An Interview with

JEAN YOUNKER

An Oral History produced by
Robert D. McCracken

Yucca Mountain Series

Nye County Town History Project
Nye County, Nevada

Tonopah
2013

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 Jean Younker and Michael Voegele at Michael's home in Las Vegas, Nevada, June 28 and July 16 and 20, 2010.

CHAPTER ONE

RM: Jean, why don't you start by telling me your name as it reads on your birth certificate?

JY: My name on my birth certificate is Jean Lower, and I was born in Gladwin, Michigan, in July 1946.

RM: Where did you grow up?

JY: I grew up in Gladwin, in a very rural area on a farm, and went to high school there. Then right out of high school I went directly to Michigan State, which is downstate in Michigan, for undergraduate and master's degrees. I studied geology and started out teaching. In those days women were not necessarily encouraged to go into the research side of geology, the industrial side, so I was studying a curriculum that would lead to teaching earth sciences in a community college–level setting. Then above the master's level was when a little bit more was opening up for women in the field. I happened to have a master's professor, mentor professor, who was very inclined to promote women into the field. And so he encouraged me to think about going on for a Ph.D.

At that time one of the people in the field that I was most interested in was in the beginning days of computers being used in some geology specialties. So I was interested in the application of that to some fairly major earth science questions.

RM: What were some of those major questions?

JY: The biggest picture would be evolution of life, but particularly invertebrates, and what I understood about earth processes, which is, as you know, that the continents are moving around as part of plates that have been in different positions at different times

throughout earth's history.

One thing that's a very big problem for people who are looking at the evolution of life is the fact that the environments where organisms have lived and been preserved are totally biased in terms of the preserved record because of the processes going on where the continents are moving around colliding with each other—one plate goes under the other. As a result, the fossil record of the distribution of life that we see is the sample that's left from the environments that were preserved, and those environments are a highly biased sample because of those geologic evolutionary processes.

So I was curious. At the time, there were three or four of us within the US who were all Ph.D. students at the same time, and we were all curious, coming at it from different directions. Would there be a way to sort out, using computer models, whether the kind of earth history that we put together—based on our sampling of the fossil record—has any chance of having any semblance of representing the real picture of what really happened? So there were about four Ph.D. dissertations similar to mine, all at the same time. My research was a fairly theoretical computer simulation that basically asked the question, "From what's preserved in the fossil record, do you really have a shot at estimating the true diversity and abundance of species through time? Or is it so messed up by the preservational biases that you will never know?"

RM: What was your conclusion? I'm dying to know.

JY: Well—interesting sidelight—the guy I got my Ph.D. with had published papers about the fossil record of a particular organism and how we were seeing real patterns and those patterns really meant a lot. I interacted with a guy at Harvard at the time who's a very famous evolutionary biologist, Stephen Jay Gould.

Well, Stephen, based on the fossil record, came out with the theory called

punctuated equilibrium, which has to do with the way species evolve. Based on my thesis conclusions, together with those of two or three others, we came to the conclusion that it's highly unlikely that you have good enough preservation to test the various ideas about evolutionary theory using the fossil record. The biases are so great that, unless you can figure out ways to extract those biases, you're not getting a strong enough signal. The signal is just overwhelmed by noise. The noise is process noise based on plate tectonics. So you can imagine that we weren't very popular. When we gave our presentations at national meetings, all the great men in the field came down on us like a ton of bricks.

RM: You were junior people.

JY: We were questioning the truth that they had revealed from the fossil record. Another great story: one of the men was famous and had done a huge amount of research on one particular type of fossil, the brachiopod. That's a critter that lived between about 500 million and 250 million years ago and was extremely abundant, very successful, had colonized the oceans, and was a very good indicator of shallow marine environments. They tended to live where trilobites lived, in about the same type of living environment. Anyway, this man had supposedly handled and classified and analyzed more of these brachiopods than any person in the world. So he was there at this big symposium where I gave my talk, and he came up to Jack, my friend who did a similar Ph.D., arm on each shoulder after the talk. He said, "I just don't understand how two such obviously bright young people would be involved in such mental masturbation." [Laughs]

RM: Challenging the truth of the field. And how have your results held up since then?

JY: A lot of people carried on from there, thankfully. I continued in the field and taught for a while and did a fair bit of research. The main thing that happened was people figured out how to attempt to extract the bias. And so they went off in a lot of different

directions. The more you can put together the paleo environments that existed at a given time in earth history, the better shot you have at taking the fossil record that you have and properly placing it, rather than making it have an extreme value in the total picture of life. So I think people benefited from what we all did as young upstarts in the field.

There were probably, in total, over a five-year period, ten of us who did similar types of work, and then people went off and did what needed to be done in order to put a little bit better factual basis into the fossil record, but they've made some changes and interpretations.

RM: So, what do you think now? Do we have a clear picture or not?

JY: Much better, we've recognized the biases and used numerical modeling to extract them.

RM: This is so interesting to me. How do you extract biases from data like that?

JY: It's numerical computer simulations. One way that you look at the distribution of life, and try to look at what is happening, is to look at the number of species that exist in a given environment—how many different genera there are, so what the diversity is. And then the abundance, the total abundance.

Those things can be numerically represented. Then let's say you have an environment that in one particular earth period is way overrepresented because of good preservation conditions that lead to a bias in the record. What you do then is attempt to recognize that's just a tiny sample, and that in fact it's a biased sample. It's giving you a better picture of life at that time than perhaps a whole period where you didn't have the right kinds of environments but now it looks like something totally different happened.

RM: But is only because, essentially, of a sample problem.

JY: Yes, exactly. There was certainly a lot of evolution going on and probably lots of

both punctuated equilibrium and classical Darwinian type of evolution. So I think people now think it all happens; it just depends on the environment. And then throw in some catastrophes here and there.

RM: Did you get to know Gould at all?

JY: Oh, yes. I knew him well.

RM: Say a little bit about that. He's a hero of mine.

JY: Yes, a phenomenal man. It's so sad that he died so young. He had a lot of influence on me. The man that I went to work on my Ph.D. with was a colleague of Steve's; his name was Dave Raup. He was at the University of Rochester. That's where I started my Ph.D. Dr. Raup left the University of Rochester after I got there, and so I had to go back to Michigan State to make it work, but he was on my committee. Dave Raup even was a kind of visiting professor at Michigan State while I was there. So he was there, and he gave me a lot of feedback on my thesis.

I paid a lot of attention to what Stephen Gould said in those days. Smart, very smart man and such a genuinely nice person.

RM: He was really as smart as, say, I would think he was?

JY: Oh, yes, and he cared about his students. My understanding is that some of the profs on the staff of Harvard at that time had a reputation for pushing their students out on a limb, and Gould would go and defend them and get them back on track. He was just a genuinely good man. He had a couple of kids. It was very sad when he died.

RM: Oh, it was awful. But, you know, he beat the disease [cancer] for a long time.

JY: He did, for a long time.

RM: I always remember the article he wrote on it. He said, "Statistically, I've got so many months to live, but," he said, "that's statistically." And he lived several years.

JY: I think he lived five years, at least, past the time they said he had almost no chance. But, man, he was just a very great communicator, just a really good man.

RM: Well, I could talk about this for hours, but let's go back to your biographical information. What is your father's given name and when and where was he born?

JY: Yes, his given name is Herman Lower and he was born in Freeland, Michigan, in 1922.

RM: And how did he earn a living?

JY: My father was educated through eighth grade, and then he quit school and never had any further formal education, except as short courses when he worked for Dow Chemical Company after he served in the navy from 1941 to 1946. He served all over the world in the navy. Dow Chemical Company sent him to short courses at the University of Michigan. So he went right from eighth grade to college courses, and, of course, did fine. He was really self-educated.

He worked throughout his teens; I don't know too much about those years. I know he worked in garages, parking cars and washing cars. He talked about having worked for Herbert Dow, who was the founder of Dow Chemical Company, as kind of a chauffeur.

RM: Did he serve in any famous ships or battles or anything like that?

JY: He did, yes. He was on one really very famous convoy that went into Murmansk, Russia. There's a book written about the convoy that went before his; he was in the convoy that went after the one where the whole convoy was lost. What they did was take supplies into Russia. I think there were 35 ships in his convoy, and they lost something like seven to German submarines on the way in, but they had to go up through the Arctic, above Spitzbergen Island, through that ocean, and back down. And underneath, almost all the time, German subs chasing them. Three months in Murmansk; he still remembers and

talks about his time in Murmansk.

RM: How interesting. And your mother, what's her name?

JY: Her name was Doris Stickney, and she was born in 1922 in the same area in Michigan where I was born. She went through high school in Gladwin, the same place that I did, and then she worked at Dow Chemical Company as an accountant/bookkeeper in training. They had at that time many training programs for people, and she stayed there for a number of years. Then finally she went off on her own and worked other bookkeeping/accounting jobs.

RM: So, you got your Ph.D. in geology. Then where did you go?

JY: I taught for many years while getting my Ph.D., and then, after getting my Ph.D., taught around at universities in the Midwest—University of Illinois, Chicago, which was the Chicago campus of the university that has its main campus at Champaign-Urbana. Taught there three years. Taught for two years at Indiana University; then Purdue University combined campus in Indianapolis, which was very interesting. And then after that I taught at San Jose State part time.

RM: Oh, you went west?

JY: Went west at the same time that my ex-husband got a position at Lawrence Livermore National Lab in Livermore, California. I didn't think I wanted to work there, because it was the "bomb factory," and I was rather not into bombs at that point in my life.

RM: Sort of a 1960s viewpoint.

JY: I was still in the '60s, but it was 1977 or '78.

RM: Once you're there you never really get out. [Laughter]

JY: Right. So I took a year, taught part time at San Jose State, was waiting for

something to come through either there or at the US Geological Survey in Menlo Park. I had a good shot. I had an offer from the USGS at the same time Livermore decided they wanted to try to get me to come to work there. I got to know a lot of the people through my husband, ex-husband, and so they really recruited me and tried to convince me there were so many neat jobs I could work on besides the weapon side of Livermore. Finally, after some discussion, I got to know some excellent people, great people, who worked there. I finally said, "Okay, I'll try this."

RM: And what year was it that you went to Livermore?

JY: I went to work there in 1980. I worked on the weapons testing program, the underground nuclear weapons testing at the Nevada Test Site. I went in as a group leader because, with a degree at that time and some university teaching, I qualified as a first-level manager. So I had what was called a group with nine people under me. All the guys that worked with me and I were called "containment geologists." Every time there was a weapons test—as I'm sure you well know, they were called devices not weapons; they are not deployed, so they're not a weapon—we assigned one or more of our people if it was a Livermore test, and sometimes even if it was a LANL (Los Alamos National Laboratory) test. Or, if it was someone else's test, they also would put geologists from a different facility on just so there would be a mix of people and ideas. Anyway, on our own tests we would always have several geologists assigned to it. Our job was to go through the data, if there were any data, and determine whether or not the site had a reasonable chance of not being a leaker.

RM: Before it was shot?

JY: Before it was shot. When I started there, they hadn't recorded very much in databases. So you pretty much had to go around and talk to all the guys that worked on

the project and say, “Oh, yeah, I see we had three shots over here, two shots over here. What do you think is going to happen if we do a shot here? What are the trends?” We didn’t have the geology for the Test Site put together very well at that time. And, although we had some broad geology, the idea that you could actually say, “Okay, if I site a new device test here, based on what has happened nearby, what’s the probability that it’s going to have problems with either caving or leaking?”

RM: Those would be the two main problems you would have—caving and leaking?

JY: Right. Generally, if it’s a cave, it’s going to leak. So, of course, you want one that’s going to be solid and not cause the plugs to fall in. So we would always have a couple of our people—I generally was on all of them and then at least one of my guys. I’m sure you know this history, but there were, after we had the major leak in . . .

MV: Baneberry.

RM: I don’t know this history, really.

MV: In the seventies.

JY: After a major leak, when we watched the cloud go north toward Canada and there was a lot of consternation, they set up something called a containment and evaluation panel, chaired by some gray hairs, very smart guys, geologists and engineers in the field, both from the USGS and from some of the engineering firms that were involved in the weapons program. And for every shot from then on, you had to take this information that you had compiled before this review panel, and they’d pick you apart. Because it was very bad press, along with not being very healthy, to have major leaks, major venting. So that containment evaluation job was probably about my first year at LANL, or a little more. I spent three years total.

RM: Did you spend a lot of time at the Test Site?

JY: I did. I spent quite a bit of time there.

RM: So you went back and forth from the Test Site to Livermore?

JY: Yes. Livermore flew several planes out there, just small private planes, that would fly directly from the little airport by Livermore to the Test Site at Desert Rock. So you could almost go for the day and back if you had to. Normally you went and spent a couple of weeks. I spent a lot of time out there.

MV: I checked: the Baneberry incident was in 1970.

JY: So right after that was when they really tightened up and began to get a lot more procedural. Part of my job, frankly, during that time in the late ’70s was to try to put a lot more procedure around how the geological data were set up and presented and made available to other people. We went around and interviewed a lot of the containment physicists, because there was so much knowledge in their heads that hadn’t been written down. We tried to document as much as we could, so we could get it into databases—at that time there were a bunch of them retiring. That was part of my job too.

But after that I got involved in a very interesting project in seismic verification, which had to do with nuclear test verification. My part of this field was very interesting because at that time there was a special test ban treaty. The Senate had not ratified it, but we were all theoretically abiding by it. It was a 150-kiloton size restriction on nuclear devices tested. So we wanted to know whether they (Soviets) were testing higher than 150 kilotons, and they wanted to know if we were testing higher than that limit. So there were all these different techniques being developed to figure out, from the seismic signals generated when you detonate, how big it is—estimating the yield of the device.

There is a lot of squirrelly stuff that goes on when the signals go through the earth materials; it makes a lot of difference what kind of earth materials. So we got really

smart, and they got really smart. Like, if you put it in salt, you can't tell how big it is; you can test a really big device and it could not be detected.

RM: Where would we test ours, then?

JY: There may have been other classified stuff going on that I never knew about, but what I knew about and what's now not classified is that we did all of our testing at that time at the Nevada Test Site. Earlier, they did some in Colorado; they did some in other places. But at that time the US didn't have the flexibility that the USSR had. They tested in a lot of different geological materials.

RM: I wonder if they were cheating.

JY: My guess, from my experience, although I don't think that it's ever been made public, is that they were testing far higher, as high as 250 kilotons. In fact, in one of the reports I wrote, which may still be classified although I'm sure nobody cares, I estimated that one of their shots could have been as high as 250 or more. So I don't think they were abiding by it. I figure that's probably one of those that's been declassified by now. It wouldn't matter, anyway, if it was or wasn't.

MV: We've declassified all of ours. Why wouldn't theirs be declassified? [Laughs]

JY: That's right.

RM: So what happened? How long were you on that?

JY: That was a year. And then, toward the beginning of my third year at Livermore was when the Livermore people within the geologic division were becoming very interested in the Yucca Mountain project. At that time it wasn't called Yucca Mountain, it was called the Nevada Nuclear . . .

MV: NNWSI. "Next November we start investigating."

JY: Yes, right! [Laughs]

MV: Actually, Nevada Nuclear Waste Storage Investigations.

JY: Yes, because Livermore had an interest in metallurgy; we had a fair amount of metallurgy going on related to weapons program development. And their thought was, "We should be able to apply this to the container materials to be used for nuclear waste storage." So they had several feelers out and were working behind the scenes, as all the national labs do, to see if they couldn't get a piece of the action on the NNWSI.

While I was there at Livermore, they did start to get a little bit of money. It turned out that one of the questions that someone asked them was, "Does the geologic environment around the canister, the container, make any difference to the container?" And they all went, "Oh, yeah, we think it does. Gee, we need a geologist." [Laughter] So I became their token geologist who helped them figure out what their environment might be like. A very small number of geologists were working at that time. You started about that same time.

MV: Well, '81 is when I started on Yucca Mountain.

JY: And this was around '82 when they were beginning to get interested. So it was about the time that the scientific piece related to Yucca Mountain started to form.

RM: Were you working on this at the Test Site, or just at Livermore?

JY: This was at Livermore. And in that same time frame was when the DOE here began to formulate a program, and then the Nuclear Waste Policy Act made it a real program.

RM: And they probably had really good geology maps of the Test Site, didn't they, from all of the years of work there?

JY: Reasonably good, yes, in a general sense.

RM: Only general?

JY: In some parts very good maps—in the parts where there had been detonations, of course. Those are eight-foot-diameter bore holes, and they take sidewall samples.

RM: And they're really deep, right? Just as an aside, I'd like to ask you, as a geologist, about something. My dad worked out there as a miner, and I think he told us one time a long time ago that they were drilling one of these really deep holes and they cut a streak of really high-grade gold. The old miners were just drooling, but they cased it over and went right on down. Is my memory accurate? Does that sound like something that would happen there?

JY: That would be on the flats, around the flats. Yes.

MV: Up around there.

RM: It's possible, then? Nevada could have that?

JY: Yes. Because normally if you're going to have any kind of localization of gold or silver or any kind of precious metals, it'll be around an igneous body that's come in because that's where the hot fluids . . .

RM: That's where the fluids bring the metals, yes.

JY: There are two very nice little igneous intrusions in the flats right at the north end of Yucca Flat, near Pahute Mesa. It's in the central-north part of the Nevada Test Site, north end of Yucca Flat.

MV: Gold Meadows and Climax.

JY: Yes, Climax.

MV: That's the one you and I talked about, where Livermore did the spent fuel storage test.

RM: And then, as a geologist, are you familiar with the old mining camp of Yellow Gold? It would be on the west side of the Test Site, north of Beatty. You would go north

of Beatty and then go to the east a few miles on the Test Site.

JY: It's probably Bare Mountain.

RM: Well, there is a hell of a deposit of gold there. I have talked to miners who think it's another Round Mountain. Midge Carver showed me a nugget, and it was the most beautiful gold I've ever seen. It looked like gold should look, unlike Round Mountain.

JY: And you think it's on the Nevada Test Site?

RM: Yes, it is.

JY: It could be up around Black Mountain, too. I don't know. There are a lot of volcanics in that area, but volcanics don't usually localize the really high-grade stuff. It's usually around granites.

RM: Granites, yes. And the granites are bringing it up, right?

JY: Yes, because they are the result of a different type of igneous activity, and they tend to have more of the right kinds of fluids associated with them.

RM: There were camps out there, and then the Test Site was expanded and covered those camps. They let the miners work there for a while, but then a few years later they kicked them off and bought their claims. Some people wouldn't sell their claims. So there's ambiguity as to who owns it.

JY: Interesting.

RM: Yes, maybe there's another bonanza out there. Who knows?

JY: Are they still mining at the one west of Beatty?

RM: At Bullfrog? No, that's shut down.

MV: Yes, but Sterling is open. It's underground.

JY: Yeah, it's underground. I knew it was underground I just didn't know . . .

RM: In Beatty?

MV: No, that’s east of Beatty—that’s Bare Mountain.

CHAPTER TWO

RM: Going back to your story—you were at Livermore.

JY: Yes, there I was at Livermore as the token geologist. My ex-husband and I made sort of a joint decision to part ways. And about that same time it turns out that Dr. Voegele here was trying to do about ten people's work, working with a contractor here in Las Vegas, and needed some help. And I don't remember how we met.

MV: Her now-husband introduced her to me.

JY: Was it through Max Blanchard? Okay. So through a DOE person that I was acquainted with through Livermore, Michael found out that I was available potentially to work, and I took the job. I moved to Las Vegas in January of '84.

RM: That would be a year after the Nuclear Waste Policy Act. And what was the job that you came here for?

JY: The best description I can give is I came here to be Michael's clone, which is what everyone always said. [Laughter] "What you have to do is do whatever Michael's doing, because that's what we need done. And we just need two people doing it instead of one." So I sort of followed Michael around, trying to figure out what he was doing as best I could. He was more of an engineering-oriented geologist, where I was more of a soft rock–oriented geologist. So we complemented each other. In the areas where you have to look at site suitability—that you've talked about with the site evaluations—I think our backgrounds tended to complement, because he was stronger in areas that I was not strong in, and I knew some of the areas that he didn't. So it worked out well.

RM: What were some of the kinds of challenges that you faced initially in your new job?

JY: This is fairly philosophical. I'll need to step back for a minute. I, in my mind, couldn't quite figure how a project like this—where we were so far, at that time, from even having an exploratory test facility of any kind—was going to be rewarding, and how we were going to feel a sense of accomplishment, when all we were going to be doing for a very long time was the background development work to get to the point where we could finally maybe start a test facility.

For the first couple of years I was very challenged—with Michael as well as the people that we then had working with us—by how to give ourselves a sense of satisfaction and some sense of accomplishment when, day by day, the most we were doing was making small progress on helping the Department of Energy write the regulations that were going to be used eventually to evaluate sites, which is what we all worked on for the first four years. This was the background work, where in the early days of site screening, of course, there was geological work going on with the US Geological Survey at the site and some other contractors.

There was enough going on with the National Labs and the US Geological Survey that the Department of Energy needed people like us to help them understand the results that were coming out and to help them figure out what those results meant in terms of using the information to evaluate a site. The NRC, Nuclear Regulatory Commission, had done some studies, but to really think about what it is about a site that would matter in making it a safe site, and put that in the form of regulations, was our job.

RM: What were some of the things that you discovered or found out?

JY: Well, I think to everyone at that time it was clear that one of the reasons that Yucca Mountain was being favored and looked at as a potentially suitable site, compared to all the other ones at that time as we were going through the site screening process, was

the arid site, the lack of water. From the beginning, at the US Geological Survey, one guy that I'm sure Michael has talked about had published papers and made a very strong case that putting a repository in an unsaturated zone location—where you're above the water table, with limited water in the pore spaces—made a lot of sense. If water is your enemy, then keep the waste away from water.

And of course, at that time, with the site screening that was going on, the Department of Energy had, from headquarters, a number of different activities going on, where the different field offices had responsibility for the different screening areas that were being studied. So we were involved, of course, in a lot of sessions where the DOE headquarters people who were managing this program would bring the people in from the various sites and put them all together in a room and basically work through what-if scenarios. The site screening requirements came out in '84?

MV: They drafted it in '83, final in '84. Yes.

JY: When they were putting those regulations together, the DOE, through the Nuclear Waste Policy Act, was given the responsibility of writing its own screening criteria. It's a kind of odd thing to do, but then, who better if you can trust them? I guess that was always the question. So a lot of our work had to do with helping DOE figure out how to write the screening criteria in such a way that they were general enough that they could apply to all of the different types of sites and different types of media that they were going to look at, and yet specific enough that you could make sense of them, so that of finding "okay" or "not okay" meant something. Because if you make it too broad, then what does it mean if you say, "Oh, favorable?"

So a lot of our time, I guess from '83 through '86, was spent both preparing the screening evaluations for Yucca Mountain but also giving advice to the people who were

putting together the whole set of environmental assessments, which is what it was called. Once you had the regulations in ’84—I’m sure Michael has gone through all this—then the idea was, “Okay, we’ve got to evaluate the sites and rank the sites against those requirements.”

RM: Did you have any interface with the other sites at all, in terms of knowing or working on their geology?

JY: We interacted with the people who were doing the same jobs for the salt sites. The bedded salt sites in Texas and Utah were in the running at that time. There were some salt domes still in the running; there were a number of different salt domes in Mississippi and Louisiana as well as the site in Washington state.

RM: Was there a feeling in the people that you worked with in the other sites that maybe there was one type of geology that was best?

JY: I actually think at that time that, among the people who worked on each of the site preparations, the site evaluation information, there was a fair bit of “nationalism.” [Laughs]

RM: Each one thought their site was the most suitable.

JY: I think that’s right. And at the time Michael and I could make good arguments for why any one of the sites were good sites. They had certain qualities. We were eventually involved in some of the very rigid decision-making approaches that they tried to apply later, and to retro-apply to the decisions of ’86 and ’87, where you try to get as systematic as you can about what features, what types of factors, about the sites really matter, and then put weights on those—formal decision analysis. We were involved enough that we both knew the other sites, not as well, but we certainly knew the other sites quite well. I didn’t know the salt domes very well, but I knew the bedded salt sites in Utah and Texas.

And the Washington site, we both knew.

RM: What happened next, leading up to the ’87 legislation? What was your involvement?

JY: I think during those days we were working so closely together that, probably what Michael has told you in terms of ’86, ’87, and the politics involved in going from nine sites to five sites would be the same as what I would tell you. It became clear that some of those salt domes sites had some very strong politicians who were going to react very negatively if a salt dome in their state was found to be suitable as a potential geologic repository location.

Then it came down to the five sites. About that time was when there was the study done that did the financial evaluation that said we can’t afford to characterize all three sites so we’re only going to take one forward in ’87.

MV: What I told Bob about that was Bennett Johnson’s perspective: “Had we been told from the get-go that it was going to cost $60 million to do all these characterizations. . . .” We went over that. Your perspectives are different from mine. They’re interesting.

