their Minerals Research and Exploration Institute (or Maden Tetkik ve Arama in Turkish) goes by the acronym MTA. I became attached to that organization.

Scott: I suppose the association with MTA gave you formal support for activities in Turkey?

Wallace: Yes, we had support of both the U.S. Embassy and the Turkish government. Many doors were opened, and the assignment required travel to all corners of Turkey. At the time, Turkey had not yet been fully discovered as a tourist attraction. My wife was cautioned not to wear sleeveless dresses, for example, but within the year, things were changing. We did, however, find villages that were almost untouched by the modern world. We would be driving along, see a big hill out in the distance, and be told that it was an old city mound that had never yet been touched by archaeologists. One of our first mini adventures was on a picnic to Gordium with Clarence Wendell, the Minerals Attache at the U.S. Embassy. Gordium is famous for the "Gordian Knot."

Scott: The knot that Alexander the Great cut with his sword when he came by.

Wallace: Clarence's wife Nancy and their two boys were with us. We rummaged around the ancient city mound, which had been extensively explored by archaeologists, but saw shards of old broken pottery still littering the ground. In the same general area we picnicked near a big bas-relief from Hittite times, carved in granite. Barely discernible trails lead up to this seemingly priceless historic treasure, and we found our way with the help of a small boy who lived nearby. There were no visible signs of tourism, and no facilities for it.

While we were picnicking, the three boys went down into a gulch to a dry river bed. Soon they came back greatly excited; "Come and see what we found." Right along the river bed were some Greek figures, a foot and a half high, carved in limestone. The base of the figures were partially covered by sands of the river bed. The carved figures were in a wonderful state of preservation, apparently untouched and unknown by tourists. In 1964 such antiquities did not seem to be fully appreciated in Turkey, although agencies had been formed that were busily trying to promote tourism. To me it was a very exciting period of transition in Turkey.

On one excursion with the Minerals Attache, we drove along the North Anatolia fault, and, even in only a quick view from the car, I saw features very similar to the San Andreas fault--showing that lateral fault movement had occurred. Such features along that fault had never been documented in the technical literature. Here were offset streams, sag ponds and the topography that is so characteristic of the San Andreas fault. What an exciting discovery!

That leads to another story about maps of the fault. While I was with the MTA we were able to check out very nice topographic maps from the MTA library, very similar to USGS topographic maps. Fortunately, I traced simple maps from the topographic maps, and in the field I photographed and sketched some of the streams that had been offset along the North Anatolia fault. I thought that when I got back to Washington I could get the library copies of these maps, so that I could prepare a technical paper about my discovery. But when I got back to Washington I could not find the Turkish topographic maps in the USGS library.

Scott: I would have thought that the USGS central library would have such maps. Why didn't they?

Wallace: Yes, I presumed that USGS must have those maps available somewhere. Someone suggested, "Why don't you talk to the USGS liaison with the military mapping program?" When I did, he said, "How do you know that those maps exist?" "I've used them in Turkey," I said. "They are classified--you aren't even supposed to know that they exist, and I can't get them for you here in Washington." I observed how ridiculous that was, and felt thankful I had traced off the sketch maps while in Turkey.

It turned out that the U.S. military had assisted in the production of the maps, which looked just like USGS topographic maps, but the military participation had led to their being classified. Prior to those maps, many maps of Turkey had had longitude and latitude slightly rotated, and other strange aberrations. This was done deliberately to make use by enemies difficult. Such was the logic of the military! I made sketches of the fault topography drawn from the topographic maps that I had been able to get in Turkey in 1964. Later I used these sketches in a paper on the 1966 earthquake near Varto, in eastern Turkey, which I will take up later.

My memories of Turkey are full of the warmth, honesty and helpfulness of people in the villages. While there in 1964 I once got stranded in eastern Turkey when the scheduled plane had to land at Erzurum instead of Erzincan, where the MTA people were to meet me. The only thing I could think of was to stay over for three days and go back to Ankara on the next scheduled return flight. So I had time on my hands.

The little downtown office of the airline closed for lunch. I wandered out to sit on a stone wall to read and sketch, and a group of children gathered around. One boy tried out a few words of English, and was glad to find that it was my language. Often in that region I had been taken for German. I drew a map in my sketchbook to show the children where Turkey was relative to the United States, and were I lived. I guess that one boy, at least, had some geography--anyway he seemed to get the idea. After a while he insisted that I come home with him, to his father's and uncle's shoe shop, located down the road. We visited with the father and uncle as they worked on shoes, and they served me tea. We had the most pleasant time. What wonderful hospitable people they were!

Scott: Did they speak any English?

Wallace: No, the father and uncle spoke no English, but they were delighted that the boy could serve as interpreter to some extent. But I always have a phrase book along and that helped. In those situations, you manage one way or another. The people I was trying to talk to loved it, and so did I. Finding that you can communicate with people in even those crude ways gives you such a feeling of closeness with them.

I asked the little boy if there were a hotel around. He took me up into town where there was a small hotel, but no rooms were available, and there was no other hotel nearby. What was I going to do? After much discussion and arguing, the manager said I could stay in so-and-so's room. The room was occupied, but I could stay there for the night, which I did.

Next morning the Turkish geologists used some skillful detective work and found me. They had driven all night, arrived in town, and started asking about the "lost American." By then I guess I was pretty conspicuous in the local community. So the planned field trip proceeded on schedule. The incident had been no problem for me. In such circumstances you just do what you can do. What a nice memory of that sojourn remains.

Visiting a Turkish Earthquake (1966)

Scott: I believe you went to Turkey several times after your first assignment there in 1964.

Wallace: Yes, I also was there in 1966, 1967, and 1968 and 1972. When the 1966 earthquake occurred near Varto, Turkey, I made a plea to go out to eastern Turkey to study it. I guessed that it was along the North Anatolia fault which had become an exciting research target for me after my travels along it in 1964.

The USGS rounded up some money, and the U.S. Agency for

International Development (AID) also helped. In terms of earthquake interest, I found wonderful things at Varto. That was a "real, live" earthquake, with aftershocks still occurring when I got there.

The Earthquake Near Varto

Wallace: The earthquake near Varto occurred in August 1966, only two months after our Parkfield earthquake in California. When I started work in the field in Turkey, I was startled to find the terrain and vegetation almost identical to that around Parkfield. I immediately felt at home.

In 1966 after reaching Ankara and checking in with the AID people and with the US Embassy, I flew to Urzurum, where the MTA people met me. MTA had a field camp in the vicinity of the earthquake's epicenter. From that camp they had been carrying out geologic mapping. I had come with a sleeping bag and was prepared to be self-sufficient. I had been in the region before and knew that it was remote and had few facilities. But the MTA had some tents and even one or two little movable shacks. So I was very comfortable, and "rode" the first night out as aftershocks rocked us.

Scott: That got you into the earthquake operation rather promptly.

Wallace: U.S. AID provided me a driver-interpreter and MTA let us use one of their jeeps. The next day we went to look at the damage, seeing collapsed houses made of cobbles and adobe, and with heavy roofs. Most of the local houses were built of big cobblestones a foot to three feet in diameter, chinked with smaller rocks and mud.

The cobblestones were piled on top of each other, big cottonwood logs were laid across the tops of the cobblestone walls, and dirt piled on top of that. It made for a very warm house; fires could be burned on the earthen floors, and the earthen roofs gave excellent insulation. But the roof was a heavy and very unstable structure, extremely dangerous, and vulnerable to any shaking or lateral movement characteristic of earthquakes.

Scott: That is typical construction in many of the less-developed countries. It makes a lot of sense for just about every purpose <u>except</u> earthquake resistance.

Wallace: That is right. The magnitude 7.1 or 7.2 earthquake caused all those houses to collapse, burying most of the people under the heavy roof material. In the village of Varto something like 80% of the people, more than 2,500 people, died.

Sometime during the afternoon we encountered a military convoy comming toward us, and they flagged us to a stop. What had we done, what was wrong? They immediately came toward us with a box containing, of all things, soda pop. "We saw you at the spring this morning filling your canteens and thought that you might like some gazoz (pop)." Of course, we would, with many thanks.

Vulnerability and the Case for Prediction

Scott: What can feasibly be done in such primative areas to reduce such terrible loss of life?

Wallace: Just as in other foreign earthquakes, we keep coming up against the total inability of people in areas like that to build earthquake-resistant structures; no money and very few or no local building materials suitable for earthquake-resistant construction. It is the same in Pakistan, although in Quetta, they have devised some very simple things like using sheet metal more, to improve safety. While these measures are feasible, they still are expensive compared with the cost of using cobbles from a local stream for construction. Certainly something else is needed than the kinds of earthquake-resistant design and construction we use in the United States.

This problem, the difficulty of building earthquake-resistant buildings, underlies one of my arguments for earthquake prediction. If the people of Varto had only known that a quake was coming enough ahead of time to get out of those houses! In Haicheng, China, a successful prediction of an earthquake of magnitude 7.5 was made in 1975. The prediction allowed the people to leave apartment buildings and houses, most of which (90 percent) were destroyed in the earthquake. Without the warning, it is estimated that more than 100,000 people would have perished. The Chinese also tell of other successful predictions.

In underdeveloped countries the idea of earthquake-resistant construction has barely begun to be considered. Even in the most highly-developed countries, an enormous inventory of existing structures does not conform to the latest ideas about earthquake-resistant construction. The constant introduction of new materials and new architectural styles, along with other changes, will assuredly lead to some inadequate structural behavior and failures during earthquakes. Given a continuing stock of buildings of inadequate earthquake resistance, reliable predictions would be invaluable; very likely the only defense against earthquakes. But reliable predictions are still far from being realized. Anyway, this is enough of my missionary efforts on behalf of prediction.

A Reconnaissance Flight and Photos

Scott: What else did you find at Varto?

Wallace: Our jeep tour through the Varto area took us by a military hospital facility housed in tents and set in a broad meadow. We saw a little Turkish spotting plane parked in the meadow by the camp. It was a small bi-plane with open seats, one for the pilot forward and one passenger behind. It was like some of the old fighters used in World War I.

I said to my Turkish driver, "Let's see if there is any chance we can borrow the plane." One must always improvise when looking at earthquakes. You see opportunities and you try to capitalize on them. So we asked about the plane and they said, "Why not." Anyway the driver sold the idea to the Turkish general, and I was able to go on a reconnaissance flight.

I got in the back seat of the plane, and had to tap the pilot on the right or left shoulder to indicate which direction I wanted to turn. From the general setting of the North Anatolia fault and the topography, I knew of some areas I wanted to fly across to look for surface faulting.

We took off, and lo and behold, there were big cracks along the fault--in fact several parallel sets of cracks, right where they should be. I got some excellent photos of the faulting, and also documented the role of liquefaction in the damage or destruction of buildings. Reinforced concrete buildings were destroyed by differential settlement due to liquefaction.

Scott: It seems that field examination after earthquakes has been a very productive activity. As you know, the Earthquake Engineering Research Institute has emphasized a program of "Learning from Earthquakes."

Wallace: There is no question that each earthquake produces some new lessons, and it has often been said that each big earthquake produces surprises. That tells us how little we really understand about earthquakes. But we have made great progress over the past few decades, even since the National Earthquake Hazard Reduction Act was passed.

In succeeding years the USGS used some of my Varto photos to sell Congress on the need for earthquake research budgets. The idea of localization of both faulting and liquefaction, which I photographed at Varto, for a time became a basis for hazard "microzoning." The idea is to locate structures off the most hazardous pieces of land. The concept of using land prudently as a defense against earthquakes has grown in importance. But it took many years and further mapping of faults before even California instituted special studies of such vulnerable sites. Much remains to be learned and done in mapping such hazards.

Scott: Tell us more about your experiences during your Varto earthquake investigation.

A Forced Quick Trip to Ankara

Wallace: The AID people in Ankara, who were helping me logistically, asked me to fly back to Ankara in two days to give them a report. I said I could not possibly do an investigation and get back that soon. For one thing, transportation was just too difficult. But they insisted, and said they would send a plane for me. I don't think they had any idea how remote the area was, about 800 or 900 km east of Ankara.

I reluctantly agreed to go back to Ankara, and on the appointed day had the jeep driver deliver me to a small airfield where I was to be picked up. But no plane. Also there was no telephone, and with my lack of the Turkish language, it would have been almost impossible for me to use one anyway. What to do? Most of the afternoon, we hung around the little shack that served as a terminal. A plane finally showed up, but it was not for me. It belonged to the Dornier-Werke of Munich, West Germany, and was on charter by the Swiss Red Cross. After I discussed my problem with the pilots, they said they could fly me back to Ankara, if I could wait an hour or so. So once again hitchhiking by plane got me out of a box.

After I got back to Ankara the AID people said, "Oh, we will develop the film for you here." I first told them "No," that I would wait until I got home, where I trusted the processes used to develop the kind of film I was using. "You've got to let us develop them." "But those photos are irreplaceable, and I don't know whether your people know how to handle this film." I finally let them go ahead, and they did a very nice job. They used them for their own propaganda in Ankara and Washington, D.C.

At that point I insisted that I be returned to the Varto earthquake site very soon. They ended up getting a big four-engine bomber to fly me back out. I was the only passenger on this huge plane. We landed at what must have been a military airport, and I climbed out of the plane. Here were some high-level, Turkish military personnel--judging from their decorations--waiting for me. I shook all their hands, but then climbed in this little geologic jeep of the MTA and off we went. The military personnel had no idea who they had greeted, and are probably still wondering who that person was who rated a four-engine bomber for transportation. After that, everything worked out fine.

First to Locate the Fault

Wallace: A day or two later, Dr. Ihsan Ketin arrived at Varto. He was Turkey's leading structural geologist, a man who had prepared fault maps of Turkey. By now, it was a full two weeks after the earthquake, and he was very miffed to find that I had gotten there ahead of him. He arrived just after I had been out to the site, located the fault, and gotten good photographic evidence. The MTA group that had been there for ten days or two weeks had not yet found the fault. This upset them very much. They knew the geology, but had not found any faulting related to the earthquake. Here I had come in, a foreign observer, and found the faulting right away. Tension reigned!

Scott: I presume that was because you knew what to look for and approximately where to look?

Wallace: I guess so. Having found two zones of faulting from the air, I was eager to go out to examine them on the ground the next day. I proposed to Dr. Ketin: "Let's join forces tomorrow and go look at the faulting." But Dr Ketin said, "No, we have other things to do." So I went out with my jeep driver and started to look at the faulting. As we hiked up over one hill, who did we find studying the fault but Dr. Ketin and three or four other Turkish geologists. They were embarrassed, of course, but in the end we all made up. In the published paper I made a point of emphasizing their help. (Wallace, R.E., "Earthquake of August 19, 1966, Varto Area, Eastern Turkey," Bulletin of the Seismological Society of America, v.58, n.1, 1968, pp. 11-45.)

Scott; I am intrigued by your finding the faulting before the Turkish geologists did, although you came in from the United States about two weeks after the earthquake. Then you found it quickly. Please say more about why you were better able to find the faulting. Of course, you had a lot of experience with earthquake chasing, and also had worked on the San Andreas fault.

