An Interview with

NED ELKINS

An Oral History produced by
Robert D. McCracken

Yucca Mountain Series

Nye County Town History Project
Nye County, Nevada

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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

This is Michael Voegele, talking to Ned Elkins in his office and later in his home in Carlsbad, New Mexico, June 8, 2001.

CHAPTER ONE

MV: Ned, please give us a little bit of information about yourself: who you are, where you were born. Who is Ned Elkins?

NE: I'm a native of Carlsbad. I was born and raised here to a potash mining family. My grandfather and father were miners; I grew up in a mining environment. I always had a passion for mining. That's different than geology—I was not fascinated with geology as much as I was fascinated with the extraction sciences. I had a very active mind and always knew that I wanted to push myself academically and professionally, so college is where I was going.

I had no real plan. I started out in pre-med, as a lot of people did, and I said, "I want to be a doctor"—that just seemed like the thing to do. I went through a biology/chemistry background, got accepted to Duke University for medical school. It was such a radical change—I'm a southwestern rat—and I was maturing, I had a couple kids by that time, and I changed my mind. I said, "I don't want to be a doctor."

The question became, "What in the hell do you do with a pre-med biology/chemistry background?" I went into graduate school. I always loved to be in the field, outside, and because I'm an active person, I went into terrestrial field ecology with my biology/chemistry background. I worked there, got into the biochemistry of soil interactions, nitrogen fixation and dynamics, and messed with that for a while.

Then I decided that the biologists and chemists didn't really understand the edaphic environment that they were trying to work in because they did not work in an

engineering construct. I developed a passion for the engineering side of things, the systems approach to environmental or ecological work. After finishing a Ph.D. program in biochemistry, I decided I was going to do a doctoral program in civil engineering.

MV: Let's stop there for a second and back up and talk about high school—you went to high school here in Carlsbad?

NE: Yes, I lived here in Carlsbad from the day I was born until the day I left for college. I had a fascination for all things science—that was my driving passion. I had a great academic career here at that time—I think I was third or something in my class (I wasn't valedictorian). In my own arrogant way, I thought there had never been a finer science student to come out of this program. I never studied, never took a book home. In other words, I never applied myself, but felt I had limitless potential.

And yet, as I said, I also loved mining, and mining was sitting there staring at me—I could have jumped in and become a miner here and probably become a senior manager for a local mining company without a college degree.

MV: I'm sure there are people who will read this who don't know much about Carlsbad. Tell us a little bit about what Carlsbad was like when you were growing up.

NE: Carlsbad was primarily a mining town in the '60s and even early '70s. Potash was the thing that gave Carlsbad its prime identity. There was oil and gas in the region so there was an oil and gas background. And certainly agriculture has always been a big part of this rural community. But potash was king, mining was king during those years—it was a dynamic town. It was, in my early youth, the third-largest population center in the state of New Mexico.

The mines began to have some turndowns starting in the mid-'60s. Carlsbad kind of slipped a cog a little bit and settled into a stable state, in a way—mining was still

important, but not what it was. In my early years, this was almost like a Pennsylvania coal town—it had that feel.

MV: But it also has an agricultural aspect.

NE: Huge agriculture. Agriculture was what formed the community and it's what drove it until the '30s, when potash was discovered. By the '40s, potash was taking off and it reached its apex in about the early '60s. There were seven large potash companies here at that time and most people in Carlsbad either directly worked for or indirectly were supportive of mining. The agriculture was still here; it was always that backbone that was sleepy, and you didn't hear a lot about it. Potash was the economic driver, but agriculture was then, and still is today, a big part of Carlsbad.

MV: You were talking about an interest in science when you were going to school. What else did you do when you were young?

NE: I was an athlete and passionate about athletics in every sport. I lettered in, I think, five different sports in my years here at Carlsbad. I think there was already a pattern being established in my life that I wasn't going to be overly committed to anything—I was going to do a little bit of everything, kind of the old jack-of-all trades and master of absolutely nothing.

I was a passionate athlete and an outdoors kid. I mean, I lived outdoors. This is a small community in the desert, but we're on the fringe—we have mountains close by, so one week you were hiking in the mountains or off messing around; another week, you were out hunting or doing something in the desert. It was a combination of hunting and fishing and sports. I was just an aggressive kid. I was hyper as could be—couldn't rest, couldn't stop, couldn't ever be still.

The two things that were kind of the quiet drivers in my life were, one, a burning

desire in the sciences; I really wanted to pursue that, and second, a passion for mining—it was just the thing that I knew. I put myself all the way through school mining. I had developed a talent early on for blasting—I was a dynamiter.

In that, just like in everything, I was driven to be the very best there was and I became an extremely efficient blaster, especially what were novel blasts—not just a normal face shot, but if you had a bad shot, going in and repairing that shot, trimming—almost the art of blasting. I was good enough at it to where, when I would come home for a break for more than one week, someone was going to hire me to blast, even for only a week or two. For a kid of that age, it was better money than you could dream to make anywhere else. I was able to completely put myself through school with summers and Christmas breaks and spring breaks—whenever I could, I'd come home and shoot for a while.

MV: That's great. Tell us about your family—your parents, and your brothers and sisters.

NE: My parents were both native Texans, products of the Depression in a very depressed rural community in central Texas. They were the sixth generations on both sides of their family that had lived in the same community since long before the Civil War so there were deep roots in my family in central west Texas.

My dad was a doodle-bugger initially—an exploration guy doing seismic work for oil and gas. That took him around regionally and he passed through southeastern New Mexico and loved it. Out of the Second World War, he had taken a job as a copper miner in Bisbee, Arizona, so he had a mining background.

When he came through here, one of the potash companies, Duval, which was a Pennzoil company, was just getting ready to sink shafts. Because my dad had a lot of

construction background and the doodle-bugging experience as well as some mining background, he was hired by an Australian company. I believe it was Broken Hill that had the contracts for the shaft-sinking for Duval here, and my dad hired on with that entity to sink those shafts.

As Duval began to form its management and initial workforce here, my dad just kept hammering them for a job. In 1951, when Duval opened, they hired him and he spent 36 years with the same company and ended up as mine foreman. He had no college background but was a proud worker, good at what he did, and that was recognized in an environment where usually you couldn't be a mine foreman or manager without a mining or geologic background in an academic sense. His experience, knowledge, and drive got him to a level that generally couldn't be achieved without formal education. But he loved education.

My mother has a Ph.D. in education. She was an educator here in Carlsbad for 35 years, teaching in schools. So both my mom and dad had a love of learning, my mother very formally. I've never known a man that just wanted to know things more than my father. I think over the years he took about 150 classes at the little branch college of New Mexico State here—he probably has more college credits that I do. None of it could have been cobbled together for an associate's degree, but he didn't care. It impressed me as a kid that a man working shift work in those mines would find the time to go take a night class; he had just a hunger for knowledge. So it was a natural environment—our dinner table in the evening was three encyclopedias and two books and questions being asked.

I have an older brother, Chris, who probably thought the rest of us were crazy. He had kind of a different bent, but he had that same environment. He went into the ministry—he became a Baptist minister and has had a very different life.

But we all shared this desire to learn, and it was probably the common trait in the family. My mother and father are both still alive. They've got a retirement home in the mountains three hours from Carlsbad, up in the Sacramento Mountains, and they're still the same today.

MV: What are their names?

NE: My mother's name is Kay—Norma Kay, but she was called Kay. Her maiden name was Anderson. She was of the Anderson clan in this central Texas community I was talking about. The Elkinses were farmers and kind of handymen, jack of all trades, but the family always had a family farm.

Finally in the '40s they sold out, and the farming history of our families, which had been generation after generation in the land, kind of transitioned over to more commercial or business-related things. My dad, as I said, got into mining; my granddad had done a lot of mining but he was always a farmer. Farming was the thing that both the Andersons and the Elkinses did but they also were into building and construction. My great-great-grandfather, who was a Texas Ranger and a captain in the Civil War, settled in Texas. Every generation from my great-great-great-grandfather came into that country—I was the first Elkins not born in Coleman County, Texas.

MV: What was your father's name?

NE: Merle.

MV: Anything outstanding you want to tell us about your high school escapades?

NE: [Laughs] I was in trouble all the time because of the type of kid I was. I never took anything seriously. Most of the science teachers, even at the high school level, believed that you really needed to pay homage to what they were teaching and I would never study. I would come in, look like a bum from hell, glassy-eyed, sit in the back,

sleep through a class, get thrown out, but make an A on every test they gave me—it just drove them crazy.

When I moved back after being gone 25 years, it was amazing that that tended to be the legacy. Most people who knew me said, "We never figured you would amount to a damn thing. You were right on the edge of being a juvenile delinquent, in trouble constantly." We were a hell-raising bunch of kids. I was the oddball in the group that I ran with because most of them lived up to all the [laughs] expectations that were accepted for us ~~most~~. I knew, even when I was doing it, that it was a phase. I was going to enjoy it, but in the back of my mind I knew I'd settle down.

The gift that the good Lord gave me was my wife. She's the one who said, "Enough of this crap." She had a dream for me, she knew a potential was there, but she knew it would never be realized without a driving motive. And 40 years later, she's still pushing.

MV: Where did you meet Cindy?

NE: Cindy wasn't born in Carlsbad, but her family came for the same reason my family was here—her stepfather got a job in mining in the early '50s. Cindy moved here probably as a two-year-old child. I knew her all my life—we were high school sweethearts; we started dating in sophomore year. We had, as kids usually do, rocky years—we were apart and together and drifted in those years trying to find a way. But I think deep-down inside, I always knew Cindy was my soul mate. We started dating in 1971 and married in 1975, so that's 40 years that we've been together.

Cindy will tell you what I don't have is a very, very healthy dose of just common sense, a logical approach to anything: Don't make it too complicated, let's get it simple, and keep your nose to a grindstone. As I said, she had a plan for us, she had a plan for

me, and she made that work.

We went to college together. We originally went different ways in college and then came back together, and we got married. I worked in the mines for a little while until I got the money, and then we started down at Las Cruces. That was the beginning of our lives together, at New Mexico State University.

MV: Is that where you started college?

NE: I started college at Baylor University and I spent some time at Hardin-Simmons University in the Abilene area, because that's where my family was—it was kind of nice there. I actually got thrown out of school. I mean, I can't overemphasize that I was a little wild. A group of guys and I ran a bookmaking operation for professional sports out of a dormitory, thinking it was a hoot from hell—it was just great sport. When we got caught, the state of Texas informed us it was a felony [laughs], and we were given some interesting choices, and in my case it was renunciation of a scholarship and to leave the state forever, basically.

MV: Was that an academic scholarship?

NE: A combination academic and athletic.

MV: I wanted to get your athletic career on the record, too.

NE: Football was the one that I was kind of built for and basketball was probably my biggest passion, but I played every sport, including state championship finals in tennis, believe it or not. People look at me and say, "How in the hell could you have played tennis?" But I was quick when I was young [laughs], believe it or not. I just loved those sports, so it was a combination.

I saw no career in athletics so academics was always a part of what I was doing, but I put a lot of effort into the athletic side. And when I had gotten into trouble, it ended

both the athletic and, for the time being, academic pursuits. I came home and went to work in the potash mines for about a year.

The branch university here had night courses, and when I was on day shifts, I could handle the night classes, and started taking some classes. That's when my wife was formulating a plan for us.

MV: Was she at Las Cruces?

NE: No, she had started at Texas Women's University in Denton, Texas—north Dallas—and had come back home and got a job because we didn't know what we were going to do. We got married and when we went to Las Cruces, it was her first time on that campus, my first time, and we took off from there; that was in 1976. That's where we got started, and I've gone through my academic evolution, which was bizarre. Maybe most bizarre is that I don't think I've ever done a job that I had any academic preparation for.

MV: I wanted to touch on the emphasis you gave us about your mining history and your mining background. It didn't sound like mining was your first choice in college.

NE: No, I didn't even think about it. Mike, I'm sure you're the same way—it's hard to go back and remember, what were you thinking as an 18-year-old? I look back and say I guess I never really thought of an academically based mining career. Mining was just a way to make money; it was a job. I don't think it was ever what I thought about as a career endeavor at that time. To me, mining was something you could do with an eighth grade education, if you had the aptitude.