RM: Yes, they are.

JY: Well, I don’t remember the same things that you do, probably. That’s true. I remember that there was something from a congressional standpoint that caused the program to be put in the position of having it cost too much to carry three sites forward. The idea had been, “Of these five, we’ll take our top three, and we’ll characterize those.” That was the next step, according to the Nuclear Regulatory Commission’s regulations: you’re going to characterize sites; now you’ve gathered a lot of data, but now you’ve got to do the next step. And there was the idea that it was going to cost less than a billion.

MV: For everything.

JY: Yes. And then we did some of those estimates where it was going to cost as much as a billion per site.

MV: This was the time when Gramm-Rudman was going on.

JY: That's right. Gramm-Rudman was at that time, too. So Bennett Johnson, as well as the other people who had the ability to influence Congress, realized that this wasn't going to fly, to spend that much money on a geologic repository program. So the best shot they had was to characterize one site. That was when they retrofit a kind of structured analysis using the National Academy to help them, and they took a lot of their own people, the DOE's people, and hired a couple of consultants. Did you help with that at all?

MV: No.

JY: We were called in to answer questions. I remember I went back and answered questions.

MV: You see, that was '86, so that's before the Waste Policy Act. Oh, the issue you may have been involved in was that they wanted Hanford to be in the top three.

JY: Yes, they did in part because it was land that was already withdrawn by the federal government.

MV: And the multi-attribute decision analysis used to rank the sites did not rank Hanford in the top three sites.

JY: That's right, because there was a concern about possible contamination of the Columbia River. It has good attributes as well, but the large water body nearby was a negative factor.

MV: I just want you to know this is one of my favorite things. You didn't end up mired in the development of new regulations like I did if you think of all we went through with

amending the Waste Policy Act and never having the regulations for unsaturated zones, having Hanford thrown in, and nobody wanting Hanford in. Eventually, with the new regulations, when all the smoke had cleared in 2009, you ended up with a regulation that's perfect for Hanford.

JY: That's true.

RM: No kidding? Why was that?

MV: Because dilution is a solution to pollution.

JY: That's exactly right, and it always will be.

MV: It always will be.

RM: So the Columbia will dilute it.

MV: That's exactly right.

JY: That's right. Now, I don't know if I was back in D.C. and you weren't there. I remember many discussions where I went in for interviews with Paul Gnirk and Tom Longo. There was big consternation because, of the DOE managers at that time, there were a couple who were following this well enough and engaged enough to realize that there was no right answer. It depended on how you weighted the important factors. That's what happens when you do one of these decision analyses. You can get whatever answer you want, but if you're honest about your values you get the right answer, within that set of values.

RM: So that's how they were getting these final five and final three?

JY: Yes, by using a structured analysis. Then, once they had all of this written up, they had the National Academy of Sciences review it.

MV: Yes, the Academy reviewed the decision model process. You're probably thinking of Ralph Keeney and Lee Merkhoffer, who were two other people involved.

JY: Yes, I think they were.

MV: Okay, because that's who Tom Longo was working with.

JY: Yes, I think that's right. They were the decision analysts, if I recall.

RM: So at this point they came up with three sites, or five?

JY: Well, five environmental assessments were written but three sites were . . . it all came kind of at the same time.

MV: The multi-attribute utility analysis was in '86, and they had picked the three sites. So we talked about Basalt being selected, which Keeney objected to. And then it was the next year that they amended the Waste Policy Act.

JY: So all within a one-year period we went from five to one—five potentially suitable [sites] to one to carry it forward, because they couldn't afford to characterize three, which is what they intended to do. Did Part 960 at that time say you were going to characterize three?

MV: Part 60 doesn't touch on that; 960 does say three.

JY: That's right. The policy act said, "You're going to characterize three." Congress said, "Can't afford it."

RM: And they had the right to say that.

JY: Well, yes.

RM: Tell me about how they got down to one site through that process. What do you know about the actual passage of the '87 legislation? Was it shifty, the "making the sausage" kind of thing?

JY: It's the same thing I've just been talking about. If you take the multi-attribute decision analysis approach and you say, "Okay, as a scientist or as a politician—whatever type of decision-maker you are—what do you think is the most important factor?" Then

the decision-makers above you and the final decision-maker put that all together and place value on what's most important to them, if they are doing an honest job of it. .

It's sausage-making in a sense but the theme behind this approach is that you'll do it, not in smoke-filled rooms, but completely in the open, with all your value systems laid out as to why you made the decisions that you made. I think that turned out to be very hard for some of the DOE people, perhaps because they would rather have done it in the smoke-filled room. They really didn't necessarily want to be clear about their values, which is not surprising given the kinds of factors that must be traded off in these types of decisions.

It does come down to some interesting decisions. You have to figure out whether you value killing a coyote in Nevada more or less than you value killing a cow in Texas. It's like, how do I place value on those kinds of things? But the truth is, in the accidents that we're going to have to evaluate, coyotes versus cows is kind of a silly way to look at it, but you're trying to value, and in the end you put value on, risks, so you have to look at the ultimate safety, or potential safety, of both sites.

RM: So that was No. 1 in people's minds, you think?

JY: Oh, sure. Yes.

RM: What were some of the criteria that were eventually weighted?

JY: If you go back and look at the way Part 960, the rule that we were all following, laid things out, it had a whole set of factors that were potentially important—ones that were sociological, socio-economic; ones that were transportation related. And there was a whole suite of geotechnical ones. So there is the whole question of whether one site is more or less likely to be hit by human interference if you put the waste there. What you do with people who know how to do this for a living is you lay out all the things that are

important, and you link them all together, and, theoretically, by studying this, you as a decision-maker figure out what it is that you really believe is important and what the most important influences are.

But in the end it turned out that the rules we were following included everything from details about the kind of rock material to which place was more likely to have a transportation accident. So we had to have, working with us, people with expertise all the way from socio-economics and transportation to all aspects of geology.

RM: Given your dissertation, which was to try and extract the uncertainties from the data, was that ever done for this? Because there are a lot of uncertainties in all of this, right?

JY: Yes, but the place where that kind of thinking plays the most into the kind of work that we did was in the total system performance assessment. Because there what you have to do is essentially create your model of the way the facility is going to perform over the long time frame and look at all of the various uncertainties that are going to play into it over the short term, longer term, and very long term. Then you figure out what kinds of probabilities to put on those various events and processes. So for me, at least, I think that my background gave me the best insight and translated the most into that.

But even with some of the regulations that we were evaluating for the final site recommendation or for the environmental assessment, either one, you're still thinking probabilistically, even if you don't say it that way. If I say the answer is yes, do I mean it's a 51 percent chance that it's yes, or is it a 95 percent chance that it's yes?

So in every case where we helped the DOE draw a conclusion on one of these findings, there was always, if not explicit, this behind-the-scenes understanding, I guess, that some of us at least had as to whether we thought it was a strong probability, i.e.,

maybe 80 percent or better, or we barely got there—51 percent. You know, we're there, but whew! I'd have to say, in the days of the EA, we weren't very explicit about that in most of what we did. But at the time of the site recommendation we were certainly a lot more explicit.

MV: Absolutely.

JY: Our probabilistic thinking was a lot better, because we had the total system performance assessment to give us some projections of performance. In the environmental assessment days, we had back-of-envelope calculations.

MV: This is the time frame when the regulations had subsystem requirements, and we moved to the risk-informed probability-based approach at the time. So this is exactly what I was telling you before, but just covering it from a different perspective.

RM: So there was an evolution in your thinking from the EA (environmental assessment) to the site recommendation? There was an increase in your own sophistication?

JY: For sure. And it was happening in part because we were getting smarter about what it was going to take to predict performance for a facility like this. The mathematics was evolving—the whole approach that we were going to use. And, in parallel, like Michael said, the regulations were evolving. The Nuclear Regulatory Commission was turning the corner on probabilistic risk assessment as a way to manage their risks; the NRC proposed the probability based/risk informed rule for Yucca Mountain in 1999. About that time the regulations in the whole nuclear area were beginning to take that PRA-informed approach. From a TSPA perspective we were probably out in front on that, I would guess. Not too many other people had the kinds of sophistication that we had in our system models.

RM: So really, the whole project needed that evolution of their own abilities.

JY: Yes, if you believe that what you needed for the site to be demonstrated as suitable was a more realistic or more defensible kind of system performance assessment. Some people don't believe that that played a very large part, or should play a very large part.

RM: Why would they think that?

JY: Well, it reminds me of the total system performance assessment peer reviews. Some of the peer review panel members truly believed that the uncertainties in the input parameter so drove the results that you got out of it that, basically, they didn't believe the results.

RM: It was your dissertation all over again.

JY: Yes. It was.

RM: What is your take on all that? I mean, you're really an expert on that.

JY: Well, there's some truth to the concerns that there are huge uncertainties in some of the parameters that are very important. So what you do is make sure that you're aware of how large they are and how they propagate into the conclusions that you reach. The site is extremely robust in that if you ever want a site to do what some of us thought the site should do, it can do it with a lot of margin. So I don't think you need a TSPA mathematical model to be a whole lot more realistic than what it is, because you have a robust site. If I was a decision maker here, actually calling the shots and choosing, and I had multiple sites, I would certainly use the Total System mathematical model results as a part of my input. But I would question. I would be very skeptical because of those huge uncertainties.

RM: Were the other sites as robust as Yucca Mountain, in your opinion?

JY: You know I'd have to say that you could probably design a safe repository in a lot of different environments. I don't think it would matter, geologically speaking. You'd do it differently, but you could probably do it in a lot of different environments.

I happen to think an arid site like this one has a lot of advantages. You could probably do it cheaper here in the long run. That is my guess. But I like it being dry. Other people like salt, because those waste canisters will go hide themselves (due to creep of the salt formation), and they will be gone, and you'll never have to worry about them. Very likely that's true. So, I suspect salt—big salt bedded—is a pretty good place to put it. Michael and I have talked about this before. Salt is so corrosive. I have a boat that corrodes all the time. Even the high-grade stainless steel corrodes. And I think, "Why would I want to put an alloy canister made of steel in salt?" On the other hand, I don't want to ever have to take it out once I put it in there. Now if you did want to take it out, potentially, to reprocess, you wouldn't want to put it in salt, because it would be really hard to get it back out.

RM: I got the impression from talking to Michael that the research for Yucca Mountain was something of a milestone, in terms of human beings trying to solve a very difficult problem. It seems to me that in your research and everything out there you reached a high point that was never given its proper due, because of the misinformation and everything put out by the opponents. Now, I have worked on EAs a little bit in the past, and I had the feeling that the EA you did represents a milestone in EAs. What is your take on that?

MV: You're talking about the EIS [environmental impact statement], though, aren't you?

RM: Yes, EIS.

JY: From the standpoint of having an incredible amount of data about all aspects of

the performance and behavior of the site—the human side, socioeconomic, transportation, and all that kind of thing, as well as the potential safety impacts long term, yes, I suspect no one has ever really tried to do this kind of a modeling effort on this scale. They may have tried it with atmospheric modeling, the kind that's done at NCAR [National Center for Atmospheric Research].

RM: Yes, but theirs are very abstract. They're not solving a specific problem with an EIS.

JY: But, for hurricane prediction, some of those models recently have become very sophisticated in the amount of data that they have and the kinds of files that they are moving around. I happened to meet someone, while we were just in Hawaii, who works on atmospheric modeling for NCAR, in Boulder, Colorado.

The kind of modeling they do is incredibly sophisticated. And it's on the same scale as the total system performance assessment model, but they're doing global atmospheric prediction. Air mass, air temperature, barometric pressure: you look at the set of parameters that they are managing, to try and predict when and where a cyclone or a hurricane is going to form and where it's going to go—that stuff is pretty incredible.

It's wonderful stuff. And he was a very interesting guy to talk to. Interestingly, in terms of the way they thought things through, and the way the overall models are put together, there's a parallel process in the evolution of their thinking to what we were doing.

RM: And yours had a different goal. Theirs was scientific understanding, whereas yours was, "Hey, we got some stuff here we got to bury, and we got to do it safely."

JY: I would say the people, though, who worked the total system performance assessment side of it were our most pure mathematics people. They knew in the end that

they were trying to predict the potential for radiation to leak out somewhere, to be in the water someplace. They were our most theoretical, most sophisticated computer modelers. And many of them were back in the areas where they never saw the end product. All they saw was their piece of the model. And it was a very large, complicated model.

One interesting sidelight on that just occurred to me as we were talking about different types of modeling: there are different approaches you could have taken to do a total system performance assessment. There isn't just one right way to do it.

The total system performance assessment is really just a whole collection of individual computer models that were all hooked together in such a way that you could basically have rainfall come in the top and come down through the repository and slowly dissolve the waste and eventually discharge somewhere down gradient in the aquifer. There are many different ways that you could have laid that out, but there are essentially two very different approaches you could use. One of the big debates that developed at the time when I was running the peer reviews on the total system performance assessment had to do with the way we had done it, which had been to some extent a pragmatic approach. We never had enough time to do it this other way, this more comprehensive, kind of bottoms-up way.

We always had another milestone we needed to meet. It's the nature of programs—you know, you have to have it now, you need a result, so you kind of do what you need to do to get that result. There were the people on the other side, who favored the bottoms-up approach. Basically, it would almost be an empirical—not empirical, but almost empirical—approach, in that you really go through and structure the information and build these models from the bottom up, until you really know the answer you get is the result of a certain set of circumstances that mimics behavior of the physical processes

at the site.

Like I told you, the way the model has been used, it has a lot of input variables that have great uncertainties. It even has some input variables that are randomly picked off from a distribution, which means I have a little program that goes in there and says, "Okay, for this run I'll pick that one, and the next run, I'll take a different variable." That says you can never really duplicate the result exactly because it's a random effect that you're modeling. For those of us who have done that kind of modeling, that's perfectly okay. But the empiricist, the guy at the other end, says, "No real system would ever behave exactly the way you replicated it." Some people who are those extreme empirical types will never trust the results of this kind of a model.

So within the peer reviews we always faced that. We didn't try not to get those people, but we tried to get a distribution of views on the panels. And when you do, you always end up with a couple of people in the peer review panel who have more of a tendency to have that viewpoint.

So, interesting controversies. It also brings up the question we were talking about earlier, which is, do you really trust the result? Well, I trust the result to be as good as the information that I put into it, given the uncertainties in that information.

CHAPTER THREE

RM: With regard to this distinction between the empirical purists, so to speak, and your probabilistic approach, what are we saying, philosophically, in terms of science and in terms of the practical job of constructing a repository?

JY: The result of the fact that there are people that have those different views means that you always have some fairly strong component of the scientific community questioning the results that are based on something like a mathematical model, such as total system performance assessment. They'll always say, "Yes, but you didn't adequately represent this one little piece, and, if you had, the answer could or would be different."

What we always tried to say was, "Well, maybe we didn't represent that, but by running this variable over this complete range, that's the most extreme effect that you could have. Had we put it in there explicitly, look what happens. We can put it over here and look at the result and say, 'Okay that's the limit of how bad it could be, or how different it could be.' "

So what it leads to is a high degree of uncertainty that then translates into differences of opinion, and I think that uncertainty reflects in the public feeling uncomfortable with science to a degree. But I think it's true for any major project that has this kind of technology being applied to make decisions. Like you said, in all of the other big decisions where EISs have been written there's generally some area where there's controversy. One of the major ones comes from very, very intelligent research people out there who just fundamentally are not probabilistic risk thinkers, in one sense. They're very empirical; they want to be able to measure it.

RM: Are they concrete? That's a tough question, I know, but to me it sounds like they are a bit overly concrete. I'm interested in cognitive differences in people.

JY: I'm on one side. It's hard for me to know, because I'm way far on the probabilistic side, and I always have been, even without knowing what it was. A perfect example is a guy who was at the University of Michigan, though I don't think he is anymore, Rod Ewing, a super-bright geochemist, a very intelligent man. He was a full professor, you know, endowed chair. He was one of the people who served on our peer review panels, twice at least, and he pegged it on that end—conservative, empirical. He sounded when he spoke as if he thought probabilistically, but I never believed that he did. He would sound like he was having that conversation, but then when he wrote it down it never sounded like that.

RM: It seems to me that there are two different epistemologies, two different ways of looking at the world, and one of them is better than the other one. Because the world is probabilistic, you know, it's a stochastic world.

JY: But there are lots of people, I think, whose worlds are not very probabilistic.

RM: Yes. And do those people get in trouble? I mean, in simple systems they don't, but in complex systems they do, because complex systems behave more stochastically than simple systems.

JY: I'd have to think about it. I'm talking more about people that I know well whose world isn't very probabilistic. They believe in right and wrong; they really don't believe in shades of gray much at all. They have a code that they live by, whether religious or not—most of these people are very religious in my experience. I've watched, and I have been curious about it, like you are. How is it that they manage to live their lives, apparently, without the need for any real probabilistic type of thinking?

RM: Sometimes better than the probabilistic-type people.

JY: It looks like it.

RM: I mean, they are living in a better house or driving a better car.

JY: It looks like they're perfectly happy, they accept that things happen for reasons that are very different from what I would place on that event, or you would. But they seem to do just fine. I try not to place value judgments on thinking probabilistically versus thinking deterministically.

MV: Can I ask the same question to Jean, just a little bit differently? Maybe what we're talking about here is not so much the difference between probabilistic approaches and non-probabilistic approaches, but it has to do with simple systems versus complex systems. Because the person who can write a top-to-bottom code for a particular geologic medium, if faced with a piece of that system that could not be quantitatively written down, would have to turn to an approach like we did with Yucca Mountain. I think, to me, that's the difference. The people who are insisting that Yucca Mountain have one of these beautiful simple analytical models, beginning to end—one single differential equation and it works—never had to deal with something where you can't write down the deterministic nature of the medium that you're dealing with. It can't be quantified.

RM: Yes, yes, that's beautiful.

JY: True. And I think the people that I'm talking about that can live without it are people who don't face those kinds of challenges. They are not trying to quantify complex systems or even quantify relatively simple processes that are still stochastic in nature.

RM: Ultimately they're all stochastic, even the highly deterministic ones.

MV: There's a difference between uncertainty in a variable, which is stochastic, and not being able to develop a complete understanding of a physical component of a system

because its nature is so random. And that's not just uncertainty in a variable. That's the system itself.

RM: To me, this is a fantastic discussion.

JY: Those two kinds of variability are very important, because in order for the total system performance assessment to be defensible it had to capture both.

RM: Yes. Satisfy both sides of the street, so to speak.

JY: Well, and to be mathematically defensible, or statistically defensible, it had to capture both that inherent variability in that process as well as the uncertainty in the parametric representation. So it had to capture it adequately to replicate or represent the processes.

RM: Okay. Now, kind of harking back just a little bit: I still can't get out of my mind that Yucca Mountain—the study, all this multi-decadal effort—is some kind of a milestone in human history of analyzing an extremely difficult, highly political, and important problem. And check me out on this: did the Yucca Mountain analysis that you were doing break a little bit from the trend of human history, that is, doing deterministic models versus going into this kind of probabilistic approach? And is that the trend of the future as humanity advances?

JY: I certainly hope it is, having found out that the oil industry doesn't do PRAs at all when they site deep wells offshore.

RM: They don't? That's a beautiful example.

JY: For siting, both for siting and decision-making related to anything, the corporations—according to the piece I have been reading in the *Wall Street Journal*, and it sounds like Michael has read some of the same things—apparently have no PRAs involved. In their regulations, even, that the DOI [Department of the Interior] regulations

do not include any uncertainty analyses. It is straight: comply to this requirement. Whatever this requirement is, comply with it.

Well, what does compliance mean when you're talking about something that has a lot of variability? It's the same thing that we've been talking about. I had thought, until recently that the whole risk-informed approach to decision-making, which is what we called it as this began to happen in the nuclear world, was permeating all areas of the federal government. I know that Health and Human Services does, because NIH [National Institutes of Health] has done risk-informed probabilistic-type approaches in medical studies and so on.

RM: In disease! Yes.

JY: So you think, well, gee, something like offshore drilling and regulations should too, but apparently that part of the federal regulatory picture has not had any PRA.

RM: I wonder if they do any probabilistic studies about finding oil here or not: "one–in-ten we're going to hit it."

JY: You have to think that the smart oil companies that are really good in exploration do, but they don't call it that. Because, from what I'm reading right now—all the interviews that I see with the people that have been out in front—if they do it, they don't call it that. They must do something. I know a guy whose specialty was financial analysis related to exploration, petroleum exploration. He did probabilistic stuff; he didn't call it that. But when they put the information together to prepare an evaluation, it included how many other wells in that area have produced and what volumes of oil. But apparently that area has not had the sort of risk-informed revelation that the nuclear side of the business did since Three Mile Island.

RM: Before Three Mile Island it wasn't probabilistic?

JY: Not very.

RM: And now it is. So that was a consequence of Three Mile Island?

JY: That's what I understand.

RM: So now the work that you folks were doing on the Yucca Mountain project was risk analysis–based to a great extent?

JY: Through time it became more and more so. I think we were evolving at the same time the rest of the world was; the Nuclear Regulatory Commission was going that direction. The Environmental Protection Agency—apparently they don't really know they did. But they used the words. They just didn't apparently really risk-inform to the same manner.

MV: I don't think they do.

JY: You don't think they do?

MV: I don't think they're risk-informed. I think they rely too much on the conservatisms in their calculations.

JY: But when you read the background of some of the rules, they certainly make decisions based on probability of human endangerment. They use those kinds of inputs.

MV: Okay, yes.

JY: So in that way they are risk-informed.

MV: If you were looking at a pure EPA rule, you'd never find something that's comparable to the TSPA.

JY: Oh, no! No, no. Mathematical modeling, yes. Something, though, that tries to capture all the uncertainties to the degree that we did, definitely not.

RM: Were you guys breaking new ground, then, historically? It sounds like you were.

JY: There wasn't anything like the total system performance assessment out there for

us to compare to, so the answer has to be yes.

RM: Yes. Okay, now, I don't know if I'm getting too philosophical here, but it seems to me like the people on the other side, the hard-nosed empiricists, determinists, are more Newtonian in their philosophical view of the world.

JY: That's true, they are.

RM: And a new trend coming in to the human process is a probabilistic approach. In other words, we will more and more, in these big analyses, move away from a deterministic analysis to a probabilistic one, because that's really the nature of the world.

JY: I think we have to.

RM: Ah. That's so cool. [Laughs] Now that we have solved that problem, I think Michael is very interested in having us talk about the TSPA.

JY: Really, you have covered it from a number of different directions, just by your questions. So you have already covered it a lot. But in the early days, in some manner you had to figure out whether the site was going to cause contamination, whether it was in the air or in the water. Those were the two possibilities. You had to do some kind of analysis.

In the early days of the environmental assessment stage, as I mentioned earlier, the mathematical models would fit on a piece of paper like this. They were Fortran code, about 50 lines, and it said a drop of water comes in the top, and it goes down and contacts the waste material and goes on down, gets in the water table, and goes up to someplace where someone has a well and pumps it out. It was about that simple. But it was still a representation. It was still something that gave you some idea of how you would do it eventually, if you ever really got to the point of doing a fully complex model for it. And that's the kind of thing we used for the environmental assessment, because even at this

stage we had to reach a conclusion about whether it would make the requirements, the standards, that had been set by the NRC.

We all knew at that time that what we had was just almost a cartoon, you know. At that time we were in the early days of widespread use of computers. We weren't at the stage of the kinds of processors we have today.

So about this time, about the mid-'80s, is when the national labs—Livermore, Sandia, and Los Alamos—were all three going through major renovations of their computing capabilities, together with a kind of risk-informed thinking coming from NRC because of Three Mile Island changing their outlook on the world. And the national labs were all going through major upgrades in their computing because of the new kinds of computers that were coming to be available at that time. National labs generally get them first, or at least early on—get the newest capabilities. So the national labs had computers that people wanted to use.

We knew the next step was going to be, obviously, to gather site data and characterize Yucca Mountain. Once '87 happened with the choice of Yucca Mountain , we knew we were going to spend some time characterizing the site and eventually write a site recommendation, or do some sort of site recommendation report that would eventually carry it forward as a recommendation that this site meets the standards or it doesn't.

But what was coming together at that time, together with your concept of the risk-informed or probabilistic thinking on the intellectual side, was the computer capabilities. And we had labs. In fact, Pacific Northwest Laboratory, Battelle Laboratories back in Columbus, Argonne—probably at least seven or eight major government research laboratories—all wanted a piece of the action. Any one of them would have loved the job

of building this computer model for Yucca Mountain. I think at one point DOE had 17 subcontractors all individually involved in doing total system performance assessment for Yucca Mountain. This was right after the policy act amendment.

RM: After the ’87 legislation?

