An Interview with

RUSS DYER

An Oral History produced by
Robert D. McCracken

Yucca Mountain Series

Nye County Town History Project
Nye County, Nevada
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CONTENTS

PREFACE

The Nye County Town History Project (NCTHP) engages in interviewing people who can provide firsthand descriptions of the individuals, events, and places that give history its substance. The products of this research are the recordings of the interviews and their transcriptions.

In themselves, oral history interviews are *not* history. However, they often contain valuable primary source material, as useful in the process of historiography as the written sources to which historians have customarily turned. Verifying the accuracy of all of the statements made in the course of an interview would require more time and money than the NCTHP's operating budget permits. The program can vouch that the statements were made, but it cannot attest that they are free of error. Accordingly, oral histories should be read with the same prudence that the reader exercises when consulting government records, newspaper accounts, diaries, and other sources of historical information.

It is the policy of the NCTHP to produce transcripts that are as close to verbatim as possible, but some alteration of the text is generally both unavoidable and desirable. When human speech is captured in print the result can be a morass of tangled syntax, false starts, and incomplete sentences, sometimes verging on incoherence. The type font contains no symbols for the physical gestures and the diverse vocal modulations that are integral parts of communication through speech. Experience shows that totally verbatim transcripts are often largely unreadable and therefore a waste of the resources expended in their production. While keeping alterations to a minimum the NCTHP will, in preparing a text:

a. generally delete false starts, redundancies and the *uhs*, *ahs* and other noises with which speech is often sprinkled;

b. occasionally compress language that would be confusing to the reader in unaltered form;

c. rarely shift a portion of a transcript to place it in its proper context;

d. enclose in [brackets] explanatory information or words that were not uttered but have been added to render the text intelligible; and

e. make every effort to correctly spell the names of all individuals and places, recognizing that an occasional word may be misspelled because no authoritative source on its correct spelling was found.

ACKNOWLEDGMENTS

As project director, I would like to express my deep appreciation to those who participated in the Nye County Town History Project (NCTHP). It was an honor and a privilege to have the opportunity to obtain oral histories from so many wonderful individuals. I was welcomed into many homes—in many cases as a stranger—and was allowed to share in the recollection of local history. In a number of cases I had the opportunity to interview Nye County residents whom I have long known and admired; these experiences were especially gratifying. I thank the residents throughout Nye County and Nevada—too numerous to mention by name—who provided assistance, information, and photographs. They helped make the successful completion of this project possible.

Appreciation goes to Chairman Joe S. Garcia, Jr., Robert N. "Bobby" Revert, and Patricia S. Mankins, the Nye County commissioners who initiated this project in 1987. Subsequently, Commissioners Richard L. Carver, Dave Hannigan, and Barbara J. Raper provided support. In this current round of interviews, Nye County Commissioners Butch Borasky, Lorinda A. Wichman, Joni Eastley, Gary Hollis, Fely Quitevis, and Dan Schinhofen provided unyielding support. Stephen T. Bradhurst, Jr., planning consultant for Nye County, gave enthusiastic support and advocacy of the program within Nye County in its first years. More recently, Darrell Lacy, Director, Nye County Nuclear Waste Repository Project Office, gave his strong support. The United States Department of Energy, through Mr. Lacy's office, provided funds for subsequent rounds of interviews. Thanks are extended to Commissioners Eastley and Hollis and to Mr. Lacy for their input regarding the conduct of this research and for serving as a sounding board when methodological problems were worked out. These interviews would never have become a reality without the enthusiastic support of the Nye County commissioners and Mr. Lacy.

Jean Charney served as editor and administrative assistant throughout the project; her services have been indispensable. Valerie Brown, Jean Charney, Robert B. Clark, Anna Lee Halsig, Debra Ann MacEachen, Lynn E. Riedesel, and Marcella Wilkinson transcribed a number of interviews, as did the staff of Pioneer Transcription Services in Penn Valley, California. Julie Lancaster and Suzy McCoy provided project coordination. Proofreading, editing, and indexing were provided at various times by Marilyn Anderson, Joni Eastley, Michael Haldeman, Julie Lancaster, Teri Jurgens Lefever, and Darlene Morse. Joni Eastley proofed most the manuscripts and often double-checked, as accurately as possible, the spelling of people's names and the names of their children and other relatives. Jeanne Sharp Howerton provided digital services and consultation. Much-deserved thanks are extended to all these persons.

All material for the NCTHP was prepared with the support of the Nye County Nuclear Waste Repository Office, funded by the U.S. Department of Energy. However, any opinions, findings, conclusions, or recommendations expressed herein are those of the author and the interviewees and do not necessarily reflect the views of Nye County or the U.S. DOE.

—Robert D. McCracken
2013

INTRODUCTION

Historians generally consider the year 1890 as the close of the American frontier. By then, most of the western United States had been settled, ranches and farms developed, communities established, and roads and railroads constructed. The mining boomtowns, based on the lure of overnight riches from newly discovered mineral deposits, were but a memory.

Nevada was granted statehood in 1864. But examination of any map of the state from the late 1800s shows that, although most of the state had been mapped and its geographical features named, a vast region—stretching from Belmont south to the Las Vegas meadows, comprising most of Nye County—remained largely unsettled and unmapped. In 1890, most of southcentral Nevada remained very much a frontier, and it continued to be so for at least another twenty years.

The spectacular mining booms at Tonopah (1900), Goldfield (1902), Rhyolite (1904), Manhattan (1905), and Round Mountain (1906) represent the last major flowering of what might be called the Old West in the United States. Consequently, southcentral Nevada, notably Nye County, remains close to the American frontier; closer, perhaps, than any other region of the American West. In a real sense, a significant part of the frontier can still be found in southcentral Nevada. It exists in the attitudes, values, lifestyles, and memories of area residents. The frontier-like character of the area also is visible in the relatively undisturbed quality of the natural environment.

Aware of Nye County's close ties to our nation's frontier past, and recognizing that few written sources on local history are available, especially after about 1920, the Nye County Commissioners initiated the Nye County Town History Project (NCTHP) in 1987. The NCTHP represents an effort to systematically collect and preserve information on the history of Nye County. The centerpiece of the NCTHP is a large set of interviews conducted with individuals who had knowledge of local history. Each interview was recorded, transcribed, and then edited lightly to preserve the language and speech patterns of those interviewed. All oral history interviews have been printed on acid-free paper and bound and archived in Nye County libraries, Special Collections in the Lied Library at the University of Nevada at Las Vegas, and at other archival sites located throughout Nevada. The interviews vary in length and detail, but together they form a never-before-available composite picture of each community's life and development. The collection of interviews for each community can be compared to a bouquet: Each flower in the bouquet is unique—some are large, others are small—yet each adds to the total image. In sum, the interviews provide a composite view of community and county history, revealing the flow of life and events for a part of Nevada that has heretofore been largely neglected by historians.

Collection of the oral histories has been accompanied by the assembling of a set of photographs depicting each community's history. These pictures have been obtained from participants in the oral history interviews and other present and past Nye County residents. In all, more than 700 photos have been collected and carefully identified. Complete sets of the photographs have been archived along with the oral histories.

On the basis of the oral histories as well as existing written sources, histories have been prepared for the major communities in Nye County. These histories have been published by Nye County Press, the county's publishing department. All the oral histories, as well as the community histories, are available on the Internet.

The Nye County Board of County Commissioners, while motivated by the study of history for history's sake, initiated the NCTHP in 1987 principally to collect information on the origin, history, traditions and quality of life of Nye County communities that would be impacted should the nation's first high-level nuclear waste repository be constructed deep inside Yucca Mountain on federal land in southcentral Nye County. Understanding such impacts would aid in their mitigation. Moreover, if the repository were built, it would remain a source of public interest for a very long time and future generations would likely want to know more about the people who once resided in the area. If the site should be found unsuitable and the repository never constructed, then materials compiled by the NCTHP would nevertheless be available for the use and enjoyment of future generations.

In 2010 the Nye County Commissioners and Darrell Lacy, Director, Nye County Nuclear Waste Repository Office, approved funding for collection of a round of oral histories from individuals who had played important roles in the U.S. Department of Energy's effort to assess the suitability of Yucca Mountain as a site for permanent storage of the nation's high-level nuclear waste. (The term high-level nuclear "waste" is very much a misnomer. The vast majority of the energy originally present in the nuclear fuel remains when the spent fuel—i.e., waste—is removed from the reactor. The spent fuel needs only to be reprocessed in order to make the remaining energy available for reuse. The proper term is thus not nuclear waste, but "spent nuclear fuel.")

The search for a permanent storage site for spent nuclear fuel was authorized by the Nuclear Waste Policy Act passed by Congress in 1982, as amended in 1987. Initially, several potential sites for construction of a permanent repository were considered; the 1987 legislation narrowed the suitability search to one site, Yucca Mountain.

Over the years, several thousand scientists and engineers participated in the study of Yucca Mountain's suitability for permanent storage of spent nuclear fuel, with several billion dollars expended on the effort. In all that research, nothing was found that would disqualify Yucca Mountain as a safe permanent storage site. Then, in 2008, in a step prescribed by the 1982 and 1987 legislation and based on the research findings, the U.S. Department of Energy applied to the Nuclear Regulatory Commission (NRC) for authorization to begin construction and move forward with development of a permanent repository at Yucca Mountain. The NRC was then required by law to evaluate the DOE's application and vote up or down on it—build it or forget it. That was and remains the law!

Beginning in 1983, the issue of possible construction of a permanent repository at Yucca Mountain gradually became controversial among many in Nevada. A number of high-profile politicians expressed strong opposition to the idea of storing spent fuel at Yucca Mountain from the beginning, regardless of the site's technical suitability. Several increased their political power through their outspoken opposition, essentially doing everything legally possible to block the effort. Public opinion in Las Vegas about Yucca Mountain, which was rather mild and mixed in the beginning, gradually became somewhat negative over the years, especially after 1987, when Yucca Mountain was singled out as the only candidate. Yet at the same time, public opinion in rural Nevada began and remained accepting of the program, especially in counties located closer to Yucca Mountain itself.

Nevada Congressman Harry Reid rode his strong outspoken opposition to Yucca Mountain to election to three terms in the U.S. Senate. In January 2007, he was chosen Senate Majority Leader by the majority Democrats. Newly elected President Barak Obama was highly dependent on Senator Reid for passage of his own legislative agenda. In order to mollify Senator Reid, all funding for any further work on Yucca Mountain was killed and the Nuclear Regulatory Commission (NRC), under Chairman Gregory Jaczko's maneuvering, was prevented from voting up or down on the Department of Energy's application to move forward with development of the repository. Many believe that a vote by the NRC was prevented because approval by the NRC staff was likely. Thus, one man—in this case, Senator Reid—in effect played a pivotal role in overriding the legal process prescribed by law. The findings of more than two decades of carefully conducted research costing several billion dollars were casually set aside.

In the meantime, spent nuclear fuel continues to accumulate at temporary storage facilities located near nuclear reactors at more than 45 locations around the country, some near very large cities, including Chicago.

About the Yucca Mountain Interviews

Dr. Michael Voegele held numerous positions with DOE contractors in assessing Yucca Mountain's suitability for permanent storage of spent nuclear fuel from 1981 to 2009, and continued after that as a consultant to Nye County. Perhaps more than anyone, he has a comprehensive view of the more than three decades of research about the safety of Yucca Mountain. He personally knew many of the scientists and engineers involved in the effort, including what their work consisted of and how it all came together. Given such expertise, he played a key role in selecting the majority of individuals we interviewed on Yucca Mountain history. Dr. Voegele assisted in many of the interviews and was also interviewed by me at length. Together, these interviews provide a boots-on-the-ground perspective of the assessment process in evaluating Yucca Mountain's suitability as a permanent repository site. Individuals interviewed were Drs. Thomas Cotton, Russ Dyer, Ned Elkins, Don Vieth, Jean Younker, and Michael Voegele.

Two Nye County officials who played significant roles in the Yucca Mountain effort for Nye County over the years were interviewed. Steve Bradhurst was the first director of the county's nuclear waste office, serving from 1983 through 1993. He was interviewed twice, in 1991 and again in 2010. Gary Hollis served as a Nye County Commissioner from 2005 to 2012 and in effect functioned as the commission's point man on the Yucca Mountain project during his time in office. He also was employed on drilling efforts associated with the assessment at Yucca Mountain prior to being elected a commissioner.

As noted, the idea of permanently storing spent nuclear fuel at Yucca Mountain became a heated political topic in Nevada beginning in 1983. To be fair and to give as broad a perspective as possible, we also conducted oral histories with politically focused individuals who represented differing viewpoints on Yucca Mountain. Former Nevada U.S. Senator Chic Hecht was a strong supporter of Yucca Mountain from the outset; he was interviewed in 2004. Former Nevada Governor, subsequently U.S. Senator, Richard Bryan, a strong and vigorous opponent of Yucca Mountain from the beginning, was also interviewed. At the conclusion of that interview in 2011, although by then I was a strong proponent of Yucca Mountain, Senator Bryan told me I "had been very fair." As a professional anthropologist, I take a lot of pride in his compliment. Bob Loux from almost the outset of the Yucca Mountain effort in 1983 functioned as the state of Nevada's anti-Yucca Mountain point man in his position as director of the state of Nevada Agency for Nuclear Projects. His job, as he acknowledged in his oral history, was to do anything legally possible to prevent a Yucca Mountain repository from ever becoming a reality. As with Senator Bryan, the interview with Mr. Loux went well.

Unfortunately, U.S. Senator Harry Reid, despite repeated requests, did not make himself available for an interview.

Three additional interviews were conducted outside this Yucca Mountain interviewing effort, though still using Yucca Mountain funds. These individuals played important roles in the Yucca Mountain assessment effort. Troy Wade previously worked for the Department of Energy; he was Assistant Secretary of Energy for Defense Programs in 1987–1988. He was interviewed as part of the NCTHP. Carl Gertz was Yucca Mountain Director from 1987 to 1993 and earlier worked for the DOE at the Idaho National Engineering Laboratory. Ed Mueller worked for a U.S. Department of Energy contractor as a liaison between the Yucca Mountain project office and counties impacted by Yucca Mountain located in Nevada and California. Both Mr. Gertz and Mr. Mueller were interviewed under the Esmeralda County History Project.

Together, these interviews comprise a body of valuable information obtained from individuals representing a variety of perspectives on this important effort in our nation's energy history. A credible history of Yucca Mountain cannot be written without incorporation of such variable knowledge and perspectives. If development of a permanent repository at Yucca Mountain moves forward, such information on how the site was evaluated and on the enormous amount of work involved in demonstrating its suitability will prove invaluable once construction begins. The same applies for selection of a second or third repository site, and for the efforts of other nations to construct repositories as well. If the Yucca Mountain effort never moves forward, these interviews still will be helpful in understanding the great effort that went into the evaluation of Yucca Mountain as a site for permanent storage of spent nuclear fuel. It unfortunately also tells how a good part of the more than $11 billion spent in evaluation was in large measure wasted, not for technical faults, but for political expediency.

Opinions expressed in this introduction and in the oral history interviews do not necessarily reflect the views of Nye or Esmeralda County officials.

These interviews have been organized into four volumes and published by Nye County Press, publishing imprint owned by Nye County, Nevada. A master index covering all four volumes is included.

—RDM
2013

INTRODUCTION BY MICHAEL VOEGELE

This series of interviews with Dr. Robert McCracken, undertaken as a part of the Nye County Town History Project, focused on the Yucca Mountain project. The Yucca Mountain project oral histories were developed as part of Nye County's efforts to record information related to the project as an ancillary part of the Yucca Mountain history exhibits in the Pahrump Valley Museum. The Nye County Commissioners believed that it was important to capture this historical information, as the Department of Energy had made every effort to disassemble the project and its records when the Obama Administration made the decision that the project was unworkable, and created the Blue Ribbon Commission on America's Nuclear Future to undertake a comprehensive review of policies for managing the back end of the nuclear fuel cycle, including all alternatives for the storage, processing, and disposal of civilian and defense used nuclear fuel and nuclear waste.

I worked with Dr. McCracken on the selection of the interviewees, and on several occasions participated as an interviewer. We consciously tried to identify interviewees who had been involved at the heart of the technical story of Yucca Mountain. Because funds were not unlimited, we needed to select carefully a relatively small number of interviewees. There were potential interviewees that we were not able to talk to because they had moved on to other venues following the Department of Energy's termination efforts and we simply were not able to accommodate schedule problems. We also tried to ensure a balance of perspectives on the project. Readers will find that the interviews tend to focus on a portion of the project's history or a major technical element of the project.

In recognition of this, we decided that there ought to be an interview that attempted to encompass as much of the project's history as possible, bearing in mind that the relevant history covers nearly 70 years.

The interview Dr. McCracken conducted with me is that document. While my tenure on the program was longer than most, I certainly do not have firsthand knowledge of the earlier parts of the program. I have, however, long studied the origins and early history of the project. My time on the high-level waste disposal program dates from the mid-1970s to the present, and I did not necessarily have significant involvement in everything talked about in that document. I am particularly indebted to Dr. Donald Vieth for the many discussions we had on the earlier parts of the program and found it fascinating how together we helped each other remember so much of the program's early history.