So I went off, as I said, in biology, chemistry, and civil engineering with an emphasis on soil systems. That was where I wanted to go—hydraulics, fluid mechanics, soils. At the end of my academic period, I was post-doc'ing—I worked with what was

called the long-term ecological research, LTER, program at New Mexico State on the experimental range. It's a land grant university and has huge amounts of land. We had a multi-thousand-acre experimental research deal, and I ran that—that was my post-graduate work.

When I went through school, especially engineering, the one peeve I had was that most of the people who taught me had never worked in their lives. They were classic academicians—they went through, got a degree, got an advanced degree, post-doc'ed, and taught. I thought it was hypocritical—how can you teach me, as a young person, what's out there, when you've never been out there, you've stayed in this cocoon? So I knew one thing, that I did not want to take a professorship.

And lo and behold, I hung around too long and they asked me to join the faculty at New Mexico State in civil engineering. I was slipping right into that and every day I'd look at myself in the mirror and say, "This is not what you wanted to do. You wanted to get out and work, travel the world, do what you were going to do, and come back and teach." I saw teaching as a love of mine, but not in the early part of my career.

At an almost happenstance meeting at a mailbox at the university, I met a gentleman who said there was a mining company in Albuquerque looking for a compliance and environmental manager. This was a strip-mining coal company so the environmental side of reclamation and the compliance side of freeing up their huge bonds that they had in mine restoration was a big thing. This fit my background combination—I knew mining and my formal degrees in education were very much bent towards environmental restoration, the reclamation end of that.

So I went into mining, but from a back door—it wasn't the classic approach of geologic engineering and mining, it was through a combination of my environmental

background and mining experience. I spent nine years as a manager, and ultimately as a director, for a mining company, and at the end of that time was running nine different mines in the southwestern United States—coal mines, gold and silver mines, silica mining, and natural gas processing and pumping. What a crucible for a young man to have.

MV: I'm interested in how you got there, Ned. When you refocused and decided to go to college and you and Cindy went to Las Cruces, did you intend to get a Ph.D.?

NE: Initially when I went, it was to get an M.D. When I decided not to do that, immediately I said I might as well get a Ph.D. And it might be for all the wrong reasons. A passion to have the knowledge and to become the world's greatest expert in an area is what drives most people in science to a Ph.D. It is an inverse pyramiding kind of process to me that always says you're learning more and more about less and less. Ultimately to me, as a kind of cynic, the Ph.D. meant I am now the world's greatest expert in damn near nothing because of that focusing.

I rebelled against the focus. I wanted to study broad and general, and this academic patchwork that I had fit that. As an engineer, I knew things about biology no engineer knew, and as a biologist I had an understanding of the edaphic system that none of them knew. I liked that—you know, you look smart to half of the world all the time. It didn't matter if you were wrong; you might get caught by your peers, but the other side was impressed as hell that you knew that.

Maybe it was a scam in a way, Mike, but I loved that diversity and I loved the broadness of it. I wanted to have the Ph.D., but I believe that even then, I realized it was an enabler. It was a credential. It was so you would be considered as a subject matter expert in an area that you couldn't fake any other way, even with experience.

Again, I was raised in a family where my dad had a certain bitterness every day. I lived with the bitterness of him saying, "I've gotten beat out of an opportunity by a 23-year-old graduate of the school of mines."

MV: He didn't have that piece of paper.

NE: There it was. I think that I was subconsciously working to say that would never be my problem. And when I finished with two Ph.D.s, I said no one is going to out-diploma me—I have that.

MV: And your two Ph.D.s are in which fields?

NE: One is in biology—biochemistry. It was an environmental focus, but biology was the root. And the second is in civil engineering, with the root there being hydraulics, hydrology, and fluid mechanics.

MV: I can only compare you to myself. I still get called to task for this. I did not enter school with the idea of getting a Ph.D. I still run into people who knew me in school and said, "What are you doing with a Ph.D.? You told me you didn't want a Ph.D." I was asked to come back later on and I did. It fascinates me that you went from the wild man you were in high school, to such focus that you wanted that Ph.D. and had a reason for wanting it.

NE: As I say, I don't think it was the right reason. Most people have the drive to learn in a specific area, and the culmination of that is a doctoral program. I truly believe that it was my family upbringing, recognizing the pain you would have in your life trying to compete in the world that I wanted to live in, a professional world of academics and science, without the credential. It was so easy for me, Mike, to go to school. My wife will tell you that the best thing I can do, my greatest skill, is taking a class.

It was a lark a lot of times. I could make an A in any class that you put in front of

me. I enjoyed the learning—I absorbed, I loved it, but I never took it as seriously as I probably should have. Certainly it was a big contrast with the people around me who had their noses in books and studied all weekend when I was out raising hell all weekend, and out at 3:00 in the morning. Had school not been easy for me, I couldn't have done what I did.

MV: Cindy has told me that you'd still be in school if it hadn't been for her telling you needed to get out and get a job.

NE: Absolutely. I was finishing up the engineering and something had turned me on to environmental law and I remember the night I came home and told her, "I think I'll get a J.D. in environmental law."

She told me, "Well, send me a postcard, because your children and I are leaving you tomorrow." [Laughter] And she meant it; there was no joke in it. She had lived in a 400-square-foot cinder block house on campus with two kids, bless her heart, as long as she was going to do it. She said, "You're either going to go out and get a job, or I'm going to go out and find another way to live." She had paid her dues—she had had enough of that.

MV: So two of your children were born while you were still both in school.

NE: That's right, and the third was born while I was in my mining company period. Because I knew mining and was technically, I think, very confident and had a lot of drive. I started as the environmental compliance manager for this company and ended up being the technical manager for all of its mining operations.

CHAPTER TWO

MV: What was the company?

NE: I was hired by a company called Transwestern Sunbelt—Sunbelt Mining Company was the official name. Sunbelt was owned by Public Service Company of New Mexico (PNM), the electric utility for most of New Mexico. That was in the early ’80s, and if you follow the public utility cycle in this country, diversification and un-diversification is a cycle utilities go through.

Public Service Company of New Mexico thought they would own their own coal mining company, feeding their coal-fired power plants in New Mexico. This company was originally set up to be a coal mining company as a subsidiary of the utility, providing coal to the coal-fired power plants in the Four Corners area which fed most of the electricity for northern New Mexico. It was a diversification period. Early in the years we were there, we began to look at gold and silver. There was an upswing in gold mining in the early to mid-’80s.

MV: And where were you living at this time?

NE: Albuquerque, New Mexico. That’s where PNM's headquarters was, and this little mining subsidiary was collocated in the same community in Albuquerque. But we had offices in four different locations at that time so it wasn’t just an Albuquerque-based deal. In 1984, I was asked to permit and design the first cyanide heap-leach gold mine operation on a placer deposit in northern California. I took a period of time and learned that work from the best in the country, hired the best in the country to teach me and help me.

We started gold-mining and ran two cyanide-based programs in the Sierra

Nevada, which was an environmental coup. I mean, getting permits to gold-mine—strip-mine, basically—placer mining and cyanide heap-leaching in the middle of the Sierra Nevada was an environmental achievement. People were impressed that a young guy like me was able to work with the various entities. The state of California is very tough environmentally, the most rigorous in the nation by far. The US Forest Service was the big federal entity—it was all Forest Service lands we were working.

I think the skill set I showed was not as much technical as the ability to get people together, to get a team together and to keep the people confident in what you could do. It was establishing faith and confidence that we're good for our word and if we make a commitment, we'll live up to it. It would have been easy for those relationships to go south in that environment. I think coming out of that, PNM and this mining entity realized that I was the guy that could put something together. It led to me becoming, as I said, the overseer of all of our mining operations—coal and gold, some silica, heavy media work; I did a lot of magnetite work during those days.

It was a good crucible for me to learn, and I would probably have stayed with it, Mike, except that I could see that pendulum swinging back away from diversification. PNM was more and more saying, "We need to get back to core utility and not be as diversified." I could see them probably spinning or selling off a lot of what they were doing. I was happy doing what I was doing, but I could see a change coming. And it was a time in my life in the late '80s when I was receptive to something new.

The second very serendipitous thing that happened was that I was in a league champion softball team in Albuquerque. A guy came up to me and said, "I've been in discussion with Los Alamos National Laboratory and the Yucca Mountain program. Nevada is rumbling along and really wanting to get serious about going to a field-phase

site characterization program." This guy was not a headhunter, but he worked in a resource company and he knew me very well, and he had had some discussions with Los Alamos, telling them, "The guy you need for Yucca Mountain is this guy in this mining company."

MV: Let's explore that—you were doing permitting and compliance principally for the mining company.

NE: It had started there, but I'd gone over to actual mine operations.

MV: That's what I wanted to understand—you were actually just as involved in mine planning and operations as you were in the compliance part.

NE: By that point, absolutely. And probably spending more time on the actual mine design and the operations facility than I was on the regulatory aspect.

MV: Ned, what caused you to consider such a significant change? I can understand one of your mining companies looking to diversify, but if you had such a strong interest in mining and such a strong mining background, why not look at another mining company? What was it that caught your attention when this guy started talking about Yucca Mountain?

NE: The simplest answer to that, Mike, was it was Los Alamos National Laboratory, LANL, which to me epitomized science and research. As I said, all my life, science and the application of science was probably what I wanted to do more than anything else. Quite literally, I don't think up until that point I had ever even dreamed of working for a national laboratory, but Los Alamos was the lab I knew the best. I'd had interactions with them in both academic and in the mining company career days—I crossed paths with a lot of people in Los Alamos, living in Albuquerque; it's part of the culture. But neither the Sandia nor Los Alamos national labs, I guarantee you, had ever been a thought in my

head as far as being where I wanted to go.

The nation's nuclear energy policy had long been an interest of mine. A lot of my background, even though mining, was environmental—it was solving cleanup-related issues, and that was being applied in the nuclear program for civilian energy. Even then I remember saying, "There is no real clear path. How can you build that industry until you've resolved the back end of the cycle?" That was probably the one thing I understood when that guy came up and talked to me.

MV: What gave you that awareness? I understand environmental permitting and compliance, but where was your awareness for nuclear waste coming from? I mean, why did you know there was a cleanup issue?

NE: I'd had discussions over the years with the set of people that I talked to in Albuquerque in the mining and cleanup areas as I came across people at Sandia and at Los Alamos. The one thread that I was continually hearing was, "We don't have that resolution yet. And Yucca Mountain is the nation's preferred solution to close that back end."

This guy who talked to me about Yucca was an employee of Meadows Resources. For some reason, they had had a lot of contact with people at Yucca Mountain. To this day, Mike, I can't tell you where it was, but I shared offices with a couple of them in a building in downtown Albuquerque and I got a little background on where Yucca Mountain was currently at and a little bit about the program.

When I pulled the thread with the guy who had mentioned it to me, I found out that he was not directly talking to Los Alamos—he was talking to a company [laughs] called SEA—does that ring a bell?

MV: Oh, yes, I know of SEA. It probably wouldn't surprise you that SEA was one of

the early spin-off companies from SAI.

NE: It doesn't. Anyway, there was a gentleman, a nanosecond physicist—his whole world was in the first few nanoseconds of the nuclear detonation. (How he had gotten wrapped into the Los Alamos deal I don't know.) It became apparent very quickly that it was he who was selling something to Los Alamos, telling them, "I can bring you the capabilities you need to engage this program," and it wasn't even a direct job at the lab.

This guy called me and I chatted with him a minute and I said, "So is this contractor a subcontractor job?" and he said yes. I don't know that it changed my interest level, but I saw it as one point removed from the lab. I don't know that I would have aggressively pursued it, but he asked me to go up to Los Alamos and meet the program management team at that time. Don Oakley, Dick Herbst, and Wes Myers were the three gentlemen that I met with and the individual from SEA was there. So he was brokering me. When I first went, I said, "Okay, I understand" that I was being brokered.

When I went there, the question that I really wanted to know was not, why is Yucca Mountain important? I had heard enough and learned enough to know that it was incredibly important to a future for nuclear energy in this country for Yucca Mountain to succeed. What I couldn't understand was why in the hell would they be interested in me? I had no nuclear background. I didn't really understand even the most rudimentary parts of the process of nuclear fuel and waste and how it would be handled.

What I did have was a hellacious background in mining, reclamation, environmental issues, and related problems. In those discussions it became apparent—something I have told young people ever since—that sometimes it's not your degrees, it's not even the academic experience that you have, it's your life experience and your aptitudes and capabilities to expand that make the difference. Because it became apparent

that it was the mining background I had that was the main thing that drew them to me.