JY: Right. Some of them had been working on the other potential repository sites, but of course now they still had money left, and they had mega computers, and they wanted to play a role. And so it was probably ’88 or ‘89, in that timeframe, when DOE headquarters said, “We’ve got 17 contractors. We don’t need 17 different contractors doing total system performance assessments. We need to consolidate this down to a few and get focused in on what we need just for Yucca Mountain.”

And of course they all wanted a piece. I don’t even remember how in the world DOE let this happen, but they put Paul Gnirk, the guy we have been talking about, and another guy who at that time was stationed at DOE headquarters who worked for a technical support contractor, and me, the three of us, on the road to review and evaluate all of these contractors. We were to come back to the committee with a recommendation to the responsible DOE manager for who would carry the TSPA forward. We were hated. Oh, we were hated.

MV: Who was the Weston person (Roy F. Weston, a support services contractor for DOE-HQ)?

JY: Larry Rickertsen.

MV: Rickertsen, of course.

JY: So there were the three of us, and we were on the road for probably about three or four months. I think we had a six-month time window to bring this recommendation into headquarters. We would ride into town. Gnirk is a cowboy; he has a ranch up in South

Dakota. He is a rock mechanics-type engineer. Rickertsen is a devout Mormon and a computer jock, but with an engineering background. And then there's me. So the three of us were supposed to put our perspectives on it. Well, we'd ride into town. So the image that people had was that into town came—this is our byline—the Mormon, the cowboy, and the woman. [Laughter] And we did not make a lot of friends.

RM: I thought they'd try to butter you up.

JY: Oh, they did. But they were all aiming to get the best recommendations and trying to explain to us why all the other people should not be carried forward. You know, they were fighting for their jobs in some cases. So of course it was a very serious thing. We took it very seriously, but it had its moments.

RM: Did you find it stressful, being on the road?

JY: Oh, yes, it was incredibly stressful. And these two men are both intense people. They're as intense as Michael is; I mean, very intense people. Right? Rickertsen, actually, probably beats Michael!

MV: Beats me. [Laughs]

JY: Beats Michael. And Gnirk. I mean, both. We would be there at 7:30 or 8:00 in the morning, and we did not stop working until it was time to go to bed, which for them was midnight.

So the three-man team, evaluating total system performance assessment contractors, did its job for about four months or five months. We wrote the report, I believe, at the end of a six-month period and then delivered that report with its recommendations. And on the basis of that, DOE went from, I think, 17 to five. So they ended up with five national labs or private contractors who had a piece of the action to put together a total system performance assessment for Yucca Mountain that would be

used in the next phase. I think it was ’88 when the SCP (Site Characterization Plan) was final.

MV: Draft was January of ’88, final in December of ’88.

JY: So this was ’88–’89, maybe early ’89. So it was when DOE was trying to get the work sorted out just for Yucca Mountain and to get their contractors in order for who was going to do what as the program went forward through site characterization to the final evaluation of the site. Out of that came a major role in the total system performance assessment for Sandia National Laboratories, mostly due to a couple of very key people who were just outstanding, and represented the types of mathematical modeling that needed to be done for a more sophisticated total system performance assessment.. Pacific Northwest Laboratories in Richland, Washington, had a key role. That was almost a check and balance role. They were to do something that was, in a sense, in parallel to what Sandia, with other people supporting it, would do. And it was to some extent a check and balance that DOE decided to maintain.

And then other support roles: there were other subcontractors that Sandia determined that they needed and DOE agreed with. So there were some other key players at that time. Intera Corporation, which was an Austin, Texas–based company, had a major role in it at that time. And a lot of people who worked for Intera actually came to work for Sandia or for one of the contractors, so they carried on into the project and ended up being key people as the job went forward.

RM: Who were the rest of the five? You mentioned Sandia . . .

JY: And Pacific Northwest Laboratory, right. I am trying to think who else we had in that inner circle, who directly held contracts with DOE.

MV: I know that Los Alamos and Livermore fitted in, but they weren’t . . .

JY: They weren't directly contracted to work on the TSPA. Los Alamos wanted to be. They competed. In those days there was a guy at Los Alamos National Laboratory named Bryan Travis. He was a hydrologist, and he wanted to take on the total system modeling. LANL management wanted it, too, but I don't believe that we recommended them because they would have had a lot further to go to build their expertise than Sandia did. Sandia already had a fair number of people with the kind of expertise they needed to support the DOE.

I'm pretty sure that Livermore had a piece of it, but it was related to the waste package performance. Intera, of course, that I mentioned. They were subcontracting to Sandia. The guy who was the head of Intera at that time, the founder, was Suresh Pahwa. He was a scientist in his own right, a very, very competent man, and had a lot to do with the way the early total system performance assessments were put together. In fact, they did some of the model construction for Sandia. There was a subcontractor in Albuquerque, I think, that played a big role with Sandia, too.

MV: A company that was called Graham originally, Krishan Wahi.

JY: He played a big role in it, too.

MV: Krishan Wahi had a lot to do with Sandia.

JY: I believe the company name was Graham, but it changed. So in the end what came out of that was that Sandia National Laboratories had the big piece, the central responsibility for the total system performance assessment.

RM: What were some of the factors involved in your decisions?

JY: At the time, I think the main thing we were looking for was that they already had a core of expertise, so that they would be able to hit the ground running. Because, of course, we always, in this project, had a deadline that was shorter than what we thought

we could possibly meet.

The next total system performance assessment that we were supposed to deliver was probably a year and a half or two years away at most, and that was a very short time to get the models all constructed and be able to test them properly. So we looked for a place that could demonstrate that they had, either within-house or with subs, a core of expertise that would be a critical mass that could hit the ground running with the job that they had to do over the next year to year and a half. Pacific Northwest had a good team, but it didn't have the breadth, and it didn't have the depth. It had a couple of very strong people who ended up going to work someplace else and working with Sandia on the total system model.

But I think that was the key. One thing that Sandia knows how to do is to put their senior management behind an effort that they believe in the long run is going to pay off for the lab. They are very, very good at that. When the three of us rode into town on our white horses, on Gnirk's horse, they had the then-director of the laboratory and the guy that was next down and then the head of the Yucca Mountain piece, all three, meet us. You know, a little reception. They wined and dined us. I mean, they knew how to do it.

They said, "Look, we're committed. We want this job for the long haul." I'm sure that that guy who was the head of the lab then probably had designs on what finally happened—where they eventually took all the science work, which happened in 2006. Knowing Sandia, they had long-range plans. They're strategically very good. They, by far, showed that they knew the pieces of the puzzle; they knew what needed to happen. They came in well prepared, and I think many of the other places that had little pieces of the work didn't have the capability to do the whole model. We even recommended where and what DOE kept, that these people can help with this piece.

CHAPTER FOUR

RM: How do you go about putting together a big model like the TSPA model? What was involved for this one?

JY: In those early days you knew how the regulations compartmentalized the site, and you knew Part 60 is important to them. And then you knew how DOE had mirrored that in their regulations. So you kind of had the compartments that you knew you were going to have to speak to. Total system performance assessment doesn't necessarily have to speak to each of those compartments, but in the end it has to have accounted for them in a defensible way, and it has to be able to provide quantitative results that are believable by at least the majority of the community.

RM: And you're plugging hard data into that model?

JY: Sure. What they are doing is they're mathematically representing, as best they can, the hydrology and the rock properties that the water is going to flow through. And then the climate change that's going to perturb that, and the potential for any other kind of catastrophic events. You have to consider things like volcanoes and earthquakes; what are they going to do? You have to have a little part of your model that actually has a volcano occur, models what happens, and looks at what kind of release—what kind of contamination—you could get from a volcano erupting through the repository.

In those days, it was a kind of thought model. It was: what are all the pieces that we will have to have to be defensible? So when somebody like you, 20 years later, is saying, "Well, now, how did you take into account the fact that the climate 10,000 years ago was much wetter here, instead of being dry like it is today? And that we got 17 inches of rain back in January?" And the answer is, "Well, let me show you how I did

that. Here is the part of my model that does that. Here's the range of precipitation I considered." And I then can show you, if I have it done well, show you the result and how that makes a difference, if, in fact, the climate changed that much.

The philosophy is a "what if," where you include anything that has a reasonable likelihood of occurring over the period of time that you are going to predict with the modeling.

RM: And then you work all these different possibilities into different scenarios.

JY: Exactly.

RM: And then work a master scenario out of that.

JY: Yes. And the NRC, the Nuclear Regulatory Commission, actually reviewed the list of scenarios in the final analysis. This is jumping way ahead now, but just before the license application, while it was being put together, there was an international group that put together a list of something like 5,000 scenarios. And this was for a kind of generic repository. Then what NRC did was, in interaction with our performance assessment gurus, screened that down, from that total long list of scenarios that the international thinkers had put together, to those that have some probability of being relevant to Yucca Mountain. So they have a document, called a screening document, where they show how they took this mega list of scenarios down to the 200 or 300 relevant ones.

RM: How did you screen out some of those variables?

JY: Some of them they screened out just by logic. You know: it isn't going to happen here because it's not an oxidizing environment. Those kinds of things. Others were not that clear, so they developed an evaluation, in many cases—some kind of mathematical model. If it was one that you wanted to screen out, where you believed you were at the 50-50 or something like that, you did the analysis. You used your total system

performance assessment model, demonstrated that it's not a scenario that you have to carry forward, and put it to bed that way. That's all documented in a very thick document that was written about seven years ago but then updated. So the one that supports the license application has all of the screening information for all of the scenarios that were dropped, as well as the list of the ones that were carried over to total system.

RM: That were run in the model, yes. And what did you come out with, then, at the end, after you ran all these different scenarios? Did you come out with a statement of probability?

JY: Yes. I don't even know how it looks in the LA [license application] anymore, because I haven't seen the current version of the LA. There is a requirement to not exceed a certain level of radiation at 10,000 and 1 million years, so the modeling produces the amounts you get at 10,000 years or a million years or whatever your time frame is. Because you do multiple runs, you get a whole family of curves. Based on that, you can then do a PRA. You can say, "Okay, I have a range from, you know, some value very far below to maybe a little bit above that number, and the probability of being above the number is over 1 percent," or whatever.

RM: And then the site is robust on top of that.

JY: Yes. Well, you might say that out of the thousand runs that you might have used for your final analysis, there might be three, because of a weird random variable combination, that exceeded that regulatory limit. So you say, and this is where we get into the philosophical question, "Well, does it still meet it or not?" I would say, "Well, yes. If 997 showed that it is well below the limit, and those three show that it's over, it's already a very conservative standard."

RM: Yes. So what happened then?

JY: First, there's a long period of fits and starts at developing the individual mathematical models, the pieces, and testing them and debating them. With many of them, as Michael said, you are representing a process that is very stochastic—you have a lot of variables. And there are lots of different ways in which you can characterize that mathematically.

MV: Just for your benefit, I have shown Bob all the TSPA cases with all of the horsetail plots. So he knows.

JY: You know the picture—what we're talking about. And so for about two years between '88 and '90, in that time frame, a lot of data were being collected. They were using what we had, basically, for the environmental assessment; collected a lot of new data for a couple of years when we didn't do any site work, because we had a stop-work order. So we had these data and a little bit more. They built the individual mathematical models, linked them all together, and got something that looked like a total system model. And then it went through a whole lot of reviews and updates and more reviews and updates through the '90s, until we got to the point where site recommendation needed a real answer.

And that was when the rubber hit the road. There were two phases of a peer review; one, the waste package peer review led by Joe Payer, at that time a professor at Case Western Reserve, who was a metallurgist. Then he was on the total system performance assessment review that was led by Chris Whipple.

The total system performance assessment peer review had a wide range of people on it, and they took the mathematical model that Sandia had developed and tore it apart. Sandia is extremely good at what they do, but they don't really take constructive criticism all that well, and we had a lot of back-and-forth. Sometimes it took a lot to change

directions. Sometimes it took DOE ordering them to do it.

RM: This was as a result of the review by these experts?

JY: And others. There were international peer reviews going on throughout the mid- to late ’90s, getting us to the point where we had something that was credible to use as a basis for the site recommendation.

RM: And were you in charge of those reviews?

JY: I tended to be the person that set up the peer review panels and would take care of the logistics.

RM: How did you select the members?

JY: Generally speaking, if it was an area like waste package, where I didn’t really know the people in the field, what I would do is call university professors who were publishing in those areas, ask them about their work, and ask them who they thought were the key people in those fields. I would talk to the national lab people, find out who they were using as consultants. They tend to tap some of the key people. Some of the national labs that were no longer involved in the program had some very knowledgeable people. So they were available as peer reviewers, since they were no longer directly employed on the project. It was mostly, really, word of mouth. National Academy of Science members many times will have an idea of who they think the best people are.

RM: So you were just calling these people up and asking.

JY: Just seeking the best information we could put together. And for each of the specialties that we wanted, in my case or in Michael’s case, we would know at least one or two of the national experts or international experts, so then we would use those people as stepping stones to identify a few more, till we found a kind of critical mass of people. And then we almost always had way more people than what we needed, so then we had

to cull it down to the best ones.

Some things got fairly political, but generally DOE really never tried to influence the selection of peer panel members. The politics was that, once people realized what it was, sometimes they didn't want to serve on the review panel; they just didn't want to be involved. And other times they really wanted to do it. Then there were those in the middle. For many of them, the question was, "Well, am I going to be under pressure to reach a conclusion that I don't agree with?" "Well, no. This is objective. You are going to be asked for your honest opinion." And as long as that was the answer, they were happy with that. We used some local UNLV people, and that was very helpful.

RM: Did you use the same process for international people?

JY: Yes. I didn't set up the international peer review. There was an international panel that was already reviewing total system performance assessments. There were other sites internationally, and that same panel reviewed ours a couple of times. I tended to be the focal point who managed the peer reviews for Yucca Mountain.

RM: What is your overriding memory of working with these experts, personal and otherwise? Was it a good experience, or was it sometimes difficult?

JY: It was always interesting and always a good experience, on the one hand. On the other hand, there were always individuals who were easier to work for or with than others. There were some that were extremely good communicators, and we identified those people right away and tried to make sure they were either chairmen of a panel or at least in a position where they were going to have some influence, because often we'd have several members who were very good in their specialty, and they played a key role, but they really weren't broad thinkers.

In total systems in particular, we had to be really careful that we got people who

could look at a system-type approach. As you said, in those days, early on, there weren't that many people who were doing this type of work and had that sort of experience. So if we were looking at a mathematical modeler, he may or may not be prepared to do this kind of assessment or think this way. So, yes, it was challenging to get good people.

On the other hand, I think with the peer review panels, for the most part, we never had a situation where they were unable to work as a group. We did have some minority reports. We didn't always have consensus. NRC had given us guidance on how they thought peer reviews should be run in a nuclear program.

RM: Was it good guidance?

JY: Oh, sure, yes. But we had the choice in their guidance as to whether it was a consensus peer review or a peer review that just was a majority and a minority report was allowed. Generally speaking, we tried to do the consensus approach. But the last total system performance assessment was not a consensus report.

RM: Did the lack of consensus bother you, in terms of your operations or getting your job done?

JY: I don't think the lack of consensus was really an issue, other than we felt it was information that had to be considered. It was a challenge we were going to have anyway, if not from the peer review, then from the Nuclear Regulatory Commission or their contractors. The combined scientific group that worked on Yucca Mountain always took that information as constructive, in that we were going to have to face this at one point or another anyway.

Now, I'd have to say that there were individuals from the national labs or from contractors who were more resistant. As I said, Sandia was notorious for not accepting criticism of their work very well. So sometimes it took some extra effort to get them to

take in new information and make the changes that were necessary. Once they accepted that it really was an alternative that had to be considered, they did it. And I have to say that, for the most part, as I'm sure you know, it was one or two people. It wasn't like Sandia management didn't want to take it. Technical staff were just protective of their work, as would be expected.

RM: What's the name of the state's guy, the difficult one? He was never on your peer review, was he?

MV: Szymanski.

JY: No, no. He worked for DOE but he did influence the kind of people that were put on the peer review panels only because, the way Don Vieth, one of the early DOE site managers and others felt, he was our inside critical reviewer. We weren't ready for prime time in those days. For most DOE managers and the national lab managers, and certainly Michael and me and the people that we worked with, our whole thought process was, "We're going to have to face these kinds of criticisms and be able to answer the questions at some point in the future. We might as well take it on now." We generally always had that attitude, with a few exceptions.

RM: I'd like to back up to this one topic that interests me so much. In your experience and your knowledge, has this kind of huge probabilistic assessment for a specific problem that is very human-related ever been done before? What I'm getting at—and I keep coming back to it—is that this is new ground and a new way of looking at the world. That's what I keep hearing.

JY: There is a low-level waste site where they did a fairly major mathematical assessment. It was in Tennessee, or maybe Illinois. The reason I bring it up is they did have a PRA type of approach, and they had a citizen's review panel that was a big part of

the evolution of the site. The citizens review panel wouldn't buy the results, so in the end the site never went forward. I'm not saying it was comparable in scale or scope to this, but I would say in the early '90s, I believe, there were some other places in the country and maybe in the world where safety assessments of the kind that we did, on a smaller scale, were starting to be used to make decisions or try to support decisions, support applications for licenses or certification or something like that.

MV: Do you remember when Milton Harr published his book on probabilistic risk assessment and engineering? That's got to be 25 years ago. Maybe it's been a long time coming, but people have been thinking about doing this for a long time.

JY: I think it wasn't getting applied to decision-making in the federal realm until, I believe, NRC and Three Mile Island. Well, road design has used a PRA type of approach for a long time in terms of curvature on superhighways and interstates. They look at accident rates and gradients. So it has been there in certain areas, and, like Michael said, probably in some of the engineering sciences there have been individual applications. But probably to apply it to a big scale like this, to problems with safety, long term, no one else would have ever done it.

RM: What I keep thinking is that, looking at the big picture historically, this is a milestone.

JY: No other facility has ever really been conceived that would have this kind of longevity that would require this.

RM: Yes, it was a burst in human history.

MV: You finally got somebody to say it in a way that I will agree. Okay, I will agree now. [Laughs]

RM: You keep fishing, and you finally get what you want. [Laughs] Who was Milton

Harr?

MV: He was a professor at Purdue.

JY: Yes. In fact, Purdue and Princeton were the two places that started out with this PRA type of thinking. And I am sure 25 years ago is about right. Probably in the ’70s, early ’80s, and by ’90 it started to be applied.

RM: To me, it seems to represent a sea change in human thinking.

JY: Well, yes. For people who are looking at what happened in the nuclear business after Three Mile Island and the PRA type of approach, I think that is how it’s being described in the nuclear business. I think what you’re picking up on is that it’s starting to be felt out in other parts of the government sectors where decisions are made.

RM: And if you were going to site a nuclear reactor nowadays you would be doing this sort of research, wouldn’t you? And, of course, you would have a lot of data based on all the other reactors and the geology and everything.

JY: Yes. Reactor licensing today is based on very much a PRA approach.

RM: Do you know about its use in other fields besides highways?

JY: NIH certainly uses it, and FDA uses it—well, a form of it. I was looking at something just the other day about the way the FDA looks at trials for drugs before they approve them. And it’s interesting: they use statistics but they don’t really use a PRA basis for decision-making about whether to approve something or not. In terms of the value it would have, the probability that it is going to help somebody, or help some segment of the population, versus the side effects, they don’t do that. But they do look at the statistics of the trials that are done.

RM: I am a huge fan of 747s, for example. Get on a 747 in Vegas and fly to London—it’s mind-boggling to me. Do they do it on their engines? Do they do it with deterministic

models or are they using probabilistic ones?

JY: I'll bet for the design of airplanes and that kind of thing, I am sure they use . . .

RM: They're using probabilistic models, you would guess?

JY: I would think so, for a long time. Because they look at safety factors. I mean, safety factors are really probabilistic.

RM: Well, my own view is that, at least at this point, if one writes a history of Yucca Mountain this has got to be an important part of the history. It strengthens one's confidence. This was never presented to the public, and maybe they wouldn't have understood it or believed it if it were. But from a scientific point of view, to me it's more valid.

JY: Yes. The public perception side of risk acceptance is very interesting. And I think in general, from what I have read—and I haven't read a lot—I gather that people want zero risk. The concept of accepting some risk in order to benefit is not something that most brains handle very well, and so the idea that the government uses some kind of a PRA approach to design roads, for example, is not received well. When you tell the public that's how the roads are designed, people react very poorly. So I think what you're saying is true.

MV: We had this conversation, too, and it has to do with people accepting risk and not knowing that they're accepting risk, as opposed to placing risk on somebody without their wanting it.

JY: No, that is true.

RM: When you get in the car and you go to grandma's house, you've got to know there is an outside chance that you're not going to make it.

JY: Yes. Having it done to you versus you taking it and accepting it, right.

RM: I am sure the psychologists have worked that over pretty well.

JY: I'm sure.

CHAPTER FIVE

RM: What is your assessment of the nuclear waste program overall?

JY: Well, it's my view, and I think the view of a lot of us who lived with this program, that the approach that was written into the Nuclear Waste Policy Act had some very good points to it, like the tax on users of energy to pay for the fund for taking care of the waste byproduct. It had some very smart things. But the one thing that you really can't do with a program like this, which, in the end, was going to be a big construction project that needed the ability to do long-range planning and commit large amounts of dollars over multiple-year time frames, is manage that program effectively and efficiently under an annual budget cycle. And I doubt you can manage it under the structure of a department like the Department of Energy.

The only successful approach we all know of for managing projects of this size and complexity is something like NASA, where it had its own budget, was off-line, and could commit to long-term projects. And so, my view is, if it was going to be a strictly government-run program, rather than perhaps something different from that, it should at least have been something like a NASA model.

My guess is that we could have been 50 percent more efficient in our use of dollars had we been able to run three-year or five-year budgets rather than have to replan every year. There were some years when we managed through reductions in staff and reduction in total operating budgets of 30 or 50 percent, and that's an absolutely asinine way to run any kind of a program. I think this kind of a national problem needs a national solution. It needs something that's centralized and managed out of a governmental agency, or government-sponsored agency. But it needs to be set up very differently.

RM: Michael, what are your thoughts on that issue?

MV: Oh, I completely agree with Jean. I would have come at it from a different perspective. The budget aspect of it is very serious. Jean looked at it from having to deal with reductions in force. I look at it from the perspective of when the Department of Energy did not meet a particular milestone or their program plans were changing, they would tend to rely on Congress to tell them what to do next. That's very different from what you would have gotten if you'd given this to a large corporation—the NASA model.

I mean, NASA knew—beginning to end—what they were going to do. If something failed, they had a fallback plan for what they were going to do next. In our program, the Nuclear Waste Policy Act led the Department of Energy to believe that they were supposed to involve the public at a very high level. There's a difference between a constitutional democracy and a real democracy, right? Not everybody should have gotten to vote in this. The Department of Energy was given a mandate by Congress, and they should have taken the reins and gone forward with it. That was just not the operating model of their managers, because they are a bureaucracy. They're not a business. And that's exactly where the problem comes down.

You compound that by annual budget reviews. Jean and I had to do more of these than a rational person should ever have to deal with in a life. But we would get these solutions from Congress, or negotiated with Congress, like the viability assessment and so forth, where we would essentially have to stop the progress toward the real goal and do something else, because the Department of Energy let themselves get into a position where they either looked like they were asking or they actually asked Congress, "What do we do now?"

Jean's NASA example is perfect. NASA would have never done that. The

opposite would have happened. Somebody would have had to catch the NASA managers and stay, “Stop that.” And that never happened at DOE; it was the other way around.

RM: Is there any way that DOE could have been formed or managed so that it didn’t have this deficiency?

MV: If this were going to be a federal corporation, there are better models in the federal government: the Tennessee Valley Authority (TVA) or the Corps of Engineers. They know how to manage big problems and programs over a long time.

JY: Yes, there are a number of cases, not a lot, that have been real successful. But the ones where it’s either a government cooperative with a private company or even the private company is simply running it for the government, there have been some of those that have worked really well. You know, even the US Postal Service comes to mind as one. I think Michael’s right, that the problem that you have giving this kind of a job to a bureaucracy like the Department of Energy is the kind of people that you will forever have coming up the ranks into the management positions, your top-level managers below the guy who’s appointed by the current administration—which is another problem. But the fact that you always will have bureaucrats in those senior management roles means it really can’t operate any differently. It just can’t.

RM: Because they’ve come up though the ranks and they have a certain mentality?

JY: They’re bred to be what they are. And, you know, there were some very good ones. Some of them were very good at what they did. But it was to perpetuate the system, you know. I can give you a great example. I can remember two of our better managers here at the local office at one point. I went over to them—this was not that long before I retired—went over to them and gave them what we had come up with as a way to really save quite a significant amount of money. It was an efficiency that would have resulted,

within the science organizations, in saving probably on the order of 25 to 30 percent of the budget for the next couple of years.

And they said, “Are you crazy? We don’t want to save money. We want to maintain and make sure that we have as much of a justification as possible for our cut of the DOE budget, of our section of it. So we don’t want efficiency. Don’t be bringing us efficiency improvements.” And these were not bad managers. These were some of the managers who did reasonably well in the roles that they were playing.