I felt it was important to offer the caveat that it would not surprise me to find that a reader remembered things differently than I did, or believed that I was mistaken in my recollections. I accept responsibility for any such errors; I can only say it has been a long time. It is also important to acknowledge the time so graciously accorded us by the interviewees. I suspect that some of them wish, as I do, that there had been references available to check some of our memories. I can only say thank you for trying to help us collect some important information.

I'd like to particularly thank Nye County Commissioners Gary Hollis and Joni Eastley for their enthusiastic and unwavering support for the interview project and the museum displays, and Dr. McCracken for his skill as an interviewer.

Michael D. Voegele
2013

Robert McCracken talking with Russ Dyer at his home in Las Vegas, Nevada, July 23 and 24, 2010.

CHAPTER ONE

RM: Russ, please tell me your name as it reads on your birth certificate and when and where you were born.

RD: I am James Russell Dyer, born June 6, 1947, in Chickasha, Oklahoma.

RM: What is your father's name, and when and where was he born?

RD: My father was David Houston Dyer—unfortunately, he's not with us anymore—known as Hugh Dyer, born December 7, 1920, in, I believe, Apache, Oklahoma. My mother was Wilma Johanna Collins Dyer, born September 20, 1924. She was born in Oklahoma, also. And unfortunately she is also gone.

RM: What was the background of her people?

RD: That's hard to determine. At least my great-great-grandfather, and maybe my great-great-great-grandfather, were in Oklahoma by the late 1800s.

RM: How did they earn a living?

RD: On my mother's side? Mother's side was farmers. On my dad's side, my dad and his dad worked in the oil fields.

RM: Oh, really? When did they discover oil in Oklahoma?

RD: It was really big in the late teens—1919, and the '20s and '30s. I know when my dad was growing up his father worked for a number of companies, but later he was the drilling superintendent for Sinclair. And so he moved from essentially oil field to oil field as the big booms came in and would supervise the drilling out of an oil field. They would move from place to place, I guess fairly regularly.

RM: Was he a geologist?

RD: No. He was literally uneducated.

RM: But he knew drilling?

RD: He knew drilling. He was very handy with things mechanical.

RM: And did you grow up in Chickasha? Where is that?

RD: Chickasha is not far from Norman; it's south of Oklahoma City. And I was just born there. My folks lived in Cement, Oklahoma, at the time, but the hospital in Grady County was in Chickasha, so that's where they went. Then we went back home, and I have no recollection of what that home was like.

My dad followed the job market and a few years later we moved to Odessa, Texas, and he worked in a refinery out of Odessa for a couple of years. That's where my brother was born. Then we moved to Oklahoma City about 1952, '53—maybe '51—and lived in Oklahoma City until 1955. We moved to Pampa, Texas, for a year—'55–'56—and then moved back to Oklahoma City in '57. They lived in that house for the rest of their lives.

I lived there until 1965, after I graduated from high school and went away to college. I ended up going to Rice University in Houston; got my degree in geology. The Navy ROTC program paid my way through college, so after graduation I owed them an amount of time. I stayed on active duty for five years and then went back to grad school and got my Ph.D. in geology from Stanford. That was in 1982. That's when I left school. I finished my dissertation and got my degree in 1983. But from 1982 to 1988 I taught geology as a structural geologist at the University of Texas at El Paso.

RM: What was it like, going to Stanford? That's pretty top drawer.

RD: Yes, it was. It was a wonderful environment. It's hard to describe because everything was just so open. If you wanted to find out about something, you could walk into the office of probably one of the world's three experts and ask them about it, and they would love to share that information with you. And if you had the time, they would set you up with a little project for a semester or a week or so, and you could explore things to your heart's content. As a place to truly encourage learning, I've never run across anything like it anywhere else. One of the other things that made it so special was that it wasn't just you, but everybody that was there had the same kind of outlook on learning—sharing knowledge and inquisitiveness. It was truly a magic place.

RM: And I gather it was very pleasant, psychologically.

RD: Oh, yes. Psychologically and physically, too; in the southern part of the Bay Area. At the time that Stanford University was formed, I think Leland Stanford—Senator Stanford, one of the robber barons of the California gold rush—was one of the richest men in the world. He decided to choose that place for his farm, his horse farm. And there was a reason—it's a gorgeous climate, gorgeous country. It was physically and psychologically very nice.

RM: I talked to a fellow once. I think he was a graduate student at Cal Tech, not a faculty member, but he said Cal Tech was a horrible environment because everybody was trying to make everybody else look stupid.

RD: Yes, that's always been kind of the persona of Cal Tech. I know a lot of people that did not like to go down and give invited talks at Cal Tech because everybody wanted to ask the question that would make him look like a fool.

RM: That's interesting. What was your focus in graduate school?

RD: Structural geology. And structural geology is understanding how earth materials behave over time. It turns out that silly putty is a pretty good approximation for a rock because it behaves in different ways at different time scales (what's called "strain rate," which is how fast you apply forces resulting in a strain or deformation to a rock). If you take silly putty and make a ball out of it, it'll bounce, so that's an elastic behavior. If you take that same ball and hit it with a hammer sharply, it'll shatter, so that's brittle behavior. Now, if you take that ball and stretch it, it behaves like a plastic material. And if you take that ball, roll it out into a cylinder, and hold it up, it'll slowly melt. So it's like a viscous fluid as it does that.

RM: And rock is like this?

RD: Rock is like this, depending on what time scale you look at. Whenever we hit a rock with a hammer, of course, we're doing something very quickly, so it behaves like a hard elastic material. But you were talking earlier, before we started recording, about how some of the rocks around Breckenridge are gneiss, with all the folds and squeezes in it, which looks like taffy that's been moved back and forth. So that's obviously a different kind of behavior.

RM: I think I've got an idea for my grandson's science fair project—silly putty.

[Laughs]

RD: So structural geology looks at how rocks behave and how, essentially, mountains are made. Now, one of the things that made this a special time to be at Stanford was that it was in the forefront of applying the ideas of plate tectonics—the late '60s really is when it started, the early '70s. And by the late '70s, we were trying to explain mountain ranges and sequences of mountain ranges and the highs and lows and so forth in terms of the movements of the plates. Some of the real early champions in successfully doing that were at Stanford. So it was a great time to be there.

RM: What an environment. That was a true revolution in thinking, wasn't it?

RD: It was, yes.

RM: Just as an aside, I took geology—I had a really good teacher—at the University of Colorado. This was in ’57, ’58. And we asked him, “Well, how come Africa and South America kind of fit together? Isn’t that an evidence of continental drift?”

He said, “People that think that are more interested in jigsaw puzzles than they are in geology.” That was the status of tectonics. And then 10 years later, you’ve got a revolution.

RD: That’s right.

RM: What did you do your dissertation on?

RD: I did my dissertation on the sandstones at Arches National Park, Utah, which had a number of parts to it. I mapped Arches National Park, which is about 110 square miles in southeastern Utah.

RM: You mapped the structures?

RD: I mapped the geology and the structures in the area. It’s made up of Jurassic sandstones that overlie a core, a salt anticline. The salt rises and has risen over time and deflated a couple of times, and on the flanks of this salt anticline are some of the most spectacular lineaments, what are called joints. A joint is a fracture in a rock, across which there is no discernible displacement. If there’s a significant displacement, we call it a fault. So this is just a crack in the rock. And some of these cracks can be seen from the air and go for miles and miles. And they are systematic. I mean, they’re not random patterns. There are very systematic patterns. Trying to figure out what those systematic patterns are and how they developed was one of the things that drew me there—trying to understand how these particular joints formed and what they could tell us about this type of fracture in rock.

And one of the reasons this is of interest . . . you may have read in the paper the other day that in West Virginia and some of the eastern counties where they're producing natural gas, one of the things they do is what is called hydrofracking of the rock, where you introduce fluid under pressure and sand or some chemical additives, and the pressure of the fluid causes the rock to crack, and then the sand that you're injecting holds those cracks open. Then it's easier to produce natural gas out of that material.

One of the sponsors for the work that I was doing was the petroleum industry. They were looking at developing a better understanding the fundamental mechanics behind the fracture in the rock. That's one of things that I was trying to understand—looking at these fractures in rock, trying to capture the tip of a fracture. So I didn't just go out and map things, I took a portable drill—it looked like a chain saw, except it had a little diamond core on it—and drilled into the rock to try to get samples of the very tip of one of these fractures to understand what was happening there.

The fractures I was looking at were in sandstone. When that fracture propagated it must have grown from something. Did it fracture sand grains at the very tip of the crack? Or did it just grow between sand grains? So trying to get enough samples where you could actually discern what processes went on there was a challenge. There were systematic sets of fractures. And some would go like this and some would go like this and overlay it.

RM: They would go in opposite directions?

RD: Well, not quite opposite. But it turns out they didn't just cross, they did one of three things. Sometimes they crossed. Sometimes they came in and curved parallel to this set. And sometimes they came in and curved perpendicular to that set. But these behaviors were consistent within a given area. You would find one of these behaviors, but you wouldn't find a mix of all three behaviors in a given area. So what was going on to cause that to happen? I did a lot of work in fracture mechanics and material science to try to understand what was going on that would create a stress field that would show these kinds of behaviors. That was really what most of the dissertation was about.

RM: And what was your conclusion for it? What was going on?

RD: It turns out if you have a preexisting fracture, then it disturbs the stress field in the material. And as another fracture is growing toward it, it senses that disturbed stress field. If you look at a mud crack, you'll see that they generally either grow where there are 120 degrees between the cracks, so that you have three cracks coming together, and they're coming together about like that. Or you'll find a crack and then another crack that grows perpendicular to it. And it turns out that if you have a free surface—if this crack already exists—the type of fractures that I was looking at grow in tension. They don't grow because of shear. They grow because either things are shrinking or things are being bent to create the tension. So as this crack is growing, it feels this free surface, and, because it's growing in tension toward this free surface, the principal stresses are absolutely parallel and perpendicular to this free face. So this new fracture comes in and must grow perpendicular to this face.

Now, if you have a preexisting fracture, and you have a little bit of sheer stress, where the two sides of the fracture are rubbing together just slightly, then you change the stress field so that another fracture growing into it feels this change in the stress field and curves like this. Kind of parallel. And I called this a "curving parallel" as opposed to the "curving perpendicular" geometry we talked about earlier, and developed the mathematics that allow you to make estimates of what the ratio of the stresses were. You couldn't say what the stresses actually were, but you could constrain what the ratio of the stresses was.

RM: By the crack?

RD: By the cracks and the orientation of the interaction between them.

RM: So you couldn't actually measure this stress field, but it's there?

RD: It's there. So that's what I ended up doing.

RM: And you figured all that out?

RD: Yes. It took me a while.

RM: How long did it take you?

RD: I was in grad school for six years. Started in '76, got out in '82, and then turned in the dissertation in '83.

RM: Are the Arches part of the San Raphael Swell in Utah? I often take I-70 across the San Raphael Swell, and it's one of my favorite places on the face of the earth.

RD: Oh, yes. A gorgeous place. But Arches is a different deal. San Raphael Swell is about, I guess, about 70 miles west. Arches is just north of Moab.

RM: Oh, okay. So your dissertation, then, helped the oil companies figure out how to do this fracturing in some degree?

RD: Well, maybe to some degree. It might have helped somebody better understand how fractures would propagate in that environment. At that time there were many ideas about how fractures developed. There was one school that said everything developed in shear—I mean, you had to be sliding things to make it happen. And that wasn't at all what I found.

RM: And does it split the grains, or does it kind go around them?

RD: As near as I can tell, it goes around them.

RM: So you did your dissertation and the defense, and then what?

RD: Did the dissertation and the defense, and then had to go get a job.

RM: Yes, go out in the "real world," as I used to say.

RD: Right. And I had a couple of job opportunities. This was 1982, and there was an oil bust coming on. From the late '70s through the early '80s there was a big oil boom. Even as grad students we used to get a call a month from headhunters looking for anybody to come work for the big oil companies. They were setting up research labs and they had a lot of money that they were willing to throw at problems because they were making a lot of money. Then in the early '80s, it kind of came back to the bust cycle, and that's when I was looking for a job. I interviewed with a number of oil companies, mostly with the research and development labs, and with a number of universities—the University of Wyoming, University of Missouri, and UTEP (University of Texas El Paso).

I went up in January to interview at Wyoming and got off the airplane, and it was, like, 20 below zero. And the guy met me and said, "I hope you brought warm clothes, because there's a storm coming in." [Laughter] They had great people there, but they told me, "I hope you like cross-country skiing, because in the winter there's not much else to do here." Two weeks later, I went down to El Paso, and it was about 75 degrees, and we went out on a field trip and walked through the Franklin Mountains there in the middle of town. Started at the Precambrian and walked up high up into the Pennsylvanian—just a totally different environment. And I thought it was a school that was more like Stanford—inquisitive and stressing teaching and so forth.

So I decided to go to UTEP, and I taught a number of courses there, mostly in the structural geology/plate tectonics arena—physical processes, the standard introductory geology courses, geology for teachers. (I never quite understand why the education department needs to have special courses for education majors. Why they can't just take a regular course?)

Then, in about 1986, I started getting calls from one of my officemates at Stanford, Dave Dobson, who was trying to hire geologists. He had come to work for the Department of Energy at this thing called Yucca Mountain. And some of the problems that they were having to face were similar to the problems I had worked on. I had done a lot of computer modeling and such while I was working on my dissertation. Dave Dobson is also a good friend of Mike Voegele's. At that time, the oil companies had kind of gone at least half cycle so most of the students that were graduating from UTEP were going into either the environmental arena or to work for an oil company. There wasn't much interest on behalf of my students to come and talk to Dave. But I told him, "I'll come and talk to you."

The first time I interviewed was, I think, in '87, when I came and interviewed with SAIC Corporation. It was the technical and management support services contract, I believe it was called, to the Department of Energy. DOE had a number of entities that were actually doing the work—the national labs, US Geological Survey. We had contractors out at the Test Site—REECo, Holmes and Narver. And then DOE was supposed to pull all of this together.

Now, this was about 1987. Let me go back to the first week I was at grad school at Stanford, 1976. Some of the esteemed professors came to talk about things they were working on at a brown bag lunch and all the grad students came to listen and learn. There were two professors. One was Connie Krauskopf, Conrad Krauskopf. One of the great geochemists. In fact, he probably wrote the most popular geochemistry textbook that has ever been. Connie had a Ph.D. in chemistry and a Ph.D. in geology, both from UC

Berkeley. He came across the bay and became a professor at Stanford. At that time, Connie was probably 80 years old, and he was like an elf, about 5’4” tall and just full of energy. He and the other 80-year-olds would race up and down the stairs in the geology department.

And the other gentleman was Irvin Remson. He was the Dean of the Applied Geosciences group at Stanford. Both of them had been involved in the National Academy of Science’s efforts to look at radioactive waste. So that was something that they introduced us to in 1976, the effort that was going on then to try to find something to do with radioactive waste. Both of them seemed to be pretty sold on salt as a media—that it works pretty well. So when Dave Dobson called me in about in ’86 or ’87, the issue of high-level radioactive waste was not exactly foreign to me. I’d had some introduction to it, knew it was as issue, knew that the nation had been working to develop some approach to it. And then, of course, the end of 1987 was when the Nuclear Waste Policy Act Amendment was passed that selected Yucca Mountain as being the only site that would be characterized for potential development as a repository.

Now, it took quite a while. I originally came and talked to SAIC. That didn’t go anywhere. Then the Nuclear Waste Policy Act Amendment was passed, and all of a sudden DOE, which had a workforce at that time of about maybe a dozen people in total, was looking at running a big program, and they needed to staff up very promptly. They needed to staff up especially in the technical and scientific arena. So I got called back to interview with DOE this time instead of SAIC and was offered a job and took it. And came aboard, I think, the first of August 1988. The first of August was either a Sunday, which is when the pay period started, or it was a Monday, when I actually walked in the office.

RM: What is your perception of the challenges that you were going to encounter in this new job?

RD: At that time I didn't know that much about Yucca Mountain per se, but I knew that what needed to be done was to observe and describe what we had. Understand as best we could what processes were operative in this system. Understand how to construct a model or series of models that would describe how those processes operated over timescales that are really difficult to understand. If you talk about geologic processes operating over timescales of thousands or hundreds of thousands or millions of years, it's difficult to test those models in a conventional sense.

But all of that had been accommodated in some of the early rulemakings and regulations that took place, which specifically said, "You're never going to have the level of certainty for this kind of thing that you might expect in the engineering of a bridge because you rely on natural materials to a large part here, and natural materials are not designed, engineered, or made by man. They exist. Your job is to understand what they are, how they work, and how you can augment them or complement them as best you can with some engineered part of the system to help the overall system work better." I understood all that but I didn't understand the details of what we had to look at. That was one of the first things that was being done.

Well, there were a number of philosophical arguments going on there. I mean, as a scientist, you can look at anything in an unlimited level of detail. I mean, we could take apart that door and look at it molecule by molecule. Now, I personally think that probably there's a break-even point, a cost/knowledge curve, and you probably get most of your knowledge before you're too far up the cost curve. If you wanted to look at it molecule by molecule, you could spend a whole lot more money looking at the door, but probably you'd get about 95 percent, maybe 99 percent, of the useful knowledge with a miniscule expenditure of the total budget that you might spend to dissect that molecule by molecule.