Wallace: There were two factors. I did have a good idea of the kind of topography active faults might produce, especially faults of the North Anatolia type. Moreover, I had examined aeronautical flight charts of the area before I left home, and already had some detailed ideas. I glued the maps onto linen for easy field use, took them with me and used them on the flight. Although the maps were not very detailed, lineaments showed clearly. Those were the zones I wanted to check, and did so from the plane.

The Importance of Air Reconnaissance

Scott: So on the map you had spotted features that tipped you off as to the most likely place to look?

Wallace: Yes. The major factor in finding the faults quickly was my brazen willingness to try to "borrow" the Turkish military plane. I feel sure that the Turkish geologists would have hesitated to confront their military and ask for such a favor. For us, it has become standard practice to try to start with an overview by air. The first thing you do is get in a helicopter or light plane and fly over the area and see what things look like from the air. I have bummed a ride on airplanes many a time to do such reconnaissance.

Planning the CENTO Conference: Turkey, Iran and Pakistan (1967)

Scott: I believe the 1966 Turkey earthquake and your investigations of it helped bring about a conference on methods of coping with earthquake hazards in the Middle East.

Wallace: Yes, the CENTO group in Ankara asked me to organize such a conference. I made trips in 1967 and 1968 with support from CENTO (Central Treaty Organization), to develop a program on earthquake hazard mitigation in Turkey, Iran and Pakistan. Great Britain, Turkey, Iran and Pakistan were the members of CENTO, but the U.S. was nominally included, I presume because the U.S. ended up paying many bills.

Formulating the Conference Plan

Scott: Discuss what you did to get the conference plan in shape.

Wallace: By no means was the conference planning all my doing. In addition to exploring the scientific side, first priority was given to seeking simple building techniques suitable for regions where only primitive construction methods were traditionally used. In so many of the world's earthquake regions, homes are built of cobbles and loosely piled rock with heavy earthen roofs. Or they are built of unreinforced adobe brick. Such construction tends to collapse completely, even in only slight earthquake shaking. There was also concern about some of the more advanced construction methods used for highrises--in Ankara, for example. With these priorities in mind, the U.S. Department of Housing and Urban Development (HUD) became involved, and Robert C. Reichel of the HUD Codes Division joined me in the conference planning.

Scott: So you and Reichel visited the CENTO countries?

Wallace: Yes, in 1967 we set off to visit the institutions in Great Britain, Turkey, Iran and Pakistan that might usefully participate in a conference. Ankara had been chosen as the site the for conference, because CENTO personnel there could help with the logistics. The date was to be in 1968. Joint funding by CENTO and the U.S. was promised.

I won't go into the details, but will recount a few incidents that stick in my memory. The Geological Survey and Geophysical Institute of Iran were very receptive to the idea of the proposed conference. I had a good friend, Dave Andrews, at the Survey who had for years worked overseas on various USGS projects.

As fortune would have it, we were to leave Tehran on the evening of the coronation of the Shah of Iran. I had contracted a severe cold and earache in the meantime and wanted desperately to stay in our hotel one more day. That was impossible because every room in Tehran was taken, including ours. So we headed for the airport, where we sat most of the night waiting for a delayed flight to Karachi, Pakistan.

When I awoke the next morning in Karachi, I thought that I had had a stroke. My mouth, lips and left eye were numb. Nevertheless we went on to Lahore on schedule, and in Lahore I promptly went to see a doctor with the U.S. Agency for International Development. He diagnosed my problem as Bell's Palsy, ordered a few days in bed, and use of tape to close the left side of my lip and my left eyelid, to keep them from drying out. Even now, although I have largely recovered, I still suffer somewhat from the palsey.

The rest of the several-week planning trip was miserable, but successful. I regret to say, however, that my partner from HUD was of little use in planning the conference--he had almost no experience with earthquakes.

Scott: That must have been a very trying time. I can certainly understand the kind of bureaucratic mismatch that occurred--it is not unusual. Your HUD colleague represented his agency, but lacked the background in earthquake engineering or seismic design that would have made him a really useful team member. Nevertheless you apparently got a lot done. Say a little about that.

Wallace: There were many things, but one stands out. In Quetta in western Pakistan, I was impressed by some very simple construction of buildings put up after the great earthquake there. The construction technique involved the use of very light-weight metal roofs and single-story walls. The lower half of the walls was good quality masonry, and the upper half largely of wood frame construction. That kind of construction would certainly be more resistant to earthquake shaking than the customary adobe construction.

Scott: Did the conference proceed as planned and on schedule?

Wallace: Yes, the 1968 conference was a great success, in no small measure thanks to Karl Steinbrugge who agreed to give the keynote address, and another key player, Nick Ambraseys, a British leader in earthquake engineering and soils mechanics. We produced what I believe was a useful volume published by CENTO. As you know, such things take a lot of work. CENTO was pleased with the result, and convened another earthquake conference in 1972, which I believe my good friend Joe Ziony of the USGS spearheaded.

The Post-Conference Field Trip

Wallace: I won't say more about the 1968 conference itself, but will use the field trip we made after the conference as a point of departure to say something about the people of Turkey, and people in general. To my mind it is the people and their outlook that really count. The post-conference field trip was very timely, because only a year before a large earthquake hit the western end of the North Anatolia fault near Adapazari. Liquefaction led to much damage, and I made a special effort to record some of the effects near Sapanca Golu (Lake).

After making some measurements and photographing a liquefaction site on the shores of Sapanca Golu, I left my tape-measure propped up against a dock at the site. It was many kilometers from where we were staying, so when I missed the tape-measure, I thought, "Well, that's gone." But a day later my tape-measure appeared on the table in my hotel room. What honesty! How did they find its owner and figure out a way to get the tape-measure to an absent-minded American who left it behind?

I am reminded of one time in 1972, when I was also in Turkey. I was at the North Anatolia fault with Professor V.V. Beloussov from the Institute of Physics of the Earth, USSR, who was perhaps the Soviet Union's most famous structural geologist. We were there evaluating the Balkan Seismo-Tectonic Map Project, another fabulous assignment about which I will say little. While travelling, Beloussov and I sat in the back of a car, both of us wearing brown Eisenhower-type jackets. He was taller and bigger, but we both had sort of blond, graying hair.

Remember this was at the height of the Cold War with the USSR. Our Turkish guide and host turned around and said to us, "You are American and you are Russian, yet you look so much alike. You are supposed to be at war with one another, and yet there you sit in peace." All people are so much the same, even if they don't look as alike as Beloussov and I did to a Turkish geologist.

I could write volumes about Beloussov, and his apparent opposition to the idea of plate tectonics and strike-slip faults. I say "apparent" because while in the field along the North Anatolia fault in Turkey, he readily accepted the obvious field relations that clearly demonstrated strike slip on that great fault.

Also, Dr. Beloussov actually accepted many facets of the plate tectonics concept when we discussed the problems personally. Nevertheless, he had very sound arguments to mount against some parts of the plate tectonics theory. The concept had gained "band-wagon" proportions, and needed some skeptical analyses; Beloussov provided these.

In 1973, however, at the International Geologic Congress in Montreal, Beloussov presented a no-holds-barred denunciation of plate tectonics. After long decades of preaching vertical tectonic movements as the dominant structural style, he had to save face in front of such an international audience.

Muggings and Extortion: A Contrast with the Villages

Wallace: The Beloussov story also includes a stark contrast between what happened in cities, and the kindness I was shown in the villages. In the city of Istanbul, I took Professor Beloussov to Hagia Sophia and the Blue Mosque, two famous mosques. I had visited both before, so I said, "I think I'll just walk back to our hotel." An hour or so later he came in very distraught. Just after I had left, a car had driven up along the sidewalk and an older lady and a younger lady had gotten out. The older one tripped and fell right in front of Beloussov. He helped her up, and the two ladies discussed things and then got back in the car and drove away. Suddenly, he realized that his billfold with all his papers was missing. He had been mugged.

Scott: He was a victim of a special kind of pick-pocket artist.

Wallace: Yes, they were pick-pockets. On another occasion five years before that, in 1967, I had been in Istanbul with Bob Reichel, from the Department of Housing and Urban Development. We were looking into organizing the first CENTO (Central Treaty Organization) conference on earthquake hazard mitigation to be held in Ankara. We went to Turkey, Pakistan, Iran, and England on that trip.

We were at almost the identical spot near the Hagia Sophia where Beloussov was robbed five years later. I turned back to return to the hotel, and Bob came back to the hotel with as disturbing a story as Beloussov did five years later. Bob Reichel was distraught, saying, "I got my shoes shined over there." There were shoe-shine boys everywhere, with beautiful little shoe-shine kits with polished brass knobs as decorations. Anyway, when the shoeshine was finished Reichel asked, "How much?" The boy said, "Ten dollars, American money." "That is outrageous, it should only be a few cents." The boy said, "My friends don't think so." Bob turned to find four thugs behind him. So he was forced to pay $10.00 for a ten-cent job, and came back to the hotel, rather shaken.

Scott: They practiced a form of extortion, using the threat of force to get exorbitant payment from customers.

Wallace: Yes indeed.

The Coup During the World Conference

Wallace: Another rather unsettling event occurred in Istanbul during the VII World Conference on Earthquake Engineering in 1980. We arose one morning and found notices posted on the elevator doors of the hotel telling us that we were all confined to the hotel (house arrest I guess) but that the hotel would provide us with meals as long as possible. During the night there had been a military coup of the Turkish government.

A small group of Americans in the hotel, most were friends attending the conference, gathered in a corner of the front lounge of the hotel to compare notes and discuss possible actions we might take. We also divided up tasks such as calling the U.S. Consul, looking into transportation so that we could escape to Greece, and possible food sources. We agreed to post news and rumor notes on the wall behind a certain set of chairs.

Breakfast and lunch went well, but shortages of food in the hotel were real. During the afternoon someone noted a few Turkish people carrying long loafs of bread up the alleyway beside the hotel. Several of us decided to take a chance to leave the hotel, even though from the roof of the building we could see soldiers on guard at every corner and many tanks at crucial corners. We sneaked down the alleyway, found the bakery and each purchased several loafs of bread. I intended to store the bread for emergencies, but couldn't resist that wonderful aroma as I carried the warm bread back to the hotel. I'll admit I ate half a loaf that very afternoon.

The "house arrest" didn't last too long, only a day or so, and as soon as we could go out again, the earthquake conference resumed. Unfortunately, or fortunately, my paper was to have been given on the day of the coup, so I did not have to give my oral presentation.

I did rather enjoy the excitement of going out to the Galata Bridge where tanks were standing at ready at both ends. I further entertained myself by painting some sketches of the bridge and skyline on the far side. I had my pocket-sized paint set, and sketch book, but no water. Peddlers were selling soda pop, so that made a handy substitute. In Japan and elsewhere hot tea served nicely.

More Good Recollections of Turkey

Scott: You had a lot of good recollections, I believe, as well as those of muggings and coups. You mentioned the forced stopover that you enjoyed in a little town during your first trip there. And the unexpected return of the lost tape-measure on a later trip. Would you give some of your other impressions of Turkey along that line?

Wallace: Yes, I want to end this section with a word or two more about the warmth and friendliness of the Turkish people. There were tea houses everywhere, and we just loved them. The tea is served from little tea glasses, not cups, and the tea glasses are delivered in hanging-type brass trays. Waiters and delivery boys could literally run with a full tray of glasses and not spill a drop. We would go to a tea house, and often they would not let us pay. "You are our guests," they would say. Incidentally, I picked up some skills in the process--I can pick up a Turkish tea tray with tea in the glasses and swing it in a vertical circle without spilling a drop.

X STORIES AND ADVENTURES (II): MORE MEMORABLE TRIPS

Scott: Talk about some of the World Conferences on Earthquake Engineering. You have already mentioned the 1980 World Conference and the Turkish coup.

Wallace: I shall have to step back a few years and report on the Fourth World Conference on Earthquake Engineering in Santiago Chile in January 1969. George Gates of the USGS also attended the conference. Following the conference George and I decided we had to visit the famous Atacama fault in the Atacama Desert just east of Antofagasta in northern Chile. Someone from the Geological Survey of Chile was to meet us at the airport, and we were pleased when it turned out to be a very attractive lady geologist. She drove us over the mountain range to the east and into the desert.

The Atacama Desert is one of the driest in the world, where years sometimes pass without any rain. We found the fault scarp easily, and hiked and drove along the fault for several kilometers. The scarp was remarkably well preserved, but no one knew when it had been formed. As we talk about this scarp now in 1995, our new techniques of paleoseismology would probably enable us to date the most recent several movements--earthquakes--that occurred along the fault.

Scott: Has anyone done that since, using the more advanced current techniques?

Wallace: Not that I know of personally, but somebody certainly may have, because so many geologists worldwide now keep up on paleoseismology. In any event, after seeing that spectacular fault scarp on the ground, George and I tried to find an airplane so we could get the kind of bird's-eye view you can see only from the air. That was easier said than done, however, because there were no planes for charter in Antofagasta. Our lady geologist escort did some inquiring at the airport and found that a flying club in Tokopilla north of Antofagasta might be helpful. A time was set to meet the plane, but engine trouble delayed that day. Next morning the plane and pilot arrived. We got in the plane to take off, but then the engine would not start. We were delayed until afternoon! Finally we did get off--with some trepidation--and we flew and returned safely. The flight allowed me to take some excellent photographs of the fault scarps--several scarps roughly parallel the east flank of the mountain range, a kilometer or so east of its base.

On that same trip to Chile, I also managed to get to Puerto Montt in southern Chile where, even nine years later, damage from the great 1960 earthquake was still recognizable.

A Break in the Cold War--Visit to the USSR (1969)

Scott: You were involved in some exchanges with the Soviet Union, I believe. When did those exchanges take place?

Wallace: One of the first formal exchanges took place in September 1969 while the cold war was still quite active. But gaps in the cold war began to show, and perhaps our trip was one of those. That came about when Karl Steinbrugge headed a team to examine earthquake engineering practices in the Soviet Union, and I was fortunate enough to be asked to join the team.

The State Department briefed us on "Do's and Don'ts" to observe while in the USSR--things like taking no photographs of train stations or airports, for example. We were advised not to be surprised if our brief cases and luggage were gone through in our absence or at night.

There were great suspicions on both sides. In Moscow, we were assigned a lady administrator to take us around--"Mrs. G," we called her. We were all sure she was with the KGB because she seemed to have entre everywhere. After Moscow, we visited engineering and seismologic stations in Tashkent, Uzbekistan, Dushanbe, Tadzikian, and Tblisi, Georgia. Then we were allowed some leisure at Sochi on the Black Sea.

We began to ask if it would be possible to visit Leningrad, but the idea was immediately dismissed. One evening in our hotel room, we loudly protested to one another about not being permitted to visit Leningrad. Lo and behold, the next day we were told that Leningrad had been added to our itinerary.

Scott: Maybe the walls had ears?

Wallace: That was our conclusion, but it may have been only our paranoia working overtime. We certainly lived with a sense of tension, however, and I will relate just one example. Before leaving the states I knew we would visit the main seismological station in Uzbek. I also knew that my good friend, Richard M. (Pete) Foose, who had participated in early test-ban treaty negotiations in Geneva, had recently returned from Uzbek. Pete Foose was most recently a professor of geology at Amherst College (although he may have been at the Stanford Research Institute at the time). I wrote to Pete to see if he could suggest any contacts. His reply did give me many names of people, but the letter arrived just a few days before my departure from the U.S, so I decided to wait until reaching Tashkent to try to memorize the names.