But back to the other train of thought: the other problem is when you cut off the head of the organization every time you have a change-out in administration and plop somebody new in there who may or may not have the right expertise and experience. Because they get there for political reasons, it’s just dumb luck if you happen to end up with somebody who really knows how to manage a big project, No. 1, and No. 2, has the right technical background. If you get the combination of technical and political smarts, you’re incredibly lucky. And it happened very, very rarely.

And then, on top of that, the Secretary of the Department of Energy likewise may or may not be well prepared for the technical parts of the job. The people who are calling the shots on the program simply, for the most part, didn’t understand the technical part of the program and did not know how to manage big projects. So what do you expect to get—50 percent inefficiency? I may be being kind when I say 50 percent.

MV: This is great.

RM: This is brilliant.

MV: I want to say something in defense of my bureaucratic friends. Now, the term “bureaucrat” began life as a pejorative term. And most people who “are one” now are proud of it. I don’t want to put as much blame as Jean and I have just done on the

individual managers. We had some awesome managers back in Washington who really tried. But it's not just the bureaucrats, it's the bureaucracy. And we had a little piece of the Department of Energy that was responsible for this. But you can never forget that there's an administrative perspective, there's a secretarial perspective, and there's a general counsel perspective—all three of those forces—on what was trying to be done by our managers back in Washington, an unbelievable force on the direction of the program.

The Government Accountability Office (GAO) is doing an investigation right now on these very types of issues. I think when you and I spoke I referred to the Waste Policy Act as the great experiment that failed. It came right out of the '60s and '70s, where the people of the United States were saying, "Our government has to tell us what's going on; we have to have a voice in this." And I believe in the NWPA the pendulum swung way too far.

JY: Yes, that's true.

MV: The Department of Energy did not know how to manage that. And they tried to. They tried to bring people into the program and then could not deal with the fact that some of the people they were trying to embrace and bring into the program did not want the program to go forward. So this was not a cooperative venture, this was an opportunity for people to destroy the program from within. And that was created by the Waste Policy Act with the best of intentions.

You can just pick these things up. Where did the Environment Protection Agency come from? Where did the Nuclear Regulatory Commission come from? Those are all things that were trying to make sure that what the federal government was doing was more transparent and visible to the citizens it was affecting.

Look at the old Atomic Energy Commission when it was split apart. We've talked

with people who were staffers on the Atomic Energy Commission, and nobody knew what the budgets were. Those were black numbers, and they just happened, and it was autonomous. The budget would come in, and the money would go out. And in ’74, I think, was when the law changed that. They split that and they created the Nuclear Regulatory Commission and ERDA at that time, the Energy Research Development Administration. That was exactly so that the agency that was doing the implementing was not regulating itself.

That’s exactly what this country wanted. That’s where clean water legislation came from. That’s where clean air legislation came from. It was the idea that we ought to be able to hold the federal government accountable for the way they’re spending our money, and we should have a voice.

This is where the problem comes. I read a beautiful editorial the other day, talking about the problems we would have in this country if it were a true democracy. The South would not be integrated today if we had a true democracy. It would not. And you have to try to weight in this constitutional aspect that there are laws that the democracy has to work within. And that’s where people have the biggest problem, believing they should have a vote and not knowing how to manage the two different aspects of this. We have law in the Nuclear Waste Policy Act that was passed properly. We were doing what Congress wanted us to do. That should not be disrupted, even though Mr. Reid is trying.

RM: Yes, he’s doing his best.

MV: The law is the law. Remember Dan Dreyfus used to stand up there and point at the Constitution and say, “This is it. We have to do what the Constitution says.” Dan Dreyfus was one of the better project managers we had back in Washington.

JY: Yes, he sure was.

RM: Well, is it fair to say that it was a failed experiment in over-democratizing?

MV: I'm not sure that Congress appreciated that DOE did not know how to draw a line and say, "I am not going any farther than this. I will take your input. Here's how I will factor it in." They could not. I pulled this up for Jean. Remember this? Thirteen drafts of the environmental assessment ready to go to press. This was general counsel messing with the DOE managers. Every single time we had to redo another one, it was because somebody had figured out if we changed some aspect of it, a lawsuit might go away. Well, guess what? No. 13 finally came out, and they got every lawsuit that they ever thought they were going to get. But this is an example of what I'm talking about, somebody trying to work the system.

RM: Describe what this is.

MV: This is a poster that several of us were awarded at the final completion of the environment assessment; I'm proud to say this was my idea. It shows a phoenix rising from the ashes on the shoulders of all the 13 failed drafts of the environment assessment. We had 13 ready-to-go-to-press drafts before the final one got out.

RM: Oh, my God. This is like a seminar in sociology. Let's say that, like the phoenix, Yucca Mountain is going to rise from the ashes. How can they do it next time to avoid some of these problems? What would you recommend?

JY: Above everything else, I would set it up in such a manner that it could be managed like a private company. I don't know how you would do this within the government, so I guess I probably would vote for the kind where it's government-owned but privately operated, the kind where they get approval for a budget for some period of time that makes sense, given the kind of work that has to be done.

I would probably go further than this, I guess, in this case, although the reason I

say that is in part because of the point that Michael was making. If you still assume, and I think I would assume, that there would still be some level of public involvement and some level of transparency in the operation that seems to still be important to the culture of our country today, I think that a private company with managers who are selected because they have the right capabilities and have been given the responsibility along with the authority to get the job done and a reward system that rewards them for accomplishing the work that has been laid out in a way that there isn't so much churning and the replanning would be good. You have to get an organization set up where the right kinds of plans are developed.

If you wanted to develop a geologic repository, there's no new technology necessary. We don't need a lot of basic R&D in order to make this work. We already have everything we need. And so I'm assuming that you can give it to a company with essentially a scope of work and say, "Go do this. Go build this." And there might be some small amount of ancillary R&D that would go on, on materials and things, where you want to improve to the best knowledge of the times. But the key, to me, would be to give the authority and responsibility, which includes budget authority, to a management team and allow them to staff in a way that they can get that work done on a schedule that they commit to, with a proper reward system. So, I'm describing a well-run company.

RM: What would be an example? The TVA?

JY: I'm not familiar enough with TVA to know whether it's really a good model.

MV: It is.

JY: But, yes, if it is, then TVA might work like that. Are they civil service, though?

MV: You're both staring at me.

JY: Are they civil service?

MV: Yes.

JV: See, I'm not convinced that I would want the people to be civil service employees. Civil service employment tends to build in some of that bureaucracy that Michael was talking about earlier. And my impression is you could have a type of tenure, like you do in the university system, perhaps, that would work more efficiently and not tend to try to perpetuate itself quite as much as the civil service system does. I don't know. Tenure's got problems, too.

MV: Yes. I would have basically said that you need a model that looks like a real business, where you perform or you're gone. I agree 100 percent with Jean. And I have her at a disadvantage, because several of us spent a fair bit of time writing Darrell Lacy's testimony to the blue ribbon commission last week. You could have been giving Darrell's testimony.

RM: He said that?

MV: Yes, but everybody said that. There are two or three points I'd like to make on top of that. Most everybody who spoke to this issue said the problem is not so much with the Waste Policy Act; you don't need a different approach; that's the approach you need. Okay, we have to do some national screening; we have to select some sites. But there is the aspect that Jean emphasized—the business aspect of it.

But there's also the fact that a lot of people compare the United States with, say, Sweden or Finland, where they're having a bit more success with their repository programs; they point to the public involvement and acceptance. The point that was made at the Blue Ribbon Commission last week is it's not the local communities that are the problems here, it's the states. We have something different in the United States from what you find in Europe. In Europe, the local communities are the authorities. In the

United States, you have a different set of authorities. I did not hear, nor have I had a chance to read, Bruce Breslow's talk to the Blue Ribbon Commission last week. But I've heard it paraphrased as "what you did wrong was you didn't offer enough."

RM: Well, I was shocked when I read that, because Nevada was informally offered the super collider and the super train, and Chic Hecht himself was offered the big multibillion-dollar research facility on the Test Site and so on. I don't know how Bruce Breslow could say that unless he's just uninformed.

MV: Well, I will react a little bit more positively toward Bruce in saying that perhaps our understanding of what was really on the table was never viewed by the state as being on the table. And that's another big part of what Darrell's talk was about. You'll remember when Lake made this offer, it surprised all of us. Lake and the undersecretary from MIT.

JY: Dr. Ernest Moniz.

MV: Dr. Moniz, whose idea was to keep the repository open for 300 years, because that's 10 half-lives of cesium and strontium. This is when it's really dangerous. And let's talk to the state of Nevada about assuming a monitoring role, and responsibly. That's the part that's always baffled me. In my recollection of what was on the table, the state could have walked up to the table and said, "Okay, we want the final say-so on closure. We want to run the monitoring program." And I suspect they would have gotten it. I do not understand why the state didn't want it.

RM: The state didn't want it because they had vested political interests. Governor Bryan, on the first day, said, "I'm unalterably opposed."

MV: Right. Well, all I can tell you is the stuff that was on the table was well received. And, in fact, it was Susan Eisenhower who, after listening to all of the people talk about

how well the relationship had worked between DOE and Carlsbad in the Waste Isolation Pilot Plant program (WIPP), and how difficult it was on the Yucca Mountain program, wanted to know if these were two completely different government agencies. [Laughs] How can this be the same government agency? And the answer is, they tried it in New Mexico. They just talked about how bad it could be here in Nevada; they never tried it. They don't have evidence that it wouldn't work.

JY: Yes, because Nevada never got into the bargaining—to the extent that New Mexico did, at least.

CHAPTER SIX

RM: Do you have any further thoughts on how the Yucca Mountain project could have been handled differently?

JY: There are a lot of individual parts that could be done differently. In the technical part of it, in hindsight, there are things that you could do differently, now that we know what we know.

The big topic that we have talked about somewhat is the total system performance assessment approach that's been taken. And we talked about how there are some fundamentally different ways that you can develop a system model with all the pieces and processes that need to be represented, and then run it in such a way that out the end comes some kind of release, millions of years in the future.

And there are fundamentally two end numbers, at least by the Sandia folks who were most influential in the most recent period of total system performance assessment. There's still a fundamental difference of opinion—two different approaches. I'd have to say this kind of simplistically, in a sense. One which is more like what we have right now—the total system performance assessment mathematical model tends to be kind of a black box. There are ways that you can trace from the beginning to the end and see what caused a given horsetail plot (release curve) coming out to look like it does, but it is very hard to do.

Whereas, there is a way, if you had constructed it differently or if you went back and constructed it using a different approach, where the whole purpose, the objective of the way you set it up, would still have probably about the same mathematics, but would be set up in such a way that it was quite clear what caused the differences in the horsetail

plots that come up, in that the stochastic aspects of it would be easy to bring out and display mathematically, so that you could then say, "Okay, in this case, this variation really is driven by this set of stochastic parameters that were selected."

Whereas, in this case, right now you can do it. You can say, "I have an early failure of the waste package material, and that's why I've got higher releases in this run," but it's really hard to say much more than that without a heck of a lot of work.

RM: Which was which? Sandia was with which model?

JY: Both.

RM: And where was Nevada—your people?

JY: Well, there really is only one system approach right now.

RM: Is it the black box or the other one?

JY: The black box is what's in the license application. And "black box" is probably a little bit too strong, because in the last couple of years they did a lot, and Sandia spent a lot of time, fixing it so that it was as transparent as it could be, given the kind of mathematical model that been developed.

This goes back to one of the peer reviews that was very interesting. One of the guys who was very influential, Mel Marietta, who I guess ran the system PA for WIPP (the repository in New Mexico), came in as a peer reviewer on the model that the Sandia folks, together with other contractors, had constructed for Yucca Mountain. He came in as a lead of a peer review panel and zapped that model, black box model, even though some of it was created by his colleagues at Sandia, for the very reason I'm talking about. He and his colleagues felt the lack of transparency, lack of fidelity, as to what caused variations in the results was so high that it would be very difficult to defend, not only to the public—certainly that was in there—but, more importantly, to the Nuclear Regulatory

Commission. He really felt, and others like him felt, that the Nuclear Regulatory Commission would demand something, a mathematical construct that would allow them to more readily be able to see what was driving the variations.

RM: Well, why go with the black box? They weren't equal, I gather, in terms of their predictability.

JY: The other approach, to my knowledge, at this point in time has never been developed unless it was done by a group that was put into a closet and had food thrown at them. Knowing Sandia National Laboratories, they may have done it as a sidebar project. They may have developed something like that, because, at one time, there was funding for them to do that in parallel with finishing off the licensing total system performance assessment.

MV: It would have mimicked the WIPP model more closely.

JY: More closely, yes. The original idea for it about five years ago was that it would give you some kind of a check and balance, in a sense, on the licensing model. And the idea was that the Nuclear Regulatory Commission was going to need something, since normally they actually take computer codes from the applicants and run those codes themselves under a variety of conditions. In this case, that wasn't really going to be possible. They have it, but whether they could really run it and duplicate the results that are submitted in the license application—my guess is not.

RM: So their outcomes wouldn't have been quite the same?

JY: Right. Well, even if they could, even if they did staff up or have a subcontractor who could set it up and run it right, I think that there would be enough variations that couldn't be explained that it would cause them to have problems.

MV: There may be another perspective on that, though. The NRC had their own model

that they were running, and they published the results of their models. Consistently, our results predicted much worse behavior than their results did, which gave them a lot of confidence, because they're not noted for being kind.

RM: Yes, you want worst-case scenarios.

MV: It has to do with the stochastic nature of the system. It's very easy to say, "Yes, I can write a differential equation from getting to point A and point B." But what you're missing in that, for a system that's as complex as Yucca Mountain, is you have to make a lot of assumptions about how that system behaves. And what our black box does is it says, "I'm going to look at things that can happen in that black box." That's why it's hard to understand.

So I could be just as critical of somebody saying, "Well, I assume it looks like this. And if it looks like this, here's what's causing this problem." That's just as weak a model as one where you can't really understand what particular parameter did it, but you know that you've covered every possibility and you've looked at all the results. So it's sixes.

RM: Yes. Looking again at rising from the ashes, which I think is going to happen, what do you recommend?

JY: Well, my preference is that, with the knowledge base we have now, if someone was to start from scratch, or almost scratch, and develop the architecture for a new system model, they would do it in a way that was more transparent. Partly because, when you build these mathematical models, like the one that is the black box one, there are many times when, if you'd had time, you would have gone back and done it differently. But what you do is, you just continue to build on what you have, because you don't have time to go back and redo it. With this program, there was always no time to redo it but

time to just keep on adding on.

No systems analyst would ever construct a model that looks like the one we have today supporting the license application. I fundamentally believe that; it's the product of probably 300 or 400 staff members who went in and built and rebuilt, and built and rebuilt, and revised and built, and added and subtracted. So, of course, you'd start with the architecture as you know it, which we know a lot better now than we did 15, 20 years ago when this thing started. And you would put it together in a manner where you could be as transparent as possible. Sure, there's still going to be stochastic process, there has to be. But those stochastic processes would be represented in such a way that you have a better chance of following through on any given run as to what are the controlling parameters.

RM: With your background in evolution, would you tie what you ended up with to evolution? In other words, the elephant is a product of evolution. Maybe if you were going to design this guy again, you might do it a little differently?

JY: Some of those shortcuts that were taken, and some that were add-ons because somebody had a question and you couldn't go back and redo it, but you did something to make it work, are probably species that should have died out. They shouldn't have made it, but they made it because of tightened money, and because DOE needed a result. So, yes, there's no question.

RM: It reminds me of the old joke. Doesn't it go that a camel was a horse designed by a committee? Is that an appropriate analogy here? The camel is perfectly good, but . . .

MV: There's one better than the camel. You know the story of the blind men describing an elephant. The piece that's not in the poem is the guy kneeling behind the elephant with his hands in the mound of steaming stuff and saying, "An elephant is warm

and squishy." [Laughter]

RM: Yes. So it works, but it could have been done a little better. And when we get another shot at it, it will be. So it hasn't all been for naught, right?

JY: Oh, no, absolutely not. And the historical perspective on it is that we had contingents within Sandia National Laboratories who had very different views about how this model should be constructed, early on as well as late in the program. We had differences of opinion back in the '80s when we were first doing very, very rudimentary performance assessments on the site. And those carried through. They're not the same staff of course, now, but probably mentored by some of these scientists.

In the end, my belief is a significant shift in the program occurred about four years ago, I guess, when all of the scientific work was shifted to Sandia National Laboratories—shifted away from the US Geological Survey and the other national laboratories. The other national labs and the USGS had pretty much been reduced to a very small contribution at that point anyway. But part of the framework for laying the basis for convincing the government that the work should really all be shifted to Sandia, in my view, has to do with this fundamental difference of opinion that we're talking about in the total system performance assessment world. Because the person from Sandia who headed up the next to the last total system performance assessment peer review, brought together a team of very talented people, the right people, as peer reviewers. This was around site recommendation time, 2002.

MV: Now, you did two versions of the Sandia one. And one of them was coming out just about 2004.

JY: You're right, it was after.

MV: Just about time for the license, for the SR.

JY: That's right; it disrupted the date for the initial license application submittal, because we were heading for a date in something like 2004. We had this peer review panel. And the hope was that they would give it enough of a blessing that DOE would be confident with the total system performance results and let the license application go forward. And what happened was that they came in with a thumbs-down.

This was headed up by the Sandia person who has the view that this black box approach is not a defensible approach. Not that it gets the wrong answers, that it's just not a highly defensible approach. I would say that he and the team, some members of the team more strongly than others, had had some other specific technical issues that they disagreed with—approaches taken in the mathematical modeling. But the fundamental philosophical difference was this one which was driven by the peer review panel leader.

Looking at it from inside, looking at what was happening, it appeared that that was used, and, as usual, very capably used, by Sandia management as part of the leverage on DOE to say, "Look, you know, we can give you a much better system model than this—in our efficiency and the capability of our staff, and we've got the WIPP experience—and bring it all together." Together with this negative peer review, it put Sandia into perfect position to go in and make the end-run on the rest of the national laboratories and take control of the science.

RM: So, it was a competitive thing as much as scientific.

JY: Oh, absolutely. No question.

MV: But it was the right thing to do.

JY: Oh, I agree.

RM: To give it to Sandia, you mean?

MV: We had too many national labs and nobody managing that one.

JY: Yes. Having the lead national laboratory and some amount of work done by the other national labs was the right move. A national lab as scientific lead makes sense. A private company could do it, but you don't really have the durability that you have in a national laboratory. So it was the right answer. It caused a major disruption in the program. And in the scientific aspects of the program, I suspect it cost a couple of years.

RM: Just over the basics.

JY: It's bigger than that, but that was a trigger point. I remember seeing some of the presentations that Sandia used when they went in to the DOE Secretary to make the case for why they should be given the scientific lead for the program. They could use what they believed to be the negative aspects of that black box approach and say, "Here's what we could do for you." And perhaps, although the person that was the lead at that time was saying they could do it in very, very short order, that turned out to be not correct.

RM: What was his name?

JY: Mel Marietta. And, as I said, he was the WIPP total system performance manager. He had the experience there. And no question—very smart guy. But I think, like Michael said, the right answer for the program was to have a scientific organization with the lead responsibility. Sandia National Labs was a perfectly okay place to put that responsibility.

But having it happen at the time that it did, right around the time of the license application, caused a major disruption in the whole scientific part of the program, so that the preparation of the license application, as well as any ongoing scientific work, was disrupted. And it's another one of those situations where there is probably a greater than 50 percent loss of efficiency within the whole technical staff while this all was happening.

So the USGS and the national laboratories were all up in arms, because they had

no idea what their future would be under this new construct. And, although at that point most of the work of compiling the license application was in-house with the contractor here in Las Vegas, there were still some key national laboratory staff who were supporting that license application, very key. On the parts that I wrote, I had key experts who helped with those pieces. And some of those people just weren't there when I called, because they were off at meetings trying to figure out what their future was under this new construct. And so it caused a significant impact to it. It's another one of those situations where, like Michael said, it was the right decision but, if you wanted to submit a first-class license application in the time frame that DOE was on at that time, that was a stupid move.

MV: There's a problem here that maybe would be best if you understood, to put this in context. The national labs could not take direction from one of DOE's contractors. They could only take direction from DOE. And they really worked that to their advantage because, individually, they each had contact points within the Department of Energy. And so, you had somebody arguing for their work in the Department of Energy. Each of the labs was different.

And what we had to deal with was the fact that—pick a particular part of science—you might have three or four people working on exactly the same thing in different national laboratories. DOE tried the model where the contractor was given the responsibility for management, managing the labs. And all that meant was the contractor was chewed out for the labs not getting their stuff done on time. But the labs would not pay attention to the contractor who was trying to manage it. So you almost had to have another federal agency-like thing to do it. And of all the national laboratories that worked on the Yucca project, Sandia is the one that has an engineering mindset. They are the lab

that implemented the weapons delivery systems, as opposed to the theoretical physics. And so it was a natural.

RM: It was a good choice?

MV: It was a good choice, yes.

JY: I agree.

MV: I wanted to point out something. Thinking of peer review and looking at Jean made me remember this. At the same time that this Sandia peer review was going on, the managers of the program were having people who were very experienced in NRC licensing matters looking at our license application document and trying to assess whether they thought it was good enough to go forward to the NRC. And they said it was. Okay, so this is the scientists saying, "We can do better," as opposed to a bunch of people who did NRC licensing saying, "Look, this is good enough to take forward. It doesn't have to be perfect." So you had that trade-off.

RM: Well, in retrospect, should you have gone forward with it? That would have put it in under Bush, wouldn't it? Was that, in retrospect, a serious error?

JY: It was ready to go over two years earlier than when they submitted.

RM: So then you would have had a different NRC—I mean, different personnel, wouldn't you? And so you wouldn't have this problem that we're having right now.

MV: You know, I hate to second-guess too many people, but I think you and I spoke about the reason that the acting DOE manager at that time had so much trouble with the license application, with the issues of handling fuel and air and things like that and not getting what he felt were good, strong definitive responses. And, in Paul Golan's defense, I think he believed he had no choice but to pull it back and redo that design. And it wasn't the TSPA.

JY: No, it wasn't.

MV: No, the extra four years of working on the TSPA did not radically change the TSPA, did not change the science. It was the design that held it back for four years.

JY: Yes, the one part that was improved that you would be interested in, Bob, is that the part that was probably the hardest to really pull the string and see how the results vary was in the disruptive scenarios—any kind of disruptive scenario, but volcanism particularly. And so that part of it was really redone and was much better, given that there was some time. That part was a complicated way to deal with the interpretation of the Nuclear Regulatory Commission's rules on how to treat those kinds of low-probability events. Now people would say, from a systems standpoint, it's not necessarily the way you'd want to do it in the future. If you had the opportunity, you'd have set it up differently for the treatment of those low-probability events.

RM: Well, are we better off as a species, or as a nation or whatever, because of doing that? Or was it, in retrospect, an unnecessary delay, given the political circumstances? I mean, Harry Reid wouldn't be Senate Majority Leader, and so on.

JY: My personal view, knowing what I know about the design, which isn't nearly as much as Michael knows, is that it really wouldn't have mattered. You would have gone through a bunch of design reviews and evolution with the NRC review anyway. And the changes that needed to be made would have been made under negotiation with the staff members who drove you the way they wanted to drive you. In the end, you probably would have come out with something that would have been equally acceptable.

RM: So, you would have had to go down that road either way, you're thinking?

JY: I believe so. There were questions about some specific design choices, like Michael said. I'm not close enough to it even now to remember, but I believe the view of

the engineers on the floor and the engineers I trusted was that what we have is probably okay. What we came up with after three more years, or four more years and a fair amount of money, was also okay.

MV: Well, this is one where we had it one way, then we changed it the other way, then we had it another way and we changed it back. You know, this is traceable to 1979 and the Interagency Review Group under President Carter that we talked about, where "we're going to solve this problem in this generation's lifetime." And it created that aspect that led to the Waste Policy Act and led to Part 60, the Nuclear Regulatory Commission rule, that has the reasonable-assurance finding pegged to the construction authorization application, rather than the receive-and-possess decision. And what that really means is you've got to get it "perfect" on your submittal of the license application.

That's not the way NRC typically worked. NRC historically would take an application at whatever level and then work with the applicant, asking questions, and modify it over a period of years. We had different programs. It's the first of a kind where it was supposed to be some level of perfection before it was submitted. And that's a root cause of mischief, because somebody could always find something. You know, what is it, "the enemy of a good idea is a better idea?" What does that do for you? It just means somebody says, "Maybe it's not as good as it needs to be," and we'd go again for a couple of months, or whatever. That is characteristic of this program. It's always trying to make it better because it has this hurdle it has to get over to get to the next milestone, as opposed to just giving it to the NRC and taking your lumps. If they don't like something, we can fix it.