It was a combination of two things—one is they said, “We’re going to go to the field. We need to get an exploratory studies facility, ESF, developed at Yucca Mountain. That’s the next big step—get to the site characterization phase.” They understood that science would be interfacing in a construction underground mining-related environment. They said, “We looked far and wide for someone who has two combined capabilities—one, mining; understanding that construction environment. Second, having the credentials and a basic understanding of science, I was someone who could understand a principal investigator's needs but could then turn around and interface that with a mining company or a mining entity in the field.

It intrigued me; I had never thought of my background as a unique combination. I had never been a serious academic. I had been a post-doc, but I had wanted to get into the real world. I didn’t have a three-arms’-length list of publications to my credit, I had maybe 20 to 25 peer-reviewed publications. The years I was in mining, I maybe only had two publications because it wasn’t a research institution—it was a blue-collar job.

I went then to Yucca Mountain and met a few people. I’m not sure if it was on that first soirée, but this SEA guy had me on a collar and was leading me around like a trained seal and saying, “Here’s the guy that you need out here.”

MV: I’m going to just take a second here to just tell you that any day I learn something neat that I didn’t know before, to me, is a red-letter day. Well, listening to the ’50s station coming in this morning, I heard a song that I’d never in my life heard—I’d never heard the Rivingtons sing “The Bird Is the Word.” So that clears up one of life’s mysteries. [Laughter]

The second thing I’ve learned today, and this really helps me piece something

together, is why Los Alamos was looking for the particular person you've described. This predates you a little bit—it goes back to the way Don Vieth ran the program; he set up the technical project officer. You mentioned Don Oakley, and Dick Herbst took his place after that. Yucca Mountain work was assigned like a job jar—whatever job came up, whichever lab's turn it was got the next job. It was Los Alamos's turn when the exploratory shaft design came up. I can't remember the guy's name, but the initial shaft design report came out of Los Alamos.

And of course, you know Wes Myers—he was the lead for the original test planning because it was Los Alamos's turn. So the penny has dropped again and I know why Los Alamos was looking for a mining-experienced person to lead that underground test program.

NE: And that fills in background that I've never really had. Because one of my questions then was, "Why is LANL in this position?" I would have expected that shaft work to be more in the realm of an operating contractor. I was just simply told, "DOE has asked LANL to take that role." The reason I recall hearing was because Los Alamos had, at that time, a 35-year history of coordinating nuclear weapons field testing activity through the old J and X divisions on the Nevada Test Site. There was kind of an unheralded group there that primarily did that work.

MV: I didn't know that.

NE: Whether it was a Livermore shot or an NDA shot or a Los Alamos shot, they were the common entity in putting that package together, inserting down-hole and making sure that the test went right. What caught my fascination was building a program. Not being the scientist, but being that central conduit for meeting the needs (not the desires, the needs) of the scientists to get a program in the field that would derive data they needed

and still withstand the scrutiny of the licensing process. The quality assurance needs on Yucca Mountain were so much beyond what normal academic needs were. It had never been done in that setting so it was novel, and I was always attracted to the novel. LANL said, "It's our job." I asked them why, and, as I said, the only answer I got was that it was the historic role they had played on the Nevada Test Site.

MV: I think there's more to it. I mentioned Wes Myers. Wes was at Hanford early on and he led the exploratory studies facility, but it was an exploratory shaft-testing program. He was involved with that when the site characterization plan was written. I'm curious—how much time did you spend with Wes as you began to move into this role?

NE: Wes was truly the guy who was my mentor. He called me two months, maybe, after the initial discussions and said, "Ned, we think you're it. But we don't want to do it through a subcontractor. If you're going to do this, we're going to hire you as a Los Alamos employee."

It was left to me to call the SEA guy, and I think he's bitter probably to this day—he reacted very badly to that. I told him it wasn't my request—it was the lab's request that they do this directly. You may have better insight than I on why the lab would have done that rather than stay with the subcontract deal. I had assumed that this guy was pushing that. He knew that Los Alamos had a need and didn't have the right technical skills mix to approach the project, and had said, "I can provide it for you." And when the lab found someone who could provide it, they decided to cut the middleman out, basically.

MV: Did SEA have a support services contract with LANL?

NE: I do not believe they had an active contract.

MV: He must have been doing this on his own. I wish I knew who it was.

NE: I'll think of his name. I did bump into him a few more times, and I could always sense a bitterness. I think he resented the fact that I ended up saying "I'll do it, but only if I'm LANL." I was so naïve at that point in time, Mike, that I was simply intrigued with the role.

MV: Ned, we could sit here and speculate all day. If the SEA split from SAI was at the right point in time, he could have had some Yucca Mountain connections through SAI before they formed SEA, and he may have been trying to get back in.

NE: That's a perfectly reasonable set of assumptions. But when I decided, okay, I'll do this, it was Wes Myers who basically hired me. Wes programmatically ran the Los Alamos Technical Project Office (TPO) for Yucca Mountain at that time, but in the line organization of the lab, he wasn't going to be my direct boss. My direct boss was initially going to be Don Oakley, but very rapidly it became Wayne Morris, then Dick Herbst. I started with an N division group. That morphed very quickly into what was the ESS—earth and space sciences.

My actual first boss, when I really got out there and got on the ground, was a gentleman named Wayne Morris. Wayne was the group leader of individuals—Dave Bish, Dave Vaniman, Schön Levy—names that I know you know so well.

MV: Yes, they were some very important scientists on the program.

NE: They were all in that group. The year before me, they had hired a gentleman whose job was to build this test management organization. His name was Hemi Kalia and he was already in Nevada. There were two guys from Los Alamos in that office permanently at that time, Hemi Kalia and Ron Oliver. Oliver had just come over from the weapons side. He had been out on the Nevada Test Site for years. And there were other players there, kind of coming and going. We mentioned one yesterday in loose

discussion, John Rowley. I don't know if you remember John, but he was in that group. There were a couple of guys who floated through there—I don't even remember their names—looking at data acquisition systems before there was anything to be collecting data on. Hemi was trying to pull that little group together.

There was another guy who was very central, Tom Merson. Tom wanted out of Yucca Mountain—he was getting close to retirement. And those guys didn't live out there. Every Sunday night they would go out, spend a week, then go back home to Los Alamos. Tom was worn out. He was the test design manager in this little structure. Tom told me, "You're replacing me." Test design interfaced with the facility construction, mining. He said, "You bring skills that I don't even have."

So that was kind of the structure. Don Oakley may have been the TPO, but basically, Wes Myers ran it. He was the division leader for the Earth and Space Science division. He was the technical project officer until about 1993. When we came out of the ESF alternative study, he moved away from that job and the position became Dick Herbst's for a period of time. Then Julie Canepa took the TPO's job. I was in Julie's group, so she was my group leader. As we began to get active in the field, LANL made the decision to replace Hemi's lead job, and I was offered and accepted the position. Hemi was left in the group, and that was a difficult interface for me for a long time. It's hard to replace someone in a lead position and have them stay in the organization.

MV: And it only gets more complicated because Hemi came from SAIC. [Laughs]

NE: I had forgotten that, but I did know it. And I think his wife worked there for a long time.

MV: Yes, she worked for us, too.

NE: To say that there was some bitterness on Hemi Kalia's part would be an

understatement, and I can't say that I blamed him. But I felt early on that there needed to be a more direct hands-on, maybe a little more aggressive, approach to this. Because at that time, we had no ESF. We were pushing a lot of paper, but there was no plan. You, much better than I, can say what drove the decision to do the alternative study.

CHAPTER THREE

MV: Before we get into that, exactly what day did you show up in Las Vegas?

NE: I got my badge, was given a plane ticket to Las Vegas at the lab, and May 19, 1989, was my first full day.

MV: So you were there in ’89—May of 1989 would have been five months after the final site characterization plan was published. So you were there earlier than I remembered you being there.

NE: The first thing Wes Myers gave me when I went to work there was the document they had produced, which was the exploratory shaft test plan. It was a two-set bound volume and he said, “There it is—start learning that.”

MV: And that document was then subsumed within the site characterization plan—Wes ran that as a separate exercise. But as the site characterization plan was developed, and we needed to have a Nuclear Regulatory Commission (NRC) review, he did a very good job of rolling those scientists into the program as a whole.

So you were faced with the objections to the draft site characterization plan, which had to do with the Nuclear Regulatory Commission's unhappiness with our exploratory shaft test layout. Do you want to talk about that a little bit?

NE: All I knew when I came in was that there had been at least an initial review, and there were a lot of problems established with that. Basically I was told there was no cohesiveness or integration, really. This looked like a massive number of totally independent-type activities that weren’t really tied together. The site characterization plan was supposed to be that decoder ring that tied everything from the needs characterization to the test activities.

My first job had nothing to do with the NRC comments or with that pushback. My first job was to understand every test that had been proposed, understand why it was there, look at where in that exploratory studies test plan it was going to be deployed. Most of them at that time weren't at a repository horizon—they were in the shaft itself. They were just down-shaft tests.

MV: Right. Let me set some background because I'd like to go to the point where you and I have a detailed exchange about the exploratory studies facility alternative. When we put the site characterization plan out in draft, there were five objections. One was on the quality assurance plan; one was on the lack of alternative conceptual models. There were three related to the underground test facility, and specifically they had to do with the shafts being too close together. That was a big issue for the Nuclear Regulatory Commission—that you didn't have enough space, the shafts weren't spread out, they were going to interfere with each other in the tests. And of course, you remember when the Nuclear Waste Policy Act was amended the previous year, in December of '87, the Nuclear Waste Technical Review Board was created. In one of their first meetings with us, they walked in and said, "What do you guys have an exploratory shaft for? What's wrong with you?" So that's where you walked in.

NE: I could see some of that history, but I didn't have it. It took me probably six months because it was a different kind of science. This was science looking at the effects of nuclear waste in that environment. Nuclear waste was not a background of mine so the types of studies and testing programs I had done in my life didn't prepare me well for absorbing that. I dropped off the planet for about six months and just read and studied. I traveled to each of the labs involved with Yucca Mountain (Sandia, LANL, Lawrence Livermore and Lawrence Berkeley) and the US Geological Survey and Bureau of

Reclamation. We just sat in offices and talked—I needed to understand.

When I got that understanding, the thing that was being told to me is, "We probably aren't going to do this in a shaft." You've put that in the context of maybe comments by the NWTRB (Nuclear Waste Technical Review Board) coming in and saying, "Why are you even doing that shaft?" I didn't hear it that directly, but I heard that this thing is probably going to radically change. I said the only way it radically changes is if you go to a portal or an adit system and you drive a different kind of opening.

But I guess that I would say in honesty, during that first year when I was learning, there was a part of me that said, "Don't get too wrapped up in resolving the issues of spatial separation and interference in that shaft because very likely they won't be doing it in a shaft."

MV: Do you remember who was leading you in that direction?

NE: A lot of those base discussions were with Wes Myers. Wes never told me we wouldn't test in the shaft, he just said, "I think that this is going to evolve."

MV: Wes was putting together those pieces I've just explained to you. It was clear to a lot of us that we had a bigger facility. You'll remember as well that it was a two-level facility. There was testing in the Topopah and in the Calico formations.

Let me explore a little bit of what was going on when you were talking to the other scientists about their tests. What struck you, talking to the scientists that made this unique in terms of a nuclear program, as opposed to a mining program? What was different? What were they telling you?

NE: I think the most obvious difference, in every case, was that each PI, principal investigator, would try to extrapolate, from the data he was going to collect, to how that data could be used—what its importance or value would be in ultimately developing a

performance assessment framework for Yucca Mountain. Even the concept of a performance assessment was new to me at that time—I had never been in that type of a program. I began to understand the underpinnings of the models and the code development that was going to lead to a PA that in turn was going to lead to a license. I think the first thing that struck me, which was different than any frame of reference I had, was the degree of extrapolation from data collection to how that data could be used, and how it was going to fit in to the conceptual models of the framework.

MV: I don't mean to make light of any of the people you were talking to, but that has to have been Wes Myers's perspective. I don't see many of the PIs in that program having that big of a grasp of where this was going.

NE: I think in fairness, Wes certainly was that voice that was constantly driving me to explore that. Maybe what you're getting at is that a PI would never just tell me that—I would be asking. I would be pushing that because I wanted to understand. Most of the PIs had very little knowledge of the project as a whole. One of the first thing I understood was there were about 50 elements in that test plan with PIs. As I talked to them, maybe four or five of them had really any knowledge of any other activities that were going to go on. It was just stovepipes.