So the question was, at Yucca Mountain there's so many different things that could be looked at. There's so many degrees of detail that could be pursued for each of these fields of knowledge. For instance, hydrology—do we need one well? Do we need a hundred wells? Do we need a thousand wells? If this were a gold mine—you have experience in gold mining—you know, that's money. And what you want to do is to drill out the ore body and define where it is so that you can mine the ore body. You don't want to mine the rock that's not ore. So you invest in putting a lot of boreholes in because there's a payback on that. But at Yucca Mountain, there was kind of an inverse thing. If you put in a lot of boreholes, that's a potential mechanism by which material could get into the repository or out of the repository at some time.

So you want to be very judicious in how you design your borehole program. And you want to know where everything is. You want to know what you've done, everything that you've done, so that if something happens later—if you come across something that was unexpected—you can figure out whether it was something you did or whether it's natural.

For instance, I don't know if Jean Younker or Mike Voegele told you, but every bit of water we used at Yucca Mountain in underground construction was tagged with a chemical tracer—lithium bromide—in trace amounts, but it is a conservative tracer, which means it goes with the water. It doesn't get strained out by anything. So whenever we encountered a body of water anywhere, we could tell very quickly whether it was a natural body of water or whether it was a body of water that we ourselves had introduced into the mountain.

Because the most critical thing about a repository—and it's not just Yucca Mountain, it's any nuclear waste repository—is water. You've got to get water into the system to degrade the waste packages. You've got to get water into the waste packages to dissolve the spent fuel. You've got to get that dissolved spent fuel out of the waste package in a liquid form so it can move away from the repository. And then you've got to get that liquid with the radioactive waste dissolved in it out to somewhere where it can't do damage to the public. All that has to happen to get *any* release from *any* geologic repository.

If you can short-circuit that anywhere, if you can put a barrier in place that will stop the water, stop that from happening, then you have a successful operating repository. And we had a number of potential barriers that we could put in place and stack up. You know, maybe this one's only 90 percent effective, and this one is 85 percent effective, and this is 95 percent effective. Whenever you take all those and stack them together it's pretty damn good.

So water became one of the most important things that we needed to investigate. But we knew that we needed to investigate other things. We needed to investigate the propensity for earthquakes, the seismic character of the area, not just for the period when there would be an operational facility there, but for the very long term after the operational facility had been closed down and you had underground facilities. Would the shaking of the rock cause roofs to collapse, and would that damage the waste packages? Did we need to put backfill in place in the tunnels around waste packages? Did we even need waste packages?

There's a feedback loop that works here, and the feedback loop works like this:

you go out and observe something about the natural system. And from that you develop an engineered system that can augment and complement the functions that you want this repository to do. Then you estimate how the natural component and the engineered component would behave.

And you ask, “Now, what more information do I need in order to, let’s say, improve the engineered system?” Maybe I want to make it of a particular material but I need to know what the pH, the acidity, or some chemical specification thousands of years in the future might be so that I can select now the right materials to engineer some complement to the natural system.

So, “Do I know how that works?” Well, if I go in and pull out the water that’s in the system now and analyze it and then say, “Well, now, okay. That’s the water that’s there now. But if I heat that water up in the presence of some different chemicals, it’s going to change its character somehow. How will that water evolve? What will it do to the chemical nature of the water? And will I have to change the specifications for the metals that I introduce in the engineered part here?”

What you’re getting is this iterative process where you observe something in the natural system, develop an engineered solution to augment and complement it, estimate how this system performs, evaluate the gaps that you have in information that you need to fill—either you’ve got large uncertainties that you need to reduce, or you have areas that you just have no information about. And then you go back out and do some more observations or experiments, some more of the fieldwork—what’s called the site characterization—to develop that information. Then, you go through the cycle again. So site characterization is the observation/experimentation phase.

CHAPTER TWO

RD: Let me just run through this again. You've got the site characterization, which is the observation. You've got the engineering, which is developing your manmade component to augment and complement the natural system.

Now, what you use to evaluate how both of these—the man-made and the natural system—perform together is what's called performance assessment. So you run through this cycle of observation, design, performance assessment. You evaluate the system, determine what you need to change in either your understanding or state of knowledge of the natural system, the level of detail that you have in the engineered system, until you develop a confidence that you understand how this whole system would work. And eventually you want to develop a high confidence that the system that you observe, plus the engineered system you propose to build in it, will meet all of your desired safety and regulatory standards for whatever period of time is specified.

One of the challenges that we had at Yucca Mountain was that safety and regulatory standards changed over time and the performance period changed over time. It started out as a 10,000-year performance period and eventually became a million-year performance period.

The safety and health regulation started out as being a release standard—a somewhat awkward construct called the complementary, cumulative distribution function—which was essentially, "You can't let more than this much material out of the repository." And later that got changed to a dose standard. Now, those are quite different things, and the way you approach them is quite different. And the way you approach compliance with those standards can be quite different. And to go back, the information that you might want to develop early on, whenever you're trying to do those, can be quite different.

So in retrospect, that's the environment I walked into—remember, when I came to work for DOE there was a set of regulations in place. We were just finishing up what we called the site characterization plan. The site characterization plan laid out to the regulator—to the Nuclear Regulatory Commission—how we proposed to go and develop information at the site that would allow us to be able to demonstrate that a repository system would be able to meet all of the regulatory requirements. It was primarily built around the regulatory interpretation, although that's not to say there wasn't science involved. I mean, there was a lot of science involved, but not every science that could be done needed to be done.

When I came to work for DOE in 1988, we were just finishing up the site characterization plan. I wasn't involved that much in the development of the site characterization plan because it had been pretty well completed by the time I came on board. But there was a bit of a struggle going on between the Department of Energy, which had had a small number of people, and the entities that had actually been doing the program up to that time. Those were the national labs. We had Sandia, we had Los Alamos, we had Livermore, and we had the US Geological Survey. Seems to me I've forgotten somebody. Well, those were the biggies at that time. Later, Lawrence-Berkeley Labs got a much more prominent role, but in the beginning, it was the three weapons labs—Sandia, Los Alamos and Livermore, and the US Geological Survey—that were doing the bulk of the scientific investigation.

And this is in large part because of the legacy that they had from working for the Nevada Test Site. They had been supporting the weapons program since the early '50s and had been doing a lot of work in the precursor to the Yucca Mountain program on and around the Test Site for decades before Yucca Mountain really got started in, I guess, ’83, ’84. They were kind of the brain trust. I mean, they were the ones that had been developing priorities, putting together plans; and now, all of a sudden, DOE comes on the scene and has the responsibility for doing this. And we took the responsibility quite seriously. So there was some head-knocking that went on as we tried to figure out, who’s really responsible for this? Who has to say that this can happen? Who has the authority to approve what? And who decides where the money goes?

We had a number of contractors that were involved in construction. There was REECo, there was Holmes and Narver, there was Fenix and Scisson. Later there was Raytheon. All of these were what are called the M&O contractors out at the Test Site—M and O means management and operations contractor. This is a contracting construct that is unique to the Department of Energy. And it came about because of, essentially, the Manhattan Project. Because the risk, the liability, was so enormous, no company—not even Westinghouse or DuPont—was willing to risk the fate of their company on success in something that had never been done before.

RM: Namely, Yucca Mountain, or disposing of nuclear material?

RD: Yes, disposing of it in the case of Yucca Mountain. But originally it was developing a nuclear weapon. So that’s where the M&O concept came from. And as we started developing the nuclear stockpile, this situation came about where the government took the liability and they paid the contractors to perform work. The contractor really didn’t have liability. They were expected to provide best effort. But in the work that they were performing, since it was first of a kind, they didn’t have to lay their company on the line and say, “You know, this will work. I guarantee this will work.” No, they just did what . . .

RM: They did what they were basically told to do?

RD: Right. I mean, there were a lot of companies—REECo, EG&G, Fenix and Scisson, Holmes and Narver, Raytheon. Those were all M&O companies that worked under that kind of a contract construct. And that's what we came into.

Now, the down side of that kind of construct is that there's little incentive for cost-savings by the contractor. I mean, essentially, you're paying for effort, and they'll put as much effort in as you have money. There's little risk that the contractor is assuming. On the government's side, you want to ensure that the government is getting fair value for what it's paying. So there's an expectation of a high level of government oversight, which is not traditional, certainly with the Test Site. They had a more hands-off approach.

I mean, the technical details were handled either by the national labs or by the construction crews. Yucca Mountain was a little different because we came in with an expectation that DOE—by DOE, I mean the Yucca Mountain office of the Department of Energy—would be intimately involved in making the decisions, setting the priorities, and ensuring that things were done kosher and aboveboard.

Let me back up a little bit. I think in 1986 or 1987, when the Department of Energy was looking at characterizing multiple sites, not just Yucca Mountain, the way that the DOE screening process had been laid out, DOE was going to look at a number of sites, do a comparative evaluation, and select the best. They started out with nine sites; narrowed that down to five sites; narrowed that down to three sites. Then they were going to do full site characterization of all three of those sites. And then, based on the wealth of information that came out of that, do a comparative evaluation of those three final sites and determine which one should be taken forward for recommendation for development as a repository.

But when they put this scheme together way back in the early ’80s, it was before any of the regulatory framework had been put in place. So they didn’t know exactly how much information was expected to be needed. Congress had an expectation, whenever this was originally set up, that all the site characterization for all of the sites would cost a few tens of millions, maybe a hundred million dollars. Well, by the time the regulatory standards from the EPA and the Nuclear Regulatory Commission were put in place, it was clear that, to answer the questions that had been provided in the regulations, it was going to cost a lot of money to get that information. It was going to cost billions of dollars per site.

And that’s when Congress stepped in, in 1987, and said, “No, that’s not what we had in mind.” They essentially said, “Okay, now what’s your best site?” DOE had done some comparative evaluations up to that point, and, you know, it’s hard to say what your best site is because there are so many different criteria, and everybody has a different way of looking at those criteria. But in every one of the evaluations for all of the technical standards they looked at, Yucca Mountain was either at the top or very close to the top for all of those. So, essentially, DOE said, “Well, Yucca Mountain, of the three that we’re looking at, is probably the best site.”

So that’s when Congress stepped in with what’s known as the “Screw Nevada Bill” and said, essentially, “Cease and desist on site characterization of the other two sites—the BWIP Site at Hanford and the salt site in Deaf Smith County, Texas.”

RM: And that’s how it’s pronounced? Deef?

RD: Deef. Yes. I know that, having lived in that part of Texas for a while. So those two programs were literally cancelled overnight. This was in ’87, ’88. Yucca Mountain was then accelerated, and they were trying to build up Yucca Mountain.

So we had a regulatory framework at that time; we had a number of contractors at Yucca Mountain; we had a number of contractors at other sites whose programs had just been cancelled and who thought that they ought to be involved in Yucca Mountain. In about 1987, the Department of Energy, who thought that they were going to be characterizing multiple sites simultaneously, had put out a bid for a contractor to oversee all of this site characterization that was going on. And that was the original integration contract that got overturned and was awarded in 1989 or ’90 to TRW. So we had this set of contractors in place at Yucca Mountain, and then, all of a sudden, there is a court-imposed contractor that’s put in that is supposed to come in and play a role that doesn’t exist anymore, which is to look at the standardization and integration of all three site characterization programs. Well, there’s only one site characterization program now. So now we’re blessed with an overabundance of contractors. And for about two years there was a substantial amount of effort spent in trying to figure out who needs to do what.

RM: This is DOE doing that?

RD: This is DOE trying to do this, yes—and the contractors suing each other and countersuing and going back and forth. And there was an uneasy truce that was developed. I should also say, during this time we were not allowed to even do any site characterization work. We were primarily doing paperwork anyway, so it’s not like there was a lot of lost opportunity. But eventually it was kind of hammered out that one M&O contractor, TRW, would do this part. The technical and management support contractor that we had, which was SAIC, would do this part. The national labs would stay on and do this, this, and this. The USGS would have a role doing this. And we would use some of the contractors from the Nevada Test Site to assist in doing drilling or underground exploration or road maintenance or whatever. I can’t remember exactly when we started site characterization. I think it was 1990.

RM: Would you expand on what site characterization is?

RD: Site characterization is the actual field activities to go out and develop the observations, the experiments, the information, the measurements of what the natural system is composed of, what processes are operative in the natural system.

One of the first activities we did, because it was fairly simple to do, was to go out and put a trench across a fault. We knew there was fault out there, and we needed to develop a displacement history of the fault. So we put a trench across it. Then, you have different horizons exposed in the walls of the trench, and if you have datable material in the horizons you can develop a displacement history. “Okay, 800 years ago there were six centimeters of displacement, but 10,000 years ago there were 22 centimeters of displacement.” So we were going to put this trench across, and it was one of the first relatively easy things to do. I mean, you go out with essentially a bulldozer, and you make a big trench. And then you have some geologist go in and observe what’s in the trench. You annotate it very closely, you take samples, you make a very careful record of what you see in there.

And then you bring out the Nuclear Regulatory Commission technical staff and say, “Well, this is what we found. Here’s what you see in the trench. Do you have any questions? Do you think there’s anything more we need to look at?” Because eventually you’re going to fill in all the trenches. You don’t want to leave open trenches sitting out there because wildlife can fall into them.

RM: How deep were the trenches?

RD: The one I’m talking about was maybe six or eight feet deep. We had some that were 15 feet or so deep. We had some smaller trenches that we had looked at. But this particular trench was part of our planned studies. Did either Jean or Mike talk about Jerry Szymanski?

RM: Yes.

RD: Okay. Work to deal with the issues Jerry Szymanski raised was going on on the side and was not really a part of the plan site characterization work. Because the scientists had specific things that they wanted to understand, develop data on, understand what processes were operative. Nobody really believed what Jerry was proposing made any sense. So there wasn’t much that was built into our plan site characterization effort that was planned to really look at what Jerry was looking at, because it was ludicrous.

RM: And, briefly, what was he proposing?

RD: Well, there are calcite deposits at Yucca Mountain. Jerry was convinced that those calcite deposits were evidence of past hydrothermal waters being raised from the subsurface to the surface of the earth. Now, at Yucca Mountain the water table was somewhere between 1,500 and 2,000 feet below the surface. That would mean that you had to have enormous excursions of hot water bringing this water up. The alternate explanation for these materials—you live in Las Vegas, right?

RM: Yes.

RD: Are you familiar with the term “caliche?” Have you ever tried to plant a tree or dig a substantial hole in your yard? You encounter caliche, which is the same calcium carbonate, but instead of being brought up from deep in the earth, it is windblown calcium carbonate that comes as dust, gets mixed in with rain, which is slightly acidic, and dissolves in the water here.

RM: That's where caliche comes from? I didn't know that.

RD: It trickles down through the dirt, down to a certain level when the water evaporates out, and it leaves this caliche layer that is like a layer of concrete, literally.

RM: In the cracks?

RD: Well, no. It precipitates at a level, and it's like you poured a concrete road down four or five feet below the surface. It's necessarily solid, but it's big chunks and conglomerations that you can't get through. If you build a swimming pool here in Las Vegas, the excavation costs, in some places, are more than it costs to actually build the pool, because they've got to go in and jackhammer it and blast it to get through the caliche.

RM: And it's windblown? Where's it coming from?

RD: Well, here it's from the Spring Mountains, just to the west of us, which are all carbonates. They're all limestone.

RM: So it's erosion coming off of that?

RD: Coming off from that. And you get a fine dust of that calcium carbonate. It's picked up, and whenever it rains, the rain is slightly acidic, and it dissolves that material, takes it down into the earth a distance—not a great distance, but a distance—and then reprecipitates it. And given thousands and thousands of years, a little blob becomes a big blob becomes a mass of this material.

And that's the same thing that we had at Yucca Mountain. It was just misunderstood and poorly represented, and, because Jerry framed himself as a whistleblower, it became a very contentious issue when it should not have been. I mean, it was just open and shut.

RM: It was just a slam dunk to any good geologist?

RD: Yes, it was.

RM: Well, how did he get away with it?

RD: As a whistleblower.

RM: So any issue could be contentious, regardless of the quality.

RD: That's right. Now, let me back up a little bit again. When I originally came to work for DOE, I came to work as a staff scientist. And within a couple of years, we had grown the staff to the size where the Regulatory and Science Evaluation Division—I think that's what we called it; it had different names—had three branches. I became a branch chief, and Jerry Szymanski worked in my branch.

RM: Oh, he worked for DOE? I thought he worked for the state of Nevada.

RD: No. He worked for DOE until about 1991, I think, when he resigned. Then he formed a consulting company and consulted for the state.

RM: How did he ever get on board at DOE?

RD: Beats the bejesus out of me.

RM: Was he there when you got there?

RD: Yes.

RM: Was he just a negativist personality, a contrarian by nature?

RD: Yes, I think so. I mean, he was looking for something spectacular and apparently he had a history of doing that.

RM: What did his colleagues at DOE think of him?

RD: Well, not much. He had a couple of people who believed him, but of the 25 or 30 scientists that we had on staff, only one did. In fact, when I was put in as the branch chief, I went and talked to the project manager, Carl Gertz at that time, and told Carl that I could not give him a performance evaluation. I thought he was a charlatan and if I evaluated him, it would be grounds for firing.

RM: And he couldn't be fired politically?

RD: He could not be fired politically as a whistleblower; there was a whistleblower law. I'm not sure what the provisions were. But Carl said he would take care of it and write the appraisal for Jerry. So I never wrote an appraisal for Jerry Szymanski, although he nominally worked for me.