RM: So perfection was the enemy of the good?

JY: Oh, yes. I think this goes back to our earlier discussion about DOE management,

too. And I know Michael really knew some of these managers very well and has a lot of respect for some of them. Watching them from a little bit further away, I have to say that, partly because of the fact that they're political appointees and they're in a position where they have the responsibility but don't have the authority to really do the job that they've been told to do, it puts some of them at a huge disadvantage.

Going back and talking to some of the grand old men in the nuclear utility world, there were people like Bill Lee from Duke, who was the founder of Duke Power Company and who said he'd go and sit down with the DOE manager, and even the secretary and say, "Look, if you want this to really work, you have got to establish a rapport with the Nuclear Regulatory Commission. You've got to have both on-the-record discussions and off-the-record discussions. You've got to develop a kind of trust where your key to success is making them successful." This is Bill Lee speaking, not me—in his lectures he would tell us what he told some of the DOE managers is, "Figure out how to make the NRC successful."

I don't think most of the DOE managers had the ability to really get into that mindset. I don't think they had either the political smarts or maybe the technical confidence. In some cases some of them were not really technically qualified, necessarily, to manage the kind of work they were managing. Some had the political strength and a good technical background, like Dreyfus, but we had very few of them that could really do what at least someone like Bill Lee and the guys who were very successful in the nuclear utility business said you had to do in order to work with the NRC.

So part of the hesitation to go ahead and submit the license application, I think, had to do with the fact that the structure of the program just didn't get the kind of

managers in there that had the right capabilities, the right talents, to do that necessary interaction with the NRC. One of the big issues was what percent the design complete has to be, based on the fact that you are going in and trying to get this reasonable-assurance finding at this early time. There were people like Bill Lee who said, you know, "No more than ten or 20 percent design complete is all you would ever want to have, because you want the NRC to work with you to bring them something that in the end is what they really want." Well, DOE gets cold feet, for good reason, because their managers don't really have, or never really had, necessarily the backing of their management. Sometimes the secretary was on board and was behind this program, and sometimes he wasn't, for a variety of political reasons.

MV: This is a perfect example of what we're talking about. Who do you think is yelling that it has to be 100 percent? The state of Nevada.

JY: Oh, it was, absolutely.

MV: Yes, absolutely. Why would they take any other position? So you've got professionals telling DOE ten percent, 20 percent; the state of Nevada saying, "Read the Waste Policy Act, read the regulations. It has to be 100 percent." And where's your DOE manager going to come in? Well, the Waste Policy Act says I've got to listen to the public, you know. So, it's very difficult.

JY: That's a very good point. That is a big part of the drive. But I also contend that the types of managers you ever would get in a program that's set up like this one, running a bureaucracy topped by a political appointee, really would never be the Bill Lee type of manager who can interact with the NRC in the way you have to, to take a first-of-a-kind program through a licensing application process.

RM: But is one of the problems that the NRC is not the same beast that it was, or at

least the heads of it, before our present president, Obama, was in there?

MV: We are sitting at the cusp of the answer to that question. If the staff is allowed to go forward with their findings, with their SER, and the commission overrules it, it's certainly grounds for a lawsuit.

RM: Yes. But then we're back into this chaos that we're talking about. And Lee is saying, "Hey, work with the NRC," when, in fact, if you've got Obama's NRC in there, it's impossible, as long as they're there. And you have to wait for an administration change and everything.

JY: For this program you may be right. I'm assuming, and I may be wrong on this because I don't know what's been going on very much in the utility licensing side of it, but there have been some fast-tracked license applications for reactors moving through, with some of the utilities having quite a bit of success in the last three years. So there's a part of NRC that still knows how to do it and is still having good interactions with the utilities. And so, it may just be the commission, which is not a small thing, and maybe the commission is the problem in this case.

MV: Well, I want to make sure that we don't go on record as condemning the commission.

RM: Oh, absolutely.

MV: I mean sitting Commissioner Svinicki is, I think, fair and honest. And there are three new commissioners: Apostolakis, Bill Magwood, and one more—I can't remember his name. Those gentlemen are very capable.

JY: Oh, they're very capable.

MV: And I don't think that they could be bullied.

JY: I'd be surprised. I'd be really surprised.

RM: Really? Even though they made the statement before Boxer's committee that they would not go against the president?

MV: No, that's not what they said. The question was, "Would you second-guess DOE?"

JY: I'm not even sure they knew what they were answering.

MV: And, in fact, all three of them could end up recusing themselves from this decision, simply because of the furor that's been raised already, because they look like they're conflicted. So I'm not going to go negative on the commission.

JY: And what I was going to say is I don't actually have any reason to believe that the NRC overall, commission or staff, would have had a problem, had DOE gone in four years ago with a ten or 15 percent complete design.

At various times, the contractor for DOE here in Las Vegas that Michael and I worked for brought in teams of managers from the nuclear utilities, who would just essentially interview staff. In the contractor offices, they would meet with DOE management. DOE management was always so uncomfortable having much interaction with the utilities, because they were afraid that it would be viewed as some kind of collusion or something.

So even the fact this type of interaction was a problem reflects a different problem. That is, there should have been maximum exchange of information. It should have been perfectly reasonable for the managers from DOE to take insights and gain information from utility managers who have been successful in the nuclear utility world. But the idea was that it was either collusion or it was inappropriate, because there were lawsuits related to DOE being late taking possession of the reactor spent nuclear fuel. After the lawsuits had been filed, they couldn't interact with those utilities. If you want

this program to run in the best possible manner and be as efficient as possible, you want that kind of information. You don't want to stifle it. So it's another layer on the conclusion that we both came to earlier, that this is not the right way to run a railroad.

CHAPTER SEVEN

RM: One of the things I'm interested in is how you were drawing on all this scientific talent from around the world. You had the national laboratories; you were working with university people. You were aware of what was being done in Europe and other places. Could you talk about this huge world talent base that you were drawing on?

JY: I think from the very beginnings of the program, with the universities in this country as well as parts of the national laboratories that were not involved but had expertise, when the license application went out for review to the NRC and when there were challenges, what was finally submitted as the scientific basis for this project had to have had adequate review so that any real fundamental flaws would have already been exposed.

So the idea from the beginning, with Michael and me helping DOE, was to get all of the ideas out there. This was before the Nuclear Waste Technical Review Board, so before we even had them helping to get the problems identified, we had probably ten to 15 peer reviews in total, with at least five or six of them happening before 1987, when the law established the Nuclear Waste Technical Review Board. .

RM: When was this?

JY: This would be right around '82, '83, when the question that the state and others were raising was, "Would you want to put this kind of a facility in a location that is prone to earthquakes and has evidence of recent volcanic activity?" Recent, geologically speaking, meaning in the last couple of million years.

And so, we had at that time a team of eight or ten of the top seismologists and volcanologists in the US. I don't think we went internationally for anyone on that one,

but it had people from universities around the country, and they sat on a review panel for us. We asked those kinds of questions. At that point, there hadn't been nearly as much scientific work done, of course, because we were only in the early stages of evaluating the site. So we gave them what information we had; brought in the technical people from the US Geological Survey. There were US Geological Survey people on that panel who were big-name people and had not worked on Yucca Mountain previously. In each case, we tried to get first-class people who were in the organization but had not worked at all on Yucca Mountain.

RM: At USGS?

JY: USGS as well as the national labs. And USGS, of course, is a huge organization. They have people working in earthquake studies, or volcanic studies, who had nothing to do with Yucca Mountain. So we were able to tap those people.

RM: In your recollection, what was their overall assessment, based on the geology?

JY: Their view was that the volcanic risks were well within the risk range of the kinds of hazards that this country is accustomed to dealing with. This was early, before we had done any modeling. At that time I think we felt the probability of a direct intrusion of magma through the facility was in the same realm as a meteoritic impact. We actually had it lower than what it came out to be in the more recent thinking. But still, the direct intrusion, I think, is still down in the same range as meteoric impact, if you look at the probabilities.

As for the earthquake hazards, the fundamental conclusion was just that technology is there to deal with it. You will have to take into account in your design basis that you're in an earthquake-prone area. In fact, I think that they said, because it will be a nuclear facility with high visibility, you'll have to design on the high side in terms of

seismic risks.

RM: And how big an earthquake were you calculating for?

MV: Then or now?

JY: Well, bigger now than it was then, I think. I think at that time . . .

MV: About 6.5?

JY: It was about 6.5 on the Bare Mountain Fault.

RM: On Bare Mountain? So there's a Bare Mountain Fault that goes through there?

JY: The east side of Bare Mountain is a major fault zone.

RM: And it goes down to Yucca Mountain?

JY: No, it's north-south, just like the one that runs along the west side of Yucca Mountain. It's another one of those major north-south faults that are common in the basin and range.

RM: With the basin-and-range tectonics.

JY: Exactly. At that time, we did not have good fault maps. We hadn't had a lot of seismic exploration out in the valley to the east of Yucca Mountain. So we didn't know much about the Midway Valley Fault, and we didn't know much about the Paintbrush Fault. We didn't know where they came through—anywhere near where there would be surface facilities. We were figuring we were designing for a Bare Mountain earthquake—ten miles away or something like that—and for a 6.5 magnitude.

US Geological Survey had done research in that area and had published reports about what kind of seismic hazards were in the Yucca Mountain area, and they were on the order of 6.5 magnitude, I think. So the view was that that was well within recently available technology, which it was and still is today, even though the design basis would probably be a little bit higher than that today. It may not be much over a magnitude 6.5,

but it's on a local fault, not on Bare Mountain.

MV: I think the long-term one gets up to almost 7.5.

JY: Does it?

MV: I think it gets pretty high.

RM: How would you have answered somebody who said, "Well, what if there's an 8.0 or 9.0 earthquake there?" What would be your answer?

JY: Well, the approach you use, on this panel as well as on later panels, is to design to a recurrence interval. So, yes, there could be a magnitude 8.0 on one of these big faults, but the recurrence of it was probably a couple of million years. And you don't design to a recurrence interval of a few million. But what that panel told us at that time, and what continued to be carried through, was that, although not explicit, but kind of implicit in licensing for reactors, the recurrence interval was something on the order of a few hundred thousand years. That was the kind of recurrence interval they expected we'd have to design to, so that's why we were looking at what would reoccur, say, in a 200,000-year interval.

MV: I know this sounds like a broken record, but who do you think it was that was arguing that you can't site a facility in southern Nevada? It was the state of Nevada. And it turns out that they were basing their argument on an NRC rule for siting reactors, 10 CFR 100 Appendix A. That has some provisions in it that can very easily be interpreted to say that you would never site a reactor at Yucca Mountain. Reactors are very active systems. They have piping that has to be held together. And the NRC really helped us on this one, because they actually went to the press and the paper and said, "If we had intended to apply 10 CFR 100 to Yucca Mountain, we would have applied 10 CFR 100 to Yucca Mountain."

RM: So, the criteria are different.

MV: They wanted the criteria to be different. This is a very passive system.

JY: Yes, it turned out that there were a couple of leaders at the Nuclear Regulatory Commission who really took that issue into the scientific world and had a bunch of conferences where they basically had all of the people who ever had a dog in the race in seismic hazard work at that time, Leon Reiter being one of them. And they all came out in support of this paper that said you don't need to apply reactor licensing criteria to a facility like this one.

Over the years, by the way, it went back and forth, where we'd get a more conservative NRC staff member who tried to take us back to that kind of conservatism. And then we'd get a little bit more of a balanced approach for a while. Anyway, that's an example of a very early review that we had using a number of outside experts. But if you move on through time, probably the Szymanski issue hits next.

MV: A couple of Szymanskis came next.

JY: Yes. And during this time, too, there was generally a National Academy of Sciences panel that was at least looking at what we were doing. When was the Radioactive Waste Board established—the subpanel? It was probably in the mid-'80s, I think.

MV: Mid-'80s, yes. The Board of Radioactive Waste Management.

JY: Yes. And they had people on it who usually had nuclear background. Some were geotechnical, but many were nuclear engineering types. So you had a panel of people who were there to be tapped if there was some specific need for critical review.

In this case it turned out to be really useful for the Department of Energy, because they had their own guy inside the house, Jerry Szymanski, who raised a question about

the potential for hot fluids to penetrate up through the facility and expose the waste—to cause corrosion and erupt to the surface through geysers. Essentially, it was a disruptive scenario having to do with a geothermal type of activity—what Jerry would picture as an Old Faithful geyser coming up through the middle of Yucca Mountain.

And it's not unheard of. If you drive a little ways to the west, over toward Death Valley, there's evidence that there were hydrothermal springs, at least, at the surface in fairly recent times, geologically speaking. There are existing hot springs in the Tecopa area. The fossil springs don't look like they were terribly explosive, although there is some evidence that sometimes they got explosive. So it might have blown a few chunks of rock up and out, at least, if not a canister. The whole question was whether Jerry's interpretation was scientifically within the realm that needed to at least be treated as a potential disruptive scenario, and on the books as one of the things we would put in the mathematical model, crank it through, and see what effect it would have. Or is it something that is so low-probability as to be ruled out as incredible? You know, where does it fall in that probability range? First we had one more internal peer review.

MV: There was at least one, and then USGS did an internal one, too.

JY: Yes, USGS did one. We had one where Szymanski picked two of the members on the panel and, I think, we picked two.

MV: That was the second. There was one that Steve Mattson ran first.

JY: Oh, that's right.

MV: Bill Sublette ran that one; and Mattson ran one earlier. Because Jerry was so upset with the first one, we did a second one where he got to pick two of the panelists.

JY: Yes, that's right. And that one came out, of course, with two of the members supporting him.

RM: So now you were catering to Szymanski.

JY: Oh, absolutely. Well, we had also some staff members who supported Szymanski. So we had some really quite rational scientific people who felt that there was enough of an issue there that it needed to be investigated, and we took it seriously. DOE took it seriously, even though they probably would rather have not. Anyway, DOE was able to request that the National Academy, with the Radioactive Waste Board, review it. And it was a 17-member board at that time. They reviewed it and issued a book that said, "Groundwater at Yucca Mountain—how high can it rise?" And out of that comes the conclusion that an explosive hydrothermal event is an incredible event. It's not something that has to be modeled and included as a disruptive scenario.

MV: And when scientists say "incredible" they don't mean "awesome." [Laughter]

JY: Less than 10^{-8} annual probability, so it's off the hook. I do use that word both ways. It just occurs to me that people sometimes may not know which way I mean it. In my mind, "incredible"—less than 10^{-8} annual probability—is not "wonderful." [Laughs] So this took a period of about a year and a half or two years, at least. It did not dominate the scientific work during that time frame. During that time frame there was a fair bit of scientific work going on. We were getting new information. The understanding of the seismicity in the region and the understanding of volcanic history in the region was moving forward nicely. Those were our two biggest credibility issues, from the standpoint of people coming at us.

The fundamental underlying science—understanding the hydrology, the rock properties, that kind of work—was continuing on at a reasonable pace, with a few disruptions due to quality assurance issues. One of the biggest challenges that we faced, but also what caused it to make huge steps forward, was bringing it up to the point where

it could meet national quality assurance standards for nuclear facilities—where our records were good enough, the treatment of information was good enough, the traceability was good enough that, when you have some specific scientist making some conclusion, you could trace back to the raw data that supported that conclusion.

In the early days of the program, the need to do that was really not recognized. Most people felt like the credibility of the scientists and the experts would save the day, not knowing that in the nuclear world you have to be able to have records when the scientists are long gone. You have to be able to put your finger on the information that was used to make that decision.

So, that the design control concept, moving it into controlled scientific information over about a ten-year period, together with the very specific aspects of quality assurance that require you to document everything you do—and a scientist might not normally write down all of those details in his little notebook—all of that was a revolution for our scientists. I guess we had 700 or 800 scientists in total, and probably more if you add the engineers, who had to go though that revolution in order to work on the project. Some left; some didn't want to do their work under those kinds of rules. But in the end, we ended up with a group of scientists who would do it and who did a good job of it.

RM: Was this a new kind of wrinkle in the science?

JY: Oh, yes, absolutely.

RM: So, this is another contribution of the project.

MV: For national lab scientists; I was doing this in the early '80s, working under an NRC QA program.

JY: Yes, anybody who had worked in the nuclear programs outside of this project had

already done it. But most of the scientists who worked on this project had never done that.

RM: And the average scientist out in the real world . . .

JY: In the universities.

RM: . . . is not doing this kind of documentation, right?

JY: They may be more, today. If they get any money from the Environmental Protection Agency, National Institutes of Health—the federal agencies now have requirements for extensive documentation. It has evolved, of course, since the ’70s, to where probably most scientists are required to document maybe ten times more than they did in the ’70s.

RM: So, this is an evolutionary development in the history of science?

JY: Absolutely, yes. No question.

MV: For scientists who are usually left to their own devices, not for those of us who are used to having to work for a living.

JY: Well, yes, but those are the engineers.

RM: But do the engineers have to document more?

MV: This is pure engineering. Quality assurance is a way of life to engineers—having your boss review the document, having two check-off signatures of independent people—that’s a way of life in engineering.

JY: And in the pure sciences, or the basic sciences, it generally was your credibility. You know, you submit a paper for review: yes, there are going to be peer reviewers, and they’re going to review it, but are they going to question the fundamental data that you used?

RM: That’s right. And are they going to want to see your data?

JY: Yes, right. No, it was your credibility as a scientist.

RM: So it's a really interesting evolving wrinkle in doing science.

JY: It was a significant impact.

RM: And it's caused by the greater complexity of the whole process, right?

MV: Caused by the lawyers.

JY: Well, it's the need for documentation traceability when you're no longer there—when you, the expert, are no longer there to explain.

RM: Because your decisions have social weight.

MV: That's right.

JY: That's very true. So, anyway, it's bringing scientists into the world of social decisions that have potentially significant impacts on public health and the environment.

RM: This is wonderful. What you're doing is taking us through the science thing, because this is key to the legitimization of the whole thing. So take us to the next stage, if you've finished with that stage.

JY: Well, in the late '80s we went through the Szymanski review. During that time frame, between '85 or '86—EA days—until the early '90s, until the early site suitability evaluation (ESSE), I don't know that we had any other major peer review.

MV: I can't think of one.

JY: We had submitted the environmental assessment in '86, which had an evaluation against the DOE siting guidelines. For every one of these conditions that somebody wrote down and said was important, about whether this was a good site or not, we had to make a finding against DOE's Part 960.

So time had gone on. We had had the Szymanski episode, and we had had a fair bit of scientific work done, and it was getting into the early '90s, about '90. Carl Gertz,

then the project manager, who was a very good manager from the standpoint of trying to manage public perception, said, “We need some kind of milestone that we can use to raise the flag again and say, ‘We’ve looked at the site, we’ve looked at all this new information we’ve gained since 1986, when we last did this,’ and give some kind of a status report. Does it look as if the information is still suggesting, as it did in ’86, that this is a potentially suitable site, or is there something in there that’s really causing us to have some question about it?”

So Carl Gertz tasked the contractor—me—and Michael and others to put together a team of people to do this evaluation, called the early site suitability evaluation, ESSE. And it was really just a dry run for the site recommendation. In the end, it was rather different than the way the site recommendation came out, but it was a rerun of the environmental assessment with updated information.

RM: When did the EA go in?

JY: EA went in in ’86. This was six years later—’92, right, is when we issued it?

MV: Yes.

JY: So we did it. And this got us to the next peer review. Of course, in order to be sure that we did that in a totally objective manner, we hired ourselves a peer review panel. We had people from the University of Nevada-Las Vegas and Reno, because the idea was, at that time at least, that the governor was not forbidding our scientists in the state to at least review and objectively evaluate the information, to go on record as to whether or not DOE was doing a credible job.

RM: You mean they were forbidden later?

JY: Later, yes. As a result.

RM: I did not know that. That’s shocking.

JY: As a result of ESSE, and the fact that that got a fair bit of visibility in the local papers and on the TV. Dave Kramer from UNLV, a hydrologist, got interviewed. And he did not say negative things. None of them came out super positive, but they did not say there was any reason to think the site wasn't going to be able to perform safely. When they were asked those kinds of questions, the broader questions, they answered that the information appeared credible, DOE was doing a good job with the science, and the national laboratory scientists were doing a good job.

So, what came out of that, the peer review, was actually more important than the early site suitability evaluation. It was more important in that it got a lot of press from a panel, a very broad, broad panel. We had biologists, we had to cover the socioeconomic impacts, we had to cover all of the same things as the environmental assessment—the transportation, environmental, and geotechnical parts of it. So, out of that came a fair bit of press. Out of that also came Governor Miller, at the time, who then told both UNR and UNLV presidents that there would be no further participation of any sort of Yucca Mountain peer reviews.

RM: Referring to their faculty and staff?

JY: Yes. About two years later, we tried to get a guy from UNR as a peer review panel. That was Jim Brune, I guess. Yes. And he said, "I'd love to, but I can't. I've been gagged." And he was the kind of guy who went public with the fact he'd been gagged.

RM: Just as an aside here, in your understanding of the history of science in America, has that ever happened before?

JY: I'm sure it has.

MV: It happened in Italy to Galileo.

RM: Yes, right. Oh, my God. [Laughs]

JY: I think if a state has a very strong political position relative to some company doing some major work in that state or some major federal project, if we went back and looked and did the research, I'll bet we would find it's not uncommon for the governor to gag the local university people, because those people are the ones that are going to bring the greatest credibility. And, of course, you don't want them saying DOE's doing a good job.

RM: What year was this gag?

JY: That was, I think, either '93 or '94. Well, Miller was still in, so probably '93.

RM: Lord almighty. [Laughs]

JY: Oh, yes. The gag was relieved later, because we were able to use UNR and UNLV people on peer review panels. But at that time, oh, yes. I called Kramer back; I called several other people. There was a guy that was a health physics expert that we had on the panel. He wasn't really an expert, but he was supposed to be an expert that we had on that peer review. I tried to get him—it was probably two years later—and he said, "I can't talk to you."

RM: And they would have lost their jobs? Was that the threat?

MV: They felt that way.

JY: They felt intimidated, no question.

RM: Lack of academic freedom.

JY: Anyway, the peer review panel for the ESSE went through two iterations. We got their comments—they gave us a long list. We published a report.

RM: Now, who was on the peer review panel? Who were they?

JY: There was a professor from Brigham Young University who was the socioeconomic impact guy. There was a biologist—I think he worked for a company in

Arizona—who was a Ph.D., well known in the area. I don't think he was at a university, I think he was with a company—that guy that worked on the little fish, the fish going extinct in the Colorado River Basin. Anyway, you probably can pull up ESSE.

JY: There were about eight or nine members.

RM: Again, how where they selected?

JY: What I would do, any time I had to set up a peer review panel, is call someone on the National Academy that I knew, call a university professor who knew the work in that particular field. Like, let's say seismicity: if I didn't know who the really well-known people were, I would just call somebody at a university. And generally, because it's a very common thing to serve on peer review panels, they will say, "Oh, yeah, I served on a panel with . . ." and they'll list off the people. Then you can say, "Well, who do you think would be best for this kind of a peer review?" You get their recommendation and tabulate that all, and then sit down with your DOE sponsor and say, "Okay, here's all the information I have. How would you like me to proceed?"

RM: What would you say to somebody who would say, "Well, you were shopping for a favorable opinion?"

JY: We tried to be incredibly careful to make sure that, if we had an expert who we knew was either neutral or possibly even negative, that we included them. So we went out of our way to get people who we thought would bring controversial ideas, because, like I told you earlier, Michael and I and other people who tried to move this program through this scientific morass felt like we were going to always be more successful in the long run if we exposed our own problems, rather than waiting for the NRC to find a problem. So we always wanted to get the best people we could and people who had differing opinions, so that we could get those opinions on the table and address them.

RM: So you deliberately searched for a range of opinion.

JY: We did. Oh, yes, we always searched for a range. Exactly. Anyway, for that peer review panel there were about eight or nine of them. They came, they did their review, they looked at our response to their comments, as our peer review procedure required. The peer review panel then made a final set of comments based on our responses to their initial comments, and that's all documented. So you can see in there who said what about whom. It's all there for the record. And if there were differing views, they're all recorded. It can be a consensus peer review or not, so you may have a minority report that says, "I disagree with this peer review panel, with the majority view, for the following reason." And in a few cases, we'd have a few minority reports attached.

MV: I'm downloading it. It's much faster than trying to search it on line.

JY: Okay. So that was in the early 1990s. About that same time, we were going through major upgrades in our quality assurance procedures. And that's the same time when we had this stop-work order due to the environmental impact.

So we had environmental issues that we were dealing with that were holding up the scientific work. Carl Gertz, the project manager, thought something like this ESSE was a good milestone to assess the information and go through again, like an update to the environmental assessment. Particularly because much of the scientific work, because of quality assurance and because of environmental regulation problems, was, if not on hold, at least not advancing at the rate that we would have liked. So we had about a two-year window there when things were rather slow. Laboratory work was going on; the field work was really slowed down a lot.