MV: That doesn't surprise me.

NE: I recall that as one of the things that shocked me the most, that there was no integrated understanding, even in that community of science. They were focused on their thing, and their thing only.

The second thing that I began to learn very quickly is something I mentioned earlier about the desires of a PI going to the field with his test, and the requirements that he would have. Every time I talked to a PI it struck me that "You have no concept of the

environment you're about to go into, and of what you really need here versus what you think you need here or want to have here."

I'm going to jump ahead—we can come back to it—but it led me, Mike, to say in 1993 that the first thing we have to do here is to develop a formal test plan—very brief—a requirements-based test plan that was driven actually by functional requirements for the test, not even really keyed on the characterization objective or ultimately how the data would be used.

I can remember the number: we started the process in 1991, and I called it Test Plan 91-5—you've got to remember that as well.

MV: Yes. [Laughs]

NE: That was the first thing that was truly my vision—that was Ned's first rattle out of the box, saying, "Let's go do that." It was, to me, the greatest effort that I made because it got me to sit formally with every PI at every test organization and get beyond what they thought the data was going to be used for. To me, that part was up to people like you—to decide if it was necessary in the great fabric of what we were going to do. I drove down to, "What are your requirements to do that? Let's really look at what you would have to do in the field." That was the knowledge I gained. Then we skinnied it down to 42 –there were 42 test activities identified in this first test plan, 91-5.

As the alternative study was completed and we changed and morphed, that grew to an actual test planning program and an actual test plan developed under an NQA-1 program for every activity. I look at 91-5 today and it's nostalgic as hell and it was naïve in a lot of ways, but we grew from that.

That test plan was developed in a TBM, a tunnel-boring machine, environment. It no longer looked at trying to do the tests in a shaft. It assumed we would have breakout

alcoves or continuous access in the ramps, and so we were first coming to grips with the outcome of the alternatives.

MV: So 91-5 is before the alternative study.

NE: It was being done concurrently.

MV: So the maturation to the mechanical excavation was concomitant with the alternative study.

NE: That's right.

MV: Let's try to get on record some of the tests that you're talking about; you probably could name all 42 of them.

NE: It would take time. I think the best way would be for me to get at that from an organizational standpoint. The USGS was primarily about water movement, permeability testing. They had a series of tests that were looking at water movement in the facility, both in the Topopah and the Calico Hills formations. There were about a half-dozen hydrology-related tests in that original plan that were different; there were variations of water movement in either one unit or the other. They were similar to some degree in design, but all had some unique characteristic that said the data here is unique from any other test.

MV: At that time, was Berkeley a subcontractor to the USGS?

NE: No, I think Berkeley already had a life of their own. I had a hard time separating Berkeley and Los Alamos. They constantly were having a battle over near-field, far-field, saturated zone, unsaturated zone, and they were scrapping and fighting over those tests. The ones Los Alamos owned were really mineralogy and petrology focused—the min-pet program was the primary thing that Los Alamos was going to bring. Berkeley did not have a single test in the initial version of 91-5. They were still forming and didn't have

anything that they owned directly at that point in time.

MV: Why is mineralogy-petrology important for the repository?

NE: From Los Alamos's standpoint, it was going to be ultimately an issue of interaction between waste and waste package and engineered elements with the mineralogy and petrology.

Now, this became important toward the end; not in most of the years I was there, but another big driver with Los Alamos was an avid concern over the fine thoracic element, that is, how the work would affect people's lungs, of what we were going to be producing by mining. They were big on zeolites, they were big on mordenite, eronite, things that had already been demonstrated to be a potential respiratory issue.

Another big driver of theirs was to look at the construction and operation environment to determine what impact the mineralogy would have in terms of worker health and safety. That was a big element of their program—not just the environment interaction part. I remember very early saying, "Why are we worried about an actual interaction of the waste in this environment when it looks like what we're trying to design here is a system that won't have the waste free in this environment?"

The answer that I was consistently given was, "We have to look at the tails of the curve out here where you lose that confinement. If you lose that confinement, what will be the motive force, what will be the pathway, and what will be the reactions along the way with the minerals and the petrologic environment?"

MV: So they also had concerns about temperature effects and how the minerals would be changed in the environment, and what that would have to do with it?

NE: Exactly.

MV: And weren't they focused a lot on how the mineralogic composition of the rocks

at Yucca Mountain conditioned the water to impact the waste packages?

NE: Absolutely. And a lot of that early interest in Los Alamos was actually the zeolite itself, so not at the repository horizon, but in the Calico Hills. It was in terms of what the zeolite's water interaction would be, first of all, and how that would be important in the repository.

Now, these PIs were good, and the ones who did know about other tests—min-pet is a good example—were planning on piggybacking on them. For instance, not a single thermal test was run through the min-pet program, not one. But as you mentioned, thermal change was important to them. They intended to do mineralogy-petrology sampling and testing as a subcomponent of the thermal tests that were going to be owned by somebody else.

In those early days, Sandia and Lawrence Livermore owned the thermal testing. The big concept of a drift scale thermal test, at least initially, was primarily being pushed by LLNL. So Livermore's focus was primarily a drift scale representation. They also had the large block test, which we ultimately did at Fran Ridge. We can talk about the successes and failures of the large block, but the large block and the drift scale tests were Livermore's focus in those early days in the program, and to some degree that never changed—they stayed right with it.

Sandia was more the single-block-type heater or the small-scale heater test, permeability changes, actual changes in base rock characteristics based on a thermal pulse in the rock. We did the first one of those ultimately in Alcove 1 and did the smaller-scale thermal block test, if you remember, in Alcove 5 before we turned the corner and did the drift scale test. That was Sandia—rock mechanics—so there were a lot of flat-jack tests and things; Sandia was looking at the squeezing and spreading of the rocks.

So Sandia, to me, was more a rock mechanics focus with a heavy thermal interest. Livermore was more focused on the demonstration of an integrated thermal test at scale, both at block scale and in drift scale. USGS was primarily focused on hydrology.

Of course, the Bureau of Reclamation had the fracture mapping program. That was a big piggybacker for others—as they were mapping, there would be mineralogy and petrology components. So there was some crossbreeding amongst these. Those were the big areas.

MV: Great, Ned. I really wanted to nail down, as a point that we can build from, the fact that this was being developed with people thinking of a shaft, and we eventually moved away from that.

NE: Right. If you remember, Mike, the alternative study had no shaft-only alternatives. We had breakouts, and there were those that did not have any ramp or tunnel-boring machines that were shaft-accessed, but none of the alternatives were shaft access only. There were breakouts in those, both in the Calico and in the Topopah. You probably have a better memory than I do.

MV: You just made me scratch my head a little bit. It's probably worth our while to dig up at least one reference so that we get this straight. I remember there were 34 options.

NE: Well, 17 with a duplicate—that made 34.

MV: All right. [Laughter] There's so much to cover here, Ned—I'm grateful you've taken the time to go over this so we get it documented. All right, so we have literally 40 or more PIs getting ready to do tests underground but we don't have an underground facility. I wanted to make sure that we touched upon that, because that was a regulatory compliance issue that we had to deal with. Tell us a little bit about the permits and the state water problems. You must have stepped into this and scratched your head and said,

"How are we ever going to get underground?"

NE: Initially I was naïve enough to say, "We better hurry because we're going underground tomorrow." It took me a while to change it to where I said, "Will we ever get underground?" [Laughter] It took me a while to become that cynical or that realistic or whatever. From the test management standpoint, to some degree, I was always shielded; I was never directly involved in the negotiations with the state; I would hear that backdrop of issues on permitting. And the NWTRB perspectives were just forming.

Actually, in some of my early interactions, the initial people in the NWTRB showed a great interest in the test program. They didn't understand the backdrop necessarily, but they showed interest. I would keep up with the regulatory licensing challenges. The ones that manifested themselves to me in the big picture were free application of water, or certainly any organic material. That was a new world to me, where you can't introduce water except in a given application. You certainly could not allow any organic release.

MV: Let's expand—why can't you do that?

NE: It was explained to me early on that it would truly affect the long-term performance of the repository. Even today, I've always struggled a little with the fact that when we were talking about the ESF, I always felt we were talking about a repository. And we were continuously being recalibrated to say, "Even the facility use, and everything you're doing here, cannot and will not be used in the repository." And that separation from, "Are you building a part of a repository here that you're going to use later or not?" drove a lot of the confusion and a lot of the issues early on. I used to ask, "If you're not going to use this as a part of the repository, why are there prohibitions on any organic intrusion—why is that important?"

MV: It surprises me that you were given that impression. Because the 1985 mission plan was the heart of the objections that came to the site characterization plan. The mission plan said, “We are going to collocate the shafts from the exploratory facility and use them in the repository.”

NE: Really?

MV: That’s when the NRC staff said, “If you’re going to do that, then you have to meet NRC licensing requirements for the repository in your shaft program.”

NE: And that was the reality of what we were told to do. But continuously, as I was learning, I was always being hit with the fact that we could take no credit for utilization of this facility in the repository.

MV: There may be a different perspective on that; it may have gotten confused in how it was related to you. We could not say it was the repository because construction of the repository, before you get the NRC license, was grounds for denial of the license. There was always this very fine line to walk—you could not be building the repository, yet the DOE’s position was, “We intend to collocate them,” and the NRC’s counter-position was, “Then you’re building this to repository specifications.” And that’s where your QA (quality assurance) requirements came from; that’s where a lot of your problems came from.

NE: That makes perfect sense. Maybe it was my naiveté, in just starting, that I sensed that there was a taffy pull to some degree between those.

MV: Oh, there was.

NE: By the time we were getting underground, I was comfortable with the fact that what we were doing could be built and would be built into the design of a repository. One thing from a testing standpoint we were always very cognizant of is that nothing you did

and nothing you built—no alcove, nothing you constructed there—could be constructed for a repository purpose.

MV: That's probably the better way to say it. I wanted to make sure we have all the background set before we start talking about this thing that's looming. [Laughter]

CHAPTER FOUR

MV: Of course, this "thing" is the exploratory studies facility alternative study, ESFAS. How did we get there, Ned? When did it come together—that we were going to have to do that study?

NE: It came fairly early in my time there. As I was on my learning curve, as I said, there were those like Wes who were cautioning me that the milieu, the environment, in which these tests were being planned might change dramatically. But we didn't know. What was on the books at that time were two fairly closely spaced shafts. As you said, part of the concern was interference because of the close location of those shafts.

But even early on, from what I was hearing and what I came to believe, putting a lot of effort into designing a cohabitation of these activities in the shaft was not where we needed to be spending time because we were going to get to a point where we're going to take a careful look at that. So it was no surprise to me at all when the project announced, "We are actually going to take a pause in what we're doing—we're just going to stop right here. We're going to engage in that study and decide what the layout of this exploratory studies facility would be."

MV: At that time, I was working on resolving the objections for the site characterization plan, and we were not successful in resolving them. The objections were on the draft site characterization plan and we thought we would try to address them in the draft, into the final, with some changes to the underground facility. They did not work—the NRC had objections to the final one as well. They just didn't buy that we could do the testing in a way that would alleviate their concerns about interference. It was all about interference—not the interference of two PIs getting in each other's way, it was being

able to separate out the effects of one test from another because they were too close to each other. Now, that's one pile we're dealing with.

The other pile is, you've got the Nuclear Waste Technical Review Board saying, "What are you doing with shafts? You've got a repository site that just begs to have a ramp access."

You may not remember this, or you may not have ever known it, but very early on, there was a study at Sandia that looked at ramp access for the Yucca Mountain site. The DOE did not want to hear that. This had to do with the fact that, how do you compare nine potential sites when eight of them have shaft access and one of them has ramp access? The DOE lawyers were always very concerned about that.

As a matter of fact, that is how we got the second shaft at Yucca Mountain; the original ESF plan from Los Alamos had a single shaft. We felt that because we weren't that deep into the mountain, we didn't need that second egress. But the DOE general counsel was saying, "You've got eight sites that have two shafts—you have got to have two shafts," and the NWTRB was saying, "What's wrong with you? You need ramp access."