So this was playing out in the early '90s. We had Szymanski bringing attention to the project, questions being raised. We were not able to start site characterization for a number of reasons. The state had not allowed us a water permit at that time.

RM: Just briefly talk about that issue—how they shut off the water.

RD: This started long before I got here. DOE had requested a permit to use water. Now, the Nevada Test Site had not ever gone through the formality of asking the state engineer for a water use permit. We thought we were going to be aboveboard and work, hopefully, collegially with the state. So we went through the formal process of putting in for a permit and the state, as I understand it, declined that permit. We put in for an appeal, and it took years for that appeal to work its way through the system. I really don't know what the basis for the appeal was. I do know that whenever we started site characterization, we actually had to import water; and we imported several tens of thousands of gallons of water. I believe we bought it from wells in California, just over the border in the southern end of Amargosa Valley.

RM: Same aquifer, probably?

RD: Oh, yes, probably so. I think we used it primarily for dust control. Then we got a ruling that allowed us to use water from Nevada. I don't know the details of how that happened. But anyway, when that happened, that became the event that really signaled the start of large-scale site characterization. Because then we could use water for dust control for roads, we could use water for drilling, we could use water for construction purposes. There were many things that needed water that we'd just been kind of waiting for.

So all of a sudden, we had these many things that we had outlined in the site characterization plan and for which we had an even greater level of detail in supplementary documents called study plans, which said, "Okay, this is what we're going to look at. These are the techniques we're going to use. This is the information we're going to collect. This is how we're going to evaluate it. Here's the analytical tools we're going to use. Here's the scientific instruments." This was all broken down to the level of even having operating instructions for a piece of scientific equipment. I mean, all that laid out in great detail. And all of that was put in place under a very strict quality assurance program before we started any of the work.

And then over time, as we developed these programs and plans and were able to field them—we would start a program, get a level of information and then have to make decisions as to, "Do we have enough information yet?" because the scientist out there is on a roll: "Jeez, I can do this for the next 20 years, and I enjoy it." But at some point somebody has to step back and say, "Um, we've got enough," or, "You know, we really don't need that at all. We're not going to do that."

RM: Going back to Szymanski. You said he nominally worked for you. So he was employed by DOE under your supervision, or in your department?

RD: Right.

RM: How did you handle that? I mean, basically, he was kind of a hot potato wasn't he?

RD: Yes, he was.

RM: How did you, as his supervisor, handle that? I mean, did you give him real jobs to do? What could you do?

RD: I could give him some nominal jobs.

RM: And that's what you did?

RD: Yes. We would have things come in that needed a review and I'd task him with doing a review of something that would take a few hours. But his understanding with Carl was that the bulk of his time would be devoted to pursuing this hypothesis that he had.

RM: And the hypothesis was that the calcite was coming up from waters from below rather than being deposited as windblown material? In retrospect—in a better world—how would an agency with a mission such as DOE handle a person like Szymanski?

RD: I think it would be difficult to do anything very different from what we did—we brought in an independent group of five individuals. Jerry never really produced anything that was very coherent. He put a lot of thoughts in a series of notebooks, but trying to get from "A to Z" was impossible; there was no logical story in there. The story changed every time you talked to Jerry or one of his acolytes. We tried to kind of get our arms around this by bringing in a group of five people, three that were named by DOE and two that were named by Jerry.

I'm trying to remember. I believe Archambeau, who was a geophysicist from, I believe, the University of Colorado, and a structural geologist from England whose name escapes me at the minute, were the two, that Jerry named to this panel. And DOE had Wes Smith, who's a very eminent hydrologist from the University of British Columbia, and Dennis Powers, who is a geologist that I knew at UTEP, who was the chairman of the group. I’m trying to remember who the third person was. I think it was John Rudnicki, but I’m not positive. Anyway, as you can imagine, this group came out with the three DOE-selected people against the minority of the two. So the accusations continued to fly back and forth that there was a cover-up going on at DOE.

RM: In retrospect, did Jerry Szymanski damage the program or was he just kind of a pain in the rear?

RD: I think he damaged the program because he gave a standing to junk science that should never have been accorded to it.

RM: And opened both of the programs to charges of inadequate science, falsely.

RD: Yes. And that was sort of the start of it. The state picked up on it. Jerry also had an axe to grind against some of the other scientific organizations, especially the US Geological Survey.

RM: Really? What was his problem there, I wonder?

RD: He felt that they were not doing as good a job as they could have been doing, that their quality control was poor. This all occurred before my time, so I don’t know if there was in fact a basis for it, but I know that there was certainly bad blood between Jerry and the USGS. And part of this—his development of a hypothesis—was an attempt to show the USGS that they weren’t as omnipotent as they seemed to think they were.

RM: Was his theory a laughingstock among the top-drawer geologists?

RD: Pretty much. I spent a couple of days trying to understand it—what was the evidence and so forth? And I just could never get there.

After we put the report out by the panel of five it became seen as inconclusive. So we needed to do something that would be conclusive. We went to the National Academy of Sciences. We asked the National Academy, “Can you help us resolve this?” So they did. They put together a group of about a dozen or 15 very eminent scientists, one of which was one of my professors at Stanford, George Thompson. And I have absolutely the highest regard for George.

So this group came out and looked at the evidence that Jerry had. He took them out in the field, showed them what it was, gave them hours of lectures about his theories and about how he thought things were happening. And they came back with a report. I'm trying to remember the name. The title of that report is something like "Ground Water at Yucca Mountain: How High Can It Rise?" And the report essentially said, "There's no basis for Szymanski's hypothesis. However, there are 17 other things that need to be looked at at Yucca Mountain, and here they are."

And we said, essentially, "Yes. Okay."

RM: Did you ever talk to Thompson, personally, about what he thought of the theory?

RD: I did. George said, "Well, he took us out. He and his acolytes took us out in the field and showed us this deposit, which was supposed to be their prime example of how this had happened. And it was as plain as the nose on my face that they're absolutely wrong."

RM: Szymanski was able to do this because of the politics of the thing, wasn't he?

RD: I think so.

RM: So, in this case, it was a conjunction of politics and science that undermined the science, inappropriately.

RD: Now, let's skip forward 20 years. The technocrats made judgments and said, "You know, it looks good. There's uncertainty, but we think we understand the uncertainty. And it's making its way through the administrative and the regulatory process." Then the politicians stepped in again. So if you're talking about developing a system that might be able to develop a solution for this, I would urge you to try to develop a system that has no politics in it. Now, how do you do that? I don't know.

RM: Yes. One of the things that's come up in my discussions with Michael Voegele and Jean Younker was—and I don't want to cast this as an anti-democratic thing—that there was too much democracy in the whole program. What it needed, they have suggested, was that it should have been handled more as a public corporation, like, for example, TVA, the Tennessee Valley Authority.

RD: I absolutely agree with that.

RM: You can't have everybody and his brother challenging a gigantic leading-edge project like this and just throwing mud at it and slowing it down.

RD: There was a lot of talk about changing the structure of the program, and a public corporation was one of the more appealing options. But, remember, the Nuclear Waste Policy Act is laid out with a series of cascading actions, if you will. You start off with site characterization, then you go to a site recommendation, then you go to the license application, then the construction authorization. And there were a number of these actions that we felt would probably be best accomplished by a governmental entity since they were built into law, up through the site recommendation, which is the action by the president.

CHAPTER THREE

RM: Russ, before we started this interview we were talking about some basic geology of where the North American continent came from. It's so interesting, I'd like to include it here.

RD: Okay, we'll try. This is not really my specialty, but the general idea is that we have a framework and a paradigm for understanding how the earth has changed over time. And the major thing that has influenced the geology of the continents has been plate tectonics, where we have not just static continents that are fixed in space throughout time but rather are mobile on the face of the earth. And continents come together and diverge. They don't necessarily look the same before and after the super-continents split up.

We've had a series of super continents that have developed over time. One of the core parts has been the North American continent—the core, or the craton, of the North American continent, which gets assembled into super-continents and then breaks up but still remains a core identity. And this is mostly around the Canadian Shield coming down through the Appalachians, down through Mississippi, maybe through south Texas, and then back up through Arizona and probably up through at least Utah and maybe as far west as Nevada.

And to that core, over time, we've added on different pieces of material. There have been island arcs. Japan is a prime example of an existing island arc. Over time, we suspect that the sea behind the Japanese islands will disappear, and Japan will be accreted to, or sutured against, the Asian continent.

We've got evidence that such things happened in the past. The Sierra Nevada was probably underneath an island arc at one time. So from at least Utah—and maybe it was somewhere in Nevada to the west—most of that material has been plastered on, or accreted to, North America over time.

We were talking about the gold at Round Mountain. One of the things that happen when you have volcanic activity is that thermal activity provides a mechanism that can help concentrate some of the things like precious metals, sulfur, some of the other economic minerals we often find in areas where there's been ancient hydrothermal activity, heat activity. I don't know if that's consistent with the story you've heard about the development of the gold there.

RM: Totally consistent, yes. But this accretion really doesn't go to farther east than, say, Utah, as it's understood. Was Colorado was part of the original craton?

RD: Yes, it probably was, although things were complex there, because we had mountain-building, and it might have been an incipient attempt to split the continent, like along the Rio Grande Rift. You know, like the continent of Africa—the East African Rift system terminates in Kilimanjaro where Africa is trying to split itself apart.

And in that series of faults—it's a relatively narrow zone of faulting and volcanism—the material that comes out of the volcanoes is typical of what you find that comes out on the ocean floor at Hawaii. It looks like the earth there is trying to split itself apart and create a new ocean. So even the continents that we have now are not stable. I mean, the whole earth is in a perpetual state of change. It's just that it happens so slowly that, in our experience, we don't notice it.

RM: Yes. Is the New Madrid Fault that goes through the Midwest—Memphis and along Missouri—a failed rift?

RD: It was, yes. What happens in a rift is that the Earth tries to split itself apart and creates what's called a graben or a valley, a deep valley. There was a very deep valley that was produced and some ocean floor-type volcanics that came out. But for whatever reason, it never really truly separated; it just got started. That's sort of the same thing we see along the Rio Grande. The Rio Grande is a rift that tried to develop that goes from northern New Mexico down to at least El Paso.

So there are records of multiple attempts by North America to split itself up, or to be split up. What we see in the Basin and Range topography of Nevada—we talked a little bit about it yesterday—is a little different because it wasn't concentrated. You didn't have all of the extension concentrated in one zone. Instead, what we had was this east-to-west extension and development of these individual basins and ranges, which acted sort of like little minirifts, if you will, but a collection of them. This is a really complex issue, so I should refer you to a Geological Society of America memoir from 1990.

RM: Were they bringing up any deep ocean basalt in those?

RD: Yes.

RM: So they went down deep. Another question I have is I've heard about a lineament fault that runs from around Tonopah or somewhere up into the gold country at Elko. Could that be related to the mineralization in that area?

RD: Could be. There are continent-scale lineaments all over but what they represent is problematic. Some of them may be old strike-slip fault zones that have been covered up by younger rocks. Some of them may have been a zone of volcanism. It's easier to find the lineaments on either satellite imagery or aerial photography than it is to explain what they are.

RM: I wonder if you could put Yucca Mountain in the context of the growth and history of the North American continent, and what that has to do with making it a good place to put spent nuclear fuel?

RD: The rocks that make up Yucca Mountain are relatively young. They're the siliceous volcanic rocks that were erupted from a series of what are called calderas, which are very large volcanic edifices. Think of Crater Lake in the Pacific Northwest, which was a large volcano that erupted many times—erupted an enormous volume of volcanic rock.

And the same thing happened at Yucca Mountain. Starting, I think, about 25 million years ago and probably somehow associated with this development of the Basin and Range, we had not only the basaltic magmas that came out, but we also had some siliceous granitic materials that came out. Now silica-rich magma, when it cools underground, forms the big interlocking crystals that we know as granite. But if you tap a granite magma chamber and let it erupt at the surface, you don't get a chance to form the big interlocking crystals, so you have the fine-grain rock that's called a rhyolite. And the rocks that make up Yucca Mountain are rhyolitic tuffs.

These are the volcanic equivalent of granites, but they have extruded at the earth's surface. Because of the nature of granitic magma, it has a lot of water in it. So those eruptions tend to be really catastrophic, unlike a basaltic eruption, like in Hawaii. We've seen pictures of the fountaining events in Hawaii and the lava oozing out to the sea—you know, the natives moving cars and evacuating their households in advance of the lava sheet that's moving slowly toward the town. It's not generally really catastrophic. But then think of something like Mount St. Helens, which blew up like a pressure cooker. That is the nature of this siliceous volcanism like Yucca Mountain experienced.

RM: So Mount St. Helens was more granitic?

RD: It was more granitic. It was actually somewhere in between the two. There's kind of two end members. You've got granite on one end and then basalt—and its intrusive equivalent is called gabbro—on the other end. So here's a relatively benign volcanic environment with the gabbro and basalt. Here's a very explosive environment on the other end of the spectrum with granite and rhyolite. And in the middle there are mixtures that have differing amounts of silica and water in the magma. And those are generally called andesites. If you look at the rocks that make up the Cascades, like Mount St. Helens, those are andesitic volcanoes. So they have more water in them than a basalt but less than a traditional granitic magma.

RM: Just as an aside, what would Krakatoa have been?

RD: Krakatoa was certainly a cataclysmic eruption; it must have been a steam eruption. It was at least andesite, and maybe it was more on the granitic side.

RM: So you just don't get those big explosive eruptions with a basaltic source?

RD: No, they're certainly not common. Those are generally fairly benign eruptions.

RM: Now, getting back to Nevada, some of the eruptions in Nevada associated with the Basin and Range 25 million years ago were basaltic.

RD: That's right.

RM: And some were more granitic. Did they have different sources?

RD: They must have had different sources. Down in the earth you must have segregation. You've got a plate of a highly siliceous material that is going into the earth. And generally basaltic magma has a higher melting temperature, so it's hotter than a granitic magma. So you've got materials that melt at different temperatures, and they tend to aggregate some way. The details are lost on me. I don't understand them, but there is something that goes on that tends to concentrate mineral assemblages like granite. Quartz is a predominant constituent in granite. And one mineral that is sometimes found with quartz is gold, of course. So that's why granite or its extrusive equivalent, rhyolite, is often a host rock for gold deposits.

RM: I have one other question associated with Round Mountain. Mount Jefferson, which is the big mountain on the east side of the valley, is pretty close to 12,000 feet high. I was told that there was a caldera there a long time ago—I think it was called the Mount Jefferson Caldera, but I wouldn't swear to it—and it blew in a catastrophic eruption that dropped six feet of ash in Kansas. Does that sound reasonable?

RD: That's absolutely reasonable. Yes, the last big eruption we had in the western United States was the Bishop Tuff about 500,000 years ago, and this is near Bishop, California. When I was working in Utah at Arches, I found a layer of the Bishop Tuff in Arches, and it was six inches thick. It's a very distinctive unit. I ran across it and I didn't know what the hell I'd found, so I sent some samples off to the US Geological Survey in Denver and said, "Can you guys help me?" And the world's expert on the Bishop Tuff, a guy named Glen Izett, looked at it and sent me a letter within a day. It said, "It's Bishop Tuff. Where did you find this?" [Chuckles]

RM: That is fascinating. The geologists up at Round Mountain told me that this layer of ash from the caldera at Mount Jefferson came down so fast in Kansas that it caught animals drinking at a spring and things like that. So they can travel a long ways and then just drop out of the sky, right?

RD: Yes. I mean, some of the eruptions from these calderas were just immense. Nevada is where calderas were first discovered. I can't remember their names, but a couple of guys with the US Geological Survey in the '60s started noticing big circular features. They're much easier to see where you don't have much vegetation and of course Nevada's a great place to get away from vegetation. They went and visited a number of these and found that they had many things in common, and they actually came up with the idea of calderas and this sequence of calderas throughout Nevada.

And these eruptions are just enormous. The stack of rocks at Yucca Mountain represents a number of eruptions. We don't know whether some of those eruptions might have occurred hours apart, or days apart, or years, or hundreds or thousands of years apart. If they don't develop a discernible surface between each of the layers of deposits, it's really hard to break out what's what.

When Mount St. Helens erupted, it mobilized about a half a cubic kilometer of material. With some of the rocks that we see at Yucca Mountain, we think a single eruptive unit might be 200 meters thick. And it is about 40 kilometers from where the source was. So think of the volume that would have been erupted to give this. I mean, that's thousands of cubic kilometers of material. We talked about how a basalt flow just oozes over the countryside. What happens with these steam eruptions out of a caldera is that apparently you get a swelling of the earth. So it's like a big pimple in the earth.

RM: Just like at Mount St. Helens.

RD: Yes. But when we looked at Crater Lake, you see Crater Lake sitting there and you see the ramparts around it and nothing in the middle. The old idea was that it just blew up like popping a pimple; it all went up. But now we understand what happened was that, as this bulge flexes up, you develop cracks around the edge of the bulge, and that's where the eruptions really occur—around this circular fault zone. The faults go down, tap the magma chamber, and it comes up. And it comes up in this huge cloud that goes straight up in the air. Remember Mount St. Helens—they had to reroute airplanes. It went tens of thousands of feet in the air, maybe 80,000; I don't remember. But then it collapsed back down on itself. Some of it blew away but much of it just went up and came back down.