CHAPTER EIGHT

JY: Let's see, when did we start the ESF?

MV: It was '94.

JY: ESF is the Exploratory Studies Facility, the big tunnel. At the same time, now, there was a lot of Nuclear Waste Technical Review interaction going on.

RM: Now, that was by the National Academy of Science?

JY: This was different. In 1988, the Amendments Act required the establishment of an oversight panel that was nominated by the Academy of Science and appointed by the president. So this select group—blue ribbon panel—was set up in 1988 and began to have approximately quarterly meetings with DOE and the scientists that worked on the program. They had a major impact on DOE's thinking in some of the engineering aspects and in some of the scientific as well, particularly with regard to what kind of an exploratory studies facility it would be, whether it would be vertical shafts, whether it would be a horizontal tunnel, whether it would be drill and blasted or mechanically mined or bored out with a tunnel boring machine.

The guy that was the chairman of the first 1988 board that was conceived through the Amendments Act was a tunnel boring machine expert, internationally. He was one of those people that went around the world making big holes in the ground, managing big holes, or being called in when they had problems with big holes in the ground. His first advice to DOE was, "Your plans are so far behind what's going on in the technology in the world, that the most you should do"—which is what they ended up doing—"is drill and blast far enough to get your tunnel boring machine in there, but buy yourself the best tunnel boring machine you can for this type of environment, and you'll be operating in

today's world, rather than 20, 30 years behind the times."

RM: What was the original plan?

JY: It was all drill and blast. It was just going to be mechanical at that time, particularly if it was vertical, I guess. I don't know—we were shifting at that same time. We were shifting to the horizontal entry, too.

MV: We did that to satisfy a board concern on the exploratory studies facility alternative study. That was at the same time we were addressing NRC objections to the site characterization plan and NWTRB [Nuclear Waste Technical Review Board] recommendations that we have the horizontal entrance, rather than the vertical shaft.

JY: They wanted the whole view. Mostly it was NWTRB, but then the NRC staff jumped on it, too, with the idea that you needed to have much better underground availability, to look at the structures as you went down into and through the mountain. If you just did a vertical shaft, you'd just see what's in one location, versus coming in and doing a kind of "U" like we have, where you get a lot more extensive rock exposure. So the Nuclear Waste Technical Review Board was very much involved during that time frame. As a result of their involvement, I think for a period there we were getting so much peer review from that board.

And they have a team of consultants that work for them, so that not only did you have a seven- to ten-man board, but each of them had one or two staff members. Then multiply that by at least a couple of consultants that they would bring in in whatever topical area they were looking at. So you had a team of probably 25 to 30 people, and almost all of them first-rate scientists and engineers, looking over your shoulder at every step of the way.

And they did have, as Michael said, a lot of influence on the way the design of the

Exploratory Studies Facility evolved before ’94, when we started construction. They also had a number of other areas where they would start pulling the string and asking questions. And, not that it caused huge deviations in the program, but quite often if they exposed something and it looked like we didn’t have a good defense, that was then a candidate for another peer review, run by DOE.

So one question that came up right in that same time frame was the question of whether DOE had the right type of material in mind for the waste package. Were drip shields really a good idea or not? What material made sense for them? But, specifically, it was the question. The Nuclear Regulatory Commission had a person doing materials research. I don’t know, Michael, did the state have somebody that was doing materials research, too, at that time?

MV: Yes.

JY: Yes, that’s right. This gets a little bit later. This is just an aside for Bob, because you’ll find it’s so interesting—the intellectual heritage of people. But the state actually had someone who was the thesis advisor for our peer review panel chairman, for our waste package materials peer review. Joe Payer was the chairman – and his Ph.D. advisor ended up working for the state of Nevada and doing research on materials. So when the peer review panel met, they would always have a public session where the public could come in and make comments to the peer review panel. Well, his thesis professor, the man he got his Ph.D. under, came into these meetings and gave comments to the peer review panel.

RM: And were they pretty far apart?

JY: Very far apart in terms of technical conclusions about the data, but huge respect for each other, even throughout the whole peer review process. And the waste package

materials peer review went on for a couple of years at least, because that was an area where, frankly in my view at least, the national laboratories didn't have a lot of expertise relevant to this project. They had it, probably, in materials for weapons design and for other applications, but we didn't have the best and the brightest in our materials area. We didn't yet, at that time.

The reason I'm saying we didn't have the best and brightest is that the materials area had always been, in the opinion of, I guess, Michael and myself, an area that had not had adequate support from the Department of Energy. They always kind of took the view, "We'll use a good stainless steel, or we'll use whatever's out there." Because some of the DOE people at that time had the view that the site is so good, the site alone will take care of the waste, so you really can put it in a brown paper bag—you don't need this very expensive high-tech solution for your waste package material, your canister material.

And so it's an area that, until the mid-'90s, or until the Nuclear Waste Technical Review Board started to put pressure on DOE, did not receive the kind of attention that you might think. In part it's because of this evolution of thinking the site could do it all by itself—arid environment, no water, safe and stable. Even the regulation didn't really want you to have to rely necessarily, over the longer term on engineered barriers. So in the early to mid-'90s was when we really started to emphasize what kind of materials in the long run will really be necessary, that you will use the best material available. This facility is going to be first of a kind—huge amount of attention. You won't go with something that's sacrificial, that's for sure. You'll go with something that has a good chance of lasting in that environment for a very long time.

RM: In the meantime, DOE was showing films about the crashes of trucks and everything. So they had a container that would stand shock and things like that.

JY: It was a transportation container.

RM: But it wasn't for the long term.

JY: It wasn't a disposal container; it was a transportation container. And it was going to be fine transporting the waste there, according to the studies that were being done. But the question was, what happens over 10,000 years or 500,000 years. At that time the Nuclear Waste Technical Review Board did not have specialists in materials on it, either. Their initial choice of board members, the kind of expertise that they put on their panel, didn't recognize the importance of materials, either. They had one man who was a professor emeritus of materials, but he wasn't really at the state of the art of this kind of materials durability, the kind that we're talking about. Michael, remember Ellis? He was such a nice man. He always complimented me on my clothes. I'd give these presentations, and he always told me I looked nice. [Laughs]

RM: Stephen Gould, where are you? [Laughs]

JY: [Laughs] The peer review panel that we formed had a man from Case Western Reserve named Joe Payer, as the chairman. He turned out to be a phenomenally good communicator, excellent in his research areas, well respected in the community, and someone who turned out to play a role in the program for at least ten years. He ended up becoming an advisor to one of the Directors of OCRWM. He became one of us, instead of being an external peer reviewer, but he ran the peer review panel. And in that same time frame, pretty early in the '90s, we had our first total system performance review peer review panel set up, too. And they may have even run in parallel.

MV: It was on the '95 TSPA in preparation for the viability assessment.

JY: Okay, so we had this peer review panel first—the materials.

RM: And it was set up when?

JY: This would be about ’93—’92 or ’93. Waste package materials. And the question at that time was really, “Are we looking at the right materials? What kind of materials are available or should be looked at for this kind of a design?” And we had on that panel people from industry and people from national laboratories that were materials experts but not involved in this program. And I think we had international consultants to the panel.

MV: The guy from Canada, Dave Shoemaker.

JY: Yes, we had a man from Canada. But we also had Europeans who were materials experts, who worked on other waste programs as consultants, because this panel formed its own consulting panel. In fact, at the end of the review, I was told that we had, within that group of scientists, all of the major materials experts in the world involved in this question on Yucca Mountain. And this was in the early to mid-’90s, heading up toward a 1995 date when we were going to have the total system performance assessment, top to bottom, kind of the first time with the big mathematical model. And ’98 was the license application date.

We had two peer review panels, because in about that ’93–’94 time frame we also were getting set up to review that big total system performance assessment mathematical model. So we moved some of the peer review panel members from the waste package materials panel over onto the TSPA peer review panel.

But, in the end, the waste package materials peer review panel played an incredibly important role in that. As I was saying earlier, I believe that DOE’s focus had just never really been on the materials, for a variety of reasons. To some extent, I believe that had to do, as I said, with the idea that you could put the waste in there in a brown paper bag, because the site was so good.

That was part of it. But, anyway, we got the focus on materials through the Nuclear Waste Technical Review Board and through the peer review panel. The peer review panel was an extremely capable, knowledgeable group. Because we didn't know the people in the industry, we had the chairman, who we knew was very well known and respected, choose his members. So he actually put his panel together. It's the one that recommended DOE move from the stainless steel alloys all the way to the alloy 22, the nickel chromium alloy. So it took them from a relatively cheap waste package to, probably, an eightfold increase or maybe a tenfold increase.

RM: By leaving the decision to him, how would you answer people who said, "It's a put-up job there. He's not selecting the diversity of opinion that you really need."

JY: The peer review procedure had in it the steps he had to follow. It was an NRC-approved procedure. And theoretically at least, it was set up in a way to cause you to be objective, as objective as you could be.

MV: I think it works in exactly the opposite way. I think you are subject to more scrutiny if you pick all the members of the panel, as opposed to finding a world-class expert and having that person decide who should be on the panel.

JY: But he had to write justifications. We had to have a basis for each member on the panel, what they brought to the panel. Each one of them had to write a reason why they didn't have a conflict of interest, in order for them to be on the panel. They had to go on record that they didn't have a conflict of interest. There were some who had relatively strong biases. And, like I told you, moving now to the TSPA peer review panel, we had at least one person on there specifically because he already, in print and in public, had stated his view that Yucca Mountain was totally the wrong kind of environment, totally the wrong place to put this kind of a repository.

RM: To make sure I understand your point, having the guy appoint his own panel is better than having you do it, you think.

MV: Coming from the design control perspective, an engineer or group of engineers would do a design. And then it is independently reviewed. That means you don't mess with the review, right? That's where this procedure that Jean is talking about comes from. It comes from the perspective of, when you get that second signature on the drawing, you had nothing to do with that review. It is completely independent. So it's legitimate to say, "This person, his reputation or her reputation, stands alone, and that person can chair a peer review of this work, and that person has the right to pick whomever they want, because they're picked because they will be critical of the work." They're not looking for a rubber stamp; they're looking for a critical review.

JY: Yes, and every now and then we would have a fit be thrown by someone within DOE, who would say, "Oh, but this person is not objective; they're on record." Like this one man in particular, who was a University of Michigan professor and who was well known to really fundamentally question this whole concept. We usually had someone at Department of Energy, for example, Max Blanchard, before he retired, who understood that's exactly the person you want on your peer review panel. But there were times we had trouble. We had to go over to the DOE and make a real case as to why you want those people now putting their information in front you, so you can respond to it, not later, as somebody bringing in a contention to an NRC licensing hearing. You want to address all your contentions now if you possibly can. That was the objective. The better DOE managers knew that. They were with us, absolutely. Anyway, that gets us up to the TSPA peer review panel.

It occurs to me that the evolution that was going on in the Yucca Mountain work

is parallel to what was going on in the rest of the country, as we went through the post–Three Mile Island changes in the nuclear utility world, licensing world. And the Environmental Protection Agency, the whole Superfund, all of the treatment of waste sites. Public awareness was going up—skyrocketing—of all of the kinds of impacts of many of the technological advances that we take for granted. We're starting to have to pay the piper. Because here's the byproduct, folks. You've got Superfund sites all over the place. You've got wells blowing up in the Gulf of Mexico. Over this 40-, 50-year period we're starting to realize we didn't get this all for nothing.

RM: Yes. Human beings are starting to deal with the implications of their technologies.

JY: Sometimes well, sometimes not so well.

RM: Let's talk a little bit about the quality of the science that was behind the Yucca Mountain project, and the quality of thinking that went into it, versus the public perception, and particularly versus how the science was being presented by people who, for one reason or another, were opposed to Yucca Mountain.

JY: The involvement of the scientific community in this program probably exceeded any other technology type of program in the world, from the standpoint of universities and private industry. The private industry involvement just skyrocketed when we went into the materials peer review. We probably had 90 percent of all materials experts. If you called one who's known as one of the big experts in the world and had him name his 49 friends who he believed were colleagues of equal importance in the materials research, we had them all, in one way or another, involved in consulting on the engineering side, specifically on materials for the waste package and the drip shield. But they got into broader questions than just specifically the materials.

So when you consider that, on both the engineering side as well as on the

geotechnical side, we had major involvement from university people and from people who worked for the national laboratories, both who worked on this project and those who didn't, the idea that the science could have had some major flaws is just absurd. There are too many independent, well-respected scientists in every specialty that we used on the program to build the basis for the license application and that have critiqued, analyzed, reverse-engineered, forward-engineered, and have done all of the types of critical analysis that you need done to assure yourself that you've got the best possible technology and technical understanding that you could have of the site and the facility you're going to place into the site. So it's just incredulous, not incredible, on the part of Senator Reid and others to say that the science is flawed, or that there are technical problems with the site. It's just not true.

RM: Yes. You've mentioned the materials specialists. What other disciplines were involved in this?

JY: Well, because of the way the DOE's siting guidelines and the NRC's technical criteria for the site were written, and the way the Nuclear Waste Policy Act set up the whole regulatory framework, the types of specialists you had to involve were all across the fields of geotechnical sciences and engineering, as well as what I'll call softer sciences, such as socioeconomics, transportation, and radiological—radiological safety having to do with operations safety as well as closure-type safety.

So you had to have scientists and engineers involved who were at the state of the art of their fields in those broader technical areas, as well as just the geotechnical that we often focus on, relative to the safety of the site. Which leads me to the conclusion again that the technology that was embedded in the license application is at the state of the art where necessary in all of those areas, including what you need to do in terms of

socioeconomic impacts of a facility like this, what you need to do in terms of transportation-related impacts, and any technology development.

So the involvement from the environmental science side of the house, the socioeconomic-impact side of the house, and the more strictly geotechnical side—from the very beginning of the early peer reviews we always included people who were experts in biological impacts and environmental impacts of all sorts. So I believe that the information that's in the license application has been vetted through a broad technical community. Not just geotechnical, but the broad type of information that had to be presented to the NRC in both the EIS and the license application itself.

RM: So you had world-class people working on it, right?

JY: I think in some areas we probably didn't go to the level of world-class because it wasn't really warranted. But, yes, in some areas, like materials, there's no question that we had world-class people.

RM: At the same time, what you were doing was being overseen by the National Academy of Sciences and their board, and they are world-class, right?

JY: And the Nuclear Waste Technical Review Board, which almost always had several National Academy members on it, or people who were about to become National Academy members, together with the board on Radioactive Waste Management that was an arm of the National Academy. So, yes, depending on what criteria you use to call them world-class, National Academy members certainly meet those criteria.

And there've been a number of National Academy of Engineering and National Academy of Science members on peer review panels and the Nuclear Waste Technical Review Board.

MV: Let me prompt you here. When you and I spoke and I told you about the letter that

was written in 1979 to the National Academy, when we were beginning to study tuff and saying, we identified some things that were unique to the site and maybe potentially difficult. Two of them were seismicity and volcanism. And those two were among the features, events, and processes that we dealt with. Those are the ones that are potentially the scariest to people. I'd like Jean to talk about the level of people that we had involved in those two areas particularly, because they're comparable to the materials.

JY: Yes, I think the area of volcanism, particularly, was the larger focus. In the case of seismicity and earthquakes, the question always was what kind of design basis will you need? It's more of an engineering question than a scientific question, per se. You've got to make sure you understand the geology well enough and the earth materials well enough to know how any earthquake that will occur there will affect the materials, and therefore what kind of design you need.

But from the standpoint of not being able to adequately characterize the seismicity of the region, there was never a question that we could do it. Of course you can do it. It's just a matter of how many wells you have to drill, how many seismic lines you have to shoot, what level of detail would be necessary. And even if you went through the back-and-forth with the Nuclear Regulatory Commission staff, the questions were almost never, "Was this site really seismically challenged?" if you will. It was always, "How much additional work do you need to do in order to adequately characterize the seismic levels—earthquake sizes and intensities—that you'll ever have in this area?"

So I always have thought of the seismicity and the earthquake challenges a little differently than the volcanism; and for that reason we treated them differently. In the case of the seismicity, we had some experts from around the country involved in various peer reviews and review panels over the history of the project. But it did not receive the focus

from the scientific side that the volcanism did. That's partly because there's an engineering solution, and it's a standard approach with reasonably available engineering solution.

In a few cases, people have tried to design something to stop the effects of volcanic activity, in terms of diverting flows and things like that. But for volcanism, from the very early days, we knew that was going to be one of the very critical areas. So we started out in the earliest days having a number of experts involved as consultants, even, just as independent reviewers, so that any of the volcanism work that was done by the national laboratories—Los Alamos National Laboratory, together with some USGS folks, tended to be the specialists in volcanism. It was reviewed on almost an annual basis, and maybe more often than that before we had the Nuclear Waste Technical Review Board looking at volcanism, by independent reviewers. We normally had a panel of three or four or more volcanic experts kind of as resident consultants, who were called upon to monitor, overview, and review the work at every stage as we went along.

Another interesting sidelight on what happened in the volcanism area is that, because we did have our in-house resident experts, it became even more critical to have external people looking over the work, because the work was viewed as potentially flawed and not objective if the person had a conflict of interest because he was a national lab employee. When he served on review boards, for example, even though we put him on because he brought the expertise to the board that was necessary in order for them to fully understand what we knew about the volcanic history, he was always viewed as potentially biased. And so the critiques and the critical reviews were most intense in the areas of volcanic activity. Did we ever do a peer review just specifically on volcanism? It always got covered as part of other reviews.

MV: Just the volcanic hazard elicitations, and those aren't really peer reviews.

JY: Right. I don't think we ever did a specific one. I think the reason is that we had that ongoing group of about five to six experts, internationally recognized experts, as a review panel that was always there. I would say they met at least a couple of times a year, maybe three times a year. They would critique, and, under formal review procedures, of course, they would offer their criticisms. Their comments would then be addressed, and some of the work would change as a result of their input. So, built into the work of some of the very best national laboratory/USGS scientists was the work of another five or six experts that were not full-time employees but were on this review panel.

RM: Was that review panel the National Academy of Science?

JY: No, this was separate. These were consultants, actually, to the subcontractors, in our case either SAIC or TRW or whoever was the operating contractor for DOE. But the National Academy of Science also did reviews of the volcanic work. And the Nuclear Waste Technical Review Board had several people and staff members who had expertise, or they hired someone as a consultant who had expertise. So that area got incredible scrutiny. I'm sure there's no other area in the world where the volcanic history is better understood than in the area around Yucca Mountain.

CHAPTER NINE

MV: There's a perspective on the scrutiny that the Yucca Mountain project received that's probably worth capturing, Bob. Jean mentioned the potential for conflict of interest, I'm assuming by Bruce Crowe. We have many scientists on our program that were of that caliber. If they were working on another program internationally, they were considered among the gods of the field, and, without question, they were unbiased. They could talk about anything. But once it was Yucca Mountain, they were viewed as being conflicted.

RM: Because they were consulting with you?

MV: They would go consult on a foreign program, and they were the world's experts. But within Yucca Mountain they were evil.

RM: Just the association was contaminating? Why was that the case?

JY: Well, maybe there was earlier evidence of it, but it started, I believe, when Miller was governor.

MV: '85 or so.

JY: It would be just around the time of the environmental assessment. The American Nuclear Society held the annual meeting here in Las Vegas. It was one of the most embarrassing situations for those of us on the science side of the program that occurred, probably, throughout the program, at least in terms of the outside perception of our politicians in Nevada. The American Nuclear Society, a very professional society, invited Governor Miller to speak. And in his speech, if I remember correctly, he called the scientists who worked on Yucca Mountain "Keystone Scientists."

RM: Like Keystone Cops?

JY: Exactly, exactly. And he was rude to the American Nuclear Society as well. That's the standing governor of the state. So that set the tone, in my view.

MV: But you know that Bob Loux wrote that speech.

JY: Well, of course. It set the tone, though, the fact that he would deliver it to a crowd like that. But I think there were others. I'm sure there were other occurrences before that, but that was the most public one I can remember that leaps in front of my mind in terms of turning public perception against the scientists on the program.

RM: Well, what was the context that would enable a governor of a state to say something like this? Can you give me some perspective?

MV: For any colloquium or symposium, they have an opening address; it's not unusual to have a ranking government official give it.

RM: But to call all of these people in the audience Keystone Cops, and here they are scientists. What is the context that enabled something like that to happen?

MV: Freedom of speech in the United States. [Laughter]

RM: Yes, but not only is it rude, it's ignorant.

JY: It's the beginning—in the early stages—of the really terrible conflict between the members of the political establishment in Nevada and the program. I think there were other examples that were equally, or almost, as bad. But to me, that was the one that was kind of like the starting point.

From that time forward, there were times when we had new project managers or when DOE headquarters had new people who would come in. Michael was much closer to this side of the work than I was. But they would come in with absolute confidence that they could win the state over, or at least get the state to the table and have honest discussion about, "What can we do for you and what will you do for us?" They would

come in, gung ho: “I’m going to make this work.” And three to six months later they were, like, throwing up their hands and saying, “I don’t believe it. You can’t do anything. You just can’t do anything.” In each case, if you go back to when Richard Bryan was governor and then Robert Miller, with the strong political figures in the state being few and far between, the ones that we have had, this has been their banner, or at least one of their banners.

RM: So, when Governor Bryan said on the very first day of the Yucca Mountain announcement in Las Vegas that he was unalterably opposed, that was the case, and remained the case, right? Rationality had nothing to do with it beyond that.

JY: That’s right. And I’m sure Michael went through some of this, but some of these guys, like Bryan, and I believe even Reid, supported the work at the Nevada Test Site and the fact that it brought jobs to the state, and were very much supportive of additional DOE funding coming into the Nevada Test Site, while not even having a clue or understanding the differences in the work there and the potential impacts on the environment or on the population—as you said, no rational comparison of the kind of work. No basis for that other than—I don’t know. I honestly don’t know. If you did a real sociological/psychological evaluation, what was it that caused Bryan, Miller, Reid to take the positions they’ve taken?

MV: You can’t understand it.

RM: In my view, one of them is that they were Democrats, because the main opposition to nuclear power comes from Democrats. There are also Republicans who are opposed, but it tends to be a Democratic thing in this world today. If the Republicans were running the Senate and Congress, and you had a Republican president, Yucca Mountain would be a reality. That’s my own belief.

MV: But you have to temper that, because this is only Mr. Obama needing Mr. Reid to work his—Mr. Obama's—agendas in the Senate.

RM: That's true, because there's a certain elemental thing about Obama. He's a bit of a pragmatist, but he has to deal with people like Markey in Massachusetts, who is as opposed to the whole concept of nuclear as anybody. My brother and I were talking about this: how was it that the US most of all in the world came to this negative view of nuclear power. What happened?

MV: You have an ostensibly better-educated populace than in many places, and you have an emotional issue that people tend to not want to take the time to understand. They know how to express their concerns and opinions through the news, through the elective process. It's more likely that they don't understand it.

RM: But that all begs the question: what about France and what about England? And now you have the Scandinavian countries moving to nuclear power.

MV: "Better-educated" was not the right term. It was more, how do I say it, a populace that's used to being heard. Whether they know it or not, they're used to being worked by the media. The media does things to sell newspapers, and so they're always looking for something to get people worked up. You don't tackle something that people understand well and have accepted in their life, you tackle things that people don't understand and have an emotional reaction to, even a fear, because they don't place it properly in the context of all the risks that they deal with. They've come to believe it as somehow worse than it really is.

RM: But why does this tend—and I emphasize *tend*—to be Democrats, as opposed to Republicans? To me, that's an important thing. Are we getting too political?

MV: No. But that's so easy. Leave a Republican alone, he'll never open his mouth.

Who is it always going to be out there demanding equality for people? Who wants change? Who wants change for the better? That's not what you're calling a Republican, that's what you're calling a Democrat. Most people, most "Republicans" are content to sit back and not having anything in their life change. "Just keep it going the way it is. It's fine with me."

RM: It does raise the question of the Tea Party, now; they're pretty vocal. But I get your point.

MV: But the Tea Party is also very simple. This is an ability to convince people that, because they've been sitting back and letting things go the way they've been going, they've gotten themselves into a position where you now have to take action.

JY: The nuclear thing, though—I mean, the further explanation of the risk part of it, not putting the risk in perspective—goes to the generation that still remembers the bomb. So nuclear energy is tied to explosions and the bomb. And the idea that we always had was that, as you got to another generation, you would have people who were further removed from the vivid memories of use of the bombs, in which case you would have less fear of nuclear energy. But the problem is that it's been perpetuated for reasons I definitely do not understand.

RM: But here's the problem with that view. You've got Japan, who experienced nuclear bombing, and they're not opposed to nuclear power plants. They have them.

JY: No, that's right.