We had come out of the environmental assessment stage of the program with this huge multi-attribute utility analysis where we evaluated all the different parameters and found a way to analyze them to pick which one would be the best alternative. I think there was a sense in the mind of a lot of the DOE managers that if this multi-attribute utility analysis worked, it would help them solve decision problems: "Here's another big decision problem—what do we do?" I can't prove it to you, but I think those four or five pieces came together.

NE: You stimulate memories. I didn't understand the one shaft versus two situation—

it happened before my time. But I do understand that we were told access had to be by shafts because the other sites were shafts—I had forgotten that. And two shafts, as opposed to a single shaft, was also driven by that thinking.

In my opinion, part of what set the stage, or what I believed set it from what I was hearing, was that the ’87 amendment to the Nuclear Waste Policy Act tended to make people think we were relieved from the question of multi-site similarities. You were going to focus on a single site, and therefore you had now a flexibility that you didn’t have before.

MV: [Laughs] Ned, you’ve hit one of my favorite topics. We had lawyers saying, “You can’t have one shaft because everybody else has two shafts. You can’t have a ramp because nobody else has a ramp.” With regard to regulations, the NRC amended their regulation to allow disposal in the unsaturated zone. DOE never changed its regulation in that rulemaking, even though they told the NRC they would. Why didn’t they change it? The lawyers did not want to have one site with one particular piece of regulation that was different from the other eight sites.

The Nuclear Waste Policy Act Amendment of 1987 gave DOE the one site that was different from the other eight; that should have taken care of things. All the reasons they had given before, saying, “You can’t have this, you can’t have that, you can’t do this,” came back to bite them because they ended up with the site for which the regulatory structure they had built was least applicable. Pardon me for getting on my soapbox.

NE: No, that’s incredible. You’re jogging memories with me. I remember a conscious thought that we were ripe for an alternative study at that point in time. The world had changed with the amendment; we had some flexibility then.

And you also jogged another memory with me—that there was a sudden fascination with multi-attribute analysis. That was part of the fabric of why we were going to use that tool—a great tool. The primary people I was hearing that from were Tom Hunter and Tom Blejwas; they were the Sandians who really pushed that. I could not tell you why Sandia gained a lead role in the alternative study. There was a gentleman—a shorter, older, heavy-set guy—who was kind of the day-to-day lead for the alternative study. I think his name was Al Stevens.

MV: Now you've stumped me. I sure remember Gnirk and Merkhofer.

NE: Did they come to the project through you or did they come through Sandia?

MV: They came through Sandia.

NE: That would make sense because Sandia had a long-term relationship with them. But when we began to have the first combined meetings, [the guy I just described] ran those meetings. He was a Sandian and he had a sidekick who was even an older guy from Sandia, I think. He was Hispanic—a tall older gentleman who had been around Sandia for 40 years. I think his last name was Sandoval. Those two gentlemen, under Tom Hunter's overall direction, ran, or at least conveyed to the test population, that they were running the alternatives study.

Mike, that's not the wiring diagram, but the wiring diagram we were given on the testing side was really a Sandian lead for this, with Re/Spec being the grand facilitator. And this guy, Al Stevens, was a jovial kind of guy. He was a good people person, wasn't tense, wasn't spun up.

MV: You're jogging my memories as well. Sandia had the design and the Architect / Engineer firm was working for them on the repository design at that time. It would have made sense if we were going to look at different designs to have the people doing the

designs leading this activity.

NE: I didn't even realize that that was a role because I was so focused in the testing area.

MV: The conceptual design that accompanied the site characterization plan was done under Sandia.

NE: That makes perfect sense. So Al Stevens and Sandoval were the two guys who came and had the initial meetings with the little test management group, which was the group that I was already leading at that time. We began to prepare for the ESF alternative study. I made kind of a stumping trip all over the complex—went to each of the laboratories, spent time with each of the TPOs and their experimental leads in terms of the testing program, to learn what role testing would play in the ESF alternative study. It was more than just a design; it was a design to optimize the characterization of the site. So there was more than just a passive wait for the answer. We had to engage on the testing side to help drive the program to get the right answers.

All the way through the alternatives study, and no matter where we were going on the debate and the differences of opinion, I was always focused on what was the most conducive to collecting the maximum amount of data that I thought we would need in that process. I was always thinking from the testing perspective about both the type of access and the total exposure that we would get and, to some degree—back to my mining experience—the rate with which I thought we'd get that exposure. How fast we could get there was always important to me. And lastly, how flexible would they be? In other words, could you change in midstream or make a change even after the fact?

I'm jumping ahead a little bit. One of the reasons I always liked the combined ramp-shaft scenario was I felt the shaft scenario gave us the quickest access to the Calico

Hills. I thought that it was one alternative that you could always deepen or put a station on a shaft. You had certain flexibilities there that I liked, in combination with the ramp. From the beginning, I was only looking at that little slice—how does testing help drive an answer to what the best alternative is? The big picture was not where I was coming from; I had a fairly fine focus.

MV: I've got to tell you, Ned, I don't know why we would have done the ESF alternative study if it weren't for that driver, because that's what the objections from the NRC were—on test interference. It wasn't who's going to put the shaft where. Now, we probably ought to describe the original configuration before the ESF started.

NE: Before the alternative study? The prime configuration was a two-shaft configuration. One of them—I believe only one—accessed the Calico Hills; the other one stopped in the Topopah. The deeper of the shafts had a breakout in the Calico and we had a mid-station breakout in the Topopah Spring. The other was pretty much a direct access with only tests that could be done in the direct shaft. So we had breakouts, but they were small in scale. There was very little at-level development, just enough to put in, for instance, a drift scale test or a test at that scale—very, very little. As much as we could would be done in station, off platform, in the actual formation. I wouldn't swear that both shafts didn't go to the Calico, but in my mind, only one of them went there.

MV: How close together were these shafts?

NE: They were about 250 to 300 feet—they were close.

MV: And my recollection was that at this point in time, the Calico Hills was a principal barrier for us. And there was a breakout, and there was testing in the Calico. I think you've got it right. Where were they located?

NE: Up in Wren Wash. They had Yucca and Wren—there were several small header

canyons. It was on the northern end, much farther north than where we ultimately sited the facility.

MV: Okay. Let's talk about the possible alternatives that could have been considered. You were talking about a shaft-ramp combination. As you said, there were 17 fundamental combinations, and to make it 34, you went in the Calico first. What was the difference in these things? Just give us an overview of what kind of alternatives they were looking at.

NE: What drove the 17 versus 34, as I recall, was timing. The ultimate configurations looked almost identical but in one of them, as I said, we put our initial focus on the Calico Hills and in the other we put our initial focus on the Topopah Spring. What we had were permutations and combinations of shaft-only, ramp-only, shaft-and-ramp. We had combinations of daylighted ramps and stub ramps—in other words, we would go and terminate not all of our ramp accesses; we were going to dead-end and leave TBMs in some and daylight the others. So you had looping ramps, which is ultimately the configuration we did, and we had stub-out ramps.

We had some, for the Solitario Canyon access, for instance, the big question was do we drive to the Solitario, or do we turn short of the Solitario? So where those ramps went weren't just a given endpoint—there were different configurations of endpoints in the ramps and combinations of spiraling access by ramp into the Calico Hills, or almost separate ramp systems. As I recall, Mike, most of them broke off from a single, and then we'd do a deeper loop off of them.

MV: Didn't we have shafts at the southern end as well?

NE: Yes.

MV: So we were looking at potentially exploring a much larger piece of the repository

block than we had originally looked at.

NE: Absolutely.

MV: Didn't we talk about raise-boring one of those shafts as well?

NE: Yes. There were subtle differences; to look at a picture they would look similar—but with the construction methodology, and therefore the real-time testing opportunities, it was important to know if it was a TBM or a different type of road header. Was that a raise bore, or was that a V-mole. Was it conventionally sunk or not? All of those questions to me were about what was the testing access, what interferences or complications could the construction method cause, and what was the timing for experimental access.

MV: We probably need to make sure that we don't get too much jargon in here. You talked about conventional excavation versus mechanical excavation. Just give our readers a little bit of an insight into the differences.

NE: They're very different. From the dawn of time, once we had explosives, the basic conventional way to develop an underground opening was drill and shoot. But looking at the questions of interference, drill and shoot has a tremendous amount of impact on the rock—your disturbed zone in a drill-and-shoot operation is very hard to control. We were led very early to a focus on maximizing mechanical excavation by a TBM or road header as opposed to any conventional-type development of the facility to minimize impact.

Once you were there, however, you still had a smorgasbord of opportunities in both ramps and shafts: were you going to do them with a TBM, a full-face miner? Were you going to do them with hard-rock mobile miners? Were you going to develop new technologies to do those, or use the ones that were out on the street? In terms of shaft access, there are multiple ways to do it. If you're not going to go with conventional

methods, if you can provide a point underground that you can access, you have a world of opportunity to drive only a pilot hole, and you can pull a raised bore up that hole.

MV: I wanted to make sure people understood that that's the tricky part. It's kind of hard to mechanically mine going downward, but if you can get underground and drill a hole and pull it upwards, you can minimize that damage.

NE: Absolutely, that was a big part of it. And it was very difficult—you can do a blind bore, but they're tough. If you can get underground access, then you can begin to do raised bores or a raised V-mole, which is a much easier way. As I recall, there were lots of different excavation types, and sequencing was always important to me. Because if you were going to have a shaft, you have a ramp first, then have a target location where you could raise bore shafts. That mattered to me because if the shafts had to go in before the ramps, that might have been different. If the ramp could go in first and then the shaft, I thought that was more realistic and more feasible and gave us more flexibility.

MV: So it's not hard to understand why there are 17 fundamentally different things we were looking at.

NE: That's right. I probably could not give you all 17, but I've focused on a couple that early on to me said these were the two. I believe Alternative 4 was one of them and I think the last one, Alternative 17, which meant 21 and 34.

MV: If you want me to say a number, 30 was the one that had the two tunnel-boring machines simultaneously starting from both ends. That had an apparent time advantage. We'll have something to say about what a silly idea that was.

NE: But getting back to the base configurations—again, you can't tell the difference. The base configurations were probably Alternatives 4 and 17, which would have been 21 and 34. Those were the four that we kind of ended up getting on.

MV: I've said what Alternative 30 was—why don't you explain what 4 was?

NE: Four was a combination of shaft and ramp systems. Most importantly, going back to just gut feelings that I had, I felt that a shaft into the Calico Hills gave you two things—a continuous record of the geology from the surface through the Calico, including the actual lithophysal target for the repository itself, but it got all the way to the Calico. Being an old miner, I knew it was going to be a fraction of the cost and I had tremendous flexibility when I got there so I liked the ramp on the repository level. The main advantage of the shaft was access to the Calico without requiring to have another TBM drive all the way into the Calico Hills.

MV: Right. And there's one difference between these—that would've been focused primarily on the northern part of the repository. Alternative 30 intended for us to look across the repository from north to south.

NE: Exactly right.

MV: We had so many sub-working groups that we were dealing with. You and I are focused now on what, to me, were the high points of the project. But we had to look at so many other parameters in this as well. When you do a multi-attribute utility analysis, you have to develop influence diagrams. Do you have any thoughts on that?

NE: Very little. I used to just get a hoot looking at these influence diagrams, these damn bubble diagrams and overlaps and arrows going everywhere. I'll tell you, Mike, I was very guilty of tunnel vision in looking at many of the other attributes that were being looked at. The committees that I spent my time on were those that were looking, core and central, at the design itself, the timing of it, and the testing program. I remember very vividly that there were many other working groups and that they were looking at the co-influence of the multi-attribute analysis program, but I don't believe I spent any

considerable time on them outside of the committees that you and I were on. You may have inhabited many of them; I did not.

MV: Yes. But we had ones that were looking at environmental impacts, cuttings from the mining, aesthetics—could you see a muck pile from the highway? There were so many committees.

NE: Yes, I remember that they were there. We also had socioeconomic considerations and drivers there, even getting down to the workforce that would be developed. You're right, there was a huge spectrum of that. As I said, I don't think I was active on any of them, other than the one that was just the maximum bang for the buck and getting a test program done.

The only ones that I did play some in, because I had to, were things like what would you do with cuttings? What were going to be the nature of the cuttings? What was going to be the material coming out of the excavation and how would you handle that? I was involved in some siting of where the muck piles would be and what the nature of the muck would be, what the environmental impacts of that muck setting on the surface would be, what kind of environmental controls for things like runoff would we have to do, and how those would be designed. I have a recollection of some of them, but they were not the ones that I was focusing on.