We arrived in Tashkent late at night, and after getting settled in my hotel room, I got out Pete's letter to study the names. All was well until I reached page 2, where I found a disturbing line I had not read before, to the effect that, "I (Pete) tried to find out how many seismographs were deployed in Uzbek, but I was clearly denied this information. Perhaps you can find out." My God, I thought, here are orders from a U.S. official to spy! I went to bed, but thought about the State Department warning that our things might be searched, and could not sleep. I pictured myself in a Soviet jail.

Scott: How did this work out?

Wallace: I got up about 2:00 a.m., determined to destroy the incriminating page. First, I held the paper by the corner and lit it with a match, just the way spies do in movies. The paper wouldn't burn, as it was damp from being packed with damp clothes and towels. Next, I tore the sheet into small pieces and stood them in an ash tray and was again trying to burn them, when I realized that I was doing this in front of a window, where, no doubt, I was being watched from another window.

I then transferred my spy operation to the bathroom, intending to wash the evidence down the toilet. Remembering how poorly plumbing worked in many places, I thought, "The paper will clog the works, be recovered during repair, and then what"? Again I resorted to burning, although after creating a lot of smoke and charred paper, I realized the smoke was being wafted over the transom to the hall, clearly to set off a fire alarm!

Finally, however, I got all the paper at least charred, the charred paper crumbled and washed down the basin, but I still was left with a mess of black smear and black hands to be cleaned. Thankfully, I drew no attention, and returned to bed to sleep well through what was left of the night.

Scott: That must have been a pretty unsettling experience.

Wallace: Yes, it was. I'm not cut out for espionage! That brings to mind all the visits I later had from CIA and FBI investigators after each of many trips to the Soviet Union. Most of the questions they asked were routine, about where we had travelled and what institutes we had visited. But on one occasion I remember the CIA man asked me to try to find out certain things on my next trip. I told him that in no way would I accept a "spying" mission, even a suggestion of such status. I described my trauma when Pete Foose had inadvertently asked to find out about seismometers in Uzbek. I told him that despite his ID cards, I felt that he might well be from the KGB and was setting me up for arrest on my next trip to the USSR. I told him that no information I had was classified, and that I would tell it to anyone who came in off the street. But I could not tolerate, or risk, going to the Soviet Union again with even the slightest aura of suspicion about my intentions or mission.

Scott: What came after the trip to the Soviet Union with the Steinbrugge team?

Wallace: There were several trips fairly soon afterward, of which the highlights were interesting trips to New Zealand in 1970, and to Yugoslavia, Bulgaria, Greece and Turkey in 1972. The trip to the Balkan countries concerned a UNESCO program titled, I believe, "A Seismotectonic Map Program for the Balkans." Two geologists, one from the Western Block and one from the Eastern Block countries, were selected to give their independent appraisals of the project. I represented the West and V. V. Beloussov represented the East. We were shown the maps and how they were being prepared, and conferred with the specialists who were doing the work. It was a great learning experience for me, and I hope I contributed in small ways.

I have already described some adventures in Turkey with Professor Beloussov while travelling on this detail, but I shall refrain from expanding the Balkan story. I might add, however, that the trip and exposure to the people and politics of the Balkans helps me understand a little of what has been happening in the 1990s. At the time we all wondered what would happen when Tito died, and, indeed, several friends in Belgrade fairly accurately predicted today's chaos.

Nicaragua (1973): With Henry Degenkolb in Somoza's Plane

Wallace: This is just a funny story about our great earthquake engineering friend, Henry Degenkolb. On June 19, 1973, I took a trip to Nicaragua, where the Managua earthquake had occurred on December 23, 1972. I flew down to Guatemala City with George Plafker, where we happened to find Pierre St. Armand and Henry Degenkolb also on their way to the Nicaragua earthquake.

Pierre was a Caltech graduate who worked with the Navy at China Lake. He was noted not only for earthquake work but also for weather modification activities. Pierre said, "Why don't you join us? I have President Somoza's plane over here, and have the use of it to fly on to Managua." So we canceled our flight reservation and joined them, climbing aboard President Anastasio Somoza's plane.

We were in a wood-panelled cabin, with a restroom adjacent. We sat and visited, and were having a fine time, flying in and out of towering thunder clouds. At some point we missed Henry, who seemed to have been gone to the restroom for quite a while. Eventually we got a little worried, and at about the same time started hearing some banging noises coming from the direction of the restroom. One of us called out, "Henry, are you all right?" Henry's voice came through the wall, "I can't get out of this goddamned place. The door won't open. I've been working on it with my knife, trying to get some of the screws out." After a little pause, somebody said, "Why, Henry, that door slides open." He had been trying to open it out like a hinged door. So even eminent structural engineers are fallible, as are we all.

Scott: On the Nicaragua trip, you didn't encounter then-Nicaraguan President Somoza himself, I take it. He evidently was quite interested in the engineering side of the earthquake, having an engineering education himself. That is the impression I got from oral history interviews with Jack Meehan. On a visit to that earthquake, Jack spoke of having a personal session with Somoza, discussing earthquake effects, codes, professional liability and the like.

Wallace: Yes, I was invited to meet with President Somoza, but declined the invitation, but will not try to explain that here.

Delegation to the USSR on Earthquake Prediction (1973)

Wallace: In September 1973 I headed the first U.S. delegation of the Working Group on Earthquake Prediction, sent to learn more about the Soviet efforts in earthquake prediction. This was done under a USSR/USA protocol concerned with protection of the environment. The agreement was chaired by Russell Train, who served as head of the Council on Environmental Quality (CEQ) in the Nixon administration. A similar protocol was concerned with science.

Scott: Was there a reason why earthquake prediction was handled separately?

Wallace: The earthquake prediction program was not "separate," but I wondered why it had landed in the environmental protocol and not the science protocol. I understood at the time that it was because Gordon J. F. McDonald, a noted geophysicist, was a member of CEQ. Anyway, it was fortunate that it was where it was, because the earthquake prediction part of the environmental program always seemed way ahead of the other elements. I am sure that was because of the pre-existing international contacts within the seismological community. We did not have to start from scratch to build liaisons, but were given an opportunity to charge ahead with ideas that were already circulating internationally.

It was a pleasure to report progress at each of the joint meetings. A Soviet delegation had visited the U.S. in early 1973, and included V. Sadovskiy Igor L. Nersesov from the Institute of Physics of the Earth, Moscow, and S.Kh. Negmatulaev from the Tadzik Academy of Sciences. Even as late as 1973, there was still a sense of uneasiness on the part of the Soviet delegates. Dr. Negmatulaev, for example, refused to have alcoholic beverages at our home during their first visit to the U.S. A year or so later he told us that had been advised by authorities not to drink.

On his second visit to our house, however, he felt comfortable, and sampled just about all the "American drinks" and wines we could provide. That first social event at our home was honored by the presence of Shirley Temple Black, who had helped negotiate the agreement under which our program functioned. We had many things going for the program.

Scott: Did you go to the USSR again?

Wallace: Yes. Several other trips to the USSR were to attend business meetings for the agreement, but that first trip on the prediction business carried an aura of Cold War breakthrough. I always took particular delight showing our Soviet colleagues the wonders of the USA, highways, shopping centers, and grocery stores, hoping in a small way to demonstrate the benefits of our way over the Communist way. The Soviet scientists usually carried home items difficult to buy in the USSR, including hi-fi equipment, typewriters, and women's undergarments of the elastic variety. We always had philosophical discussions about religion, the democratic process, and the role of the private sector. Perhaps our propaganda served a larger mission in undermining Communism.

First Foreigners on a Fault in Kirgiz (1975)

Scott: You seem to have gotten to quite a few places while chasing earthquakes and faults.

Wallace: Probably the "first" that I cherish the most was a trip into Soviet Central Asia to examine the Talas-Fergana fault. In total the Talas-Fergana fault extends from Kazakh SSR across Kirgiz SSR and into Xinjiang Uygur Zizhiqu (Autonomous Region), China, a total of 900 km. In 1973 the U.S. Geological Survey and the Institute of the Physics of the Earth of the Soviet Academy of Sciences agreed to conduct a joint program on earthquake prediction. Soon afterward, I began to press for a chance to compare the great Talas-Fergana fault with my favorite, the San Andreas fault in California.

At the time I was chairing the U.S. delegation, so was given rather special treatment. After several exchanges back and forth, in August, 1975 I was scheduled to visit the Soviet field experiment in Garm, Tadzik. En route to Garm, my host, Igor Nersesov, told me that it had been arranged for me to see the Talas-Fergana fault. What excitement!

Scott: Talk a little about that experience.

Wallace: I'll start the story with our farewell dinner in Dushenbe, Tadzik, after formal meetings there and field work in the Garm area. Igor interrupted our meal to tell us we must leave to catch our plane. My wife, Trudy, who fortunately was able to accompany me on this trip--at our expense, I must add--said, "but I haven't finished my delicious fish." "Take it with you", Igor said. So Trudy wrapped two beautiful trout in paper napkins and stuffed them in her purse.

In an Aeroflot YAK 40, the small jet aircraft, we flew to Osh in eastern Uzbek SSR, not far from the Chinese border. At Osh we were picked up by jeep and driven, we sometimes felt "hurtled," through Andizhan and up the narrow road winding through the canyon of the Naryn River. Somewhere along the route we stopped for tea, during which Trudy took her fish out of her purse. But her purse wreaked of the fish and had to be discarded. Tea was served not at a table but rather at a raised platform which resembled a four-poster, hard, double bed.

Dark descended as we continued up the canyon, but finally we turned off the gravel road and found ourselves along the Karasu River in a grassy meadow. In the field of our headlights several tents were visible and we were shown to one of vivid orange and blue, complete with a flower in a small glass on a box beside the cot assigned to Trudy. The camp was lighted by a portable generator.

We gathered in the mess tent for a snack at the end of a long day's journey, and the snack was accompanied, as was so often the case, with plenty of vodka and toasting. Igor offered a toast to us, "To the first Americans, no the first foreigners, on the Talas-Fergana fault". That wasn't as newsworthy as being first on the Moon, but it was very special, nevertheless. Clearly the Institute had gone to great effort to get us there and to make our field investigation possible.

Scott: Was your camp actually on the Talas-Fergana fault?

Wallace: Yes. The deep, linear valley in which we were camped was an expression of the fault itself.

Scott: What did you find during your investigation?

Wallace: The next morning after a quick wash in the river and breakfast, we started out along the fault. My Soviet colleagues briefed me on the disagreement among Soviet geologists about the type and timing of movement on the Talas-Fergana fault. In 1961 the Soviet geologist V. S. Burtman had proposed that there had been a 250-km horizontal slip (right-lateral slip) or offset in about four hundred million years (since Devonian time). I gathered that our co-host, geologist V. N. Krestnikov, who was doing field investigations in the area, was very skeptical and not at all convinced that there had been such great movement, or indeed any movement in recent times.

The disagreements and different interpretations of field evidence reminded me of the decades-long contest between geologists of the Berkeley school and those of southern California about slip on the San Andreas fault. Now, in 1995, with overwhelming evidence of great strike slip gathered over the decades, and repeated records of strike slip during earthquakes, I think everyone accepts the idea of great offset along the San Andreas, but the consensus was slow coming.

Scott: Would you say a word or two more about the Berkeley and southern California contest, to make it clear what the two positions were?

Wallace: At the University of California, Berkeley, it seemed that the belief was that the San Andreas fault had had no more than about a kilometer or so of lateral (horizontal) displacement, whereas in southern California everyone seemed to believe in at least tens of miles of displacement. Mason Hill, who with Tom Dibblee later demonstrated hundreds of kilometers displacements, reviews the history of such concepts about the San Andreas fault in his 1981 paper. (Hill, Mason L., "San Andreas Fault: History of Concepts," Geological Society of America Bulletin, Part I, v.92, 1981, pp 112-131.)

On the second day out during our visit to the Talas-Fergana fault, we were driving along and I shouted, "Stop the car." Just across the valley, the fault was marked by small terraces and linear drainage channels. Most importantly, here were numerous small gulches clearly offset to the right where they crossed the fault trace. Having seen this, there was now no question in my mind but that strike slip characterized movement on the fault, and that there had been movement within geologically recent times.

Scott: It must have been pretty exciting to make such a definitive find?

Wallace: I was very gratified, but it did pose a sensitive problem. How was I to convey what I thought to Professor Krestnikov, the specialist and active investigator of the geology of the region? I pointed out the features carefully, and I believe that Professor Krestnikov was convinced that the only interpretation was the one of relatively recent horizontal slip (strike slip).

Later in the day we climbed up a 200-meter-high wall of huge angular boulders of dolomite and limestone. From the top we looked to the south where a great amphitheater was cut in the high cliffs. I had no doubt that this was the head of a giant landslide, and, indeed, Igor said that the landslide had been dated by Soviet geologists at between 6,000 and 7,000 years old.

The size and age of the landslide and its presence along the fault suggested that it might have been triggered by a large earthquake. Although this particular segment of the fault currently is rather seismically quiet, in 1946 a magnitude 7.5 earthquake occurred along a 150-km segment of the fault west of the Naryn River. Anyway to sum up, as a first, in my estimation our trip to the Talas-Fergana fault can scarcely be topped. (Wallace, R.E., The Talas-Fergana fault, Kirgiz and Kazakh, U.S.S.R., U.S. Geological Survey, Earthquake Information Bulletin, v.8, n.4, 1976, pp.4-13.)

The Philippines and the Marcoses (1976)

Wallace: One of the most memorable trips was in 1976 when Imelda Marcos, wife of President Ferdinand Marcos and first lady of the Philippines, was putting on a conference called "The Survival of Humankind: The Philippine Experiment." Just before the meeting was to convene, there was a great earthquake (M 7.9) off the southern island of Mindanao. The earthquake was actually centered under Moro Gulf. It killed a lot of people and caused a great tsunami.

I received an invitation to attend the conference, and liking the title, I happily accepted. Mrs. Marcos had invited about 40 foreign specialists on everything from food to hazard reduction. I was included among those concerned with hazards, which included weather hazards, earthquakes, and so on. My wife still has my name tag from that conference, "Dr. Robert Wallace, Natural Disaster," which she thinks is very appropriate for me to wear at all times.

An Evening at the Palace

Wallace: After I signed on for the conference, a man in New York City, whose name I have forgotten, became my contact. I will refer to him as Mr. X. Clearly he was a close associate of the Marcoses. The Philippine government was to pay my way and the authorization for me to attend came through the U.S. Department of State.

I was to deal with Mr. X in New York. When the big earthquake happened, I called Mr. X and asked if it might be possible for me to go to Manila a week or so early, so I could go down to see the Mindanao earthquake. He thought it could be arranged. This led to his saying, "Meet me at the Hyatt-Regency in Manila on such-and-such a day." So when I got to Manila, and to my room at the Hyatt, I called his number. He asked me to come over to his room, and added, "Would you like to go over to the Palace for dinner tonight?" "Yes, that's fine." "Do you have a formal dark suit?" "No, I have this old gray suit." "Well, that will have to do."

So I met him at 6:00 p.m., and a car and driver came for us. We were whisked off to the Presidential Palace, got out with much fanfare, and walked up the broad, gradual steps leading to the second floor. Freshly cut ferns surrounding lighted candles had been set out on each step to illuminate the stairs. Mr. X said, "We are supposed to meet them up in the music room." I wasn't sure just what was going on.