MV: But you're talking about a society that is not used to being able to make noise. This is the last thing I'm going to say. A year or two ago I had an opportunity to help a middle school group in a debate. They had drawn the bad side of the debate—they had to present the case for nuclear and Yucca Mountain. And I spent very little time with them,

mostly just helping them find the arguments and put it together themselves. They put together a beautiful position on the debate.

The thing about this that I'll never forget is that everybody was willing to admit that they were kind of right and they kind of won the debate, but the antis won the debate. It carried the weight, not because of any rational perspective. These kids did a wonderful job. They put Chernobyl in perspective, they put Yucca Mountain in perspective. They did their research, they got it right, and they could not be refuted. But they still lost because of the emotion of this.

JY: Well, see, that brings you back to why it is so easy for the politicians to influence the people about the idea that the science is bad. Obviously, if I am already preconditioned to think that things nuclear are more dangerous and likely to harm me and my family, and now somebody who is an authority figure in my world is saying that the science supporting this thing is bad, well, it makes it easier for me to believe it, I guess, right?

RM: You know, I did an interview with Chic Hecht not long before he died, and he believed that Bryan cynically used the Yucca Mountain issue against him, and that that's one of the reasons why Bryan beat Chic Hecht and wound up in the Senate. Steve Bradhurst said a fear issue is always wonderful for a politician. Whether it's Bryan or Reid, they knew it would work because there was a reservoir of fear out there.

JY: And I think probably it hurt Yucca Mountain, too, that in the same time frame there was a fair bit of publicity about the downwinders and the impacts of the Nevada Test Site radiation releases. That tended to flavor and give additional support to the idea that DOE and the people who work for DOE had done bad things in the past, and they're going to do bad things in the future. The overall loss of support for scientists in this

country in the last ten, 15 years is well known and well documented. So all of that comes together to make it easier for Senator Reid's bad-science claim.

RM: Yes. Could you talk a little bit about the involvement of other international scientists and their academies of science in the Yucca Mountain effort, as well as what they were doing on their own?

JY: I'm not as familiar with the international work as Michael is, so he can probably give you better insights on that. At various times, we had people from the International Atomic Energy Agency as reviewers or as members. One guy, Dan Galson, was a total system performance assessment expert and worked for the French program, didn't he, for a while?

MV: I think principally the British program. He probably worked for the French program as well.

JY: But he then took a position at IAEA, and he consulted either for Sandia National Laboratories, after they were the total system performance assessment contractor for DOE, or for some of the other individual laboratories that had contributed to the program, and served on the review panel at one point.

But I am not very familiar with the international programs, other than to know that, to my knowledge, none of them had the type of program we had in the total system performance area, developing a system model of the complexity and detailing out the processes to the extent that we did. Because I know when we would go to look for a comparison, there really weren't any. There weren't any that were similar enough.

MV: There are two perspectives on that. One of them has to do with our regulatory structure, which is much more complex and much more rigid than any other country's. And the second thing to bear in mind is that there are international cooperatives from

many perspectives, the IAEA, the Organization for Economic Co-operation and Development (OECD), and NEA, the Nuclear Energy Agency—international agencies that put together teams that would work problems to the level that they were common between the different programs. And one thing that was done was a series of tests, if you will, for total system performance assessment, where people, even though they had different types of approaches, were given the same problem to see that all the models would give the same answer.

JY: Or differences that you could explain, which was how it really worked.

MV: Exactly. So that's how the international groups were using each other for peer review.

JY: The one area where there was really a lot of international influence on this program, I believe I mentioned before. But just to make sure we have a good understanding, and that was where Michael just started to talk, it is the way you go about figuring out for a given site all of the features, events, and processes that you need to include in a mathematical model in order for it to be defensible and credible.

This was under the sponsorship of IAEA, I think. And what they did was to convene a panel that had representatives from all of the countries that had programs that were developing geologic repositories, or something like a geologic repository. And they had them get together and brainstorm all of the possible kinds of generic features, events, and processes—things like volcanism and earthquakes—that need to be considered. This is the combination of everything that needs to be considered, taken to a level in this group where they identified all possible features events and processes, and it was something like 7,000 or 8,000.

Then what each of the countries did—and we have this all documented in great

detail—is go through very explicit organized decision-making in order to wean that down to those that had to be included in the mathematical models, those that were of sufficient probability to be credible enough that they needed to be included, given regulations about what's credible.

So for this site and for other sites that are being screened and then prepared in other countries, that type of a process—I presume other countries did it. We have an incredible amount of documentation about how we got to the select set. I don't know how many are actually included now.

MV: 230.

JY: Yes, 280, 230, something like that.

RM: So, from 7,000 you went down to 200 and some?

JY: Yes, under 300, the last I knew. They were adding a few here and there.

RM: Can we get that list for our archive? Again, this strengthens the whole idea.

MV: Well, it makes it very difficult to argue that we missed something.

JY: Yes, that approach. Then, of course, that particular list, as it got winnowed down to the set of features, events, and processes that was used to build the total system performance model, was challenged each time we'd have a peer review. They would come in with a lot of "what ifs," because that's what scientists do. They come in and say, "Well, my expertise says that this particular thing has not been considered."

You pull out this book, and you go in, and you say, "Okay, here are the three places where we have it considered. Now, is this adequate or not?" In some cases yes; in some cases no. And it would cause us to go in and change the way we represented some particular feature about the site or some particular potentially adverse condition about the site, because of the way the scientific expertise came in to us after we already had, say, a

set of 300 or 400 that we were working on. That happened over and over. And as you know, when you bring experts in, you're asking them to do that. You're asking them to critique and look for holes in the work. And so they look for holes.

MV: When the license application was being reviewed, there were plenty of lawyers reviewing it. And my recollection of several of the DOE lawyers is that the parts that they hit the hardest were the arguments in the features, events, and processes screening because that's what everything hinged on.

RM: If you miss something, it's not in the model.

MV: If our argument for excluding something was not really defensible, they wanted to know why. They wanted more information.

JY: Yes, I was there for some of that. Some of the conditions and processes were excluded because these are "incredible" by NRC standards, less than 10^{-8} annual probability. Well, the basis for that was expert judgment of the scientists who were brought to bear on this. That wasn't enough for the lawyers. In many cases, you had to go back and put in additional information to answer, "What was the basis for your expert judgment that it wasn't credible?"

In many cases, the ones that had something close to a 50/50 column, whether it should be included, were the ones that they attacked. And the lawyers who were doing this were very good at finding the ones that were a little soft, digging in on those. So we spent, I think, at least six months on the scientific side just beefing those up. The exclusion bases are the ones that are most important. If you include it, okay, it's in. But if it's being excluded, those are the ones that are going to be red flags later. So you want them as strong as you can make them.

RM: I think we have covered this, but just to go over it a little bit once more: in the

history of human experience, has a model of this complexity ever been developed? Can you think of one?

JY: We have to both answer this, because we both have views. I think what Michael said a few minutes ago is probably a key point in this, and it hadn't really occurred to me. But regarding the fact that the regulations had evolved the way they did in this country, which helped lead us to the complexity of the modeling that we have, I believe the answer is probably "no." I think there's probably no place else that has done the type of computer mathematical-based model that we developed. Others have done computer modeling, but I don't think they've taken it to the same level, with the number of features, processes, events included. I just don't know that there's another country that has put the kind of effort, scientifically, into the process.

RM: And if they did, what field was it in?

MV: Well, here we go. You've asked the question a little bit differently now. My standard response to you doesn't work now, because the technical achievements that I think are far more significant than Yucca Mountain were not based on numerical modeling, because it didn't exist at that point. But now, the way you've asked the question today, I'd have to scratch my head and say, "Some of the human genome modeling in biological sciences has to be as complicated."

JY: Oh, I believe it's getting there now. And I believe that atmospheric modeling also has been very complex as well. I mean, they can take minute differences in ocean surface water temperatures and filter that through to what that did to the path of the jet stream, any day of the week.

RM: Did any of them have a little under 300 factors—what I would call factors?

JY: My guess is that the atmospheric models and the molecular biological–type

models, my guess is they probably have.

MV: They probably don't have the black box stochastic perspective that we have, because they could never model those types of things the way we did by making sure that whatever could possibly happen was in the box. They're more likely building a model and testing a model.

RM: That's right. And correct me if my logic is wrong, but they're looking for linear outcomes more than you were. You were looking for stochastic outcomes. They were looking for linear outcomes.

JY: Atmospheric science has some stochastic stuff like that. Turbulence is stochastic process.

MV: I'll give my perspective and give Jean a chance to think about this. What I'm trying to say is that they're certainly nonlinear systems. "Linear" means something to a scientist. Whether they're stochastic or not, they're very nonlinear. But what I'm trying to capture is the discussion we had with Jean the last time we met, with the difference between the proposed Sandia model, where they actually would try to write an equation for each piece of the system and link them together, and the way we've done it, which we call the black box for shorthand, where we might not necessarily have known exactly how to model what happened in there, but what we did captures all the conceivable outcomes. And my guess is that the climate model is more like the former than the latter. I don't think you could handle climate process on a world scale the way we did it. Even the most complex models have a relatively small little piece of the world they're trying to understand with all its inhomogeneities.

My suspicion is they're not treating the world model with the same level of inhomogeneity that we're trying to model within Yucca Mountain. It's still nonlinear, but

we had to adopt stochastic approaches because of the two different types of uncertainties. We did have uncertainty in the data sets that we understood. We have the unknown-unknowns uncertainty as well that we have to deal with. And I think that will be a consideration to them, but they're not building their model around unknown unknowns like we had to at Yucca Mountain. Now listen to the expert. [Laughs]

JY: No, that's right. The other key difference, I think, is that we specifically had to be able to run this model over multiple simulations to simulate the long time frame. Although the atmospheric models, and some of the microbiological ones that focus on evolution, try to simulate time in a different way, none of them had this necessity to produce the multiple simulation results, such that you could run over a range of input variables.

I mean, the question they're answering is quite different. If you're wanting to predict climate over some time frame, you're looking at probably weeks or months at best, not thousands of years or a million years. So I think that time frame also puts some other constraints or some other challenges in the way you build your model and the things you can do.

There are a lot of stochastics in the atmospheric modeling, but it's for different purposes—to take into account ranges of variable conditions and things like that. I would bet they have the complexity that we have in our TSPA model, but just with a different purpose and objective, because of the long time frame as well as the point that Michael was making.

RM: So at Yucca Mountain you're dealing with an extremely long time frame in human terms, but it's almost a microview—this little spot of the universe here. Whereas, with atmospheric science, you're dealing with the whole earth system, which is a bigger

view. I don't know if I can put genetics in that paradigm; I don't know whether it's a small view. It certainly doesn't have much time depth, unless you want to talk about the evolution of these genetic traits.

JY: Well, yes, except when they try to do the gene matching and mapping that they do to look for a similarity among and between species, and that kind of thing. There's not a lot of stochastics in that, to my knowledge. There is if you want to start tying it, like I did in my background, to the environmental changes. Then you have to put in some stochastic processes. But for the most part I think it's probably more mechanistic or more deterministic. The random events in that world are the mutations, and mostly they're random.

RM: And I think the genes interact in a random way.

JY: It's possible.

CHAPTER TEN

RM: What other countries were developing geological repositories at this time? Or did they have other thoughts about how to dispose of it?

JY: Michael knows those programs better than I do.

MV: The simplest way to say this is, literally without exception, any country that has nuclear power has a repository program.

JY: Of some sort. For some, it's just a test program, though.

MV: Very few countries are as far along as the United States is. Sweden certainly is; as is Finland making its way. France is coming along. Great Britain had a great start, then backed off; they're coming back again.

But the important point is that even the countries that reprocess have a repository program. So don't buy the argument that you don't need a repository if you're reprocessing.

One of the things I did with the DOE lawyers when we were working and commenting on the Environmental Protection Agency standard was to try to show the EPA how horribly restrictive and stringent their rules are compared to what other people are trying to do. Now, the goal is the same. The goals are to protect people. This goes all the way back to the 1990 National Academy report, "Rethinking High Level Waste," where the National Academy said that the regulations in the US are too prescriptive, that you have to find a different way to do this. You cannot, ahead of time, prescribe everything that's going to happen. You have to be flexible. You have to adapt.

And the one that hits us the hardest is the one about 10^{-8} —one part in 10,000 over 10,000 years. Nobody else has that level. What that means is the United States is

considering things, as Jean and I have both mentioned, on the level of a comet striking the earth and wiping out all life. That does not strike me as the level of risk that a human should be considering in making decisions that affect the immediate future. Look at other countries that do one in a million—we would not be having the discussions about volcanism and earthquakes that we're having at Yucca Mountain if we had those types of probability cutoffs. It's only because the EPA has made such a restrictive standard that we have to consider things about which most countries would say, "That's pretty bizarre, that you're trying to consider things happening with that probability."

RM: Did the EPA err in this, in your opinions?

MV: Well, I'll be happy to answer that. Yes.

RM: Was it the fear element?

MV: Yes, we talked about this. And I'll let Jean hear what I had to say to you and let her react to that. It goes back to the original EPA standard, where we talked about how it was promulgated. The EPA's own science advisory board said it was at least an order of magnitude too restrictive. That's the one where it equates to one additional cancer death in the world's population every ten years. You cannot measure that, yet we're being held to that standard.

When the EPA was challenged at that point in time, they said, "This is not a technology standard, we're doing this because it's achievable." So they promulgated a standard that they thought could be met, not based on risks that should be imposed upon society or accepted by society, but that could be achieved.

In that world, nobody will ever back off on that type of rule, once it's been promulgated. It just keeps propagating through time, to the point now where we have orders of magnitude more risk associated with radon that is not regulated than we ever

would have with a repository. It does not make sense to me. So did the EPA err? Yes, in my opinion.

JY: I agree totally. We have just recently learned that the minerals management department within the DOI doesn't even use a risk-based approach to write their regulations and implement them for deep water or shallow water exploration or for anything.

RM: Michael drew a book off of his shelf: *The Control of Nature* by John McPhee.

JY: He got that because we were talking about how there are a few cases where man has tried to engineer against disruptive processes like volcanoes.

MV: This particular one is Vestmannaeyjar in Iceland, where a volcano was erupting and the town was about to be overcome by lava. They just started bringing pumps in from all over the world, and they just pumped water back up there and stopped it.

JY: Froze the lava on-site. It's been done a few other places, I think, besides Iceland, too.

Back to the other point. I think my comment on what Michael said is that we don't have any sort of consistent risk-based approach across the various agencies of government that regulate hazards of all sorts, whether exposure to blowouts in the Gulf of Mexico, or the spoils that come when you are mining earth materials, or radiological hazards related to operation of a facility, or release from any other kind of chemical facility, for that matter.

That there's no consistency is just appalling. It means that we're not spending our dollars in an intelligent manner whatsoever. We're paying way too much in one area to avoid some kind of hazard with such a low probability that you can't measure it, nor will anyone ever be able to measure it with any kind of technology we'll ever have, versus

another area where we're not regulating at a level that's even reasonable given the safety risk.

You start listening to some of the FDA findings that are quite amazing, and you will find that the probabilities associated with bad—sometimes quite bad—side effects of drugs, they accept are one in 100. Sometimes they'll accept five or ten bad outcomes in 100, if it's a really nasty problem that they are trying to treat. I don't believe there's any way that there's any consistent approach to how we accept risk in this country. And FDA is one good example.

MV: They've proven it over and over again. We can give you numerous publications where you look at true risk, compared to what the public will accept and compared to what the experts say is a real risk, and there's no correlation. It's almost random.

JY: But it's one of those things where it's been made extremely prominent because of the recent oil spill in the Gulf. The technology for doing risk-based decision-making has been around, in this country, for at least 30 years, maybe 40 years, and has been at a level where there are programs that you can put the information in and it will spit out a decision for you now. It's basically organized decision-making. We're just not using it in major parts of the government. It's really beyond me.

MV: Well, if the EPA were to do that and truly do a risk-based analysis of their regulations, they would find that they are over-regulating. And if they were then to try to move back into a level that is more appropriate for the risk that's imposed upon society, they would be criticized.

JY: Of course—any time you back off on a regulation.

MV: Any time the Bush administration ever tried to take a regulation into a more rational realm, look what happened.

RM: Can you think of an example, just quickly?

MV: I can't. Probably some of the carbon dioxide standards. They've gone full swing both directions.

JY: Yes, that's true.

MV: They tried to relax them in the previous administration. Now they're coming way too far in the other direction. But none of that, to my knowledge, is based on a true calculation of risk, nor what the public's emotional fears will allow them to accept, because that is zero. It's always zero. How much residual contamination do people want you to have after you've cleaned up a CERCLA site? The answer is zero. Well, you can never get there.

JY: Plus, you can't measure it anyway, so how would you know?

MV: The perfect example is no threshold for radiation exposure, since we don't know what the effects of radiation are at low levels, because they're based on pretty significant exposures from, for example, Hiroshima. They don't know how to extrapolate it downward and say that below some level there is no risk. Scientists are split on this. Realistically, we, as a species, have evolved in a radiation environment—sunlight. Take your pick—background radiation. This is going to sound callous, but because so few of us die of radiation-induced sicknesses, there has to be a threshold below which you ought not to be regulating. But because we don't know what that threshold is, we say it's zero. That's where a lot of this comes from. When you draw that line down through zero, you get some pretty silly numbers. But in the absence of an agreed-upon number for a threshold, we're going to go with zero.

RM: This is a little bit off the topic, but I remember reading several years ago that a little bit of radiation is good for you.

JY: That's the same topic. That's exactly what Michael is saying. You've evolved to an environment where you have a little bit of radiation. In fact, you have quite a bit of radiation.

MV: Take your pick; we live in an environment that, depending on how you want to define it, what the correlation factors are, is hundreds of millirems of background. And yet, the EPA standard for Yucca Mountain, where we ought to be able to accept a little bit of risk for this—15 millirems a year. That's a couple of dental X-rays. I guess that dental X-rays are even safer now. It's a couple of cross-country airplane flights. Nobody thinks twice about hopping on an airplane and flying back and forth across the country. And yet, we're trying to calculate those kind of releases for millions of years at Yucca Mountain.

RM: The next thing I'd like to move onto is the state's own science program, and Nye County's. Was this your domain?

JY: Somewhat, yes, and Michael's too. Michael knows a lot more about parts of that than I do. But the state had a hydrologist who did a fair bit of kind of independent work. She took the information that was gathered by Yucca Mountain scientists and then did her own analysis, developed a hydrologic model, did some specific groundwater modeling that then could be compared back, and tried to use it to show that some of what the project scientists were doing was nonconservative or even incorrect, perhaps an incorrect way of representing the hydrology. And she was someone who had a reasonable reputation. She was a scientist from the University of North Dakota or South Dakota or somewhere back there.

MV: I know she went back to Minnesota to work on a Ph.D. with Otto Strack.

RM: Do you remember her name?

MV: Yes. Linda Lehman.

JY: Linda Lehman, right. And she was quite a capable person. My view was that much of the state's sponsored work was really constrained by the politics of the situation. If they got results that either complemented, or were not very different from, what the Yucca Mountain program scientists were presenting, that was not a good outcome. The outcome that was desired by their managers (and our state politicians) was something that would at least bring questions or show potential flaws. As a result, I think the scientists that worked for the state tended to take fairly extreme approaches. A good example that we already talked about is the materials man who worked for the state, who was a professor from Minnesota as well, wasn't he? Staehle?

MV: He was a chemical engineer at Minnesota, but he was at Catholic University later in his career.

JY: At the time he was a consultant to the state of Nevada. He and some of his students did some original materials testing. This is at the stage of the program where we had evolved to the very robust type of materials—very high chromium nickel alloys that would survive, almost unmarked, in quite extreme conditions. Well, in his experiments, he took them to more extreme conditions than what you could even imagine if you put them into a scrubber for a coal-fired power plant. I mean, they were under very extreme conditions with high acidity. What he basically did was engineer the solution that could attack that material and then put it in it and said, "Oh my God, the coupons of alloy 22 are being corroded."

Unfortunately, as a citizen of Nevada, I would have liked to have seen the state project office, Bob Loux's office, sponsor some work that was critically looking, just like other independent reviewers did, in an honest, objective manner. My opinion is that, for

the most part, consultants for the State were forced to take extreme positions by the very fact that Bob Loux's office's job was to try to undermine the program. Therefore, as a result, very little of that work really contributed to our understanding of the scientific bases for Yucca Mountain. You know, it could have been helpful. It could have been helpful as another piece of objective information.

And there was some of that. I'm not going to say there wasn't any. I think some of the volcanic hazard work that they did, or the volcanic critiquing that they did, was probably helpful. They asked questions in a different way and forced us to think about things differently.

I believe Linda Lehman's work probably drove some of the unsaturated zone models to be better than they were. By some of her work, she got outside scientists to critically question. So, I think that Linda Lehman's work, as supported by the state of Nevada, was useful in pushing some of the scientists in directions we might not have gone otherwise, and in understanding the unsaturated zone processes better. I can't think of other scientists who worked there.

MV: I want to make an observation and then ask Jean a question. Listening to Jean, her perspectives always make me say, "Why didn't I see it that way?" The observation I want to make is, in particular with respect to the corrosion experiment, in order to get that corrosion to happen, they had to do it inside of a contained pressure vessel at relatively high pressure. If those particular characteristics could be achieved at Yucca Mountain, the rocks would have to be so tight that water couldn't get into the repository. Do you see what that means? The site would be so good that corrosion would not matter. You cannot have it both ways.

JY: That's very true.

MV: Now, this is what popped into my mind. Did the state's approach to science change when Carl Johnson left and Steve Frishman became more prominent?

RM: First, would you tell who those two people are and what their role was?

JY: Michael's talking about a change in the staffing in Bob Loux's office, with a fellow who had been in the salt repository program for the first ten years or so of his career at least, named Steve Frishman. He had played the same kind of role for the state of Texas. Did he work for the Texas Bureau of Economic Geology?

MV: Either that or the governor's office, which is probably close enough to the same thing.

JY: Yes. Anyway, for the state of Texas. When Texas had a site in the running, he was in the same kind of role. Well, then he was hired here in Nevada in Bob Loux's office. And when he came in, he tended to be someone who, just by his style, pushed you to those extreme positions and took extreme positions as a matter of course. That was just the way he went about his criticism. And my opinion of Carl Johnson, the person that had been in that role previously, is that he really wasn't as competent a person or as bright a person as Steve Frishman.

MV: I'd agree with that.

JY: As a result, I don't think that Carl Johnson's critical work, or the people who did critical work for him, were all that effective, mostly because he just wasn't a particularly creative person.

MV: I was just wondering if Linda Lehman's role in the state changed when Frishman came in. I'll say this bluntly and you can edit it out if you want. She's a very competent scientist, and she might not have been willing to work under Steve Frishman's conditions.

JY: I think she did continue, though, to work for a while, because I can remember both of them being at NWTRB meetings. I can remember Frishman being there, and me being aware that he seemed a little uncomfortable because her reports tended to be more objective. When she took a topic, she critically looked at it. When she brought it to a technical body like the Nuclear Waste Technical Review Board, she didn't spin it. She presented what she had found. Generally speaking, she wasn't often in an extreme world where those things just wouldn't ever happen anywhere, let alone at Yucca Mountain.

As I said before, the head of our peer review panel for materials was a student of Roger Staehle, the materials man, who ended up working for the state. And we knew people who knew him. They were all astonished that Roger Staehle would take some of the positions that he did, under the circumstances. When they invited him to give a presentation to the peer review panel we set up for Yucca Mountain, he became so hostile and argumentative toward his own students that it was really a very uncomfortable situation.

So it may be something like Michael said, that the constraints he found himself working under were probably making him very uncomfortable. I think his discomfort and his reactions may have been in part driven by the fact that he was being constrained by the state to do work that was really very extreme.

RM: So, under Frishman, you had more predetermined outcomes on the research, whereas before there were fewer?

JY: Yes. I think that's right.

RM: And when did that transition take place?

MV: '88. Right after the Waste Policy Act.

JY: Yes, it would have been when he no longer had a job in Texas.

RM: Do you think there was a connection there? Did Nevada's position then harden because of the new legislation?

JY: Well, it was after the "screw Nevada bill," so of course the politicians used that to make the public feel stronger about it.

MV: Not just the politicians. If you sat down with Steve Frishman and talked about this, he would tell you that the Nuclear Waste Policy Act was passed because it was a compromise, that no one state would have to take all the waste.

JY: He believed that.

MV: He believed that with all his heart.

JY: Yes. I guess, in terms of other work that the state has done, they did a fair bit of work or they had someone who had some expertise in transportation and socioeconomic impacts. So I think they may have contributed in those areas. Some of their work may have helped, I don't know.

MV: I wish I knew how to say this properly. Bob Halstead will give the state's position at any hearing that we ever have on transportation. But I've been told that when he talks one-on-one with our people, he's very complimentary of the work that's being done. That's second-hand information.

JY: Another person that they had in recent years was a quality assurance specialist. I may have mentioned her already.

MV: Susan Zimmerman.