MV: My mind is wandering, too, trying to recollect all this stuff. It almost didn't matter what our access looked like. The fundamental testing program layout was not that different, if I remember correctly. We had a testing setup that would have been developed off of these shafts or ramps, but there was not a radical difference between them. We had solved the problem of getting the tests far enough apart so we didn't have interference in the tests and we got away from the shafts or ramp, whatever it was. I thought we were

going to be able to keep the NRC staff who were worried about test interference happy.

NE: Right. As I recall—you jogged my memory again—the concept at that time was we would have an actual test panel layout. All of these options had a test panel layout. A lot of my early work was designing test locations to minimize interference and maximize what they were trying to get in a room-and-pillar, almost, design layout. That was common—I think there was very little deviation from that in any of the options. It was just moved around and sited at different locations, both off-shaft.

MV: How many of the exploratory study facility principal investigators were involved in the studies facility? Did you represent them?

NE: Pretty much. There were very few of them who ran the tests themselves.

MV: Who were all the people in the room? I remember having 20 or 30 people in the room when we were doing this. I know we had a lot of designers.

NE: The big groups that I remember were the design group, and we had a lot of what I would call simple DOE oversight, and DOE oversight filled a part of those rooms. The Re/Spec group, the facilitator group, would be good for four—Bill Boyle sitting in the corner on a machine of some kind taking notes [laughs] like a court reporter, or whatever Bill was doing in those days.

Then from the testing perspective, generally it was just me. Given the nature of the meeting, I can remember a few times asking and having Bob Craig, for instance, there from the USGS and Frank Hanson or Larry Costin representing Sandia. I don't know that I ever had Los Alamos or Livermore too directly involved but because of the big design scale tests that we were looking at there, I remember especially Sandia occasionally having a little bit of PI influence. But for the most part, based on the previous work I'd done with 91-5, the test community was already looking to my organization at that time

saying, “Represent our requirements, represent our needs.” So they weren’t too directly involved.

MV: Do you remember the elicitations?

NE: Vaguely, yes.

MV: When we were doing the influence diagrams—the bubble diagrams, as you referred to them—what sticks out in my mind so much is Paul Gnirk, with his fishbowl full of marbles, trying to calibrate people on estimating probabilities.

NE: You’re exactly right. There was an educational process for us in the multi-attribute analysis and it was obvious to me, and to all of us, that Gnirk felt that the most important thing we needed to understand is what probabilities were and how they played in what we were doing. Of all the elicitations, Gnirk and his marbles are the ones that I remember the most—trying to drive home, what does that one-in-a-thousand shot really mean? What is the real role of probability in this process?

MV: While Gnirk was trying to get us to appreciate differences in probabilities, Lee Merkhofer, who was actually running this, says that as the decision analyst he was trying to convince us, every chance he could, that we were terrible at estimating probabilities and we needed to be cautious in estimating probabilities. That sticks with me.

NE: Yes it does, because they kind of played against each other. That was Meckhofer’s prime role to me—to be conscious we’re not good at this and we will overestimate or wrongly apply.

MV: I remember some little quizzes, like getting people to put bounds on things, even the height of Mount Everest. It showed how many people in the room, when given the chance to say it’s between something, or what it was exactly, would get it wrong. Those were fun and heady days. Anything else you want to talk about before we get to the

punch line? I've been looking forward to what's coming next. [Laughter]

NE: No, I think that set the groundwork pretty well. I'm actually sitting here trying to turn the pages in my mind about the differences in the 17 base configurations. The things that I remember were means of access, the actual types of access—the numbers of TBMs involved was actually a big driver. Was it two TBMs or four TBMs? Did they start simultaneously? Were they staggered?

I'd have to get calm and quiet to think about those nuances and how those permutations played through. The big difference was one versus two loops, shaft versus no shaft. Remember the one that took a single TBM and actually corkscrewed it into the Calico Hills? I can remember that obvious difference, which was fairly significant.

I would be hard-pressed in coming up with some of the subtleties in those 17 base alternatives that we looked at differences in access. But I think we've kind of covered the background as best as my recollections allow—what led us there, what was the objective, who were the main players in that process. As a test management group, we were representing the PIs through that process.

MV: Let me throw something on the table and see how you react to it before we talk about the final results. This is an idea I have about what drove the final results of the ESF. My recollection was when we did all the elicitations, the big management elicitation at the end was the one that probably had the most influence on which way the final thing came out. I think we had resolved any issue among the testers about whether there were preferences one way or the other. In my recollection, the big preferences were cost and time, and do you satisfy the NWTRB and the NRC? Those are the four that pop out to me. I don't think cost was a constraint to those managers.

NE: It surely wasn't. Where we ended up, you couldn't make a case for cost having

been a driver.

MV: Option 30, which is the one that was declared the winner, if you will, was the Calico Hills first exploratory program with four tunnel-boring machines starting, two from each end simultaneously. It had the apparent advantage of very rapid access to both the Calico and the Topopah in getting the test programs going. It was very expensive, and it looked like it would satisfy the NWTRB and the NRC concerns. Is that fair?

NE: That's very fair. The only thing I'll pick at a little is that in my recollection, it wasn't absolutely simultaneous—there were not four portals.

MV: No, not four portals, but four TBMs going, one behind the other, as soon as you get started.

NE: That's right, one behind the other. You actually made your drives on two TBMs and then took off immediately and began to go to the drives into the Calico Hills.

MV: Now let's talk about the practicality and the pragmatism of something like that. It looked to be a darned good solution. So what happened?

NE: What happened in the decision process, or what happened in implementation?

MV: What happened in implementing it?

NE: In the implementing, the first thing that went away was four TBMs—we weren't going to have four TBMs. In fact, very early in that process, we began to question whether we were going to go to Calico at all.

MV: Those are the two points I want to get on the table. The Department of Energy managers read Congress wrong in terms about their willingness to spend the money to do that.

NE: Yes, sir.

MV: When it became clear that Congress would not fund the program to do the alternative that looked like it would get the program moving quickly, why didn't DOE go back and say, "Okay, what's the next best choice?" Why did they stick with one that was chosen only because it looked like it had fast access after it became clear Congress wasn't going to fund it?

NE: Mike, that's a tough question. Part of my answer would be because these awarenesses and coming to grip with reality didn't all happen at the same time. This was a slow evolution of realities setting in. On Day One, if we'd known what we were going to be able to do and not be able to do in the next few years, maybe we could have gone back and revisited it. But it was a slow evolution of change that, in my opinion, kept us from ever coming to grips with the fact we were not going to get there.

I would turn and ask you a question—the decision that disturbed me the most was very early, a decision I had no part of and was never at a table when it was decided—that the Calico Hills access was really not necessary.

MV: Right. Not only was Calico a first option, how did we end up in the Topopah? I'll start by giving you my thoughts on that, Ned. I think Carl Gertz, bless his heart, wanted to be able to have members of Congress walk through the Topopah.

NE: You're exactly right. I remember that vividly—it was a desire. But why not walk through the Calico?

MV: I think he wanted them to see the repository horizon.

NE: At any rate, we bought a TBM. I think part of the reality that set in was in developing a TBM that would meet the quality assurance and performance-based criteria that we had. Whoever thought that that was going to be an easy exercise was wrong because that group up in Seattle struggled mightily. We ended up with a TBM that is probably still sitting there today because not another entity in the world wants the damn thing. Who wants a pneumatic-only TBM? It was a weird tough beast. The maintenance schedule on it was out of sight. But we played around with that in the design and construction phase for the first TBM. My guess is, someone was sitting there saying, "I'm going to go through this three more times? We got one—let's just put it to work."

One of the greatest problems to me with what we ended up doing, is when we finished the first loop, at the time we daylighted, it had pretty much been decided we would not do a deeper cross-drift, so what became the second drive was down in the horizon where we drifted across the block more east-west than north-south. At the time that we daylighted, no one was talking about that. In fact, those who were doing it were saying, "We'll never do that second drive—there will be no cross-drift in the ESF." One

of my great problems with it was that the five-mile loop we just did really didn't even look at the horizon where most of the repository would by sited—in the lower Topopah.

MV: We're going to get to that, Ned. You and I have some experience there, too. But first, you brought something up that had never occurred to me. You mentioned the tunnel-boring machine and how difficult it was to develop. That tunnel-boring machine didn't make any sense to me—what were we doing in hard rock with a fully shielded tunnel-boring machine that probably caused more problems than the excavation itself? Why was it done that way?

NE: That was a Peter Kiewit and Morrison-Knudsen design decision, as far as I know. I can give you the name of a guy who I think could probably give that history—you know him as well as I do—Dick MacDonald.

MV: It always bothered me, to have everybody complaining about how difficult it was to do ground support so far behind the face. You have to ask yourself, what were they doing in a hard-rock tunnel-boring machine with those shields in the first place?

NE: That was the Chunnel design, basically—the TBMs that were driving under the English Channel into Europe. The only difference was that they allowed hydraulics in the Chunnel machines and these had to be strictly pneumatic; that was the main design difference.

MV: So those were set pneumatically, not hydraulically?

NE: Absolutely.

MV: Oh, I see—they didn't want to spill oil.

NE: As I said, it was a performance issue—they couldn't take the chance of dripping hydraulic fluid all over that tunnel.

MV: Is there anything you'd like to talk about that we missed about the ESF

alternatives study?

NE: No, in terms of setting the stage for decisions and the beginning of actual work out there, I think we covered it, Mike. I would say, let's get a discussion of the final decision and the implementation.

MV: This is probably the best opportunity you'll have to comment on Option 4 being a better option than Option 30.

NE: I've made no secret of the fact. I pushed hard for Option 4. You probably won't believe me when I tell you that part of my reason for that was I never believed that we could simultaneously drive four TBMs—that was the old miner in me. And so I said, "This thing is going to change." I didn't know at that time how it would morph and how it would change, but I didn't believe we would implement Option 30. You've been gracious about saying that was maybe a mistake. I will be just as gracious to you and say, had we done 30 the way we made the decision on it, it would have been the best option.

But if nothing else, I thought there would be a huge cost advantage with Option 4. Cost, as you said earlier, didn't seem to be a driver too much, but with a shaft access, taking advantage of the north ramp, on the north end, going down to access the Calico hills, not only to get to the Calico, which I felt was the only way we'd ever get there, but more importantly, I think, doing it at a fraction of the cost of the TBM drive, and having that continuous geologic record that we weren't going to get any other way on the actual repository block. With access later, we could have stationed up and run tests or did anything. I feel today, as I did then, that that option gave us maximum exposure in the most realistic fashion and gave an opportunity to see at least the repository horizon. The cross-drift, to me, was driven by the concept that, in fact, the initial loop that we drove with the TBM had very, very little exposure in the actual repository horizon.

MV: I wanted to point out that another thing the NWTRB was concerned about was the fact that in our original repository layout, the main drift for the repository layout went right through the center of the block. We were getting criticism from the NWTRB for having that exploration in the block. At the same time that we're doing the exploratory studies facility analysis, we've got people doing a repository design change, moving that main drift off to the side. You mentioned this earlier—that put us in a position where we didn't really have information going across the block. Maybe we should talk a little bit about the study that was done to remedy that?

NE: Well, first of all, to me, we didn't have anything east-west that directly related to the repository block, because you couldn't count the ramp accesses—they were above horizon. But even in the drive—I can't remember the numbers, Mike, but in a vertical sequence, the lower lithophysal rock was really not explored in that loop, and the lower lith was a huge part of the actual repository horizon. It was not only a concern about the extent and character of the repository block east and west, but it was also looking at the geologic cross-section vertically, where I felt we had missed the boat. In my opinion, the decision was strongly held at DOE not to do the cross-drift. The NWTRB, I felt, was instrumental in saying, "Thou shalt do that."

MV: Absolutely.

NE: Ed Cording and Russ McFarland are the two I was most actively involved with. Certainly from a testing perspective, the scientific community felt that that cross-drift was important. I mean, there was a lot of support there. But it was not a popular decision. I don't know—you would know, again, better than me—was it economics that led to that decision?

MV: In my recollection, Dennis Williams was really opposed to it in terms of schedule

for getting the license application done and money being spent that was needed for other things.

You were right, there was resistance to doing that cross-drift, but you couldn't have put this more clearly—we didn't have the shaft. We had no information about the lower lith. In fact, there's a whole geologic section through there that we had little information on.