And when you have one of these really big eruptions like happened 12 or ten million years ago when Yucca Mountain was formed, you had this big eruptive cloud of glowing particles of rock shards. I mean, they're still molten. They're entrapped in a super-heated gas. They go up in the air and then they fall down on themselves. They've still got the gas trapped in with it, so when it comes down you've got this kind of buoyant cloud of glowing shards that comes down and then flows over the countryside. And it's flowing on a frictionless surface of this gas.

So it's flowing at hundreds of miles an hour, and it may be a thousand feet tall. I mean, we have 150, 200 meters of solid rock now that was originally a single wave that might have been thousands of feet tall. Imagine that. You know, that could ruin your whole day.

RM: Yes, it's really stirring. Well, let's say you have a certain stratum at Yucca Mountain—can you find the spot it blew out of? You know where the calderas are, I guess.

RD: We know where the calderas are. At Yucca Mountain there was a series of calderas. And they're what's called nested calderas. One caldera was active for a million years or so and then, for some reason, another caldera formed and it partially obliterated the previous caldera. And then it became quiescent, and a third caldera came, developed, and partially obliterated the previous calderas. So there's a good record of what the last caldera was. The record of the previous volcanic events is not as complete because they're partially obliterated.

RM: How big an area do these calderas that made the Yucca Mountain deposits cover?

RD: The last caldera is to the north of Yucca Mountain, and I'm trying to remember what the diameter is. I think it's about 30 or 40 kilometers across, the circular feature that's left. You've been up to Beatty?

RM: Oh, sure.

RD: Northeast of Beatty . . .

RM: Okay, out on the Test Site?

RD: Yes, it's mostly on the Test Site. Actually, I've got a satellite photo I want to show you. [Pause as Russ gets photograph]

RM: Russ has just shown me an aerial photograph of the whole Nye County and central Nevada area from Amargosa–Indian Springs up to Beatty and beyond. He was pointing out where the big calderas were and also the smaller eruptions.

RD: What I've got is a somewhat old satellite image from the Thematic Mapper. This imagery is from 1987, and the imagery covers an area probably 150 miles east to west and maybe 200 miles south to north. It's centered on the Nevada Test Site. So, of course, we get the Yucca Mountain region on it. What's very distinctive on here is this big, large, circular complex that's in the western center of this particular image, which was the last of the caldera complexes that gave rise to the volcanic rocks that make up Yucca Mountain.

I was telling you, Bob, that this is the last of at least three nested calderas that were active in the region from about 15 million years ago until about eight million years ago. We can see the rocks that make up Yucca Mountain down here. And they're probably 40 kilometers or so from the circular fault zone around the caldera that was probably the major source of these volcanics.

Because these volcanics come from a relatively common magma body, they have a commonality in their chemical signature, in their isotopic signatures. So we can look at volcanics in various places and trace those volcanics back to their source. By their source

I mean generally this caldera—not to this particular vent in this caldera but a general ability to source the materials.

And we've also had good success in age-dating these rocks using isotopic techniques. So you can build up a very good story about the sequence of events, how things happened here, not just at Yucca Mountain but also in some of the other caldera complexes that were active throughout Nevada and the associated regions from the time of about 20, 25 million years ago to about ten million years ago. Nevada was a very active region for these calderas.

RM: What was the reason for that activity? What was the underlying driver of it?

RD: Well, it is suspiciously synchronous with the development of the Basin and Range. So it may be tied to the interactions between plates. Remember, we were talking yesterday about the gap that was left when part of the plate—it probably wasn't the Pacific Plate, it was probably the Nazca Plate—got subducted under North America. And when, all of a sudden, the end of that plate got subducted down and there was nothing to shield the continent from the underlying material, so the upper mantle . . .

RM: So this stuff, the upper mantle, just started breaking through to the surface.

RD: At least it was transmitting heat through that was causing something to happen. And in places, it broke through. Now, superimposed on this, this episode of volcanism ended about ten million years ago and has been quiescent. We don't see any signs of any kind of a relic magma body here; there's no elevated heat flow. I mean, you will have some hot springs around here but not like you would see at a place like Yellowstone—we know there's a magma body under Yellowstone.

After that ended, things were very quiescent for quite some time. And then, starting about two million years ago, we started getting some small episodes of basaltic volcanism in the area. And that's some of these basaltic cinder cones we see out in Crater Flats. I think in the Crater Flats region the oldest cone that we've dated is about two million years old. And the youngest down here at the Cinderlite quarry and the Lathrop Wells cone is about 70,000 years old.

The general progression in age is older to the north to youngest in the south. Now, we see that same general progression in the Hawaiian Islands, where you go from Midway being the oldest events in the Hawaiian Islands. Apparently there's a hot spot that the Pacific Plate is moving over, and it melts the oceanic crust as the crust moves over it, giving rise to the volcanoes that make up Hawaiian Islands. So Hawaii, the big island of Hawaii, is the next-to-youngest island. There is a new island that's forming beneath the sea to the south and east of Hawaii where there's active volcanism going on under the sea that we can't see. That's going to be the youngest Hawaiian Island.

RM: So there was a progression, north to south, on the cinder cones. Was there a progression over the state for the big calderas?

RD: I do not know. There are thousands, if not millions, of intriguing questions. In this study we were focused on what was in proximity to Yucca Mountain, what was around here that could either pose a threat, a risk, to a facility at Yucca Mountain, or what did we know in order to better understand either the properties or the processes, the behavior of those materials, at Yucca Mountain over the very long term.

RM: Now, Timber Mountain—how wide did you say it was, east to west or north to south?

RD: I think it's about 40 kilometers in diameter.

RM: And it's sort of east and a little bit north of Beatty?

RD: Yes. Beatty is right here. Actually, there's a watershed that heads up in here. This is now a depressed area. And for the life of me, I can't remember what the name of that watershed is, but it's the headwaters of the Amargosa River up in here.

RM: Oh, so the headwaters are up in the Timber Mountain Caldera?

RD: Yes. And that's one of the reasons Beatty is concerned about this. Here's Pahute Mesa up here. And these little dots that you see are locations where there were large weapons tests. Several years ago there was information released that talked about a tritium plume that seemed to be moving south and west off the Test Site. I know DOE is trying to understand, well, where exactly is that plume? Is it going to come down and at any point in time contaminate the waters in this area, which provide some of the drinking water for the Beatty region?

RM: Sure. Was there a scientific judgment made about that?

RD: Well, it seems to be that the plume is moving very slowly. Fortunately tritium has a fairly short half-life, about 14 years or so.

RM: By the time it would get down to where it would really affect people it's . . .

RD: We would think it would be of insignificant concentration. But if it's moving faster than we thought. I know that they put in a series of monitoring wells, working with Nye County, to try to better determine where the plume front is, how fast it's moving, and some other details to get a better idea of what the real risk is.

RM: Was Nye County doing this work in association with Yucca Mountain? They drilled a number of wells. Was that part of that program?

RD: No. Nye County, in association with us, drilled most of their wells along Highway 95 and in proximity to Yucca Mountain. What they were looking at was trying to better understand the pattern of water flow from north to south, from Yucca Mountain down into the Amargosa Valley.

RM: Now, how would you answer a critic, somebody who was hostile to the whole idea of Yucca Mountain, who says, “Look, you know that there’s a lot of volcanic activity there in the past in the area, catastrophic activity. How do you know that’s not going to happen in the next 10,000 years or 500,000 years, if a million years is your criterion?”

RD: Well, this gets back to the concept of risk. To evaluate risk, one has to tabulate, “What are the things that can happen?” I mean, there’s a universe of things that can happen. “What’s the probability that each one of these things will happen?” And then, “What’s the consequence if it does happen?”

The formal definition of risk is probability times consequence. So we look at, let’s say, the siliceous volcanism scenario—that’s one of the scenarios that we built into the performance assessment. If you look at what the probability is, you can say, “Well, it’s happened in the past. Is there anything that would provide an indicator that suggests that we have a similar scenario that is either imminent or significantly probable in the future?” And when we look at the indicators that one finds around active silicic volcanic fields like Yellowstone, you don’t see those around here. So you don’t see any of the indicators suggestive of either a reemergence of this or of the development of a new field here. The only volcanic scenario that is probable, or, we think, even possible over the next million years, would be the basaltic volcanism scenario. Whenever we talk about volcanism at Yucca Mountain, we say, “The probability of this is insignificant.”

RM: Of the siliceous volcanism.

RD: The consequence would be catastrophic, but it would be catastrophic not just for an operating repository, it would be catastrophic for life in general in southern Nevada. Not to say that we shouldn’t look at it, but the probability is so low that it actually falls out of the analysis. This, however—I'm pointing at the basaltic cinder cones out here. This has a low but higher-than-a-threshold probability of occurring during the life of the repository. But it turns out the consequence is not particularly significant. So in our probabilistic risk assessment, we built in what would happen if basaltic volcanism occurred at some time in the future. And we used some very conservative estimates for what would happen. The probability is exceedingly low.

RM: On the order of what?

RD: Well, there's a cutoff in the regulations that says if an event has a probability of less than one chance in 10,000 of occurring over 10,000 years, then you need not consider that event. What that works out to is an annual probability of 10^{-8}, one in 100 million. Now, that's a very low probability. In fact, it is incredibly low.

Let me give you some examples from geologic history of events that have a probability on the order of one in 10^{-8}. About 65 million years ago—that's 65 times 10^6 years, or I'll call that about 100 million years (10^8 years) ago—we had an event happen that led to the demise of the dinosaurs and many other living organisms on Earth. It was probably associated with the impact of a meteor down in Yucatan, but it gave rise to global effects that significantly changed many things about the earth, one of which was the life forms upon the earth.

About 220 million years ago, at the end of the Permian Period, there was a similar extinction. We don't know what caused it, but about 85 percent of life on Earth died. So, about every 100 million years, we have an event happen that leads to the extinction of most life on Earth. So we have 10^8 years, 100 million years; two times 10^8 years, 200 million years very roughly. That's the order, that's the magnitude, of events that is associated with a probability of 10^{-8}—huge events.

RM: Yes, so it's about as likely to happen as an extinction event.

RD: Yes, a major extinction event, not just the demise of the dodo bird.

RM: Maybe you've already said this, but what's the driver on the basalt versus the driver on the big siliceous event—the Nazca Plate situation?

RD: I don't remember my plate tectonics maps very well, but I think it was the Nazca Plate that was subducting under North America. And whenever the San Andreas grew, it snuffed out the Nazca Plate, and it grew like a zipper from south to north. So it truncated the subduction of this plate that was going under. And behind the void left as that plate continued to be sucked down, that filled with something.

RM: And that was the heat and the energy coming up, which was not basaltic but was of a more siliceous type that erupted?

RD: Well, there was at least heat coming up, and maybe that heat caused some of the existing iron-rich and silica-rich rocks to re-form themselves and produce either the magmas that came up through the calderas or some of the basalts. There are some things about that explanation that don't solve all the questions. For instance, if it's tied to the development of the Basin and Range, the Basin and Range we know started in the east over by the Wasatch Front, and has migrated from east to west. If it's driven by this particular plate configuration that I was talking about, that started in the west.

RM: Yes, you'd think it would begin in the west, wouldn't you?

RD: I haven't really followed this area for 20 years. Maybe somebody has a better explanation for it now. I'm just not aware of what it might be.

RM: But what's causing those little basaltic eruptions? And they are little on a geological scale, aren't they?

RD: They are little on a geologic scale. They appear to be just leaks that come up.

There's probably some kind of a weakness in the earth's crust here, so that periodically you get a little blob of magma that can make its way up through a bunch of fractures and come up to the surface. And you find a couple of fields around here. This small field here in Crater Flats. Of course you've driven from Las Vegas to L.A., and right down just east of Baker, there's a volcanic field in there.

RM: Yes, you can kind of see some of it from the interstate, can't you?

RD: Yes. There's a significant volcanic field out there, the Cima volcanic field; same kind of rocks, but much more extensive than what we see right here. I don't know of a real good explanation for why these particular features developed there.

RM: But the judgment of the program was that the probability of an eruption of something like this on Yucca Mountain, or impacting storage at Yucca Mountain, was low?

RD: Right. And when we looked at the probability of one of these volcanic events occurring within the repository, that probability was on the order of—I want to say 1.6 times 10^7, something like that.

RM: And, for a layman, that would be . . .

RD: Well, it's just a little bit more than the 10^8 cutoff, below which it becomes an incredible event.

CHAPTER FOUR

RM: Okay, we have the volcanism; that's what you were dealing with. The other big issue was earthquakes. What is the overview of that from the program?

RD: Well, there is a correlation between earthquakes and faults. A fault occurs where you have a weakness in the earth. You accumulate strain along this weakness. It's sort of like having a couple of blocks and you're pushing one against the other. There's a bit of friction between the blocks. If you continue pushing long enough, eventually you'll develop so much accumulated strain in there that the friction bond is broken, and you have a sudden movement of the earth on one side of the fault relative to the earth on the other side of the fault. That sudden movement releases energy, which is an earthquake. And so, the reason we look at old faults is that we know it's much easier to have movement on an existing fault, this existing weakness in the earth, than to create a new fracture, or fault, and then break it. That's why we concentrate on where faults are, because those are places that you can concentrate strain on that could be released in the future as an earthquake.

There's nothing in the way of enormous faults in the Yucca Mountain area, nothing like the San Andreas system. The biggest fault system that we see is down in Death Valley, and those are very large faults. They are capable of generating a magnitude 7-plus earthquake. The faults that we looked at in this region around Yucca Mountain are, on the grand scale of faults, relatively modest faults. But they're still capable of creating a lot of ground motion, especially if you're very close to the fault.

Now, one of the contentions of the state was, "How can you think of building a facility here when there are, depending on your count, 27 earthquake faults in the area?"

Well, there are faults everywhere. The Decatur Fault Zone runs parallel to Decatur Boulevard here in Las Vegas. If you go down to Vegas Drive and drive along Vegas from Decatur to the west, you'll go up a rise that's about 20 feet tall. That's a fault along there. It is a very capable fault. There are a number of faults here in the valley. We have a number of what I would call critical facilities that are built near faults here. I'm not sure people understand that. But things are built to building codes. The building codes are meant to accommodate the potential consequences that might happen if a fault were to release energy by having an earthquake. I wonder how many people in the Las Vegas valley have earthquake insurance. I do.

RM: And insurance typically wouldn't be any good, would it, if you got zapped by an earthquake?

RD: No, it won't. So, for an earthquake rider, I think I pay about $500 a year.

RM: So there is a probability of an earthquake here?

RD: There is a probability, and I consider it a high enough probability that I'm willing to pay insurance for it. But we know how to deal with seismic motions in an engineering sense. I mean, there are nuclear power plants—the Diablo Canyon Plant in California is built within—oh, I don't know—30 or 40 miles of the San Andreas Fault. There are critical facilities throughout Japan—it's a very seismic-prone area. And an earthquake can be the trigger for other things—tsunamis (near coastlines), power failures, communications failures, bridge and road collapse, and so forth.

If we look at the seismicity of Nevada, and we've had the University of Nevada, Reno, the seismological lab, monitoring the seismicity of southern Nevada with a very advanced high-tech seismic network for at least 20 years, we find that there's actually a paucity of earthquakes around Yucca Mountain. You see a lot of earthquakes around

Reno, Carson City, up to the north, in the western part of the Basin and Range.

There were a lot of events around the Nevada Test Site, but most of them are associated with the nuclear weapons program. I mean, when you release energy into the earth, you create an earthquake. And if you don't thoroughly screen the records, you can't really discriminate very well an anthropogenic, a human-induced, event. You get the same thing from large mine explosions. There's been a lot of explosive work associated with the mining industry in Nevada. There's a little fault that runs through the southwest corner of the Nevada Test Site, the Rock Valley Fault. It's a strike-slip fault that comes from south of Highway 95 through this region right here.

RM: Which would be the Point of Rocks Gap, or whatever it is, right out of Mercury, down Highway 95 from Mercury?

RD: Yes. It's actually maybe eight or ten miles east of Indian Springs. Let's see, Indian Springs would be about here. It comes through here like this and goes up on into the Test Site. This is a little strike-slip fault. And this is where we had the 1992 event.

RM: Oh, the one they talk about and everything.

RD: Yes, this was the magnitude 5.6 event. That's been the largest earthquake we've had in proximity to Yucca Mountain.

RM: Yes, and it would have had no consequence for spent fuel stored at Yucca Mountain?

RD: No. Now, in our facilities that we had out here, north of what used to be called Amargosa Valley . . .

RM: Okay, Lathrop Wells.

RD: And the fault then would be about eight to ten miles east of Lathrop Wells. In this area, we had facilities that had been used in the old nuclear rocket program, and we inhabited those in the late ’80s, early ’90s, fixed them up, used them for office spaces. When that earthquake occurred, it damaged some of those facilities. I mean, it broke out the windows, it dropped all the ceiling tiles. And because these buildings were built in the ’50s, they weren’t built to any current codes at all. And the state hopped on this and said it did millions of dollars of damage.

Well, the real damage was replacing the glass and putting the ceiling tiles back up. And because the fire codes said that in a stairwell the mortar has to be airtight—you need to keep an air seal in the stairwell so that you can use it for emergency exit—some of that mortar had become cracked, so we had to repoint the mortar in there. The total bill was in the tens of thousands of dollars. The million-dollar bill that was trumpeted by the state was what it would cost to bring these facilities to current code status. And, you can imagine, for any building here, it’s often more economically effective to tear the building down and build a new one than to go back and rehabilitate it.