JY: Yes, but not Zimmerman any more. It's Lynch. Susan Lynch. She was a quality assurance expert. She had that background, and she went to work for the state. She came in as an observer for the state when we were doing internal quality assurance audits. She was invited as an observer. I would say that she was an addition. She often asked very

good questions. She had a good critical eye.

I think that, when she was a member of a team doing the quality assurance review, she took a fairly balanced position and, I think, contributed. When she had to represent herself as a member of the state, of Bob Loux's office, at a meeting, she would take a much more extreme position, if she commented. But I think her behavior during the actual quality assurance reviews and audits was quite helpful.

RM: Can you think of a particularly egregious example of predetermined results, or somebody who was really known for that?

JY: To me, the most extreme is Roger Staehle, in terms of an empirical experiment that was clearly so far outside of any conditions that we would ever experience at Yucca Mountain. Steve Frishman, in other situations, took very extreme positions, whether in front of the Nuclear Waste Technical Review Board or in other public meetings. But, generally speaking, I would have to say that he was quite good. I don't know if he was trained in law, but he was quite good.

MV: Yes, I think he has a law degree.

JY: He would in some manner cast the issue in a legal framework and very often brought to bear some questions that perhaps people hadn't asked. Even though he tended to be extreme, I don't think his role as an antagonist was necessarily always unhelpful.

MV: There's a very good point that I'd like to add, to complement what Jean is saying. He never tried to hide anything from us. You might have thought that the state would keep all these little theories they had about how they would attack us during the license application to themselves. That's not Steve Frishman's style. Everything was always out in the open. He was always telling us, "This is where I'm going to come after you folks."

JY: He did, he did.

RM: Then that gives you a chance to get your ducks in order, right?

MV: I think Steve kept hoping that we would see the light someday. Jean, what about Szymanski and all the way through Jean Cline, Professor at UNLV? The state kept her work funded for quite a while.

JY: Yes, there's no question about the whole Szymanski thing, which we've talked about before: the question of whether you have a reasonable chance of some kind of a geyser erupting through Yucca Mountain and either causing major corrosion problems because of the nasty chemistry that it creates, or physically transporting the canisters into the atmosphere—whether that had any probability. And all of the science that went together related to that probably would have been terminated at least five years, maybe ten years, earlier, had it not been for the state continuing to push on that as a potentially destructive catastrophic failure scenario. And going through that screening process we talked about, where you screen out all of the conditions and processes that are so low in probability of occurrence that you don't need to consider them in your mathematical model. Many aspects related to that whole catastrophic type of geyser erupting through the facility were screened out. A few aspects of it having to do with the potential for thermal waters coming into the facility, accompanying a volcanic eruption in the area, were screened in and were treated in the mathematical model.

So there was work related to how you would model that, and honest, legitimate good technical work was done by some people, both paid for initially by the state of Nevada. Jean Cline's a chemistry professor at UNLV. She did that work first under the state of Nevada and then eventually, under direct funding from the Department of Energy, continued the work. Some of that chemistry work was very helpful. And Jean Cline and her students, I think, contributed to our understanding of some of the

conditions, environmental conditions, that could develop under future scenarios where there was volcanism in the area and there were thermal fluids that could possibly penetrate the facility.

MV: I think, just to close this, you might want to comment on Jean Cline's final report to the state and what the state's position was. Much of this is second-hand, but my understanding is Bob Loux called her and said, "Is this true, that this is not upwelling water?"

JY: Oh, you're right, he did. And that was when, in fact, her funding was shifted over to DOE, because the state cut her off. I hadn't thought about that in a long time. Her final report concluded that, with the kinds of deposits she was analyzing and the evidence that she brought to bear, there just wasn't any evidence in any period of recent history—hundreds of thousands of years—of those kinds of upwelling fluids coming through the area around Yucca Mountain. That was her conclusion. And that's when the state fired her and Department of Energy began to fund her.

MV: That's what science is all about. It's about answering the question.

RM: If you don't like the answer, too bad.

MV: Well, no, the state obviously didn't like it.

JY: That's right. You're absolutely right. I had forgotten that she got caught in that.

RM: What year was that, do you have any idea?

MV: In the site recommendation time frame—2002, 2003?

JY: It was finished by then.

RM: Is there any way we could go back and reconstruct that a little bit through documentation?

MV: You can find all that stuff on the LSN [licensing support network].

RM: Do you have any idea how much the state was getting for their little scientific efforts over the years, or is that too variable?

MV: I can get you the monies that were being given to the various oversight agencies. I don't know how Bob Loux was spending it. Certainly he came under a lot of criticism in audits for using that money for "lobbying." He certainly gave a lot of that to Judy Treichel, whose job really was to try to stir up the anti–Yucca Mountain perspective.

You originally asked this question of Jean, about money being spent on scientific oversight, and until Inyo County started spending money a couple of years ago on a drilling program, nobody was doing independent scientific research in any meaningful way except Nye County.

JY: Except for Linda Lehman, I think.

MV: I'm sorry, yes.

RM: If you were the gods who were going to redo the program—go back and start all over—should the state be given the kind of power that they had? Should they be given even scientific oversight and the assets to play the political role that they played?

JY: In a perfect world, I think they should. I just don't know how to do it in a way that it wouldn't turn out the way it did this time. But, yes, I honestly believe that with a major government project like this one, whether it's run by the government or run by a company for the government, having some level of state oversight is important.

RM: Were they given too much, particularly in terms of a veto?

JY: Well, the veto and the Nuclear Waste Policy Act were kind of, "Oh, never mind anyway," because all it took was, like, 51 percent, and Congress could override it.

MV: Took a supermajority.

JY: Did it take a supermajority?

RM: 60 percent?

JY: It did. Michael may have better insights on that. It's not something I really pondered.

MV: I'm going to answer it almost exactly the same way Jean did, but I'll emphasize it a little bit differently. If you were able to find a state who would enter into this with the integrity that you need if you're going to put somebody in charge of it, I'm all for giving the state not only oversight responsibilities, but giving them a true veto. If it's based on technical, and not political, reasons, I have no problem with that. But how do you do it in the real world? That's what you were hearing at the blue ribbon commission presentations a week or so ago. Nye County's presentation couldn't have been clearer on

that. You should give the state those authorities. You should give them meaningful responsibility. But my question is, how do you ensure that?

RM: The only thing I know about that blue ribbon thing they had was what Bruce Breslow, I think, said—that the state needs to be offered things, money or whatever. They were. Doesn't he know the history?

JY: Well, when we talked before, I think Michael said, and I agree, that we thought the state had been offered things, but are we really sure that it was that clear cut that those things were really on the table?

RM: They were. I know one of them was, because Chic Hecht told me, and he was there. And Troy Wade confirmed it, because he was there, and it was dead on arrival, a multi-billion dollar medical research facility on the Test Site that in a few years would have more Nobel Prize winners working there than anywhere in the country. That was given by Secretary Herrington to Chic Hecht with Troy Wade there.

JY: My question to Michael is, could Herrington have delivered?

MV: If you were to ask the state for their perspective they'd point to the Nuclear Waste Policy Act, which was something like $10 million a year, and that's not a reasonable amount for this. What the state will not acknowledge is, if you read the other part of the Nuclear Waste Policy Act, it says that the state is required to enter into a consultation/cooperation agreement. And, from any perspective, that's where the negotiations begin.

RM: Talking about offers, Steve Bradhurst, who was Nye County's Yucca Mountain man, was talking to the head of NEI, or something high up. And he asked Steve, "What would Nevada take to take the repository?"

Steve said, "How about the Super Collider?"

And he said, “Okay, what else do you want?”

Steve said, “How about the super train to L.A.?”

The guy said, “Okay, what else do you want?”

And Steve said, “I have to get back to you.”

JY: I think those kinds of discussions very well may have happened. I’m not questioning that. But whether they were at the level of the right people talking, and whether those people could really deliver is the question.

RM: Here’s my take on that: in the real world, in an emotionally and politically charged environment like this, this is how the offers begin.

JY: Well, I think that’s right.

RM: You’re not going to get a bill introduced in Congress saying, “We’re going to put the Super Collider in Nevada if they take the repository.” That isn’t going to work.

JY: The feelers go out, and then the discussions would occur at the right places.

RM: That’s right. It’s like Chic told me. He said he went to Bob Maxim at UNLV, because it was going to be affiliated with UNLV. And he said, “It was dead on arrival.” Maxim told him, “If I signed onto your proposal right now, I would be out of a job tomorrow.”

JY: I believe that’s probably true. I believe, as I said, from Miller and before Miller on, the governors as well as the state senators have taken a stand for reasons that I purely don’t understand, other than they need an issue.

RM: I think Chic was right. The fear issue is it.

JY: I don’t know that any one of them could answer, if asked specifically, “What is it about the health and safety and the quality of the environment that you believe is going to be seriously impacted by this?” I’m not sure they had that level of understanding of what

the potential risks were. It's a cause that's easy to get the public reaction, therefore easy to get the public to pay attention to you.

RM: You've stated it beautifully. Let's move onto Nye County's science program. I don't know how much science they were doing when Steve Bradhurst was in the office.

JY: This is one where Michael really has to be source because the only work I really know very much about what was called their early warning drilling program. That was a series of bore holes that they got permission to construct. And I don't know how many were ever drilled—at least six or eight.

MV: They've drilled more than 40 bore holes out there.

JY: Have they really? Okay.

RM: Deep ones?

MV: None of them are very, very deep. They're into the aquifer down around Highway 95.

JY: When you get south of Yucca Mountain, the depth of the water table is a lot shallower. So, where they proposed to put their drill holes was in the areas where the aquifer is only a few hundred feet, in most cases.

RM: Oh, that big wash there, just out of Lathrop Wells.

JY: As you go south, south from there. Although it had, I think, public appeal, the potential for it to really have a payoff, of course, would be a long, long time in the future, if the mathematical models are correct, in that you would never have any possible contamination that would show up in those wells for probably a million years.

Nevertheless, the idea of having an early warning system had a nice ring to it. It was okay from the standpoint of contributing to the hydrologic understanding, because they were able to put some boreholes in places we didn't have any. Therefore, we got

better data to pin down positions of the water table. So our models were better as a result of their work, no question about it. DOE was very happy to have them going forward with their drilling program, because at that point in time we were not doing much fieldwork anymore.

MV: Let me try to put a shell around that. Jean's exactly right. The early warning drilling program was, in fact, exactly as Jean described it. It was a way for the Nye County people to do some scientific research that looked like it was really applicable to what the county needed.

But in 1992, when the National Academy of Sciences told the EPA to change the way that the Yucca Mountain site was regulated, all of our site characterization work up to that time had been predicated on a particular type of regulation. So we were not focused on water movement within the water table. We were focused on the unsaturated zone flow and compliance points very close to the repository. And what really benefited us from the Nye County program was, when the Waste Policy Act was changed, the DOE wanted to get the license application put together. Nye County had as much, if not more, useful saturated zone geological data than the DOE did. So it was very instrumental.

JY: Down gradient.

MV: Down gradient, and the DOE needed the information. Now, something else that's kind of neat here is that, because of the way the state dealt with the DOE in terms of permits for drilling, it was much easier for Nye County to get drilling permits. So there were drilling programs where DOE said, "Nye County, would you do this characterization program, because it will be done cheaper, it will meet our quality assurance requirements, and you'll get the permits much faster than we could ever get them."

JY: There was a very good cooperation developed with them.

MV: It was, and I want to add to what Jean said. There were a couple of other things. They had some transportation and environmental specialists that did very good jobs in participating in the DOE's environmental impact statements. In fact, Nye County achieved the role of cooperating agency, in these EISes, and did, I thought, a credible job of making the EISes better with their comments, particularly as they dealt with Nye County–specific issues.

And the one other piece—they had not just the hydrology in the flow system. They had a lot of structural geology work being done by graduate students around the country in that area. They understood probably more about the tectonics of what's known as a gravity fault and some of the way water moves in the Highway 95 area down there than the DOE was ever able to put into their models. Some of the most critical comments Nye County has to make are, in fact, about how they have geotechnical data showing what these faults are doing down there that is not well represented in DOE's model.

Now, the comments that Nye County made to the NRC to achieve intervener status for the licensing hearing were made from the perspective not of, "You should disqualify the site because you've got the data wrong," but of, "We know how to help you make this better with these programs." And that's how they wanted to do it.

JY: Makes perfect sense.

MV: And the final one I want to mention is a lady—her name is Lubna Hamdan. She was a student at the University of Texas at El Paso, one of John Walton's students. They had a program for quite a few years. One of the things we never did was look at how the water moved within the waste package. It was something that we could bound relatively simply, and we could live with the consequence of not understanding it. Well, she just

finished her Ph.D. thesis, and she had done some modeling of what could actually happen inside a waste package. It proved beyond a shadow of a doubt that we are way too conservative.

JY: Well, it's very good, very interesting.

MV: The Nye County science is just impeccable. And it's done to a quality assurance program that meets the NRC requirements. I can't say enough good about it. They spent the money the way Congress intended that oversight money to be spent.

JY: So it's a model for how it should be done.

RM: So if somebody wanted to reconstruct this kind of thing again, they should look at the Nye County effort.

MV: I'd like to mention Nick Stellavato, particularly. He left our program and went over and set up the technical program in Nye County.

JY: That's right. He did a good job.

RM: Is there a source that we could mention here for somebody in the future to go to and learn about what Nye County did?

MV: Nye County has a website where their publications are available. And to my knowledge, they intend to keep them there. Their work, because it was funded by DOE, is all in the licensing support network. I guess those two ways are probably the best way to try and find it.

RM: Can we say the same thing for the state, that their effort is publicly available to examine?

MV: I did not follow this as closely as I probably should have after I left, but to be a participant in the licensing hearing, one of the commitments you have to make and milestones you have to meet is to have your documents in the licensing support network.

My recollection is the state did not put many documents in the LSN. There certainly are legitimate arguments about where the state's technical bases for these contentions that they were putting together were.

Remember, I told you the Nuclear Regulatory Commission's staff position was that very few of the state's contentions should be admitted. And then the board judges came back and said, "Well, that's not consistent with the intent of the Waste Policy Act" and admitted them all. But, unless something dramatically changed, I do not believe you will find the technical backup for all the state's contentions in the licensing support network.

RM: Is there anything else we should cover?

MV: Jean, what would you like people to remember? What have we not talked about from the program that you had some involvement in? The reason I asked you to talk with Bob was you were so instrumental in so many of the legislatively required documents and technical reviews.

JY: I think we've covered the major ones thoroughly, and we covered the total system performance assessment. I think we've covered it pretty well.

MV: I think I beat the viability assessment to death.

JY: Well, you were much closer to the receiving end of that one.

MV: And, man, we sure covered the site recommendation documents. When I was off doing site recommendation documents, Jean was back here working on trying to get the TSPA peer reviews so the license application would go out.

RM: How do you see your years in the program, in retrospect?

JY: Oh, I couldn't have asked for a better job, a better role to play. With my background in computer modeling and broad background in earth sciences, I can't

imagine any kind of a job where I would have been able to apply that kind of training and expertise more broadly and had better career opportunities. It was excellent.

RM: When you see where the program is now, politically, what goes through your mind?

JY: I still have the impression, which I think you agree with, Bob, that the current politics will be turned in the years to come. Under almost any scenario, meaning even if we go to full-scale reprocessing and we develop technologies that allow us to do a lot of additional burn-up and we do all of the things that are on the table, we will still need some small volume of waste disposed of somewhere in a place where it's safely contained and isolated for long periods of time, because it will be very nasty—hazardous, both radiologically and chemically.

And I think, unless you're going to put it in a guarded facility in some kind of containers of the same kind of basic material that we proposed to use underground, I can't imagine a place that would be any safer than Yucca Mountain. So, under any of the scenarios that are going to go through the blue ribbon committee's evaluations, and future blue ribbon committees, I still imagine something like a Yucca Mountain facility being necessary. The isolated nature of it, the dry nature of it, the location—it meets all the right criteria. Even if it's going to be a permanently guarded facility, it still could be made to be a very safe facility.

MV: I said something to you, Bob, that I'd like Jean's reaction to. You led into it by commenting that you believe the Yucca Mountain site is a good site. What I'd like your perspective on is the fact that, from my perspective, it's very easy to say that a salt repository could be better that Yucca Mountain, or it's very easy to say a granite repository would be better than Yucca Mountain. And my reaction to that is that's only

because they've never been studied at the level that Yucca Mountain has been studied.

JY: Well, I agree with that completely. Yes, I think people can make rather grandiose arguments as to why a granite material as a repository, or a salt material as a repository, would be a good alternative to Yucca Mountain. But if you look at it from the standpoint of long-term behavior of that material, if you look at it from the standpoint of relative absence of water— under any approach you ever use, whatever you place value on, it will always be high value to minimize the amount of water contacting the material—in either case, you would end up having to engineer the facility in such a manner, I believe, that you might as well leave it on the surface. Whereas, at Yucca Mountain, the advantage you gain by putting it underground is that it really has some good properties—the dry rock, together with the fact that it's in a location that would be very difficult for any type of intrusive behavior to occur, whether inadvertently or on purpose. The granites and the salt sites tend to be in places where you just don't have those kinds of advantages.

MV: One of the things that I hope I mentioned, and maybe I can get you to talk about, was that people always believed that one of Yucca Mountain's weaknesses was we can't put a true diffusive barrier around the waste packages, and you easily could do this in granite, for example.

RM: What do you mean by a barrier, in this case?

MV: Something like a clay barrier where the permeability is so low that the materials would not travel advectively.

RM: Why couldn't you do that in Yucca Mountain?

MV: Because the wastes are too hot. Diffusive barriers are really probably what you would like to have in any repository system, and they typically are clays. The clays, if they're tightly packed around the waste packages, are so impermeable that the water

cannot move through them with any reasonable speed. In fact, the likely way that the radionuclides travel is not by being carried along with the water as much as by diffusing through the water within the pore systems. Those are extremely low travel times. If those systems work, there's virtually no way radionuclides are getting out.

One of the reasons that people talk about granite repositories performing so well is because the models that they've done show that if you have a diffusive barrier working properly the radionuclides are not going to move. There are two reasons you can't do the type of barrier many are looking at in Yucca Mountain.

One of them has to do with this country's commitment to disposal. Historically, we have not been interested in long-term disposal. In order for a diffusive barrier to be made of clay and to be able to work, the wastes have to be relatively cool, because if you raise the temperatures of the clays above boiling, they no longer are clays. They change into something else. And when the temperatures drop back down again, they may not change back into the same clay that you thought you had. Clays are a silicate material and have a lot of water hydration associated with them. With heating, the water is driven off, and the clay material turns it into something else.

So it's the heat, and the fact that the US does not want to store this material on the surface for hundreds of years, that they want to dispose of it, which is the first problem.

The second problem is we're at Yucca Mountain because there's little water in the rocks. You're not going to keep clay moist enough to be retain its behavior as a clay, so we've thought about other types of diffusive barriers, and there is a particular one where we did some studies where we thought we actually were on track to have a diffusive-type barrier, or one that actually kept the water from contacting the waste packages directly. But we did not take the time to fully develop that design because DOE wanted to get the

license application out.

JY: Yes, the nice thing about Yucca Mountain is that it's simple to engineer a diffusive barrier, because whether you put it above or below or around, the diffusive barrier in an unsaturated zone is simply some kind of material that has a sizable porosity. Because the water travels through that material so slowly, you measure it in tens of thousands or hundreds of thousands of years per meter. So a few meters of some kind of a rubble material—small gravel or gravel-size material—is all you would need. I mean, talk about simple; this is not hard to do at all. If you added that to your mathematical model, it would add literally hundreds of thousands of years to the travel times for radionuclides. So, I'd say, as Michael will say, it's almost a no-brainer. I mean, it's such an easy thing to do. In retrospect, there was a guy who used to be one of the major contributors to the program, and who's now the head of a national laboratory, who basically proposed, "Why don't we just basically strip off Yucca Mountain and engineer it with these layers." I mean, it wouldn't cost you any more in the long run.

MV: Especially today.

JY: Especially today. Now, if you asked, "What do you think about that idea that you had 25 years ago?" It literally would be a simple engineering solution that would buy you very long travel times.

MV: This is where Steve Frishman would come back and say to you that you didn't care that you'd engineered a perfect solution. His interpretation of the Waste Policy Act is the geology had to do it.

JY: That's right. But if this is just the rubble we took out of the excavations it is the geology. You just put the rubble back in.

MV: Bob, we're rambling here, but I want you to understand this could do away with

the drip shields, which was one of the states' biggest criticisms.

JY: Oh, yes. It could buy you so much.

MV: I've always believed that, once we got the license application docketed, a rational applicant would say, "Okay, now, I've got a couple of things that we never did get a chance to really flesh out. I'm going to work on them now for a while." DOE's model would be, "Well, I got it done without that, so why would I spend any money on that?"

JY: "Why would I have to?" That's exactly what they would say.

MV: Whereas a real commercial venture like we were talking about would say, "Crap, I can save billions of dollars by spending a few millions or tens of millions now. I'm going do that."

JY: It would not be an expensive solution, not compared to drip shields.

MV: And the way the licensing processes work, you could always come back and say, "I have got a better solution than you've already licensed. Let's reopen our hearings and talk about it." You can't change things without talking to the NRC, but you can always go back and say, "I can improve my performance this way. Let's talk about it."

RM: I have one more question. It's something I've wondered about over the years. Quite a few years ago, I struck up an acquaintance with Tony Hechanova in his office over there. And I was always fascinated by the things that he would tell me. From following Yucca Mountain, as an interested layman, I never understood why the kinds of things he was doing—transmutation and reprocessing and all that—never got into the dialog on Yucca Mountain. It was always like it had been in '82 or '87—we're going to keep it there a million years. My view is that there's no way in hell that nuclear waste is going to stay in a repository for a million years. There's too much energy in it.

JY: I think the main issue is that the way the Nuclear Waste Policy Act was written,

the money that's in the Nuclear Waste Fund can't be spent on those issues.

RM: They couldn't have done it legally.

JY: Right, no.

RM: But intellectually, it didn't seem to have gotten in either. You hardly ever heard it mentioned.

MV: I don't know. I'm going to show you something. It got as far as a draft. That's the draft EIS that would have taken care of it. That was done under the Bush administration.

RM: Could you talk briefly about this document?

MV: What I pulled out was the programmatic EIS for what's called GNEP, the Global Nuclear Energy Partnership document. This is what the Department of Energy was trying to do to bring reprocessing forward, in parallel with the repository program. Now, this document says you do need a repository, but they believe they can reduce the volumes and the amounts of wastes. They have numerous different approaches in here, using different reactor technologies to reprocess and consume the wastes. And there always will be a waste stream. As I said, this was ready to go in the Bush administration. And the Obama administration did not take it forward. They cancelled this program.

RM: So if a Republican had been elected, this might have gone forward?

MV: Well, you have to be careful with this. The National Academy had been critical of this work, because it has a little pie in the sky flavor. It's 20 to 30 years from now before we ever get to the point. It's very easy to look at this document and look at one of the technologies and say, "That's exactly what I want." The problem is that's not the technology we could implement today. And there's a lot of research, a lot of development, that has to go on to get to the real technology we want.

And the reason I pulled this out is very simple. It's because it's a direct answer to

what you were talking to Jean about. This document has a reference that says, essentially, the way this is laid out—the assumptions that are made for how this is going to be paid for, how the electricity is going to be generated, how many new reactors are going to be built in this country—we will never go back and get the legacy wastes. And the reason simply is, in this model we will be building reactors and reprocessing plants in parallel. And we will never have excess reprocessing capability to go back and get the legacy wastes.

RM: Okay, so that answers the question.

MV: So, we may never take that. Now, the answer is simple: you spend more money.

JY: Oh, sure, parallel programs. So the answer is it could be done.

MV: It could be done. It's just that, with the premise for this document, which is what Tony was working on, reprocessing, we would have never gone back to get the legacy wastes.

JY: The answer is there are policy decisions as to why we went the direction we went.

RM: Why isn't there any R&D going on with this somewhere?

JY: There is.

MV: There is. It's just called something else. It's no longer called GNEP. It's called used fuel disposition.

JY: And, actually, the Obama administration has put a fair bit of money into the R&D.

MV: Nothing like what this program would have spent.

JY: Yes, but it's better than it's been in the last ten years.

MV: Yes. And, in fact, if you want to be really fair and honest, given the National Academy criticism, that a few Nobel prizes having to be earned before we can do this,

it’s probably the right thing to do.

RM: Okay. So we have covered a lot of ground.

MV: I feel like I’ve monopolized parts of your interview, Jean.

JY: No. There are a lot of things I have not been active in or thought about recently. So it was very helpful to have you poking and prodding.

RM: I’ve really enjoyed it, and I’ve learned a lot. I appreciate it, both for this project and just as a citizen. I thank you, Jean, and Michael, too, for what has been a high-level discussion.

INDEX