NE: Yes, sir. As far as the actual implementation of the cross-drift, you're getting at the twilight of my time. It was while that drift was being excavated in 1998 that I left Yucca Mountain so I'm not a good repository of information on that.

MV: I wanted more to explore with you how we got to the point where it played out the way it did. In my memory, that was a pretty interesting story.

NE: And you probably have a better memory even than I do. I remember that we took a careful look at the geology—we were very involved. The Bureau of Reclamation played a big part in that because they had been our mapping team and they understood the fractures and the fabric of the block. The point we picked as a takeoff point to swing out and Y off of was a decision made primarily, I think, based on information that we had gained on the fracture orientation and densities and not so much on other points.

There was an advantage in terms of rock competence at that time. Quite literally, Mike, we had gained a lot of knowledge and lost a lot of confidence in our ability to drive a competent tunnel with a TBM. I mean, people look back on that tunnel-boring drive as being a slick operation. But if you walk that tunnel and look at the degree of steel sets and lagging that are in there, the truth of that was, especially in the north ramp drive, there were times when we wondered if we were going to be able to proceed. The amount of blocking and lagging we were having to do—up above that structure, we were ratting

out. I know there were places where we ratted out maybe 80 feet above the TBM profile. It was just a terrible thing to do.

As to the location of getting across that block with some degree of desire to where we were going to try to contact as close to the Solitario as we could on the other side—I wasn't in the decision-making process entirely, but we had our inputs to that. As I recall, a lot of it was our understanding of just what we had learned in mapping fracture densities, competent rock, for TBMs.

MV: It's amazing to me how you trigger things in my memory and I trigger things in your memory. You brought up something that none of us, I believe, have talked about, which is the trouble we had with the tunnel-boring machine in the north ramp. Maybe you could talk a little bit about wanting to start the tunnel in relatively competent rock, but then having to go through a valley where there was so little cover. Maybe you could explain a little bit about why those problems were there.

NE: I felt that the takeoff that we needed was not going to be at repository horizon. In other words, the dead-level part of that loop was a bad place to try to start off that. We needed to be up the north ramp or the south to get that full exposure of cross section we were after.

And we finally hit a zone where we made good time. In fact, this is a weird remembrance, but I believe that DOE and Kiewit particularly, and Morrison-Knudsen, took credit for setting some TBM driving records. As much trouble as we had in the north ramp, we had that section where it went spectacularly well. It was in that zone where we had good competent rock that we targeted and said, "There's your takeoff point."

MV: The other thing I wanted to talk about was that the main drift runs north-south and we've talked about an east-west drift. You and I were both very strong proponents of why

you didn't want to go east-west—there was a better way to do it. Maybe you might want to talk a little bit about why that drift is laid out the way it is.

NE: The east-west drift, the cross-drift?

MV: The cross-drift, yes. It was the perfect layout to expose the entire geologic section of the Topopah, and it could not have been done going east-west—it had to be at that strange angle.

NE: That's right. I mean, you had things that were just so basic, such as, how steep could you run a drive on that TBM and still support it? You had to have the combination of an amenable degree of slope and still get the whole target horizon across there. So the tweaking of the angle had to do with the fact that you couldn't dive that thing too steep but we wanted to get that entire Topopah horizon—we felt it was important to get it.

And that orientation ended up getting us that with a performance confirmation as well. Now we're back into that gray area. I remember a lot of discussions about the ability to take advantage of having that drift later on because it would underlie the primary panel layout for the repository as it was designed at that time. You'd have under-access between the Calico Hills and the repository and could do a lot of monitoring and performance confirmation from that drift.

MV: The cross-drift is above the repository horizon.

NE: That's right. And we just felt that it was going to be good.

MV: The access that you had.

NE: Absolutely.

MV: You wouldn't be interfering with repository operations.

NE: Not at all.

MV: And you could get bore holes and things into where you needed to be.

NE: Exactly right.

MV: Okay. Now, you talked about the lower lithophysal rock. Maybe you could expound a little bit on what you meant and where the repository horizon was relative to the different types of Topopah rock.

NE: I'm not the geologist, but the rate of cooling of that tuff and the pressures that were there gave rise to very distinct horizons. What we considered the easy answer was a monolithic pyroclastic flow of tuff. As it cooled, it was anything but uniform. One of the main characteristics that distinguished that was the amount of bubbles, or actual vugs, called lithophysae —the amount of void. The way the repository was being laid out at that time, the actual herringbone configuration of a repository would have been, I think, around 80 percent in the lower lithophysal rock. And the lithophysae that were there, the fact that there was pore void space, we felt would have a significant effect on thermal and hydrologic processes of drying and returning moisture as the heat changed.

The science community felt, first of all, that you're missing being able to test the repository horizon if we don't. And second, you had to look at that lower lith because there was true belief that there would be different performance in the lower lith than there would have been in non-lithophysal or upper lith.

MV: Just to clarify, the main ESF was in the middle non-lithophysal rocks.

NE: Yes, exactly. We did get a fairly good look at the upper lith—there is a lithophysal zone above. But we were in a spot where, from one standpoint, you'd say it was fairly uniform, and it was. But it wasn't in the horizon as you developed these flat panels because there was a slant to that. A lot of people didn't understand that. Well, it should have been where your main drive was. If you were going to develop a repository off that, it would have been in the middle non-lith. No, as you began to develop off that,

you quickly got into the lower lith.

MV: That's exactly why, when we did Alcove 5, we had to go down before we came back to stay in the rock we wanted to be in. Now, unless you've got something else you wanted to bring up about how we got to the overall structure that we had for the ESF, I'd like to move into the testing program.

NE: We can come back if I think of something, but I think we've given it fairly good coverage.

CHAPTER SIX

MV: The best way to approach talking about the testing program might be to take it by alcoves. Do you want to highlight for us what the alcoves and niches were for, and maybe end up with the test to end all tests?

NE: First of all, in the original, when we came out of the ESF alternative study and we finished test plan 91-5, at that layout, I think there were 13 alcoves identified for different test configurations. And I think there were alcoves done after I left, Mike. But in my recollection, we had seven, at most eight, alcoves that were ultimately developed.

There was evolution and change as we went forward and tested. And there were cost and time drivers. Instead of putting in a whole new alcove complex, could we deepen an alcove, collocate two tests, look back at the interference-related issues?

MV: Here we are using jargon again—what's an alcove?

NE: An alcove is an excavation that is orthogonal to or off of your main drive. It's like a room, and it's a room developed, specifically in this case, for our test program, because there were a lot of tests that you could not do in the main tunnel, even though it was a massive 25-foot drive. You weren't going to set up permanent equipment. You had to have air flow, you had the track laid for the train access—you didn't want PIs standing around dodging trains.

So you had to get this test away, and you also wanted to get out of the disturbance zone that we created with the TBM. We thought smaller alcoves made less impact on developing a disturbed rock zone, depending on the way we mined them. We did a lot of controlled mining—controlled mining was a big part of what we did for the primary purpose of trying to minimize how much you were affecting the native state of the rock

so that the tests would not be testing disturbed rock. To the extent you could, you wanted that data to represent non-disturbed rock. We did that with a combination of mining techniques and we also did it with our bore hole configurations. A lot of boring, a lot of drilling—a lot of this test program wasn't done on the face that you excavated, it was done into the rock mass to further avoid disturbed rock impacts.

But when we started, the only drill and shoot that we had in the ESF was the launch chamber. I remember, none of us could stand on the pad—I'll bet we were half a mile out there in the desert. [MV laughs] We did what was called the first center burn—a center burn is simply a way to make a place for rock to break into. We had to burn a hole in the center, then we began to break rock, and we developed a launching chamber—the starter tunnel—which was done with drill and shoot. I'm trying to remember if we directly used alpine miners in the development of that—I think we shot the whole damn thing.

MV: I think you shot it.

NE: That's my recollection. This is not probably the best reason in the world, but again, I think, to some degree there was a political driver for that. We always wanted to have a test program in the brow, the rock in front of and above the TBM, but as we were developing the starter tunnel, instead of developing it completely and outfitting it, then looking at an alcove, we put that first alcove in real time. As much as I'd like to say there were good technical drivers for that, I believe there was a desire to show that the underground test program was underway.

So we were primarily focused in Alcove 1. Our primary focus was hydrology near surface right as we went into the tunnel. USGS had a big part of that program in permeability tests. We also developed an actual turn, or a dogleg, in that alcove to expose

an isolated corner where we had two faces. Sandia did an extensive series in that of not only permeability, but excavation impacts and cross-hole testing using tracers. So we had a big Sandia program on thermal mechanics and some transmissivity and permeability testing by the USGS; we were underway with that before the TBM ever even came underground.

The next alcove that we wanted to do—there were originally going to be two, and we ended up just doing one. Fairly soon after you went to the TBM transition at the end of the starter tunnel, we had the first significant fault contact. From the beginning of time, there had been significant issues. That was a pretty good displacement fault—80 feet or something of displacement.

There were lots of very good base science questions—what was going to happen across that interface? So the goal was understanding the big faults that we were going to encounter, and that one was expected to be the biggest. I don't think it turned out to be the most problematic, but it was expected to be the best contact we had.

We drove an alcove complex there that was smaller than Alcove 1 in terms of cross-diameter, but it was done in such a way that we actually cut across that and got real good fault contact in that alcove. Again, primarily it was to examine what was going to go on hydrologically across that displacement fault.

We were in very soon after the TBM pass—we didn't put Alcove 2 in until TBM operations were going. Alcove 1 and Alcove 2 were our primary test locations in the underground; for the most part, really, the most significant thing we did until we reached the bottom of the north ramp and made the turn. There were other small alcoves, primarily hydrology-related, as we passed the different units. We didn't want to look at the upper lith. We wanted to look at the contact between the upper lith and the middle

non-lith.

So we ran a series of tests and I can remember the small alcoves, 3 and 4, were not big developments. USGS did some hanging bottle (constant head) tests—as you remember, there were some bizarre things done in some of those. And I'm not going to defend [laughs] the science; you can go to the USGS for that.

When we began to get into some of the respiratory concerns we went back to those alcoves and developed them as formal refuge chambers so they had a non-testing significance in the latter part of the program.

The largest activity, really, in terms of most consistent and biggest effort was the mapping and mineralogy-petrology program that we did on the TBM, not in the alcoves. But we rode that TBM shift by shift, hour by hour, day by day.

MV: It was unusual for a mining operation to have a car devoted to the scientists.

NE: And maybe that is something we stepped back on. Part of the complication on the design of this TBM is that we spent hundreds of hours helping the designers of that machine understand what access was going to be needed. There were lift platforms and accesses on that TBM that no one had ever thought about on a tunnel-boring machine before because it was so important to have our nose on the rock as we excavated.

MV: That's a really good point—this was not a tunnel-boring machine used to excavate a tunnel—this was a tunnel-boring machine used to support a science program.

NE: Perfectly said; that's what it was. There were entire sections of that train gear that were only there because we had to run this test program. There were other things, but the access for mapping in detail in real time as we went through there was primary, trying to catch as much as we could. We didn't want to come in and map afterwards. I've heard different stories over the years as to why, but in any mining activity, you get a huge

percentage of your rock reaction at the time you mine it and then that tails off, and you certainly get long-term response. But we believed it was essential that, as quickly as possible after the rock had been relaxed after you cut it, that you did your mapping then and didn't wait six months.

MV: I'm going to interrupt you once more. People often talk about water dripping in the tunnel—there are actually state of Nevada people who have said they stood in the tunnel with water dripping on their head. Do you want to comment on that? Nobody I know spent more hours in that tunnel than you did—how much water did you see dripping when you were excavating the tunnel?

NE: There was never a drop of natural water in that thing, in my opinion. And you're right, I spent as many hours in that tunnel as anyone during the excavation and after.

MV: There was a big rainstorm and where the tunnel was very near the surface on the south ramp, and they thought they had water coming in from the surface.

NE: I heard that. Again, that was after I had left. There was a lot of interest in water right near the brow there. There was a concern that we had fractured basically all the way through the brow, all the way to the top. Hemi Kalia on my staff was probably the primary person who was pushing those concerns at that time. We did not share them in the overall test coordination group, but we felt that the concern had to be fairly heard, and DOE and its technical family had to make a decision.