We paused by the music room, and a minute or so later Clare Booth Luce and her granddaughter arrived. We introduced ourselves, and promptly President and Mrs. Marcos arrived, as well as General Carlos Romulo of World War II fame. (General Carlos P. Romulo, 1899-1985) We went into the music room and gathered around a grand piano. Drinks were poured by servants, and the Marcos children came in. That was our little get-together.

Soon, however, Mrs. Marcos, who clearly was in charge of things that night, said, "Well, I think we had better go down to dinner." This was all a complete surprise to me. We marched down the stairs and walked into an enormous banquet room where a hundred or more people were seated at tables surrounding a big dance floor. Mrs. Marcos said to me, "We'd better have a receiving line." She went over to a little green-carpeted platform, and signalled for me to join her. We stood there and "received", shaking hands with all the hundred-plus people, and then sat down at a long head table under a beautiful canopy of capis shells. It was really glorious. Each place setting was superb, with many forks and knives in gold, and four or five wine glasses.

I was seated next to a famous Philippine writer and publisher, Karima Polotar. Toward dessert time dancers and performers of all sorts emerged with flames-and-sword acts, and the dance floor was aglitter with extravagant entertainment. Finally it was time to leave. Of course, there was nothing to do but "de-receive" everybody; to shake everybody's hand once again, as people left.

Scott: Who was in the receiving line?

Wallace: I have no idea who was received. All I can remember is standing next to Mrs. Marcos and shaking hands with strangers. I can't recall that President Marcos helped in the "de-receiving."

Scott: You were treated as the special guest that evening, weren't you?

Wallace: It seemed that way. Fortunately, I did not have to make a speech. I think it was all decided on the spot, with Mrs. Marcos leading the way. Then, after everybody left, she said, "Let's go down to the library." President Marcos and a few others, including Clare Booth Luce, a party of five or six, also went downstairs, but Mrs. Marcos took me alone into the library to show me clippings about the President.

She clearly had something else in mind, and started telling me about the development of Manila Bay that she had wanted. It was already started and many buildings had been built. She wanted to know about the earthquake problem, so we talked about the problems of building on filled land in Manila Bay.

While we were talking, I asked whether there would be a chance to go down to investigate the Mindanao earthquake. She said, "Yes, can you be ready at about 6:00 in the morning?" "Of course." "Well somebody will pick you up at about six." Soon afterward we retired for the evening and I went back to the hotel with Mr. X. I had stepped into a world I had never imagined before.

Visiting the Mindanao Earthquake

Wallace: There was a knock on my door at 6:00 a.m. the next morning, and here were two military men who had come with a car. I got my knapsack and followed them out to the car. Military police were in a car ahead of and behind us. Through Manilla we went with sirens screaming and lights flashing. At the airport we drove out to President Marcos's private plane. I climbed aboard and was directed to a single swivel chair, obviously for President Marcos, in the nice cabin behind the pilot. "If you need anything, just let us know."

Scott: That seems like a fine way to begin an earthquake site visit.

Wallace: I should say so. I had assembled all the maps I could get before I left, mainly of the Philippine fault, but also Warren Hamilton's superb USGS Professional Paper on the regional tectonics. After we got airborne the co-pilot came back and asked where I wanted to go. I showed him the maps and he suggested that I sit in the co-pilot's seat and guide the pilot. So down the Philippine fault we flew for several hundred miles, and then turned off to our destination at Cotabato in Mindanao.

There we found two big military helicopters waiting for us. George Pararas-Carayannis, from the tsunami warning center in Hawaii, was on board one. I sat on one side with a machine gunner behind me, and a row of riflemen in front of me. George was similarly situated and equipped on the other side. Off we went to look at earthquake effects.

One of the first places we landed was on Bongo Island in the middle of Mindanao Gulf. It was very pleasant there; a small village of bamboo and reed houses, mostly very primitive buildings, a lot of them on stilts along the edge of the water. There were also frame buildings which served as a school, I believe. The island had been hit by several tsunamis. We wandered out along a trail above the homes on the shoreline.

Suddenly the pilot said, "We must leave at once." Back to the helicopter we went. After we got airborne, I asked the pilot why he had gotten so upset. He said, "You didn't notice? There were no men in the village. We were being set up for ambush." In fact, a few days before the earthquake a half-dozen missionaries had been killed nearby on the mainland.

There were very active hostilities at the time. The Philippines were actually cut several ways. There were the Moslem insurgents in the south, the Marcos government, and then the Communists. One of the Communist leaders was captured while I was there. I was not prepared for all of this, but learned some current events as I went.

We left Bongo Island and went over to Cotabato, where we looked at the damage. Several reinforced concrete buildings were missing their first floors, and the several-story Harvardian School had tilted over and was a total loss. We spent the night in Cotabato. The floor of a small military office served as our bed.

The next day we flew to Lebak, where a tsunami had caused some amazing damage--beached boats and enormous piles of debris. From there we flew west across the Gulf to Zamboanga, where disaster headquarters had been set up. We learned that casualties numbered over 5,000.

The next morning we were to fly south to another site of major earthquake damage. By morning, however, the military had decided that the trip would be unsafe. In the past few days planes had been strafed by gunfire while flying through a narrow pass en route to our planned destination. Clearly we were in an active war zone.

At one of the villages on Moro Gulf several people volunteered the information that lights had been seen in the sky out over the gulf during the earthquake. While earthquake lights are still mysterious, clearly they have been seen during many earthquakes, and some observers claim they occur before earthquakes. The villagers pointed out to where the epicenter was. A big fault perhaps 200 km long ran under the gulf. One side of the fault had stepped up, displacing huge volumes of water and generating a tsunami.

We then met the President's plane again and flew back to Manila, circling Taal volcano, which was cooperating by erupting at the time. What more could a geologist ask?

The Conference on Human Survival

Wallace: Following the earthquake excursion, I attended the conference on the "survival of humankind," which was what I had really gone for. Both President and Mrs. Marcos gave talks at different times in the meetings. They seemed to have a sense of mission and message--the President especially. He spoke of the Philippines as a potential go-between, linking Eastern culture and Western culture. He dwelled on that theme, and seemed to think that he personally had a special place in history.

Admittedly, with all the ostentation at the palace, such as the ornate place settings at dinner and the like, one couldn't help be aware how greedy and corrupt the Marcoses must have been. But on the other hand, there was this nugget of their seeing themselves and the Philippines as doing something good--as a potential for world betterment.

For the duration of the conference, each one of the forty attendees was provided with a brand-new car, a military driver, and a military escort, to serve our needs. We kept very busy attending the meetings and writing up the findings of the various workshops. But Mrs. Marcos had also arranged many pleasant parties and outings. Each one of the delegates was set up with a couple of matronly ladies to serve as our personal hostesses. And then at parties we always had several younger beauties in their butterfly dresses, not for any purpose--I should add, lest there be some misunderstanding--other than to serve as decorations for the occasion. Opulence prevailed. While it was very pleasant, I also found it disturbing.

Scott: Yes, it sounds like it was almost too much! On the other hand, it seems to have been a serious conference.

Wallace: Yes, it was a very serious conference. In addition to us forty foreign delegates, there were dozens of Philippine specialists. Attendees represented a wide variety of subjects--they ranged from rice and food experts to science and technology specialists.

An Outing on the Presidential Yacht

Wallace: Although it was a very serious conference, it had its lighter moments too. At about noontime on the last day, Mrs. Marcos announced that she thought we had been working too hard. In fact, we really had been working, trying to write up reports on the sessions. She said, "Let's retire and go out to the President's yacht." You could see, however, that the lower-ranking people of the administration were thrown into a panic, not having been asked to plan for this. There was a lot of buzzing at the podium, and then she said, "They can't have the yacht ready at 1:00, but let's gather at about 3:30.

So in due course our cars began taking us out to the yacht, and by 4:00 everything was ready for us. The big fantail was full of hanging ferns, and a spectacular spread of food had been laid out. We all sat down. In the seating arrangement, we were alternated with a matronly hostess and a young beauty with a butterfly dress. There was a band and all of us men danced with Mrs.

Marcos.

At one point President Marcos came out and announced, "I've just received a telegram that Chairman Mao of China has died." There was a lot of buzzing conversation; it was a major moment in history. A little later four of us were with President Marcos in a small lounge just off the fantail. We asked what he thought the death of Mao might mean. He said that he disagreed with Mao on many things, but also commented, "The old hands create stability. Now we don't know--things are likely to be unsettled. The younger people may cause instability in China." He continued, "We were planning to have our daughter go over to Beijing to the university, but now I don't know--I would be very uneasy." To hear that kind of personal reaction from a head-of-state was a once-in-a-lifetime experience for me.

The yacht headed for Corregidor, although I understood that no outing there had been planned. However, we did land at the dock near Corregidor's fortress and about 50 people disembarked for a tour. By now it was dark, and there were only two flashlights available for the fifty of us to use as we wound our way up through the crumbling Corregidor remains. It was a wonder nobody tripped or was injured. Nevertheless, being part of a private tour of Corregidor led by Ferdinand Marcos was something to remember.

A Traumatic Trip Home

Wallace: I was due to leave for home right away, but came down with the flu and could not even get out of bed the next day. My wife called to say that her sister had died, and she needed me home. On the leg of the trip aboard Philippine Airlines between Manila and Hawaii, I truly thought I was going to die. I could not raise my head off the seat to have a cup of tea!

That is the end of this story about one earthquake trip. Some of my friends kid me about mingling with the Marcoses, and other dictators, but it would be hard to get a more personal insight into a bit of history of the time. Another dictator story involves a trip to Managua, Nicaragua, which I mentioned earlier.

With a Delegation to China (1978)

Wallace: In 1978 I went to China with a delegation led by George Housner, which was primarily concerned with earthquake engineering. Among other things, under Henry Degenkolb's tutelage I looked at construction, for example picking mortar out of masonry walls. Henry showed me the hardness test. At the weak end of the spectrum is mortar that can be pulled out using ones bare fingers--not very good, of course--and from that it grades up to very good tough mortar. During the years of chasing earthquakes, I learned many bits of structural engineering lore like this from people like Henry Degenkolb, George Housner, and Karl Steinbrugge.

The China delegation included George Housner (delegation chair), Paul Jennings (reporter), Ray Clough, Henry Degenkolb, Joe Penzien, Teng Ta-Liang ("Leon" Teng), myself, and a few others--numbering twelve members in all. The National Academy of Sciences report on the trip was published in 1980. (Jennings, P.C., Earthquake Engineering and Hazards Reduction in China: A Trip Report of the American Earthquake Engineering and Hazards Reduction Delegation, National Academy of Sciences, CSCPRC Report n.6, 1980.)

Scott: (Editor's note: Members of the delegation were: George W. Housner, chairman, Ray Clough, Genevieve C. Dean, Henry J. Degenkolb, William J. Hall, Paul C. Jennings, Liu Shih-Chi, R.B. Matthiesen, Joseph Penzien, Teng Ta-Liang, Robert E. Wallace, and Robert V. Whitman.)

Wallace: At one point, Leon Teng and I, the two earth-science representatives, separated from the main delegation in order to look into earthquake prediction in the Cheng-Tu area. We wrote up our findings, which were published in the Bulletin of the Seismological Society of America, as well as in the official report of the trip. We looked into the Chinese use of animal behavior, and what they call macroscopic evidence of earthquakes before they occur, gas emissions from the ground, and many other things like that.

In the BSSA version, we included an editorial comment on the Chinese program on earthquake prediction. Leon and I both found it to be lacking in rigorous scientific analysis, but on the other hand, with hundred of thousands of volunteers contributing, they had gathered a vast amount of data, data such as we would have a difficult time accumulating in the United States. I have had a lot of requests for copies of that paper. (Wallace, R.E. and Teng, Ta-Liang, "Prediction of the Sungpan-Pingwu Earthquake, August 1976," Bulletin of the Seismologic Society of America, v.70, n.4, 1980, pp.1199-1223.)

While in Cheng-Tu I got up one morning and soon began to feel very faint. I returned to my hotel room immediately, and was provided a nice lady Chinese doctor. There did not seem to be anything really wrong. The delegation had to continue on with the trip, because all the reservations were made, so I was left behind, but Leon Teng stayed with me.

It may seem strange to say that the illness proved fortunate, but that is how I view the turn of events. The result was a marvelous sojourn for Leon and me. Leon speaks fluent Chinese, including the dialects. He was born in China and went through a most amazing history in getting out of China during the Japanese invasion in World War II. He told of flying with an uncle in a plane dead-heading for Taiwan, and eventually going from there to the United States to earn his Ph.D. He became a fine and highly respected American seismologist and professor.

While I was on my sick bed, the greatest comfort came from Joe Penzien. Whenever I see him, I compliment him on his bedside manner. Had he chosen to become a physician, he would have soothed many a patient. After a few hours of illness I recovered, however, so Leon and I wandered around Cheng-Tu with no formal schedule. He was my guide, interpreter and partner. As we visited museums and parks, he explained Chinese history, poetry, literature and art. Americans being rare in China then, wherever we went a hundred or more people gathered around. They stared at us, followed us, and watched everything we did. In the hotel where we stayed, some of the help were trying to learn English. They had a funny little old record player, and records of some very old and very British English. I remember the voice on the record carefully pronouncing and spelling "Nay" instead of "No."

The Chinese loved to try their English on us. One day we were taking pictures in a park, and I was doing some little sketches. One curious observer there found out that Leon could speak Chinese, and a long discussion ensued. As a vignette of Chinese life, we learned that our friend was waiting for his wife to get off work. He had a job in another city 100 kilometers away, but had gotten a leave to come to Cheng-Tu to visit her.

<u>Ancient Canals and Trenches in China (1984)</u>

Wallace: I am tempted to tell of one more overseas trip chasing earthquakes and faults. In 1984, after a meeting of the International Geological Correlation Program (IGCP) in Kobe, Japan, our geologic colleagues at the State Seismological Bureau in Beijing, China, took Bob Bucknam, Tom Hanks and me to Ningxia and Gansu provinces. We were interested in examining ancient fault scarps which had been created during ancient great earthquakes of known date. Only in such places as China, with its long recorded history, could we hope to get the data on ancient events we needed for our study of scarp degredation rates. We were sure that from such data we could learn how to tell the age of old scarps of unknown age, and provide useful criteria for dating scarps everywhere and, thus, prehistoric earthquakes.

Fault Offset of the Great Wall

Wallace: On a previous trip we had learned that the Great Wall had been offset by faulting accompanying an earthquakes in 1739 in Ningxia Huizu Zizhiqu (Ningsia Hui Autonomous Region). We actually had passed by the site on the train a few years previously, but could not see the offset wall at all well at a distance and through the train window. We also had begun to hear stories that in Gansu province there were scarps related to the big earthquake at Gaotai in 180 AD. We were excited by the prospects of what these two sites would reveal.

The offset of the Great Wall was only a few kilometers from the small village of Shizuishan where we stayed, following a train ride from Beijing. We were followed everywhere by 100 or more people curious about these foreigners. Each day our crew from the State Seismological Bureau and we would drive in vans out to the Great Wall where it had been broken and offset by fault movement in 1739. We measured fault scarps and offsets of the Great Wall, as well as documenting the construction of the Great Wall itself.