RM: So this is another instance of the state’s distortion of the whole effort, right?

RD: Oh, yes. And it was one of the early ones.

RM: When was that earthquake?

RD: It was June of 1992. I don’t remember the exact date, but I remember it vividly because I was on vacation. I was up in Elko in a travel trailer in Lamoille Canyon and got a call that I had to come back for a press conference the next day. So my wife took me to the Elko airport, and I flew back and did the press conference, and then a day later I went back and joined my wife. By that time, she and the kids had gone to Salt Lake City. So I flew into Salt Lake City and joined them, and then we went on up to the Tetons and Yellowstone and Glacier.

RM: Well, my reading of the history of the Yucca Mountain effort was a continuous propagation of lies and myths. I would personally like to do a book on that—the myths and lies that were put out regarding spent fuel.

RD: There were a couple of things that I found personally disturbing. One was the half-truths—take something that is said but take it out of context and blow it up and make something out of it that is not true. And there's a perfect example of that. Some years ago, we were giving a talk to the Nuclear Waste Technical Review Board. We were talking about the engineered system and the adequacy of the engineered system, and what we showed was if you took away the engineered system, how the system would perform.

But there were some very important caveats on this. We assumed that there was no retardation by any of the natural system. We assumed a lot of things, and what it showed was that the engineered system helped with a lot of materials. That's because, in this particular evaluation, we didn't give any credit to any parts of the natural system for stopping these materials. So it looked like if the engineered system were to fail, there would be doom and catastrophe. And the state picked up on this. They knew that they were not being truthful, but Bob Loux and Steve Frishman trumpeted this every chance they had. And it was deceitful on their part.

The other thing was the big lie. And this comes, I think, from the Joseph Goebbels school of public interaction, which is if you tell a lie big enough and just keep telling it, people will believe it. We talked about the slams against science. And I consider that a big lie.

RM: Discuss that a little bit; do you have any more examples? To me, that's really important.

RD: Well, a little bit. I think one of the most disturbing things I heard was—at that time it was Governor Bob Miller, so this would have been in the early '89, '90 time frame. I think Richard Bryan had just gone to the Senate and Bob Miller, who was the lieutenant governor, had become governor. This was either his interim term or the first term that he was elected in his own right. He was addressing a group—I think it was the International High Level Radioactive Waste Conference here in Las Vegas. It might have been the first one, and we tried to bring together opposing viewpoints in order to have a dialogue.

And the state never was interested in a dialogue. The state never worked with us, unlike Nye County. With Nye County we could have collegial, professional exchanges—not agree about something, but understand what the viewpoint of the other party was. The state was always against whatever we were for, looking to show that whatever we had done was inadequate, unprofessional, abysmal.

Anyway, Bob Miller came and gave a talk to these people, an international representation, and essentially said, "You're not welcome in this state. I don't want you, and the activities of DOE are best represented by their"—what did he call them?—"Keystone Kops scientists." I can't remember what event might have happened that gave rise to this.

It was about the time that Jerry Szymanski was very popular. And, of course, after Jerry resigned from the Department of Energy, he formed a consulting firm, went to the state, and said, "You know, you pay me money, and I'll prove that Yucca Mountain is not suitable." So they paid him money, and that has been the character of the state scientific investigations—not that they were interested in objective science, they were interested in a particular outcome and were paying for the outcome, not for the science.

After Jerry Szymanski, there was a Russian, Yuri Dublyansky. He got involved peripherally though Szymanski. But he came up with an idea—some observations—that there were little bubbles. In minerals that cool from a molten state, you trap little bubbles. And in those bubbles, you're trapping the environment that the material cooled in, or was developed in. So if you can tap the gas and liquid in those bubbles, you can figure out what environment it represents. So the first thing that happened was that Dublyansky said that there were these bubbles. And there was, unfortunately, a response from some pretty reputable scientists that said, "No, those bubbles don't exist." Well, it turns out they did, so Dublyansky was right on that point.

Then he claimed that they were evidence of very young hydrothermal activity at Yucca Mountain—and that went back and forth for several years. And Dublyansky was supported by the state of Nevada for part of this. It became quite contentious, trying to understand, well, where does the truth lie? This was something I was personally involved in. I talked to a professor at UNLV, Jean Smith. She was an expert in looking at these kinds of bubbles in ore deposits and reconstructing the history of ore deposits.

I said, "Well, can you do that for us? Can you look at this as an independent party? We'll go in, we'll get samples, we'll give you a sample, we'll give a split of the sample to some scientists for the program, we'll give the split of the sample to Dublyansky. But what we're interested in is what you find. And you're not under any conditions that you have to come up with a particular answer. What we want is *the* answer."

So she had a graduate student, whose name I can't remember, and they did a wonderful study. Took them about two-and-a-half years to do it. What they found was, yes, there are bubbles. It turns out the minerals deposits didn't accumulate all at one time, but rather they were layers of minerals that accreted over time inside cavities or fractures. So you had a history of things developing. And what they showed, because she was able to put an age date on them, was that the higher temperatures associated with these bubble formations occurred at least six million years ago and probably closer to ten million years ago when the rock was laid down. They were not recent deposits by any sense of the imagination. So this blew Dublyansky's hypothesis out of the water. Of course, he continued to pursue it.

RM: He was saying that the bubbles were recent?

RD: That the bubbles were quite recent, hundreds of thousands of years ago, and he never accepted the great body of evidence that Jean and her student put forward. That was a great piece of work. And, actually, one of the oversight bodies that we had, the Nuclear Waste Technical Review Board, thought very highly of the work that Jean had done.

RM: And later Governor Kenny Guinn forbade Nevada University scientists to work on Yucca Mountain. Is that true? That was my understanding.

RD: He discouraged it, but we certainly did it.

RM: You did it?

RD: Yes, and I'll tell you how that came about. When I was the project manager—this would have been about '98, I think—I got a call from a staff member for Harry Reid, who said, "You have to fund this particular scientist. He's a Cal Tech scientist."

And I said, "Why do I have to fund him?" Well, he had done an initial survey, using a new technology, some of the satellite GPS location system stuff. And his initial survey across Yucca Mountain suggested that there was a lot more movement of the earth going on than what our existing information had shown at that time.

Well, we went to the existing expert on that, a fellow named Jim Savage with the US Geological Survey, who was probably the foremost expert, and he said, "I see what he's done. I used another technique that I think is much more reliable and stable. And I can see where there are potential sources of error in his approach."

But this guy just absolutely would not hear of it, would not hear that there was any possibility that he might have some bias built into his program. So he had gone to Reid and said, "If you get me money, I will prove the Yucca Mountain site is unsuitable." So that was the genesis for this phone call I got.

And what ended up happening was that I said, "Okay, I need to create a mechanism to pay you." And he was looking for millions of dollars a year.

RM: So do you think he saw a cow he could milk?

RD: Yes. Well, I thought it was pure and straight extortion. And the extortion was absolutely forced by Reid's office. I was told that I had to fund this. I said, "Well, okay, I will."

RM: Did he have the authority really to tell you that, or just the political clout?

RD: No, it was political clout, because he had also called higher-ups in DOE and said, "If you want your budget . . ."

RM: Yes, sure.

RD: So I put in place a program that would fund this particular thing but also would fund other activities through the University of Nevada system. So I funded that to the level of, I think, ten or 12 million dollars a year.

RM: For him?

RD: No, the total. So he got a minority part of this, and we actively sought out proposals from engineers and scientists within the University of Nevada system, UNLV, UNR, and DRI, the Desert Research Institute, that were doing work hopefully that was relevant to Yucca Mountain. If it was directly relevant, it was very easy for us to sponsor this as work that could contribute to us. We never told them what the results had to be. We just told them, "You need to complete the work. We're not just paying you an extra stipend, but there needs to be something that comes out, a publication or something. And if there's some particular aspect that we are absolutely interested in—maybe you're doing a seismic analysis of structures in a certain frequency band and we want to look at a slightly different frequency band—would you please also include this?"

Anyway, the other thing I did was, instead of having the money go directly to this guy at Cal Tech, I put it through the state geologist. So Jon Price became the investigator of record. And I told him, "Jon, you're responsible for making sure that what comes out of this program is technically credible." So Jon hired a world-class mathematician who does this kind of work to essentially look over everything that was coming out of this Cal Tech geologist's lab and make sure it was on the up and up.

So there was some good work that came out. It turned out that his original survey was, in fact, flawed in some ways. And when everything got sorted out, the end results were not that far from what Jim Savage and the USGS thought had been going on anyway.

RM: Did he admit that he was . . .

RD: Oh, no.

RM: They never do, do they?

RD: No, the best light that was put on it was, well, we've now put in place a permanent system of stations which can tie into a larger national network. And Jon was able to use some of that information to look better at the long-term stability in southern Nevada. If you're looking at, "Well, are the current building codes adequate or do we need to revisit the building code?" this is a bit of information that you could use to evaluate whether we're in reasonable shape or whether the code needs to be updated.

RM: Do you have any other examples of the misuse of science by opponents and critics?

RD: Well, one that came out most recently came out after the license application had been submitted.

RM: And this was when?

RD: We submitted the license application in the summer of 2008, as I recall, June of 2008. Shortly after that—I guess it was in late 2008, early 2009—there was a paper that came out in a technical journal called *Geomorphology*. It was by a couple of Italians, and they had taken a mathematical model and applied it to Yucca Mountain and said, "Well, it's obvious that Yucca Mountain will be eroded down to nothing in 10,000 years."

It was an absurd use of an inappropriate model, and they ignored existing published data, including an observation that the surface there is right now a million years old. There's no basis for thinking there will be a significant change in 1,000, 10,000, or even 100,000 years. And when I got to reading the acknowledgements in this paper, the paper was reviewed and changes suggested by Steve Frishman of the state of Nevada. Now, what I do not know is whether the state of Nevada financially funded this at all, but certainly . . .

RM: Do you remember any of the Italians' names, so a person could maybe track them down?

RD: No, I don't, but I've got it somewhere in the six boxes of paper that I took out of my office.

RM: Any more examples like that? To me, they're significant and also very interesting.

RD: No, the state did not fund much in the way of scientific studies. In the early days, they had a fellow named Carl Johnson who ran their science program. Carl had a level of ethics that was pretty good. I mean, he paid for work that turned out to be very good work that we used. I think his bosses were very disappointed that it came out the way it did. That was about the time they quit funding really objective science. What they focused on was essentially just fighting Yucca Mountain and fighting the political perception. So they never really produced very much. All they did was attack the things that we did.

RM: And when did that change take place?

RD: I'm guessing about 1990, '91, somewhere in there.

RM: What was it like for you and your people to have this kind of misrepresentation and constant criticism? How did that affect your people?

RD: Well, it was very discouraging. Here was the state on their soapbox, and they had open access to all of the media. All they had to do was make an inflammatory claim about Yucca Mountain, and it was in the papers, it was in the 5:00 news, it was wherever. We had our hands tied pretty much, because at that time—this is before the site recommendation had come out, so we were charged with doing an objective evaluation of Yucca Mountain—all we could say was, "Well, we haven't finished the investigation yet."

So there would be ludicrous claims that came, and we were silent. But one of the things that we started doing was a more active public outreach program. Now, Jean Younker and Mike Voegele were both heavily involved in the public outreach, and that was probably one of the most effective things we did. But it was done on a one-to-one basis. We would take tours to Yucca Mountain. About nine months of the year—we didn't do them in the extreme heat of the summer—we would take tours out there. And we'd have 250, 300 people sign up for one of these tours, because we ran them on a weekend. You know, on a Saturday we'd have eight or ten buses coming from Las Vegas or from Beatty or even down from Tonopah, and take the people around and give them a chance to talk to individuals involved in Yucca Mountain.

Building confidence is a difficult thing to do. Building confidence with a corporate entity is really difficult to do. It's much easier to build a level of trust and confidence one-on-one, with an individual. So we gave people a lot of opportunity to talk one-on-one with people that were working on the project, scientists that had come in and were willing to talk about their work that was going on. It was very positive. And it was so positive that I think it was Reid who arranged to have our budget structured so it prohibited any expenditures for public tours.

RM: They actually did that?

RD: Oh, yes, they did that.

RM: So they were actually, in effect, sabotaging your public communication effort.

RD: Sure, the effort to communicate, yes.

RM: So if you had tried to develop a comprehensive public education program, they probably would have squashed it, right?

RD: Probably so. We had various things that we tried. We tried working with the teachers, with the idea that the key to this is education and communication. In retrospect, I'm not sure that's true, because people don't have the patience to sit and learn something. It's much easier to get a visceral reaction from a catastrophic sound bite than from a learned discussion about, "Well you know, here's the truth, here's the pros and the cons." Whenever I look back, I see the same thing play out in the political arena. I mean, when you look at how a political campaign is waged, that's what was waged against Yucca Mountain—the political snippets and sound bites. It was run by political operatives. I absolutely believe so.

RM: It wasn’t just the state’s program but other players?

RD: Well, by the state, and I consider it was by our senators and congressmen, too.

RM: So in your view, they had an active, deliberate program with people who knew how to do some of this?

RD: Yes. They might have been amateurs in the sense that they weren’t paid political operatives, but what they used were the mechanisms of political operatives—sowing seeds of doubt, emphasizing the negative.

CHAPTER FIVE

RM: Do you think that there was an evolution in the populace and in the posture of the state of Nevada in their effectiveness against Yucca Mountain? My perception early on was that people really didn't care. I mean, the public that I talked to seemed to feel, "Hey, they're shooting A-bombs off out here." You know, this sounds like a good economic thing for Nevada. And there was fertile ground for a long time, but it gradually changed to where the position of Reid and Bryan and the rest of them became increasingly dominant in any dialog in the state. Do you agree with that? And even before you got here, do you remember people's attitudes and everything in the program?

RD: I had looked at some of the history of Yucca Mountain. Remember Bullfrog County? I mean, there was a point in time when Nevada was actively pursuing this as an economic opportunity. We had the Test Site operating. They were blowing up 50 or 100 weapons a year on the Test Site. And there was an educated workforce. The Department of Energy at that time was a significant contributor to the economics of southern Nevada. Since that time, the Test Site has become just a memory, essentially. The gaming industry has come to totally dominate. It is the only industry in Las Vegas, for all practical purposes. And although the gaming industry never came out either for or against, they were slightly against. I mean, "What if something happens? It will scare the tourists away."

RM: Why did that attitude change? How did it evolve into where it is now, where the program is in hiatus, at best?

RD: I am not quite sure.

RM: And how much of it was Reid? It was Bryan at first, but Reid is now carrying the ball.

RD: When Reid and Bryan were both in the Senate, Bryan was the one that was the vocal opponent of Yucca Mountain. Reid was quiet. I personally thought when Bryan retired, then because Bryan had been such a strong opponent of the Test Site that Reid would probably become a proponent. But he became an even larger opponent. And I think it's because it was the absolute guaranteed way that he could get a headline any time he wanted one.

RM: I did an interview on this with Chic Hecht not long before he died. And, of course, Bryan beat him when they ran for the Senate in 1988. Chic thought that that was the main reason that Bryan was able to beat him. He told me that he thought both Reid and Bryan's opposition was cynical. In other words, it was, "Hey, this is a way to make political capital."

My perception was Nevada was sort of for it, kind of lukewarm in the beginning. I said, "Well, how did Reid and Bryan know that this was going to be a good political issue?"

And Chic said, "An issue involving fear is always a great issue for a politician."

RD: Yes. And the other side of it was that at that time, DOE couldn't fight back.

RM: Because?

RD: Because we were still trying to determine whether or not we felt that Yucca Mountain should be a repository. So we couldn't defend it.

RM: Oh, yes, that's a very critical point, because DOE kind of left the field to the negativists. And there was a reason for that, because you couldn't really get out and advocate. Even after '87 you couldn't be in a strong position of advocacy, could you?

RD: No. Even the things we did out there—we did not build a permanent building out there until 1996.

RM: No kidding? That’s a critical point, too.

RD: Because we didn’t want to give any hint that we had prejudged the adequacy of the site.

RM: And you really hadn’t. That’s really interesting.

RD: I took Senator Murkowski out there, the senator from Alaska. And, man, he wanted a repository built now. He wanted to know why we didn’t have a labor camp of 4,000 people out there. And he didn’t want to hear any of this nonsense of busing construction people from Las Vegas. He wanted them on site working 24 hours a day.

RM: Just like the Test Site in the early days, the camps at Area 12 and so on.

RD: Exactly. And we could not do that. Now, when we talk about political pressure, the most vocal media has always been the *Las Vegas Sun* and its outlets. They were opposed to the Test Site. Much of that was associated with Hank Greenspun’s death from cancer. The family blamed his cancer on his observation of some of the tests out at the Test Site. Do you understand the political connection between Brian Greenspun and Bill Clinton?

RM: No, I don’t know that.

RD: They were roommates at George Washington.

RM: That’s interesting. This is a little bit off topic, but it’s one my brother and I have wondered about a lot. How is it the Democratic Party became the anti-nuclear party and the Republicans basically are more pro-nuclear? What’s going on there, in your view?

RD: I don’t know.