We walked and worked atop the Great Wall and along its base, and measured trenches and berms that paralleled the north (Mongolian) side of the Wall. Later we learned from Professor Arthur Waldron, a China scholar at Princeton, and an expert on the Great Wall, that such defense trenches had never been accurately measured and documented in the literature.

After about a week's work at the north end of the 1739 fault scarp, we moved to Yinchuan about 200 km south along the Huang He (Yellow River) and near the south end of the zone of faulting. There the scarps were even more spectacular and in places exceeded 5.5 m in height. Up until that time, I believe, these were the oldest and biggest scarps to be measured accurately for scarp calibration purposes. What an opportunity!

(Zhang Buchun, Liao Yuhua, Guo Shunmin, Wallace, R. E., Bucknam, R. C., Hanks, T. C., "Fault Scarps Related to the 1739 Earthquake and Seismicity of the Yinchuan Graben, Ningxia Huizu Zizhiqu, China", Bulletin of the Seismological Society of America, v.76, n.5, 1986, pp. 1253-1287.)

Ancient Canals in Gansu Province

Wallace: Our next target was the reported faulting produced during the 180 AD earthquake near Gaotai in western Gansu Province. After stops at the west end of the Great Wall and a tourist side trip to the incredible Magao Grottos carved in volcanic ash, where a treasure of documents was found, we were taken well off the main highway along the Old Silk Road to see the faulting.

We labored up some dry washes which carried drainage off the Yu Mu Shan (Elm Mountains), and finally came to a halt amid walls and columns of fairly firm alluvial gravels. As Tom Hanks exclaimed, "This is the most bizarre land I have ever imagined!" I thought to myself, "These are fault scarps? No way," It did not take long before I said to our SSB colleagues, "These are the remnants of canals".

Scott: Were these canals known before, and to what and when did they supply water?

Wallace: Those were some of our first questions. Even after later historical research by our Chinese colleagues back in Lanchow, only indirect references seem to have been made of the canals earlier. The canals head at the Li Yuan He (Dasha River) and reach about 50 km to the abandoned walled City of Camels.

Where the wash that gave us access to the canals crossed them, we found four parallel canals. These seemed to range widely in degree of degradation and thus antiquity. Eventually we decided that the oldest might date from before Christ. The largest canal is about 30 meters wide and 6 meters deep. Our tentative interpretation was that the canals had served as water conveyers and possibly also as defensive barriers.

Unfortunately, it started to rain a few hours after we arrived in the dry wash, so our examination ended all too soon. We did prepare a short paper that has been published, and we hope others will note this and someday carry out adequate studies. (Wallace, R.E., Bucknam, R.C., Hanks, T.C., "Ancient Engineering Geology Projects in China: A Canal System in Ganzu Province and Trenches Along the Great Wall in Ningxia Hui Autonomous Region," Engineering Geology 36, 1994. pp.183-195.)

Other Trips

Scott: What other trips or explorations stand out in your mind?

Wallace: So many images well up in my mind, but I don't want this to turn into a travel log. Two months in Japan in the early spring of 1984 are memorable. I was hosted by the Geological Survey of Japan, Yoshi Kinugasa specifically, and all expenses were paid by the Japanese Industrial Technical Association. My wife, Trudy, was able to accompany me.

Yoshi Kinugasa and many other geologists, T. Matsuda of Tokyo University's Earthquake Research Institute, A. Okada professor at Osaka University, among them, took us throughout western and central Japan to examine active faults. We plodded through rice fields in snows of February, stayed and ate almost entirely in Japanese hotels and restaurants, and travelled by car, bullet train, and busses. We headquartered at Tsukuba, the science city where the Geological Survey of Japan is headquartered. Wouldn't you know that I would run into another U.S. earthquake chaser there. One morning in the Sunroot Hotel when we went for breakfast, there was Henry Degenkolb having his breakfast. Earthquakers seem to be everywhere.

Scott: Yes, it seems that when each earthquake occurs world wide, some earthquake engineer, geologist or seismologist happens to be nearby. As we talk today, the terrible earthquake just happened in Kobe, Japan, and a conference on earthquake disaster response was being held not far away in Osaka.

Wallace: The first on-site radio report I heard the day of the Kobe earthquake, January 17, 1995, was by Charles Scawthorn, a member of EERI.

Scott: Would you like to talk about any other trips?

Wallace: I can't end this set of travel logs without remembering a fabulous trip to China in 1985 to attend a conference and field trip for the International Geological Correlation Project, No. 208. The project was under UNESCO and was created to compare active faults world wide. I can think of no better way to dispense information throughout the world, and to learn as an international team, than by such UNESCO projects. The importance cannot be overemphasized, for example, of standing on a fault with colleagues from many countries and debating and pointing out to each other what we see and how we interpret the evidence. Each of us has had unique backgrounds and experiences.

Scott: Elsewhere we have talked about education techniques, but this would seem to one of the most effective.

Wallace: I should say so. I strongly agree.

Another trip in 1990, which I considered to be quite an honor, stemmed from an invitation to help the Geological Survey of New Zealand celebrate New Zealand's 200th anniversary. I was asked to give a series of lectures in Lower Hutt and Wellington, and to be available for press conferences.

Trudy went with me and, inasmuch as we had many friends in the country, the several weeks there was delightful. They even managed to have a rather strong earthquake on the North Island in the vicinity of Hawke Bay. I guess earthquakes tend to follow us earthquake nuts.

Scott: You have surely had some fascinating experiences in your geological and earthquake travels.

Wallace: Yes. As Bob Bucknam said as we stood atop the Great Wall of China where the fault break occurred in 1739, where no westerners had stood before, "What duty we have drawn! Ah, but somebody had to do it!"

Over the years, wherever I was doing geologic studies throughout the US or overseas, I repeatedly pinched myself, and said, "And you get paid to do this?" The exploration and discovery have thrilled me always.

XI USGS DIRECTORS: MY RECOLLECTIONS

Scott: Now that you have completed your review of the USGS programs, and before you take up your own post-retirement activities, this seems like a good place to give your own recollections of each USGS director that you knew.

Wallace: Yes, I would like to do that. I have known eight of the twelve directors of the Geological Survey. Also I should mention the history of the Survey was written up very nicely by Mary Rabbit, who is still around USGS, retired, and still working on history. (Mary C. Rabbitt, A Brief History of the U.S. Geological Survey, U.S. Geological Survey, 1974, 1980, 1984; Mary C. Rabbitt, The United States Geological Survey, 1879-1989, Circular 1050, 1989.) We also have an official historian for the Survey, Cliff Nelson, but his project has started and stopped and started again. As you might imagine, it does not always get very much funding.

I do not want to try anything like a full history, but perhaps I could provide a few stories and vignettes about directors--some things that others might not know. They were real people with human strengths and weaknesses.

Scott: You might begin by simply listing all twelve of the directors USGS has had in its history.

Wallace: The list begins in 1879, when the USGS was created out of four earlier geographical surveys of the west. Clarence King led one of those early surveys and was selected to be the first Director of the new Geological Survey. It is said that William James considered King to be perhaps the leading intellect on the Washington, D.C., scene at the time.

1. Clarence King: 1879 to 1881
2. John Wesley Powell: 1881 to 1894.
3. Charles D. Walcott: 1894 to 1907

(List continues)

4. George Otis Smith: 1907 to 1930
5. Walter C. Mendenhall: 1931 to 1942
6. William E. Wrather: 1943 to 1956
7. Thomas B. Nolan: 1956 to 1965
8. William T. Pecora: 1965 to 1971
9. Vincent E. McKelvey: 1971 to 1978
10. Henry William Menard: 1978 to 1982
11. Dallas L. Peck: 1982 to 1994
12. Gordon P. Eaton: 1994 to present

Scott: The USGS directorship did not change frequently--tours averaged over 10 years. How did that come about?

Wallace: I believe the long tenure was related to the fact that, fortunately, the USGS was never politicized in the sense that when Presidents changed, USGS directors would automatically be changed. Such a pattern never developed. The director of the USGS serves at the pleasure of the Secretary of the Interior, but the selection has never been based primarily on politics, although each nominee requires confirmation by the U.S. Senate.

The Secretary of the Interior picks each new director from a list of about five or six nominees compiled by the National Academy of Sciences (NAS) from various sources, principally earth science and technical groups. Qualifications for directorship emphasize national and international recognition for leadership in earth science and technology, including personal scientific and technical contributions. When selected, many directors had already been elected to the National Academy of Sciences for their innovative research in earth sciences.

Scott: So the tradition of choosing highly qualified directors was established right at the outset in the days of King and Powell and has been maintained throughout the life of USGS. That is a remarkable history.

Mendenhall (1931-1942), Director When I Started

Scott: You joined the USGS IN 1942. Who was USGS director then?

Wallace: Walter Mendenhall was director in 1942 when I joined the USGS, and his eleven-year tenure ended that same year. I only met him a time or two, when I returned to Washington D.C. from field work in Alaska in the Fall of 1942. As a lowly "Junior Geologist", the designation applied to entry level professionals, I had little chance to hobnob with directors.

Wrather and the War Years (1943-1956)

Scott: As you discuss the various directors, would you characterize their leadership qualities, particularly of those you feel you actually got to know reasonably well?

Wallace: Yes. I want to comment on that intangible thing called leadership. We all seem to know it when we see it, but it takes so many forms, it is difficult to define, and even more difficult to generate. Leadership seems to be born with the individual.

As to the ones I got to know--I had much more to do with Bill Wrather than I had with Mendenhall, especially after I became involved with the permafrost program in 1945. He invited me and others to lunch several times, and was very much interested in the studies of permafrost, which was somewhat of a departure from then traditional programs of the USGS. In fact, Bill Wrather's interest in so many new facets of geology and his encouragement to all of us to move ahead was one of his great contributions.

Wrather had become wealthy in the petroleum industry before he became director of the USGS. He had a stately bearing and was gracious and engaging in every way. Although I never attended Congressional hearings then, other people reported that even Congressmen treated Bill Wrather with deference: "Yes Dr. Wrather," "What do you think, Dr. Wrather?" "What can we do for you, Dr. Wrather?"

During Wrather's tenure, of course, World War II was in full fury, and the Survey was deluged with tasks, and given plenty of money to carry out the work. When money is plentiful, people love their leaders, because those leaders can see to the fulfillment of project dreams. But of course, the war and military needs dictated the projects. We all knew we were doing important things for the war effort. It is unfortunate, really sad, that we seem unable to manage such a unity of purpose without a war or other big crisis. I guess awareness of a threat to your very survival is the big motivator. I know I felt that.

Scott: Describe some of the special wartime programs.

Wallace: A Military Geology Branch was created to answer the military's specific needs, such as tractability for tanks and other motorized vehicles at invasion sites, water supplies for troops at battle sites, special topographic maps of bombing targets and Army travel routes.

My own projects reflected the war orientation. First, in 1942 the mission was to find mercury (quicksilver) deposits in Alaska to fill the shortage of that metal needed for ammunition detonators and anti-fouling marine paints. The project in early 1945 related to developing the North Slope (Alaska) oil potential. Within a few months I was reassigned to explore for uranium needed for the atomic bomb. Then only a few months later the military asked the USGS how to cope with the ravages permafrost inflicted on airfields along the air ferry route to the Soviet Union, and off I went again to a new project. At the outset I knew nothing about permafrost, but neither did anybody else.

The Nolan Years (1956-1965)

Scott: You have already mentioned the next director, T.B. Nolan, in connection with your Nevada mapping work.

Wallace: Yes. Before becoming director, Tom served under Wrather, I believe as an associate director, but I don't know just how many years he served. When Wrather left there seemed to be no question but that Nolan would be the successor. As an Associate Director, Nolan had learned all the disciplines and techniques of management. But he also had a superb track record as a scientist and was a member of the prestigious National Academy of Sciences.

Tom loved field geology, and for his entire tour as director, he spent summer months in the field working out of Eureka, Nevada, running the USGS by telephone and mail from there. This was not motivated by anything but personal devotion to field geology and science. The message of his example was clear to everyone: Tom Nolan was a champion of fundamental field geology and excellence in science.

Scott: That has to be classed as a form of leadership.

Wallace: I should say so. Leadership by example is one of the most powerful forms of leadership, in my opinion.

Scott: Say more about Nolan's role as USGS director.

Wallace: Tom was a meticulous detailer, perhaps even to a fault. In field investigations, he and his assistants worked at a scale many times more detailed than was intended for final map publication (generally one inch equals one mile).

Scott: I suppose such detail documented his field observations extremely thoroughly?

Wallace: Yes, I should say so, but unless later investigators search out the original field sheets from archives, much of that detail essentially is lost. In running the USGS, Tom also kept track of detail. One of his common evening pastimes was playing bridge, and I learned from some of his bridge partners that during a game, Tom would place the day's "chron file"--a pile of hundreds of carbon copies of all correspondence in the USGS--on a chair beside him. Whenever he was "dummy" or had a moment free, Tom would read the files. He was a rapid reader and had an amazing memory, so he knew everything that transpired in and around the USGS.

Scott: *Did any of his penchant for detail rub off on others?*

Wallace: Well, I can report one instance where it seemed to have. I stepped into the office of Associate Director Julian D. Sears one day, and found him running his finger down a manuscript page of water records. He seemed a little embarrassed, and volunteered that for years he had been looking for errors in such tables from the Water Resources Division, and he had finally found one. He had the responsibility to review all manuscripts for approval.

Scott: He himself must have felt almost a proofreaders responsibility that the published results should be error free.

Wallace: As essentially a chief executive officer for an office of 8,000 plus employees, he had the responsibility to review all manuscripts for policy content, not editing. The editing and proofreading was supposed to have been done at a much lower administrative level. In any event, in Nolan's time there was no question who was the boss. Nevertheless, others had great influence within the USGS, whether they carried titles or not. Jim Gilluly was one of those.

Scott: You haven't mentioned Gilluly before, who was he?

Wallace: I want to stick to the Nolan story, but will digress for a moment. Jim Gilluly was a senior scientist, considered by some to be the greatest USGS geologist since G.K. Gilbert. Jim was a prolific producer of new geologic ideas. Just by the power of his personality--he only once took on a temporary administrative job as Branch Chief--Jim had enormous influence, on occasion perhaps more than Nolan. But they saw eye to eye most of the time. Sometimes, however, Jim's personality seemed to be counterproductive, making some youngsters fear him, and inhibiting the characteristic "welling-up" of ideas within the USGS.

Scott: Thanks for that explanation. Now back to Nolan?

Wallace: One characteristic of the Nolan administration was his seeming to build a brick wall around the USGS. He did not trust the Department of Interior. On one tour of headquarters, I felt I should get to know the Department of Interior's Program Office, so off I went for a visit. At the time, I was working for Harold Bannerman in the Geologic Division's Program Office. Not many days later, I was called on the carpet for my visit to the Department, and was told, "Any contact with the Department must be through the Director's office." That was Tom Nolan's way of trying both to protect and promote the USGS.

Scott: You mentioned that Nolan was a bridge player. Did he have other hobbies?

Wallace: I mentioned that he was a birder. Tom was an avid birder all through the years, and having followed that hobby most of my life, we always had birds to talk about, as well as our shared interest in Nevada geology. Other birders in USGS were Ed McKnight, a noted mineral resources geologist, Art Baker, Associate Director, Luna Leopold, Chief of Water Resources, and Bob Smith, who was noted for work on volcanism, especially ignimbrites. Members of the group aided and abetted each other, and jokes were sometimes made about the USGS being populated by birders.

Ed McKnight was the ringleader of the USGS birders. He organized annual Christmas bird counts for the Audubon Society at Brooke, Virginia, and at the estate of Harry "Fergie" Ferguson across the Potomac River from George Washington's Mount Vernon. I felt privileged to participate in the counts during several years while on tour at headquarters. I later started a similar count in Palo Alto, California.

Scott: There seems to have been a real camaraderie within the USGS.

Wallace: There surely was. That feeling was helped by such things as the annual Pick and Hammer Club's annual show, a lampoon of all that was wrong with the USGS. Laughing at oneself does help so much to keep things in perspective. Those activities helped create personal bonds between people in very different parts of the organization.

Bill Pecora (1965-1971): Scientist-Politician

Wallace: After Nolan, the next Director was Bill Pecora, an outgoing person and born salesman, as well as an excellent scientist. He was known for his work on the petrology of volcanic rocks in northern Montana as well as many other things. Bill was the ultimate scientist-politician. It came so naturally to him. Since the days of John Wesley Powell, I doubt that any of the other directors surpassed

Bill in this talent. He seemed to have Congress in the palm of his hand. Congresswoman Julia B. Hansen from the State of Washington chaired the House Appropriation Committee's Subcommittee for Interior and Related Agencies. She seemed to like what the USGS was doing and what Bill thought the Survey should do.

Scott: Having someone who viewed USGS favorably in such a powerful position must have made life easier for Pecora?

Wallace: Indeed it did. I wish I knew some details, but somehow Bill Pecora early-on managed to get the authority and money to launch a satellite, now called "LANDSAT," earlier known by another names, I think one was "EROS" for "Earth Resources Orbiting Satellite," which proved invaluable for studying natural resources. This was accomplished over the strong objections of other departments and agencies. I believe Bill and Congresswoman Hansen together arranged this, although I can't verify that.

I should remember some of this better, because for a few years I was deeply involved with "remote sensing," a fast-developing new discipline, based primarily on satellite imagery. I was encouraged to help the USGS push ahead with it, and I did some "ground truth" work along the San Andreas fault for the infra-red imaging experiments. Bob Moxham was the infra-red specialist and my partner in this effort.

Scott: I presume that "ground truth" refers to comparing interpretations of imagery with what is actually found on the ground to see how they match. What did you learn from this remote-sensing work?

Wallace: Well, I learned nothing new about the San Andreas fault, but the IR beautifully displayed sheep droppings which had been spread out by sheet wash to form thermal blankets over very large areas! The usefulness of that information seemed limited.

The techniques of remote sensing were rather primitive at first, and for several years, technical sessions seemed to be made up of papers that said, "Look Ma, I can tell that grass is green by remote sensing," At the time, I thought it would take years of evolution before remote sensing--other than good aerial photography--would really pay off geologically. In contrast, air photos had become such an absolute necessity to me that I concentrated on them. Now, however, remote sensing has progressed to a high level of sophistication. I admire all the images of the planets made by an assortment of sensors.

Scott: Do you have other stories from Pecora's time?

Wallace: Oh, there are many! In terms of getting money, I think of Bill's proposal to search for "invisible gold" under a program called "Heavy Metals," which he built into an image of mystery and high tech. It rested on the very real basis that abundant new gold would enhance the nation's international monetary position and create jobs here at home. It was very reasonable, and today most gold is produced by open-pit mining of very low-grade deposits.

Scott: You could almost say that Pecora had a vision and it became reality?

Wallace: I will say again that Bill was always several steps ahead of others. He was articulate, even glib, but always with the talk rooted in good science and technology. Nevertheless the politician in Bill sometimes did get the best of him. For example, when trying to be politically correct in one direction, he got burned in the press in the infamous "muzzling of geologists" incident.

The story revolved around the fact that Marve Lanphere and Brent Dalrymple, both working mainly on geochronology (dating of geologic time) projects for the USGS, submitted criticisms to the press about the earthquake hazards of a housing development being built on the shores of San Francisco Bay. The developers objected strongly, and to some, the USGS was put in bad light, especially because Lanphere and Dalrymple were not earthquake specialists or engineering geologists.

Bill insisted that Marve and Brent cease and desist. But when the press learned of this, the USGS was then portrayed in even a worse light--"Good science should be heard and not hidden just because developers might suffer financially." So Bill got headlined in the press: "USGS Director Muzzles Geologists," and he backed down. The very positive outcome was that all of us suddenly had far easier access to the press, whereas previously all press contacts had to go through headquarters review.

Scott: Had Lanphere and Dalrymple by-passed the earlier process?

Wallace: Yes, they operated as concerned citizens, not as USGS staff. The greater freedom we got has served us well, inasmuch as we could not have functioned in the old way when we began to get into the era of fast-breaking earthquake reporting. Almost uniformly the technical staff has displayed enormous individual responsibility and skill in talking to the press.

Scott: Interacting with the press is always a delicate matter.

Wallace: Another time Bill's concern about politics caused some of us difficulties. During the time I headed up the San Francisco Bay Region Project, the State Department of Water Resources became upset with our findings. The press did us dirt by reporting, falsely, that the USGS would be issuing a report saying the Bay would become a "Dead Sea."

Scott: I see how that could cause quite a stir. What happened?

Wallace: After the director of the State Department of Water Resources flew back to pound on Bill's desk, Bill agreed that the State could review our reports before release. I happened to be in Bill's office when this happened, but the ruling really hit home when Bill returned a manuscript produced by our project and asked me to forward it to the State for review.

Scott: I can understand how you might find that hard to accept. What did you do?

Wallace: On a Sunday I got Dave McCulloch, one of the authors, and one or two others together. We decided that we could not have our technical findings subject to censorship by a state agency. I sent the manuscript back to Pecora saying that he could send it to the State, but that I would not. I suppose that was insubordination, but I never heard another word about it.

Scott: Do you suppose Pecora sent the manuscript to the state?

Wallace: I suppose so, but don't know.

Scott: Despite a few incidents like these, however, I gather that you feel strongly that Bill Pecora was a good director.

Wallace: No question about that. He may have been the most skillful director the USGS ever had. Rogers Morton, Secretary of the Interior, enlisted Bill's services as Undersecretary of the Interior. Both Morton and Pecora died before their tours of duty ended. In my mind their combined deaths was a national disaster.

Scott: Would you elaborate on that?

Wallace: That was a period when the nation was experiencing a shortage of energy. Morton had ideas of a national energy policy, and was thinking about the creation of a Department of Energy and Natural Resources. He had the political clout to pull off such a deal. Bill Pecora had the technical skills and knowledge about earth sciences to keep policies on a realistic and practical track. With the two of them working together, I do believe the country would have been far ahead in energy policies.

The McKelvey Years (1971-1978)

Scott: Who followed Pecora?

Wallace: Vince McKelvey was the next. He had been Chief Geologist, that is, head of the Geologic Division of the USGS, before moving up the administrative ladder to become Director.

Scott: Was McKelvey more or less a contemporary of yours?

Wallace: Vince was indeed a close and dear friend. We had been together in the Spokane office, an outpost of the Branch of Mineral Deposits in the late 1950s and '60s. I was working on the Coeur d'Alene, Idaho lead-zinc-silver deposits with Warren Hobbs, Allan Griggs, and Art Campbell, and Vince had a big group working on phosphate deposits in western Montana near Dillon. The small Spokane office seemed to spawn other people who grew in importance to the USGS, including Monty Klepper, who became Associate Director, George Becraft, who became Chief, Office of Mineral Resources--Warren Hobbs also held that post in later years--Art Campbell, who headed up a branch of Regional Geology in Denver, and Dick Shelton, who became Chief Geologist.

Several from the Spokane office moved to Menlo Park at about the same time. Here we frequently had bag lunches together with other colleagues. I remember that Vince was one of the first of my friends to become passionate about the problem of global overpopulation, and he would wax eloquent about this again and again at lunch.

Vince and I, plus ten others, formed the Homewood Investment Club (named after Homewood Place, the street on which our office was located). That name rather naturally became "Homeless Investment Club," especially to our wives. But after a dozen years, after Vince had moved to headquarters, we cashed in our ten-dollar-a-month investment after the total reached a thousand, and recorded a three-fold profit. The main benefit, however, was the learning we gained as well as the camaraderie.

Scott: You formed the club in order to meet regularly, each make modest contributions to the club's investment pool, and see if you could make some money on the market?

Wallace: Yes.

Scott: How would you characterize McKelvey as director and as a person?

Wallace: The "epitome of the ethical man," would be a good way to characterize Vince. He worried and fought to have the USGS and all its members be ethical, not just substantively and legally so, but in image and appearance as well.

Scott: That's an excellent goal, if not carried too far. How did he try to promote it?

Wallace: One way was to issue a statement of his philosophy, which then became USGS policy. His statement was issued within a few months of his becoming director. I was Regional Geologist at the time, and picked Vince up at the airport when he came out for his first formal visit to Menlo Park. His very first question was, "Has anyone paid attention to or used my statement on ethics?" I could cite a few examples but not to the extent he had hoped for. Vince went on to say that he had assigned several different people the task of writing a statement on ethics. None of the drafts pleased him, so he said, "One weekend I sat down and wrote my own." Vince was a fine writer, and superb editor.

Scott: What else did he do regarding his ethics goal?

Wallace: Vince forced many colleagues to divest themselves of stock, some inherited, and interests in companies that had any holdings whatsoever in natural resources, just as called for in the 1879 Act that created the USGS. Many felt that Vince went too far at times, because he considered that many large, diversified companies with only a very small interest in natural resources were all off limits. Often one could not even determine whether a large conglomerate even held interests in natural resources.

Vince took a strong position against Survey personnel taking advocacy positions, especially based on Survey findings. I had long discussions with Vince about how to separate statements of geologic facts from the appearance of advocacy. For example, if a USGS scientist issues a geologic map showing an earthquake fault passing under a housing development, the developer generally feels that the USGS has taken an advocacy position against the development. Is stating a geologic fact an advocacy position? In my opinion, no.

Scott: In trying to put in place an ethics policy statement that differed significantly from what was already being done by common consent, do you think he overdid it a bit--in trying to make USGS folk so pure?

Wallace: Yes.

Scott: How was McKelvey in terms of program and politics?

Wallace: I would say that Vince was an excellent director as far as programs go, but perhaps history will suggest that he ran against the political tide to some extent.

Scott: What do you mean by that?

Wallace: President Jimmy Carter was fighting the oil and gas shortages and had taken the position, for example, that the U.S. should impose an import tax. Vince, always the optimist, as well as knowledgeable about natural energy resources, took the position that by applying the excellent creative power of U.S. technical people, especially the USGS, of course, the energy shortages could be overcome.

Scott: If he was thinking about petroleum, that would have seemed overoptimistic, considering our dependence then and now on oil from elsewhere. Recalling Carter's position at the time, I can see why there might have been some displeasure in the administration.

Wallace: Vince's position was not popular at higher levels of government, and he began to be eased out. The Washington Post reported that he was "fired". (Nov. 11, 1977) Vince's tenure as Director overlapped into the next administration so that the USGS directorship would not be looked upon as political. Fortunately, the tradition of having the USGS remain non-political is still intact.

Scott: McKelvey's tenure was 1971-1978, so he came in during the Nixon administration and went out after about two years of the Carter administration. Do you have any personal asides concerning McKelvey?

Wallace: Vince was a chain smoker of cigarettes. It was long before the anti-smoking era. He joked to me once that he hoped the cigarettes would help assure that he went quickly. Ironically and sadly, his fate was to suffer the worst possible last years under the terrible affliction of Lou Gehrig's disease.

But Vince kept up a remarkable correspondence. Although word processors were new, the USGS provided a word processor at Vince's bedside, which Vince learned to use. Shortly before he died, I had a marvelous letter from him that he had to type one letter at a time, using a wooden pencil held stiffly in his hand. In his final months he held the pencil in his mouth. In this same way he wrote a touching poem about his disease.

William Menard as Director (1978-1982)

Wallace: Bill Menard--Henry William Menard--came to the USGS after years of teaching and research at the Scripps Institute in La Jolla, California. He was an excellent scientist, who had explored the floor of the Pacific Ocean and had contributed importantly to the plate tectonics revolution.

He was at Caltech when I was there as a graduate student, and we took a course in field geology together, but I was not a close friend. I did visit his office and lab at Scripps on a few occasions. On one memorable trip, Bill and his student Tanya Atwater described Tanya's research on the migration of the East Pacific Rise and other Pacific spreading centers. Tanya became one of the major contributors to the plate-tectonic revolution.

Scott: After that, did you keep up with Menard over the years?

Wallace: No, I did not see much of him again until he became USGS Director. I have one story that casts some light on Bill's personality. About six months after Bill became director, I met Bill and his wife at Caltech where I had been invited to give the Buwalda lecture. A sherry party and reception was held before my talk, and Bill's wife came over and asked me a question, which I did not understand at first. She asked, "What is the climate like at Menlo Park?" "What do you mean, I asked?" "Well what do the USGS people think of Bill?" I replied, "I would like to discuss that with him." She pressed on, but I resisted saying more to her, because at that moment Bill's image around the USGS was not good.

Scott: How did you handle that touchy situation, and did you have a chance to talk to him?

Wallace: A week or so later Bill was scheduled to come to Menlo Park, and I had a call asking me to meet with him. We met and spent almost two hours in very personal discussions. Bill opened with, "I thought the Survey was sort of a closed group, but I never knew how tightly." He continued, "When I sit down across a table from people, they just clam up and I can't get a word out of them!"

Bill had authored a book in which he had been very critical of the USGS, its slowness in investigations and publications and several other serious accusations, some true, but others not. I reminded him of this and explained that people were afraid of him, that he had power over their funds and projects, and even their lives. Indeed, his appointment had been rumored to have been in part political, although I saw no evidence of that myself.

I raised the political question, and he scoffed, "My God, I have been devoted to science all my life." He also added, "I have been in office six months and nobody has invited us to their home yet." I expressed sincere surprise at that. We ended on a very friendly note, and after he left, I wrote him a several-page, hand-written letter to try to express my views better than I had orally. That very day I called Dallas Peck, who was Chief Geologist at the time, and very promptly the sociable Pecks invited the Menards to their home. The ice seemed to have been broken.

Throughout his tenure, however, Bill seemed somewhat uncomfortable in meetings. Although I felt he developed into a fairly effective director, he always seemed to me to have a basic sense of social inadequacy. As for his science, he was anything but timid or inadequate.

Scott: People come with many varieties of personality, including those in leadership positions.

Wallace: Here is another related story. At a centennial dinner where directors of geological surveys from many countries around the world were in the audience, Bill was accepting a present from the Director of the Indian Geological Survey. It was a plaque featuring a peacock. Bill held up the plaque and expressed his appreciation, but then blew it by saying, "In this country we call them turkeys and eat them at Thanksgiving time." I suppose he actually could have mistaken the peacock for a turkey, but as you might imagine, a gasp rose from the international group gathered for the dinner party.