RM: The Democrats are almost goofy now in their anti-nuclear feeling. It’s like France doesn’t exist, and Japan, and so on.

RD: Right. I take it back to Jimmy Carter.

RM: And what about Hank Greenspun's vigorous opposition?

RD: Hank, I think, died about the time we got here; maybe before, even. But he certainly had made quite a reputation as a vocal opponent of many things. And I think his son, Brian, who took over the editorship of the *Las Vegas Sun*, carried that on and carried it on with a fervor. And one of the things that he picked up as a personal challenge was Yucca Mountain.

I don't know if he and Bryan were of similar minds. Bryan, remember, was the governor when the Climax facility test was going at the Nevada Test Site. That's the test where we brought in fuel from Turkey Point—the nuclear power plant in Florida—and placed it underground at the Nevada Test Site at a place called the Climax facility and observed it. Well, this was done safely for a period of several years. It was before the program outlined by the Nuclear Waste Policy Act had really gotten underway. And apparently, then-Governor Bryan demanded that the fuel be removed from the Nevada Test Site.

RM: I did not know that.

RD: The test was stopped; the fuel was removed. It was repackaged and sent up to Idaho, where it remains to this day. But that was a successful test that was ongoing that was terminated at the demand of the Nevada governor.

RM: Why was he doing that? Did he say, or did you have any thoughts on that?

RD: I don't know. That was before my time. You might ask Michael Voegele, because he was involved in the Climax test. It was one of the first things he was involved in when he got here.

RM: So Bryan's opposition to spent fuel predated the Nuclear Waste Policy Act.

RD: That’s right, yes. And I don’t know what the basis for that was. We were talking about the Democrats, and, if you’ll remember, Jimmy Carter . . .

RM: Who was a nuclear engineer, of all things.

RD: But in the presidential debates with Ronald Reagan, the thing he brought up as the thing he was most fearful of was nuclear proliferation. It was because of that that Jimmy Carter decided that, although we had been pursuing a policy of recycling our spent fuel—we were going to reprocess it and remanufacture it—he terminated the reprocessing of spent fuel. Now, that subsequently was overturned and that prohibition on reprocessing has been lifted. It was lifted during the Reagan Administration, but it’s not economically viable.

RM: At this point it isn’t, yet France does it, and other countries.

RD: France does it and Japan does it, but they do it not for the economic reasons but rather as a matter of national energy security. So they’re willing to pay the extra expense because of the security that it gives them at the national level for their energy supply, rather than being at the whim of imported oil.

RM: Oh, that’s what it is. How interesting. But do you think we’re going to get more involved in reprocessing and everything?

RD: Well, if you look at what your options are for dealing with spent fuel, there was a study done in 1980 that was a precursor and laid the groundwork for the Nuclear Waste Policy Act for the selection of deep geologic disposal as this nation’s preferred alternative. It looked at a number of alternative ways that one can deal with spent nuclear fuel. If you were to do the same kind of study today—and I think they will, with the blue ribbon commission that Secretary Chu has commissioned—they’re going to look at the same things that were looked at.

The study that was done in the 1980s looked at the universe of things that could be done to spent fuel. If you did that same study today, or did a comparable study, you'd have the same list of choices. And in that list would be reprocessing; transmutation, which is taking some part of the spent fuel and making more benign products out of it; indefinite storage, which everybody agrees is not a permanent solution; and I have real strong feelings on that. Shooting it into outer space in a rocket—you know, that will work as long as your delivery system is pretty close to 100 percent reliable. And our experience to date does not support that.

RM: No. Plus it would be too expensive, wouldn't it?

RD: It would be very expensive. They looked at putting it into deep sea beds, which has pros and cons, but right now is against the law of the sea, which the United States is a signatory to. We looked at putting it in the ice caps. We looked at putting it in islands. We looked at disposal in deep boreholes. We looked at putting it underground and melting the rock around it to make a synthetic rock, essentially fusing it in place.

And of all of those things that were looked at, the one that appeared to be most achievable, most practical, and for which the technology already existed, was deep geologic disposal—mining a facility, putting it underground, monitoring it for a period of time, and then closing the facility.

Now, let's talk about storage for a moment. I mean, storage is part of the system. Whenever you're dealing with waste, you've got to have some kind of storage facility. But everybody agrees that indefinite storage cannot be a permanent solution, because in order for there to be no risk to public health and safety you've got to have some kind of perpetual institutional control. There is no institution that lasts forever. Some last a long time, but, when I was growing up, starting a professional career, there was an event that kind of signaled what can happen when you lose institutional control.

It actually started the whole Superfund thing. Remember Love Canal? Hooker Chemical closed down a plant. They had used the grounds for dumping chemical waste for decades. But they closed down, moved out. I think the company went out of business. And somebody came in years later, bought that land, and developed a housing development there, and you had green slime oozing out of the ground and kids playing in it. But all institutional knowledge of what was there had been lost when Hooker Chemical went away. They no longer had a duty to do anything.

So when you lose institutional control, you have a possibility of things like that happening. I had a personal experience that is relevant to this. When I started teaching at the University of Texas at El Paso, within the first year that I was there, there was an event that happened across the border in Juarez, Mexico. About half of the students that we had at UTEP were Mexican citizens that came across every day to go to the university and go back. The students kept us pretty much up on what was going on. And there was a family in the barrio in Juarez that had gotten very sick. Actually, some of them died. And what they eventually found—this is a convoluted story—was that the husband, the patriarch of the family, worked as a janitor at a medical facility in Juarez. In the warehouse in this medical faculty was an x-ray machine that had been donated as a gift to be installed in the hospital. But the hospital never had the power to feed the machine, so they had never installed it. Systematically over a period of time, this janitor had dismantled the x-ray machine and made junk out of it—I mean, beat it up with a hammer and sold it at a junk yard. One of the things he beat up was the cobalt 60 source.

RM: And that's very radioactive?

RD: That's very radioactive. And it had rolled around in the back of his pickup truck there in the neighborhood. The kids had played with it. So they determined that these people probably had radiation poisoning. Fast forward about six months, and there was an event that happened in Los Alamos. A truck loaded with steel rebar from Mexico came in and asked for directions. This is the story I got. The driver was lost and the guards told him where to go. They have radiation detectors at the gates at Los Alamos, just like we do at the Test Site. As it was leaving, it triggered every radiation monitor.

They backtracked where this came from. This load of rebar had come from a steel factory in Chihuahua, Mexico, that had bought junk from the junkyard in Juarez that the guy had sold the stuff from the x-ray machine to. EG&G went down and did surveys. I read that report; I don't think it was ever formally published. Each of the little cobalt 60 needles is about the size of a bit of lead pencil that's maybe a couple of millimeters long, so they're tiny things. And it's about a 300-mile drive from Juarez to Chihuahua City.

But they were able to fly the highway and find individual needles on the highway that had fallen out of the truck as it was transporting the junk down to the foundry. There were pictures of the truck that the janitor had driven. And it had been covered with concrete to shield the radiation source from them.

As far as I know, this is the worst non-military accident involving radiation that's ever happened in North America. There were people that actually died in this. Nobody died at Three Mile Island. And again, this happened because of the loss of institutional control. Everybody assumed that institutional control would be maintained. It was maintained by the manufacturer; it was maintained by whoever bought and used the x-ray machine first. It was presumed it would be maintained when it was gifted to the medical institute in Chihuahua. But it fell down.

RM: It fell down. Well, now, just playing the devil's advocate here, what would you say to the person who said, "See, that's the reason why we shouldn't have any nuclear power, because these things can happen. And we just can't control all the factors and variables."

RD: Well, it's certainly not unique to nuclear. Why would you have chemical factories? Why would you have anything that would have any risk? My God, why should we let airplanes fly? I think there needs to be an evaluation of risk versus benefit; I think the benefit is so enormous that it greatly outweighs the risk.

RM: Another thing I would add—and I'm not sure that my facts are totally straight—but I don't think the world can get out of the energy corner that it's painting itself into without nuclear and maintain anything resembling this standard of living.

RD: Well, it's not just the standard of living, but also if you want to do something about this climate change, the anthropogenic—the man-induced—climate change that seems to have been accelerated, or even brought about, through the Industrial Age and the burning of hydrocarbons. If we're going to lessen our dependence on hydrocarbons, and there are good reasons for doing that besides global climate change . . .

RM: One, we're going to run out.

RD: We're going to run out. You're absolutely right. It may be a large supply, but it is finite. And renewable energy is very attractive if you can make it economic. I used to watch people in Juarez burning tires for warmth. And they do it because of the economics.

RM: One question I have here is what it was like for you and your people working with the labs—Sandia, Los Alamos, Lawrence Livermore.

RD: Well, that was a relationship that changed over time. When I originally became affiliated with DOE, we had a small operation. Most of the technical staff had formerly worked for the Test Site. And they were not really technical staff. I mean, they weren't comfortable developing a scientific program. So one of the things that the early leaders of DOE's effort, Max Blanchard in particular and Dave Dobson, did is they brought in people that they felt could stand toe-to-toe and nose-to-nose with the prima donnas in the lab and discuss technical aspects, and that also had some level of administrative and managerial capabilities that could manage a scientific program.

One of the first people that Max brought in was a fellow named Uel Clanton. Uel worked for NASA for 25, 26 years, and was heavily involved in putting together the lunar receiving laboratory down at Johnson City, Texas, and he used his experience there to put together our core library, the sample management facility, out at Yucca Mountain. Because we knew it was so important to have a continuous chain of custody record for any of the materials that came in that would be used eventually in licensing, we knew that there would be controlled environments that we would need to maintain. We knew that there were processes that needed to be developed and maintained and rigorously applied, and a quality assurance program that would be associated with that. So Uel was deeply involved in putting that program together.

Although there had been a lot of work involving the national labs prior to the establishment of what I'll call the Yucca Mountain office, it had been piecemeal. I mean, there was a little bit of overall kind of direction given, but in the case of, like, USGS scientists, their tradition is for each individual to go and do a piece of work and then write a report, generally on an area or a particular aspect. But normally you don't interface that much with a bunch of other people; you're not part of a large team.

So putting together the framework and the mindset where you have a common objective, you set priorities, you assign resources to achieve those objectives in that priority (and it may not be the objectives that the individuals personally would like to pursue) and then you enforce that [laughs]—that was really the framework and the approach that had to be put together in the early days. And there was a lot of head-bashing back and forth.

RM: Were you the one that was doing that—or were you involved—for the geology part of the project?

RD: Well, I was peripherally involved in the early stages. When I got here in ’88, within the regulatory site evaluation division three branches had been established. Two branches had been staffed, nominally—there were, like, three or four people per branch.

RM: What were the branches?

RD: There was a site investigations branch and a technical assessment branch and the regulatory interactions branch. I think I started with the regulatory interactions branch because, after all, we were trying to answer regulatory questions. So we were charged with interacting with the regulator, the Nuclear Regulatory Commission. The flow of paperwork—I told you about the site characterization plan and the follow-up, the study plans. This is kind of formalizing the agreements—“This is what we intend to do in order to answer the questions, the regulatory questions.”

The site investigation branch was run by Uel Clanton, so Uel brought in some people—like Roy Long, who was a drilling engineer—because we had some unique needs. Usually when you’re drilling a well, you use water or mud as the circulating media . . .

RM: That carries the stuff up.

RD: It washes the stuff out; it cools the drill bit. But because what we were interested in was the water content in the rock, if we introduced water, we would be contaminating the well. So all the investigation wells that we drilled were drilled dry, using air as the circulating media. That way, we didn't introduce any water. But we had to work with the manufacturers to develop equipment that had capabilities that could meet our requirements, because we needed to drill deeper than they typically did at that time. And usually when you're drilling a well, you only take core samples of rock periodically. Mostly what you do is just look at the bits of rock that come up out of the hole. But we wanted to have intact cores as much as possible. So there were many holes that we cored 100 percent of.

RM: How deep were some of those?

RD: Some of the holes were 2,000 feet deep.

RM: Two thousand feet of core? Oh, my God. You can do that, technically?

RD: Yes, we did it.

RM: So those cores became a part of your library.

RD: That's right.

RM: And where is that library?

RD: That library, as far as I know, is still located at the Nevada Test Site at Yucca Mountain. We took two of the buildings that were out there and refurbished them, put in coolers and protective fencing, security measures, and processing equipment to put that core in and essentially make a rock library, an archive.

RM: I wonder what's going to happen to that in the chain of command, which you talked about.

RD: Yes, that's a very big question. I was asked what should be done. And my response—this was before I retired—was that you must maintain the core library and all the quality assurance records associated with it and all of the QA records for the program until everything, all of the litigation, has cleared the Supreme Court level.

RM: And how long do you think that will be?

RD: Twenty or 30 years.

RM: It's going to go that far?

RD: I think so.

RM: It's hard for me to imagine that in 20 or 30 years they won't be building Yucca Mountain out there.

RD: I don't know. The whole idea of what to do with nuclear waste is driven in some sense by a level of fear. Now, the standards that are put in place are almost irrational. I mean, you're looking for a level of confidence, and there's a presumption that nuclear waste is so much more dangerous than any other material around. I'm sorry, but microgram for microgram, anthrax has it hands down.

RM: I didn't know that.

RD: But we have almost made this bogeyman out of nuclear waste by making it seem so much more horrible and inflating the real risk that I believe is associated with it. So, I'm thinking the facility that we were planning—yes, it could be built. It would, I believe, have worked well. But I think it's probably much, much more than really needs to be done to give an adequate level of protection to us humans and to the environment. And I don't know if there's ever going to be a reprioritization of what's really important.

RM: As nuclear power evolves, it will become more obvious to people that we've got to have nuclear power, given all the energy problems. I've talked to Tony Hechanova over at UNLV from time to time over the years. I get the impression that the kind of thing that he's working on, transmutation of nuclear material, is inevitable in terms of the technology that human beings are going to develop in the future.

RD: It probably will be.

RM: And it may well happen at Yucca Mountain, or on the Nevada Test Site. The Nevada Test Site, to me, is a perfect place for it, with the exception of the water issue, which I think is a little bit of a canard.

RD: That's probably the downside. If you have a process that's water-intensive, it's not a good place. But reprocessing will almost certainly come to be. It's just a question of whether it will take decades or centuries to develop a technology that's adequate.

RM: You don't see present reprocessing technology as adequate? The kind, say, that the French are doing?

RD: The efficiency needs to be improved by a factor of at least ten.

RM: Really?

RD: I would say so. I mean, remember, the French technology is, like, 40 years old. So there's a lot better way of doing things now, but nobody's willing to make the expenditure. And who would make the expenditure, industry? Why should they, because right now it's cheaper to mine uranium and use it first generation. Whenever the economics are such that reprocessing becomes economically attractive, I would expect industry to embark on reprocessing.

RM: And do you think transmutation associated with power production is in our future in, let's say, 40 years?

RD: I don't think so. I think transmutation is an intriguing idea, along the lines of fusion energy.

RM: You would put it up there with fusion?

RD: It would be a panacea if we could do it.

RM: The old joke about fusion is that it's always been 30 years away. [Laughs] For the last 70 years it’s been 30 years away.

CHAPTER SIX

RM: What about working with the M&O companies? In the same sense that you had to work with the labs and everything, you had to work with the M&Os, right?

RD: Yes. Well, we touched on that a little bit yesterday. When the original M&O, TRW, was brought on, the job they were hired to do was not the job that they had bid on because the job description that was advertised and that they bid for was to manage and integrate the three parallel site characterization programs (at Hanford, Washington; Deaf Smith County, Texas; and Yucca Mountain, Nevada). By the time that contract was let, there was only one site characterization program. So they essentially didn't have the same job. They were promised a job, so the job they were awarded was the job that was already being done by the labs, who were doing the engineering work and everything else, and by SAIC, who was the technical and management support services contractor.

So there was a lot of turmoil and chaos and blood-letting trying to figure out some kind of—"equitable" may not be the right word—but some kind of reasonable distribution of responsibilities that would allow people that had been doing a job and doing it well to continue doing some part of that, yet still letting the M&O contractor in.

Now, once we had TRW, the die was kind of cast. And we, over time, reassigned the roles and responsibilities. Initially, when I came aboard, there was DOE, with SAIC directly supporting DOE. And then reporting to DOE were a number of essentially independent contractors, each of the national labs—Los Alamos, Livermore, Sandia. PNNL had a small role at that time. We had a number of contractors out at the Test Site: REECo, Holmes and Narver, Fenix and Scisson. EG&G was involved, Johnson Controls—a multitude of contractors, all of which reported directly to DOE.

Whenever an M&O came onboard, things were shifted around so that DOE was here, and then all other entities worked in some way through the M&O contractor. So DOE's involvement was through the M&O. Now, that was a very sticky point with the national labs, because the national labs are DOE entities. So the M&O was depended on to help provide the overall strategy and game plan, but then the labs were expected to interface and work with the M&O to improve that strategy. And then DOE provided essentially the objectives, the direction, and the priorities, and we were responsible for getting the budget and disbursing the budget.

The relationship between all of the actors kind of mutated over the years. The labs maintained a very strong independence for a long period of time and were actively resisting, and even fighting, the authority of the M&O.

RM: They didn't like that subordinate position, did they?

RD: No, they had no reticence about directly calling somebody from DOE to interfere on their behalf in order to overturn some decision that either had been made, or was being made, somewhere in the construct of the M&O. I mean, I was involved in an awful lot of that. Sometimes the labs were right, and sometimes they weren't.