Scott: That does sound like a serious gaffe. Perhaps he thought it would be funny, and in some circumstances, I suppose it would be. But international occasions like that are notoriously tricky. You said something about his feeling uncomfortable in meetings. Maybe he was just a little short on some of the so-called social skills.

Wallace: Yes.

Dallas Peck as Director (1982-1994)

Wallace: Dallas L. Peck, another Caltech product, succeeded Bill Menard. He graduated from Caltech and earned a Ph.D. from Harvard. Dallas was a lot younger than I, but was a close friend--one of the gang at Menlo Park. He was always gregarious, bright, and outgoing. So was his wife Sue. We socialized with the Pecks, as did many other couples here in Menlo Park. Organizationally, many of us were in the Branch of Mineral Deposits (later the Office of Mineral Resources). In terms of his scientific pursuits, his geology was focussed on work in the Sierra Nevada, particularly in and around Yosemite National Park.

Dallas always seemed to be striving for higher posts in the USGS.

Indeed, after he was first called to serve in some deputy capacity in Washington, we were all sure he would move up the ladder. He became Chief Geologist in due time. It seemed perfectly natural when the National Academy of Sciences nominated him to be director, and he was chosen.

Scott: How did he work out as director?

Wallace: I believe that I am too close to Dallas, and it is too soon in time to provide proper perspective. He served in a very difficult time under less-than-superb Secretaries of the Interior. All too often the Interior Department's top post has been filled on more of a political basis rather than on one of capabilities. I do give Dallas great credit for keeping the USGS alive under some very threatening political situations.

People have said that Dallas always wanted to be loved, and he did put up with a lot from the powers above. I believe that he was truly liked as a person by everyone, but that now appears not to have been enough. A common perception seems to be that the USGS drifted under Dallas, that opportunities were missed, and that clear goals and plans for reaching those goals were less clearly defined than they might have been. But we all knew that Dallas was devoted to scientific excellence, and the USGS did survive.

One story will illustrate the type of difficulty Dallas had with James Watt, the Reagan administration's first Secretary of Interior. At a meeting with Secretary Watt, Dallas was enthusiastically reporting about how USGS water scientists had used new microwave imaging techniques to map previously unknown underground water channels in rocks a million or more years old. Secretary Watt shook his head and retorted, "That can't be so, Dr. Peck, the world is only 6,000 years old!" When I asked Dallas about the story, he said it was absolutely true, and added, "What could I say or do, other than just stop right there?"

Scott: That sounds like Jim Watt, for sure.

Gordon P. Eaton (1994 to Present)

Wallace: Gordon P. Eaton came after Dallas Peck. Perhaps others should tell you about Gordie Eaton, from the vantage point of years beyond 1995, but I will say just a word or two after a little over of a year with Gordie as USGS director. He is an old USGS hand, having been with the Survey until he departed to serve as provost at Texas A&M. Subsequently he was president of Iowa State

University, and director of the Lamont-Doherty Geological Observatory (later Lamont-Doherty Earth Observatory), before returning in 1994 to become director of USGS.

I must record that since Eaton became Director, the USGS has faced a severe challenge. The Republican Party, now in control of the Congress, presented its "Contract With America", which, in an appendix at least, calls specifically for abolishing the USGS. With help from many sources, Eaton successfully thwarted this action. But he has also shown clearly that he wants to change the geologic program drastically, and to emphasize short-term needs and solutions over fundamental, deep-rooted understanding of earth-science problems. He has emphasized management over world-class science and scientific excellence. He has spoken out repeatedly against "curiosity-driven" research in the USGS.

He selected for Chief Geologist a person who is not from the Geologic Division where superb candidates abound, a person Eaton himself praised primarily as a manager. This clearly declared Eaton's vote of no confidence toward the Division and its personnel. Eaton's position is, as best, difficult to comprehend, and even can be interpreted in sinister terms. Scientists of the Division have led the world in innovative science and application for over a century, and no group has displayed greater dedication to serving society than has the scientific staff of the Geologic Division.

Many senior scientists in USGS view such decisions as anti-science. In Eaton's defense, however, we must acknowledge an anti-science, anti-government sentiment that is abroad in the United States. Perhaps Eaton will be proven to have shifted directions in an appropriate way to permit the USGS to survive. But the upwelling of non-administrative leadership, of which Pres Cloud spoke so eloquently in his centennial history of the USGS (see below), seems now to be in jeopardy. I believe that in the long run science in the USGS will almost surely suffer. I also must report my own pessimism about the USGS maintaining its reputation as the preeminent earth-science organization in the world. I hope my pessimism proves to be ill-founded.

But let me end on a more optimistic note. The USGS has many superb scientists in early to midcareer who, given the proper encouragement, should be able to regenerate the tradition of excellence in science, dedication to individual initiative, and loyalty to the USGS and its goal of service to society which flourished over the past century.

Retrospective: The U. S. Geological Survey

Scott: This might be a good place to give your overall perspective of the USGS. You have touched on many aspects of USGS. How would you sum it all up?

Wallace: To respond, I'll focus here on the people angle, rather than the Survey's duties as stated in its organic act and later directives, i.e., to provide factual, unbiased information on all earth-science issues. I think it bears repeating that USGS people have displayed unusual individual initiative and integrity, and have been dedicated to excellence in science and service to society. USGSers have been an unusually bright, enthusiastic group of earth scientists, with a consuming sense of individual responsibility.

Scott: You clearly have a high opinion of the USGS.

Wallace: It is also obvious that I am biased! I cannot imagine a better outfit to have worked for, or a more exciting group with whom to have been associated. I doubt that I can characterize the USGS as I have known it any better than Preston Cloud did in his paper of 1980. (Cloud, Preston, 1980, "The Improbable Bureaucracy: The United States Geological Survey, 1879-1979: Proceedings of the American Philosophical Society, v.124, n.3, June 1980, pp.155-167.) On page 155, Cloud wrote:

> The USGS, although a bureau of the Department of Interior, defies conventional perceptions of bureaucracy. Instead of being narrow, rigid, formal, dependent on precedent, and lacking in initiative and resourcefulness, the USGS characteristically responds to challenges in fresh and enterprising ways. With few exception it has done so from the beginning under nine different directors and hopefully will continue to do so under its now tenth and future directors.

Cloud added:

> What I would stress ... is the importance to USGS distinction of the non-administrative leaderships that welled-up and continues to well-up under the traditional Survey policy of encouraging and rewarding individual initiative. (p.161)

Scott: "Non-administrative leaderships" refers to people whose formal training and professional work has been in substantive science. In contrast to those whose educations and careers have largely been in administration and management.

While management skills are essential in large organizations, it is a big mistake to freeze out the subject-matter people for high-level administrative jobs. Cloud's words highlight a remarkable and very valuable USGS tradition.

Wallace: Yes, the dedication of individuals to the USGS and the encouragement of individuals by managers to accept personal responsibility for innovation and scientific creativity have characterized operations within the USGS. The distinct change in these attitudes in 1994 and 1995, which places more faith in management than in the individual scientists is a sad shift indeed. I hope in time the great traditions of the USGS are rekindled and once again flourish.

Scott: What you have been saying is a very good statement on the importance of USGS research and outreach. It fits right in with the powerful case that can be made for the importance of other such publicly supported facilities in addition to USGS, such as the National Institutes of Health, and the national laboratories such as at Los Alamos, Livermore, etc. They provide some crucial services of kinds that we cannot expect to get from private sector alone.

Wallace: I agree.

XII RETIREMENT AND REFLECTIONS

Wallace: I retired in 1987 a few days short of age 71, after over 40 years service.

Scott: For the record, at this point would you summarize your working history with USGS?

Wallace: I started with the USGS in May 1942, and worked fulltime until the summer of 1946, when I moved to teach at Washington State College in Pullman. I taught there until the summer of 1951, when I returned to USGS full time. During the years of teaching, I believe I kept a status of "when actually employed" (WAE), although it may have lapsed for a few months. Also I actively worked for USGS during summers, from 1943 until I rejoined the Survey fulltime in 1951.

In short, I may have been at USGS for 53 years, with five years of dual service at Washington State and the Survey. Civil Service, however, would credit me with only a little less than 42 years for annuity calculations (cheating me out of a couple of thousand dollars per year). duty.

Scott: When you retired, did you immediately become "Emeritus"?

Wallace: No, at the time I retired an "Emeritus" program had not been established formally, but has been since.

Scott: Say a word or two more about the "Emeritus" designation.

Wallace: I am called both a "volunteer" and a "Scientist Emeritus" or a "Geologist Emeritus." Fortunately, the USGS is reinforcing the Scientist Emeritus program. I do enjoy the association with my colleagues here at the USGS, and at age 79 I seem to be able to continue in my happy state for a few more months at least. The USGS has been the best of all places to work.

On retiring in 1987, I still had many research papers based on foreign travel and work in the Basin and Range to finish, and we had long talked about an overview paper on the San Andreas fault. For the first few years of retirement, I operated on a part-time, partial-pay basis, but the formal retirement status gave me a chance to be more flexible in meshing personal matters and work for the USGS. John Filson, then Office Chief, as well as former Office Chiefs, Bob Hamilton, and Rob Wesson were kind enough to encourage me to stay on.

Volume on the San Andreas Fault System

Scott: You mentioned the volume on the San Andreas fault. It is a handsome job of publishing and seems very comprehensive. Would you say just a few more words characterizing the volume, how it was done and what you tried to accomplish with it?

Wallace: Thank you, I will. The San Andreas fault had received such world-wide recognition that visiting scientists came to our office seeking a trip along the fault and an overview statement about it. And as I mentioned, for years we had talked about doing an overview paper about it. Several people had pointed to me as the one to prepare such an overview paper. I knew that I could not do it alone, however, because it involved so many specialties in geology and geophysics. But in retirement I felt I could take the time to ramrod a cooperative effort.

Aided by Joe Ziony and Bob Brown, we developed a plan and a team of USGS authors to tackle the job. I wanted USGS people for two reasons: expertise was here in abundance, and I would not have to arm-wrestle people at a distance to get them to submit their chapters in a timely way. The Professional Paper (perhaps better referred to as the book) ended up with ten chapters by 14 authors, 283 pages and abundant photos, maps, and diagrams. The volume was intended to reach primarily a fairly well-informed earth-science audience, but also a general-science and lay audience. It has enjoyed rather wide circulation, including adoption as a text or reference in many classes.

I view the book as a brief review of a very complex earth-science problem. In the preface I observe: "This volume represents but a small punctuation mark in the early stage of our understanding of the San Andreas fault system ... Most of the story has yet to be learned."
Thanks to some priorities set by Dallas Peck, USGS Director, we had a colorful USGS Professional Paper printed in 1990. I am happy with the product.

(Wallace, R.E., (ed.), The San Andreas Fault System, California. U.S. Geological Survey Professional Paper 1515, 1990.)

Scott: Do you think of any other elements that have influenced the evolution of the earthquake hazard reduction program?

Wallace: The story I have told has been very selective and incomplete. I apologize for important omissions of people and institutions that had major influences. Nor can I begin to paint a story of the constant and chameleon-like changes inherent in the flow of people and activities through a program like this. I can identify periods of five to ten years in which the same names appear on one advisory panel after another. Then ten years later, the whole cadre of names has changed, preferred agenda change, and institutions disappeared and new ones have been created. Individuals retire or change jobs - some much-needed individuals are inconsiderate enough to die. The fact of constant change cannot be overemphasized.

Closing Observations

Scott: We are nearing the end of a very fruitful set of oral history interviews. Before you end, are there any further observations that you would like to mention from your half-century of experience in USGS?

Consensus and Conflict

Wallace: One thing I would emphasize is the central nature of the processes of consensus-building. Consensus-building is what it is called, but "consensus" is really too benign a word for the process I observed and in which I participated. It often involved a real struggle, sometimes a fierce struggle, over the distribution of money to support one program element or another. That really cannot be avoided, so the battles are inevitable.

Scott: Yes, that is a universal feature of life in a complex world. You see it in the public and private sectors, and among practitioners and researchers. Sometimes it seems rather pronounced in the worlds of academia and research. I guess the main hope is to conduct the struggle with a degree of fair play and civility. But that is hard to do, especially when people see reputations or maybe even careers as being at stake.

Wallace: The personal factor is important. Individuals ultimately determine outcomes, working through their personalities, and with their strengths, weaknesses, insights and persuasiveness. Understandably the process can be abrasive and may appears messy, disorderly and contentious. Strong personalities emerge now and then to create partial order within the disorder, at least for a time. Sometimes a welcome sense of community prevails and consensus seems to flower, especially when funds are adequate for the would-be players of the moment. But funds always attract additional players, and new power struggles ensue.

Those in the heat of battle sometimes forget that honest differences of opinion are legitimate. Too often, such battles can turn into destructive, personal vendettas. On the other hand, some elements of tension are always healthy for a dynamic intellectual environment. But there are never enough funds to carry all programs along at the speed every advocate hopes for.

We have seen major disciplines contending for influence in the earthquake program, including earth sciences, engineering, emergency response planners, social sciences, and public administration. Also there are analogous contests among subdisciplines within the major disciplines. Similarly, various agencies and organizations have been fighting for roles and funds.

A Few Generic Lessons

Wallace: I can close by summarizing some simple generic lessons I have gleaned from my own experience as set forth in these memoirs. National agendas have to be hammered out over time with a lot of effort. They are created by dedicated people who must expend enormous amounts of energy. While such agendas are not necessarily long-lived, they do seem to thrive if the principal individuals continue to be personally dedicated and energetic. On the other hand, competing national priorities can overwhelm smaller efforts such as the earthquake program. Also, priorities are transformed in wartime, and in peacetime they shift as economic factors wax and wane.

It is a truism in the earthquake field that each disastrous earthquake creates a temporary unity of purpose and a renewed determination throughout the community of specialists to seek better means of disaster reduction. Damaging earthquakes also generate a vivid sense of urgency for action within the public.

Scott: Yes, and in the immediate aftermath of a disaster, it becomes easier to get a consensus on action to help forestall or reduce similar future disasters, and to make significant progress on earthquake disaster programs.

Some Darkening Shadows: Changing Concepts of Government

Wallace: As these words are written, a major issue concerns the very role of the federal government itself. For a time, some members of Congress targeted the USGS for abolition. "Reinventing government" has become a popular phrase. An antagonism toward all government seems to pervade some parts of society, and anti-tax sentiments flourish. In such a climate, support of science is being challenged daily.

Conclusion: A Great Half Century Ended

Wallace: The joyful and satisfying memories of a great half century will be with me forever. But I also feel an overwhelming sadness at no longer being a fully active and effective investigator, explorer, and participant in the earthquake hazard reduction program, and other socially and scientifically important activities of that "improbable bureaucracy"--the USGS.

APPENDIX

SELECTED PUBLICATIONS

by Robert E. Wallace

1946 - Wallace, R. E., A Miocene mammalian fauna from Beatty Buttes, Oregon: Carnegie Inst. of Washington, Pub. n. 551, Contributions to Paleontology, p. 113-134.

1946 - Wallace, R. E., Terrain analysis of the vicinity of Northway, Alaska - with special reference to permafrost: U.S. Geological Survey Permafrost Program Progress Report, n. 3, p. 1-34.

1948 - Wallace, R. E., Cave-in lakes in the Nabesna, Chisana, and Tanana River Valleys, eastern Alaska: Journal of Geology, v. 56, n. 3. p. 171-181.

1948 - Wallace, R. E., A stereographic calculator: Journal of Geology, v.45, n. 5, p. 488-490.

1949 - Wallace, R. E., Structure of a portion of the San Andreas rift in southern California: Bulletin of the Geological Society of America, v. 60, n. 4, p. 781-806.

1950 - Wallace, R. E., Determination of dip and strike by indirect observations in the field and from aerial photographs; a solution by stereographic projecting: Journal of Geology, v. 58, n. 3, p. 51-58.

1951 - Wallace, R. E., Geometry of shearing stress and relation to faulting: Journal of Geology, v. 59, n. 2, p. 118-130.

1953 - Wallace, R. E., Hobbs, S. W., and Griggs, A. B., Magmatic source of Idaho ores; a discussion: Northwest Science, v. 27, n. 2, p. 73-76.

1955 - Cady, W. M., Wallace, R. E., Hoare, J. M., and Webber, E. J., The central Kuskokwim region, Alaska: U. S. Geological Survey Professional Paper 286, pp. 1-132.

1956 - Wallace, R. E., and Hosterman, J. W., Reconnaissance geology of western Mineral County, Montana: U. S. Geological Survey Bulletin 1027-M, p. 575-606.

1956 - Wallace, R. E., and Calkins, J. A., Reconnaissance geologic map of the Izee and Logdell quadrangles, Oregon: U. S. Geological Survey Mineral Investigation Field Studies Map MF-82.

1959 - Wallace, R. E., Graphic solution of some earth-satellite problems by use of the stereographic net: Journal of the British Interplanetary Society, v. 17, n. 5, p. 120-123.

1960 - Wallace, R. E., Griggs, A. B., Cambell, A. B., and Hobbs, S. W., Tectonic setting of the Coeur d'Alene district, Idaho, in Geological Survey Research 1960; U. S. Geological Survey Professional Paper 400-B, p. B25-B27.

1960 - Wallace, R. E., Tatlock, D. B., Silberling, N. J., Intrusive rocks of Permian and Triassic age in the Humboldt Range, Nevada: Geological Survey research 1960: U. S. Geological Survey Professional Paper 4400-B, p. B291-B293.

1961 - Wallace, R. E., Deflation in Buena Vista Valley, Pershing County, Nevada; in Geological Survey research 1961: U. S. Geological Survey Professional Paper 424-D, p. D242-D244.

1962 - Wallace, R. E., and Tatlock, D. B., Suggestions for prospecting in the Humboldt Range and adjacent areas, Nevada, in Geological Survey research 1962: U. S. Geological Survey Professional Paper 450-B, p. B3-B5.

1964 - Wallace, R. E., (1) Topography; (2) Structural evolution, in Mineral and water resources of Nevada: U. S. Senate Document, 88th Congress, 2d Session, n. 87, p. 11-12, 32-39.

1964 - Wallace, R. E., and Silberling, N. J., 1964, Westward tectonic overriding during Mesozoic time in north-central Nevada, in Geological Survey Research 1964: U. S. Geological Survey Professional Paper 501-C. p C10-C13.

1965 - Hobbs, S. W., Griggs, A. B., Wallace, R. E., and Campbell, A. B., Geology of the Coeur d'Alene district, Shoshone County, Idaho: U. S. Geological Survey Professional Paper 487, 139 p.

1966 - Wallace, R. E., and Moxham, R. M., Use of infrared imagery in study of the San Andreas fault system, California, in Geological Survey Research 1967: U. S. Geological Survey Professional Paper 575-D, p. D147-D156.
1966 - Plans for a Department of Geological Sciences at the University of Costa Rica: U. S. Geological Survey Technical Letter CR-12, 45 p.

1967 - Wallace, R. E., and Roth, E. F., Rates and patterns of progressive deformation, in Brown, R. D., Jr., and others, The Parkfield-Cholame, California earthquakes of June-August 1966 - Surface geologic effects, water-resources aspects, and preliminary seismic data: U. S. Geological Survey Professional paper 579, p. 23-40.

1967 - Silberling, N. J., and Wallace, R. E., Geology of the Imlay Quadrangle, Pershing County, Nevada: U. S. Geological Survey Geological Quad. Map GQ-666.

1968 - Wallace, R. E., Earthquake of August 19, 1966, Varto area, eastern Turkey: Bulletin of the Seismological Society of America, v. 58, n. 1, p.11-45.

1968 - Wallace, R. E., Notes on stream channels offset by the San Andreas fault, southern Coast Ranges, California, in Dickinson, W. R., and Grantz, Arthur, eds., Proceedings of conference on geologic problems of San Andreas fault system: Stanford University Publications in Geological Science, v. 11, p. 6-21.

1968 - Brown, R. D., Jr., and Wallace, R. E., Current and historic fault movement along the San Andreas fault between Paicines and Camp Dix, California, in Dickinson, W. R., and Grantz, Arthur, eds., Proceedings of conference on geologic problems of San Andreas fault system; Stanford University Pubs. Geologic Science, v. 11, p. 22-41.

1968 - Wallace, R. E., Geologic factors in minimizing earthquake hazards: American Institute of Architects Journal, v. XLX, n. 1, p 55-69.

1969 - Silberling, N. J., and Wallace, R. E.., 1969, Stratigraphy of the Star Peak Group (Triassic) and overlying lower Mesozoic rocks, Humboldt Range, Nevada: U. S. Geological Survey Professional Paper 592, 50 p.

1969 - Wallace, R. E., Geologic factors in earthquake damage: Symposium volume on earthquake hazard minimization, Central Treaty Organization, Ankara, Turkey, July 22-27, 1968: Office of U. S. Economic Coordinator for CENTO Affairs, p. 123-133.

1969 - Wallace, R. E., Tatlock, D. B., Silbering, N. J., and Irwin, W. P., Geology of the Unionville quadrangle, Pershing County, Nevada: U. S. Geological Survey Geological Quadrangle Map GQ-820.

1969 - Wallace, R. E., Silberling, N. J., Irwin, W. P., and Tatlock, D. B., Geology of the Buffalo Mountain quadrangle, Pershing and Churchill Counties, Nevada: U. S. Geological Quadrangle Map GQ-821.

1970 - Wallace, R. E., Earthquake recurrence intervals on the San Andreas fault: Bulletin of the Geological Society of America, v. 81, p.2875-2890.

1970 - Vedder, J. G., and Wallace, R. E., Map showing recently active breaks along the San Andreas and related faults between Cholame Valley and Tejon Pass, California: U. S. Geological Survey Miscellaneous Geological Investigations Map I-574.

1972 - LaMarche, V. C., and Wallace, R. E., Evaluation of effects on trees of past movements on the San Andreas fault, northern California: Bulletin of the Geological Society of America, v. 83, p 2665-2675.

1973 - Wallace, R. E., Surface fracture patterns along the San Andreas fault, in Kovach, R. L. and Nur, A., eds., Proceedings of the conference on tectonic problems of the San Andreas fault system: Stanford University Publication Geological Science, v. XIII, p. 173-180.

1973 - Wallace, R. E., Plan for zoning Managua, Nicaragua, to reduce hazards of surface faulting: Earthquake Engineering Research Institute Proceedings, Conference on Managua, Nicaragua, earthquake of December 23, 1972, v.1, p. 173-180.

1974 - Wallace, R. E., Goals, strategy, and tasks of the earthquake hazard reduction program: U. S. Geological Survey Circular 701, 26 p.

1975 - Wallace, R. E., The San Andreas fault in the Carrizo Plain-Temblor Range region, California, in Crowell, J. C., ed., San Andreas fault in southern California: California Division of Mines and Geology Spec. Report 118, p. 241-250.

1975 - Wallace, R. E., Basin and Range province, in Fairbridge, R. W., ed., The encyclopedia of world regional geology, pt. 1, Western Hemisphere: Dowden, Hutchinson and Ross, Inc., p. 541-548.

1976 - Wallace, R. E., Earthquake waves made visible by stroboscopic effect: Seismological Society of America Bulletin, v. 66, n. 5., p. 1771-1772.

1976 - Wallace, R. E., The Talas-Fergana fault, Kirgiz and Kazakh, USSR: U. S. Geological Survey Earthquake Information Bulletin, v. 8, n. 4, p. 4-13.

1977 - Wallace, R. E., Time-history analysis of fault scarps and traces -- a longer view of seismicity: World Conference on Earthquake Engineering, 6th, New Delhi, India, Proceedings, v. 2, p. 409-412.

1977 - Wallace, R. E., Profiles and ages of young fault scarps, north-central Nevada: Bulletin of the Geological Society of America, v. 88, n. 9, p. 1267-1281.

1978 - Wallace, R. E., Geometry and rates of change of fault-generated range fronts, north-central Nevada: U. S. Geological Survey Journal of Research, v. 6, n. 5, p. 637-649.

1979 - Wallace, R. E., Map of young fault scarps related to earthquakes in northcentral Nevada: U. S. Geological Survey Open-File Map n. 79-1554.

1979 - Wallace, R. E., Earthquakes and the prefractured state of the Western part of the North American continent: in Proceedings of the International Research Conference on Intra-continental Earthquakes, 1979, Ohrid, Yugoslavia, p. 69-81.

1980 - Wallace, R. E., Degradation of the Hebgen Lake Fault scarps of 1959: Geology, v. 8, n. 5, p. 225-229.

1980 - Wallace, R. E., and Teng, Ta-liang, Prediction of the Sungpan-Pingwu Earthquakes, August 1976: Bulletin of the Seismological Society of America, v. 70, n. 4, p. 1199-1223.

1980 - Wallace, R. E., Active faults, paleoseismology and earthquake hazards: in Proceedings of the Seventh World Conference on Earthquake Engineering, Istanbul, Turkey, Sept., 1980. p. 115-122.

1980 - Wallace, R. E., G. K. Gilbert's studies of faults, scarps and earthquakes: Geological Society of America Special Paper 183, p. 35-44.

1981 - Wallace, R. E., Active faults, paleoseismology and earthquake hazards in the western United States: in Simpson, David W., and Richards, Paul G., eds., Earthquake Prediction -- An International Review, Maurice Ewing Series 4: American Geophysical Union, p. 209-216.

1981 - Wallace, R. E., Paleoseismology: Geological Society of America Annual Meeting, Cincinnati, Ohio, Nov. 3-5, 1981, p. 575.

1982 - Wallace, R. E., Patterns of late Quaternary faulting in the Great Basin Province, western United States: Proceedings volume, International Symposium and Study Tour on Continental Seismicity and Earthquake Prediction, Beijing, China, September, 1982.

1983 - Wallace, R. E., Tangshan, six years later: Earthquake Information Bulletin, May-June 1983, v. 15, n. 3, p. 102-107.

1984 - Wallace, R. E., and Whitney, R. A., Late Quaternary history of the Stillwater Seismic Gap, Nevada: Seismological Society of America Bulletin, v. 74, n.1, p. 301-314.

1984 - Wallace, R. E., Patterns and timing of late Quaternary faulting in the Great Basin Province and relation to some regional tectonic features: Journal of Geophysical Research, v. 89, p. 5763-5769.

1984 - Wallace, R. E., Fault scarps formed during the earthquake of October 2, 1915, Pleasant Valley, Nevada and some tectonic implications: U. S. Geological Survey Professional Paper 1247A.

1984 - Wallace, R. E., Davis, J. F., and McNally, Karen C., Terms for expressing earthquake potential, prediction and probability: Seismological Society of America Bulletin, v. 74, n. 5, p. 1819-1825.

1985 - Hanks, T. C., and Wallace, R. E., Morphological analysis of the Lake Lahontan shoreline and beachfront fault scarps, Pershing County, Nevada: Seismological Society of America, v. 75, p. 835-846.

1985 - Wesson, R. L., and Wallace, R. E., Predicting the next great earthquake in California: Scientific American, v. 252, n. 2, p. 35-43.

1985 - Hill, D. P., Wallace, R. E., Cockerham, R. S., Review of evidence on potential for major earthquakes and volcanism in the Long Valley-Mono Craters regions of eastern California: Proceedings Volume, U. S.- Japan Conference on Earthquake Prediction, Earthquake Prediction Research 3, p. 551-574.

1985 - Zhang, Buchun, Liao, Yuhua, Guo, Shunmin, Wallace, R. E., Bucknam, R. C., and Hanks, T. C., Fault scarps related to the 1739 earthquake, and seismicity of the Yinchuan Graben, Ningxia Huizu Zizhiqu, China: Seismological Society of America Bulletin, v. 76, n. 5, p. 1253-1287.

1985 - Sieh, Kerry, and Wallace, R. E., The San Andreas Fault at Wallace Creek, San Luis Obispo County, California: Geological Society of America, Cordilleran Section Field Guide, p. 228-238.

1986 - National Research Council, Studies in Geophysics, Panel on Active Tectonics (Wallace, R. E., Chairman), Active Tectonics: National Academy Press, Washington, D. C., 266 p.

1986 - Wallace, R. E., and Morris, H. T., Characteristics of faults and shear zones in deep mines: PAGEOPH, b. 124, n. 1-2, p. 107-125.

1987 - Wallace, R. E., Grouping and migration of surface faulting and variations in slip rates on faults in the Great Basin province: Seismological Society of America Bulletin, v. 77, n. 3, p. 868-876.

1987 - Wallace, R. E., A perspective of paleoseismology: in Directions in paleoseismology, Crone, A. J., and Omdahl, E. M. (eds.): U. S. Geological Survey Open-file report 87-673, n. 7-13.

1988 - National Research Council, Panel on Seismic Hazard Analysis, (Aki, K., Chairman, Wallace, R. E. and others members), Probabilistic Seismic Hazard Analysis: National Academy Press, 97 p.

1989 - Wallace, R. E., Fault-plane segmentation in brittle crust and anisotropy in loading system: in Proceedings of Workshop XLV, Fault segmentation and controls of rupture initiation and termination, Schwartz, D. P., and Sibson, R. H., (eds.): U. S. Geological Survey Open-file report 89-315, p. 400-408.

1990 - Wallace, R. E., (editor) The San Andreas Fault System, California: U. S. Geological Survey Prof. Paper 1515, 283 p.

1990 - The Governor's Board of Inquiry on the 1989 Loma Prieta Earthquake, Housner, G. W., Chairman (Wallace, R. E. and others, members), Competing Against Time: Report to Governor George Deukmejian; State of California, Office of Planning and Research, 264 p.

1992 - Wallace, R. E., Ground-squirrel mounds and patterned ground along the San Andreas fault in central California: U. S. Geological Survey, Open-file report n.91-149, p. 1-21. Also: Geological Society of America, Cordilleran Section, 1992 Abstracts with Programs, v.24, n.5 p.88.

1992 - Wallace, R. E., Survival: Earthquakes and Volcanoes, v. 24, n.3, p.94-95.

1992 - Brown, R. D., Jr., Wallace, R. E., and Hill, D. P., The San Andreas Fault System, California, U.S.A.: Annales Tectonicae. Special Issue - Supplement to V. VI, p. 261-284.

1994 - Wallace, R. E., Bucknam, R. C., and Hanks, T. C., 1994, Ancient engineering geology projects in China; A canal system in Ganzu Province and trenching along the Great Wall in Ningxia Hui Autonomous Region: Engineering Geology , v. 36, p. 183-195.