RM: But that was part of the bloodletting that you mentioned.

RD: Yes. I guess it was about 1997 that it was time to recompete the M&O contract. Now, this was about the time that we wanted to do something called the viability assessment.

RM: And what was that?

RD: It was a report that was requested by Congress that essentially was going to be an interim status report that said, "You know, you spent a lot of money in Yucca Mountain. Now, do you want to spend any more? Based on the money you've spent so far, what does it look like? Is it probable that it's going to turn out to be okay? Are there some significant things that have already been turned up that look like things are headed south?" So that's what the viability assessment was. We were scheduled to turn out the viability assessment, as I recall, in about 1997, 1998.

And things are getting a little cloudy here. I'm not sure whether it's the viability assessment or the site recommendation that was the critical product, but it was one of those two products that we were depending on our existing M&O contractor to wrap up in a very short period of time. But we had this mandatory recompete coming up. I was with Lake Barrett in Denver when he was talking to Secretary of Energy Bill Richardson and saying, "You know, we really need to extend this contract for about a year, and then we'll be happy to recompete it after we get this major product out."

And Richardson turned around and said, "I don't care about the product, you will recompete." This was in the Clinton Administration. And I am convinced that Richardson had directions from the White House to drag things out. I mean, progress is not necessarily a good thing for a politician.

So by recompeting, we had a turnover in the workforce, we had a turnover in management, and the product that we thought we were going to get out in about six months got delayed by about two years.

RM: And again, you think that Clinton was not a supporter of the whole concept?

RD: I think that's true. I think Clinton was paying back some debts to his college roommate, Brian Greenspun.

RM: That's really interesting.

RD: I was never in a meeting with either of those two.

RM: Sure. And so, again, you have the Democrats for almost extraneous reasons being drawn into this anti-nuclear position.

RD: That’s right. I don’t think it started as a national policy. I think it was a personal agenda that was forwarded through personal ties, not through Democratic Party process.

RM: I, myself, am a big believer in the stochastical nature of society. And this is a stochastical thing here. If Clinton had had a different roommate, it might have been a different story.

RD: Yes. And I think the other side of that is that Harry Reid, prior to the last Democratic presidential primary, essentially before he came down and supported a candidate, waffled for a long time as to who he was going to support. And he extracted from Obama that the price he wanted was Yucca Mountain.

RM: And again, if Harry Reid had been dropped on his head by his mother when he was a baby, we wouldn’t be having this conversation. [Laughs]

RD: Right.

RM: What exactly was your job as division chief there?

RD: I had a number of jobs through my tenure with Yucca Mountain. When I came in 1988, I was a staff geologist. And the things I worked with early on were establishment of a quality assurance program, developing some number of these things called study plans. And I got involved with what’s called performance assessment and essentially took over that job because this was the regulatory demonstration of the adequacy of the performance of the system. So it became part of the responsibilities of the group that I was in. I later became the chief of the technical interaction branch and had essentially all of the science programs.

RM: You were in charge of all of them? When was that?

RD: Oh, I was only in that job for about a year. That was maybe ’92 to ’93, something like that. And then I became the division director for the regulatory site evaluation division, which had all of the science programs and all of the regulatory programs.

RM: That whole one division, and you were head of that?

RD: A whole one division; I was head of that. And then I became the deputy project manager, which was over everything.

RM: When did you take that job?

RD: That was 1995 to 1997.

RM: What was that like, to be the deputy of a huge project like that?

RD: It was good training, because in 1997 I became the project manager.

RM: I didn’t know that.

RD: Yes, from ’97 to 2002.

RM: Well, let’s talk about that. That’s pretty interesting.

RD: Yes, it was. But, of course, you get further removed from technical interaction. Most of the things you’re dealing with are administrative or managerial in nature, mostly people problems, either within the DOE group or interpersonal issues between the contractors, the labs, and DOE or the state, or dealing as the kind of the public front of the project. I did an awful lot of public presentations for a long time.

RM: What would be an example of that?

RD: Oh, discussion with the National Association of Regulatory Utility Commissioners, NARUC. These are the guys that determine whether or not each state is going to grant a rate increase to a utility, based on the facts presented. I testified before the state legislature of Nevada several times.

RM: Was that a hostile experience?

RD: No, it was not. Actually, the oversight committee, the state legislative oversight committee, was quite friendly and receptive. And, I think, secretly supportive. Now, the political committee that started out as the McKay Commission—and now I believe ex-senator Bryan is the head of it—is a totally political committee. Their agenda is to stop Yucca Mountain. But dealing with the individual senators and representatives was a real pleasure. I gave many, many tours of Yucca Mountain to US congressmen, senators, industry CEOs, and so forth.

RM: Did you interface on a regular basis with the congressional delegation?

RD: Not on a regular basis. I mean, I personally took Harry Reid out to the site three times. The first two times he was pretty non-committal. That was while Bryan was still a senator, also. It was really hard to read Reid, but I got the impression that he was not particularly opposed to the site. I mean, he seemed to be in favor of resurrecting above-ground nuclear testing. Why would he have been opposed to dealing with nuclear waste?

RM: Yes. I'm probably too speculative, but I was wondering what accounted for his increasingly hardened position.

RD: I believe it was a cynical calculus that, "This is the issue that will get my face before the voters the most."

RM: I would agree with that, yes. I was at the first meeting they had on Yucca Mountain in the spring of '83 at UNLV, where Governor Bryan came in with his entourage like he was Jesus Christ, and announced that he was unalterably opposed to the proposed repository. And he was the first speaker. To me, the key word there is "unalterably." That means irrespective of the evidence.

RD: That's right. And I do not think the state's position changed from that utterance.

RM: But Reid sent a surrogate. He was a congressman then, and it didn't have the dramatic effect there was when Bryan came in, and then went out with his entourage as soon as he had made his statement. I'll never forget it. I didn't know much about nuclear waste or anything then. I thought, "Wow, it's kind of weird for a guy to use the word 'unalterably' where they want to come and spend a few billion dollars in your state."

RD: Yes, and, "I don't care what the facts are."

RM: Yes, right. Now, do you have any other thoughts on being head of the whole project? How do you look back on it, and what are your feelings about it?

RD: Well, it was often frustrating, but never boring. I mean, every day was a different problem. I was very fortunate in that I had some very supportive bosses in Washington who greatly isolated me from the true day-to-day political machinations. They made it possible for us to get an awful lot done.

RM: Would you like to mention any names?

RD: Lake Barrett's the first one to come to mind. Dan Dreyfus did a great job, also. But Lake was the acting director on and off probably for more time than any other director. And he is responsible for a great amount of what we were able to accomplish at Yucca Mountain.

One of the other things was that DOE tried a new experiment here. Rather than taking people through the management development cycle in DOE, where you grow up and you're sent to different assignments to learn something, and then assigning a manager to a technical problem where the manager may or may not have any experience in that particular technical arena, what they did here was take technical people and bring them in and then construct a management team from the technical people they had brought in. So, the emphasis was always on the technical competency, rather than the management side. And I don't think we did a bad job with management, either. But if your emphasis is on the management side and the bottom line, then there's always a level of suspicion. Did you slight the technical side? And we always wanted to get the technical story right.

RM: What did you do after you were director— is that the right term?

RD: I was the project manager until late 2002. Then we had a new DOE program director that came in, and she chose to bring an individual up from Albuquerque. And we did away with the Yucca Mountain project. We changed the name. We had a reorganization. We reorganized about every five years, it seems like. So I stepped back to a supporting role for this new individual for a period of time.

RM: What was her name?

RD: Her name was Margaret Chu. She was the director, formerly, of nuclear waste programs at Sandia Labs and had a lot of experience with rad waste, primarily through the WIPP program—a little bit through Yucca Mountain, but mostly through the WIPP program. And then she left, and we had an interim director who came in, and then he reorganized again. And I got a new role at that time, which was chief scientist. In the last four years, I think from 2005 until I retired, I was the chief scientist.

RM: Chief scientist. And what was that like?

RD: That was really a fun job because I got to concentrate on the things that I thought meant a lot.

RM: What did you see as those factors that meant the most?

RD: Well, making sure that we had sound evidence in place, that whenever we said something we could demonstrate that it wasn't opinion, but there was a basis for it. And that we didn't just cherry-pick things, that we systematically went through things. If there are a number of potential ways to address this, what are the pros and cons of each of the ways? What's the answer that comes out of each of these ways? What can give the worst result and the best result? And which one is more probable? And discuss all of those, rather than just saying, "The world is red," or whatever.

RM: Had the science been pretty well nailed down by the time you took that position, or were there still a lot of open questions?

RD: Well, there will always be open questions. But let me rephrase your question: Did we feel confident enough about the science at that point in time to be able to go to the next step? Because the way this program is designed, it is a series of step-wise iterations, step-wise improvements, or moves forward. And the idea is to never have an irreversible step. You can always go back to the previous step and do something else. Even when you get to the point of building and operating a repository, there is the provision in the Nuclear Waste Policy Act that says you've got to be able to retrieve everything if you have to. So even when you get to the point of having an operating repository, it is not irreversible.

RM: Yes. Did you do any interfacing with the professional review?

RD: The Nuclear Waste Technical Review Panel?

RM: Yes, those people.

RD: An awful lot. It's difficult to talk about the nature and character of the Nuclear Waste Technical Review Board because the character and nature changed with the makeup of the board. The character of each of the boards was set by the chairman of the board. We'd been through, I guess, about seven different chairmen, among them John Cantlon, who was an early chairman of the Nuclear Waste Technical Review Board, and who, by the way, was a graduate of UNR.

And the last chairman that I knew is John Garrick, who is a very eminent scientist. Some of the boards were much more political. Supposedly, the members of the board were nominated by the National Academy of Science and appointed by the White House. It's clear that there were some boards on which it was much more important what your political affiliations or gender or race were than what your technical qualifications were.

RM: So there was a big political-correctness dimension in all that?

RD: Oh, yes. I think the first boards were much more technically competent and the last boards were much more technically competent. There was an interval in the middle, especially during the Clinton Administration, when it was much more important what you were and what you represented.

RM: Again, we're coming back to this. That's really interesting.

RD: Science is not democratic, and if you try to apply democratic principles to everything, it doesn't work all that well.

RM: It's like the movie *Casablanca* where Claude Rains comes out and says, "There's gambling going on in here. I'm shocked, shocked."

RD: I'm shocked and appalled. [Laughs]

RM: But who would say this here? [Laughs] Did they influence you a lot?

RD: Oh, yes.

RM: In what ways?

RD: Well, let's take the first board, which actually was chaired by Don Deere, who was an eminent mining engineer, I think from the University of Illinois. At that time we were looking at our site characterization plan and how we were going to investigate the underground at Yucca Mountain. Our plan at that time was to put a vertical shaft in, go down about 1,000, 1,200 feet from the surface, and put a couple of rooms in place, do some fairly simple experiments. But it was a relatively small-scale endeavor.

Don Deere was a champion of mechanical excavation, tunnel-boring machines. He was one of the big consultants on the Chunnel under the English Channel, between France and England. And he brought this essentially and laid it on our steps and said, "You must do significant underground exploration, and the tool to do it with is the tunnel-boring machine."

So instead of this pretty small facility, what came out was—I guess it's a five-mile long loop, the exploratory studies facility, the big tunnel, the 35-foot diameter tunnel with an additional two-mile-long 25-foot diameter tunnel. The change from a very limited underground site characterization to a much more extensive site characterization effort was entirely due to the Nuclear Waste Technical Review Board.

RM: Was the big tunnel warranted, in your view?

RD: In retrospect, yes, although it took a significant amount of the project's resources, and it took about three years longer that it would have.

RM: Just because of this board.

RD: Yes. The other thing that intruded—we had been getting budgets on the order of $450, $550 million dollars a year for the program. And in 1995 we got a budget of zero dollars.

RM: Really?

RD: Hazel O'Leary submitted a budget request of zero dollars.

RM: Again, that was Clinton, wasn't it?

RD: That was Clinton.

RM: Why did she do that?

RD: Well, this was a pissing contest that she had with Congress, because each of the cabinet departments has a cap put on it as to how much total budget they can request.

And she said, “Well, Yucca Mountain is funded out of the Nuclear Waste Fund. I mean, the funds don’t come out of anywhere else. They’re sitting there waiting to be expended. The funds from Yucca Mountain should not count against my cap. So I’m not going to put any Yucca Mountain funds under my cap.”

And Congress said, “Well, okay, that means zero for Yucca Mountain.”

Well, finally, in conference there was a move to fund Yucca Mountain for $250 million, which was about less than half of what we were planning on and what we had had the previous year. That was the year, Christmas Eve, we laid off something like 1,500 engineers.

RM: Oh, my God. Did it ever regain its momentum after a layoff like that?

RD: I don’t think so. I mean, we came back to it several times, but that really kind of broke the back of the engineering group, because what our emphasis was on at that time was site characterization, which was the scientists. Remember, I was talking about the iterative process of the observations and then designing something? Well, we were busy making the observations, and then all of a sudden we fired everybody who was charged with doing the engineering part. So we had essentially decimated our ability to try to design the engineered system that could augment and complement the natural system.

RM: Given this and other problems, if you were recommending how to design a new program, what would be some of your thoughts on that?

RD: I absolutely agree with Mike Voegele and Jean Younker that it ought to be, at best, a quasi-governmental enterprise. TVA is the model I have in mind. I actually went to TVA and talked to them for a couple of weeks to try to understand how we could bring that model of management and interaction with the Nuclear Regulatory Commission to Yucca Mountain. I’m convinced it could be done. Or do it as a private corporation.

RM: Where you would have continuity of budget and less interference politically and maybe less "visibility and democracy."

RD: Yes. Now, to return to the directors we had. Ward Sproat came on as our last director, and Ward was the most qualified person that had ever sat in that job. He had worked for the nuclear industry for years. He had been program manager for the Pebble Bed reactor project in South Africa. He had interacted with the Nuclear Regulatory Commission for decades. I sat down with him and told him my take on things. I said, "You're in a very unique environment here. You are the most qualified person that has ever sat in this chair. Just remember, as you're putting together programs and plans, there's a significant probability that your replacement is going to be a dentist, somebody who has absolutely no experience, no role, but is being paid back for political patronage."

RM: Looking back at the program, how much of it was science and things that really had to be done to make a really successful project, and how much of it was just dealing with all this extraneous stuff and the politics and laying off and then having to hire again and all of that?

RD: You know, we replanned the project every year, which means that we went through and essentially did a zero baseline, building from the bottom up, every year. Because we had to re-look at what the resources were, we had to reevaluate what our priorities were, based on what we had determined the previous year. We had a new budget. Often, in the federal system, you don't know what your budget is for the year until the year's halfway gone. So do you spend at a very low rate and hurt yourself in the long term, or do you spend at an optimistic rate, hoping to hell you don't go to jail if the final budget is not what you're betting on?

RM: So people who are dedicated to this project and know a lot about it and everything essentially don't have tenure, right? I mean, it's, "Maybe I'll be here next year, maybe not."

RD: That's right, yes. It has been amazing to me that we have been able to maintain the level of technical and professional competence on this program that we have in spite of that incredible uncertainty. I know that for years people's insides were churning because there was so much uncertainty as to whether things were going to be here next year or not. And usually it's the contractors that face that uncertainty. But they kind of become acclimated. With the federal staff there's generally an expectation of some level of security and certainty. We didn't have that at Yucca Mountain because there were always talks about disbanding the problem, you know.

RM: So the federal employees didn't even have tenure?

RD: Tenure, no. Even among the federal staff there was a lot of angst and apprehension. At one point, I drew an analogy between the people that worked on Yucca Mountain and people in the Peace Corps. They weren't there just for the job; it was something more than just the job. There was a sense of personal satisfaction that was involved. It had to be part of the equation, because the money alone could never have paid you for all the anger and turmoil and heartache that you had to endure.

RM: And it probably wasn't like that on a lot of other big projects like this, like Boeing building a new airliner or something. How could somebody have made your job better and more productive?

RD: Well, trying to isolate the individuals from the day-to-day political infighting. I think some people were very successful in doing that. But it's hard to do when you pick up the daily newspaper in the place where you live and you're accused of being a charlatan.

RM: Yes, a Keystone Kop.

RD: That's right, yes. Even though your bosses say, "You know, this is just politics, don't pay any attention to it," it's hard not to pay attention to it.

RM: Yes. Looking back, how do you feel about what was really almost a career there?

RD: Yes, pretty close. Well, I'm very proud of what I did and the contributions I've made. I'm even prouder of the overall contributions that we were collectively able to accomplish. I'm immensely proud of the technical work and the unfailing integrity of the technical staff. I am sorely disappointed in the gutless politicians who are afraid to allow a technical process to come to fruition.

RM: Well, the last chapter may not have been written.

RD: We'll see.

RM: We'll see, yes. Is there anything else?

RD: I can't think of anything. I mean, we could go literally year by year, but I think we hit on the big points. You know, after we talked yesterday, I lay in bed thinking, "Now, what are some of the things I want to make sure come out?" And I made sure that those were addressed at some point today. So I can't think of anything in particular that I wanted to make sure got in the record. There's a lot more of minutiae and trivia that we could go through, but I'm not sure it's all that relevant.

RM: Well, Russ, I thank you for talking with me. This has been very interesting and informative.

INDEX