We did a lot on the surface. I can remember Flint, I think it was, and Tom Buscheck cut a series of trenches. I think there was an early trench that you-all had done long before we even started. But we came in and cut at least several new trenches on the top because of great concerns that it was going to rain in right on the north portal.

My recollection of the outcome with Hemi's concern was that it wasn't that badly

fractured. What we were seeing and assuming were stress fractures developed from the cutting were actually a part of the normal fracturing in the mountain. But I remember a tremendous amount of interest we were getting underground related to hydrology in Alcove 1. We did a lot of testing in that first alcove. But we did a lot on the surface up over the brow and that got exacerbated—believe it or not, there were times when people were afraid, when we were having the excavation issues in the north ramp, that there was a possibility we could daylight.

I used to sit there and say I don't think there's a way in the world you're ever going to daylight. But once those concerns were brought up, you could not just ignore them; we had to take every one of them seriously. We spent, I remember, a lot of effort and concern fearing that on the backside of the brow on that side you could actually daylight the excavation.

MV: You're reminding of the Dave Dobson observation. I loved to listen to him talking about the scientists. He would say something like, "Our seismologists don't think we have a seismology problem, our hydrologists think we have a seismology problem. Our hydrologists don't think we have a hydrology problem, our seismologists think we have a hydrology problem." And that's what you were dealing with—PIs who were out there every day looking at various things.

NE: [Laughs] That's right. And even though they may not have had much to do with each other's tests they were free with opinions, and they would always be more critical of the test in the next alcove. It was just part of the dynamic that we had to deal with. But these weren't people off the streets, they were career professionals, scientists. If one of them had a concern, as much as you or I or anyone might have thought, "That's just total crap," you couldn't be that cavalier about anything like that.

MV: Not in the environment we were in.

NE: No, sir, we had to take each and every one of them very seriously.

So that, to me, was the north ramp. I have some recollections on the TBM and the programs; we did modify things over time. We never were able to implement all the science off the TBM that we had hoped we could but the design was heavily driven by PIs and access. And there was a tough interface, always. That machine was Peter Kiewit's domain, with Morrison-Knudsen managing it. I always had more interface problems trying to do science on the TBM. Once we had an alcove, we were kind of out of the profile. But we had an extremely lively dialogue on a daily basis between what we were doing on that TBM and that construction.

MV: And that's because, even though we have agreed that the tunnel-boring machine was there to support a science program, you put miners on a tunnel-boring machine and they're interested in mining.

NE: That's right. And at the end of the day, as far as the envelope in terms of safety and the conduct of operations, there's no way that we could turn that over and say, "That's a PI organization's responsibility." At the end of day, safety and conduct of ops belonged to the M&O, or the excavation group.

Peter Kiewit felt that we were at best, from a science standpoint, a necessary evil in this. They approached this like any other tunneling job: "Stay the hell out of our way—we'll drive you a tunnel. But if you keep these damn PIs down here, you're just . . ." [MV laughs] There were times when Kiewit would stop work and say, "We won't continue until we work this out." And that was exactly the right thing to do—"Let's stop, let's pause." We had a thousand meetings in the old trailers up there, sitting with a few PIs and some very hot construction people saying, "What they want to do and what we're going

to do don’t inhabit the same universe." We had to work through those, and we did. It was a success story. No one said it was going to be easy, but I think that we did an outstanding job of running that program.

MV: You used another term, the test coordination office. What was the difference between the exploratory studies facility testing planning committee group and the TCO, the test coordination office?

NE: On the one hand, there were two entities involved; on the other, it became just the one. The test planning part of this had to involve the science community. That was the interface—what do you really need? How do we supply that? How do we establish those functional requirements and how do we implement them through an operational basis? That was a very broad committee that included each and every one of the PIs and their test organizations and the construction entity and the design group. That was a very dynamic group during those times.

When that collapsed down to a plan, for the most part, the PI organization was not there when we actually did what we did. That was the test coordination office—they represented the needs and requirements of the PI organization. As I said before, other than mapping, I’m not sure that a single PI resided in Nevada. These people traveled in from the national laboratory structures—or from Golden, Colorado, for the USGS.

The importance of having the test management group, to me, was borne out in the fact that these test beds—the alcoves, the test preparation—could be done without having the PI on travel sitting there all that time. He could reside wherever he was and be confident that that interface was being taken care of. It allowed the design groups and the construction groups to minimize the noise of how many people they were having to try to keep satisfied at one time.

I always felt it was one of the most important facets of running that kind of a complex test program, to have a group that knew the testing that the construction people could speak to. The people that I brought in, for the most part, all had actual tunneling or mining experience—not just a scientist's background, but also they had the interface element. As I said before, I think it made me kind of unique—I could spit tobacco and wear a pair of overalls and get right in the nose of a construction guy's face, but I could clean up and go into a room and talk to regulators or the NRC or the science-based organizations. And we tried to have a group of people who pretty much understood that all across the board.

I'll go back to the original question on the difference between the groups. The test management group were not the PIs, but they had to be enough of a scientist to understand what that test really required. They would be the ones that would do the interfacing.

The other thing that had to happen is the test organizations had to have an implicit trust of the test management group. If they didn't trust that the test management group was there for them and was going to ensure their needs were met, the project would have come apart. One of the good things that I can say, looking back on it all, is I think we had an outstanding relationship between the actual scientific organizations—and there were so many of those organizations involved—and the construction design side.

I don't think we ever lost the trust of the science organizations. In fact, there were a lot of times when I felt that outsiders mistook the test management group for the test program because we were the ones you saw and we were speaking confidently about what was going on. The ownership of the science going on in there was never the group that I had. We didn't own the science—we owned that interface. It was a compliment that

people put that big of a weight on us; we became kind of the pseudo test organization, but that wasn't our role.

MV: You've given us the perfect opportunity to talk about what it meant to manage a whole lot of PIs on a single test. Do you want to talk about Alcove 5?

NE: From the beginning, Alcove 5 loomed as the big litmus test for everything that we were learning anywhere else. That test was the drift scale test, and it was that test that was not owned by any organization—it was truly the project's test. All of the organizations came together in the one test we asked them to work together on. I felt that that was also the epitome of what DOE had wanted—there was great value in being able to take someone down and show them what was going on. I mean, consider the fact that we had to put a window in so people could look at that test.

MV: What was the drift scale test?

NE: The drift scale test was a demonstration of an actual repository concept, where you were going to simulate the waste and waste-generated heat. A primary characteristic looked at in that test in terms of sensile effects with that mountain was heat. So we wanted a drift that was much longer than the standard—it was long enough to not have edge effects or boundary effects. It had to be a fairly long tunnel and it had to be heated throughout.

It was going to look simultaneously at thermal mechanical change as well as hydrologic change so it was a bore hole-intensive test with no access to the bore holes inside. So we had to develop a design that allowed a companion drift. The access drift getting down to that test location became a very important drift for an array of bore holes that went out over and below and beyond the actual heated drift. We had instrumentation stuck in from every configuration and orientation around that thing, knowing that for

many years, no one was going to get in there.

Again, this shows the importance of the political aspects. It was not easy to design an actual window that we didn't feel was either interfering or was a safety issue because we wanted people to be able to come and put their nose on that glass and look in there and see that row of inline heaters. It was an impressive set of canisters for those heaters, very big—these were large-sized heaters.

MV: How long was the drift?

NE: The drift was about 450 feet long, I think. And we had nine inline heaters—each of those heaters was almost 30 feet long—in huge canisters with space in between and some buffering on both ends, so it was an extremely long drift.

MV: I think there were 6,000 channels of instrumentation?

NE: It was multiple thousands—I wouldn't argue with that number, Mike. One of the big challenges we had was data collection. Anywhere else in the ESF, data collection and the assurance of quality in that process was—I won't say trivial, but easily managed.

But because of the sheer numbers of channels, the different types of instrumentation, and having to have capability for data collection and storage of that data, this test was truly a marvel of data collection. We had our own part of that—a little stub alcove that was simply and only data acquisition—it was a world of its own.

We did practice, if you remember. As we went down Alcove 5, we actually put a single heater test in and we learned a lot in that process. Even though there was valid science, to me, the main reason to have done that was to practice a lot of what we were going to do later.

MV: There's a timing point, too—this is just about the time of the site recommendation. My recollection was that with the single heater test you were doing

multiple things—you were getting practice, you were getting information that you could use to refine the predictive models for the bigger test, and you had real data coming out to support the site recommendation.

NE: Exactly right; so that timing was very critical for all of those reasons. From the time that we originally envisioned the ESF, from the time that we came out of the alternative study, the final design was just in time. We were still tweaking and changing. I think you might remember some of those early design tweaks, where we had drifts above, drifts below—that was a Livermore vision. When I first saw it, I said, "That's a nightmare that is never going to happen." And so coming up with the dipping access, getting down into the lower lith, taking advantage of the opportunity to alcove off and do a single test, taking advantage of our bore hole arrays—it was a very elegant design.

My mark of pride in leaving is that, of all the tests at that time, I think it was the most complex geotechnical test ever done anywhere in the world.

MV: I still think it is.

NE: It may still be. And because we were prototypical, there was no "go buy a heater off the shelf." These heaters had to be very minutely detailed in design on the performance criteria. There was redundancy because losing one halfway through the damn test was not an option. The complexity of the heater systems, the data acquisition systems, the control that we had to have on our drilling programs—we had learned all the way down. Drilling a line-of-sight hole in some of these holes was essential—you couldn't have any deviation.

Had we started with that test we'd have failed miserably, but we had two years of good data. We had learned a lot, and those lessons learned culminated in flipping the heaters on for that test ahead of schedule and within the budget. I think that was a

remarkable compliment to all of the test organizations that were there, to the construction entities, the design organizations. That test at least started well.

MV: And it ended well, Ned. The only comment I would make to you in retrospect is there were probably more channels of data collected than they could actually handle.

NE: I would not argue that. We knew that was going to be a daunting challenge when we started.

MV: I'd rather have too much than too little.

NE: And we knew that going in. Again, we recognized that if we lost some, we wouldn't lose anything critical because there was a lot of redundancy and a lot of overlap. As I recall, we flipped those heaters on before Thanksgiving 1997. I believe it was a November start, and I was gone from the program the following February. So when we got that test going, I was at the end of my time. But I look back on it as still the absolute culmination example of what that test program was.

MV: Okay. Any other points you'd like to make about the underground test program? Here's one for you to react to—I tell this to people all the time, that yes, it was a very expensive program. It costs us a lot to go underground, it cost us a lot to field-test underground. A test the size of the Alcove 5 test was a first-of-a-kind accomplishment.

NE: That's right.

MV: How do you go the Nuclear Regulatory Commission and convince them that a model is appropriate if you haven't done underground work? From my perspective, what you did in the ESF could not have been done from bore holes, could not have been measured at the surface. We had to go underground.

NE: I guess that would have been better coming as a comment from me, but all I can say is amen. The actual ability to do these tests in many cases had to have hands-on

access from an alcove or underground—you could not have done it from a surface drilling program.

One of the other things that I evolved into in the years that I was there was not just the test manager, I also became the spokesperson for the program. I mean, how many tours did you and I lead together?

MV: Yes. [Laughs]

NE: You were the big picture in compliance and regulation and direction, and I had my little focus. But that test program was the proof of principle. Without the underground test program, the public, the politicians, the people who came there could have stood on the surface with a drilling rig and you could have told them what was down that bore hole, and they would have gained no confidence. I watched hundreds of pessimists and naysayers, people who were against the program, go down there. The department's commitment to that empirical data collection at the actual location of the repository underground, I think, gave the public much more confidence in them.

I know that where the data ended up in the models that were codified in the performance assessment was critical. But I'll tell you that I think there was an almost equal value in the visual demonstration of that commitment of a science-led program—it was not a political set of decisions. People went down there and said, DOE is being driven by the science that they learn here, and we have confidence in that. You experienced that even more than I did, and for a much longer period of time. But I had some of our worst critics go down there and come out and say, "Why not here? I don't see why not."

CHAPTER SEVEN

MV: I couldn't have asked for a better lead-in to one of the questions I wanted to ask you. Was Yucca Mountain a good site, was it workable?

NE: I've been gone 13 years, and I'm heavily invested in a salt program here at WIPP, but from a technical standpoint, I still believe today, as I believed the years that I was there, that Yucca Mountain is a workable site.

MV: Now, you left the Yucca Mountain project and came home to work on WIPP. Do you want to remark on any comparisons between the programs?

NE: The Los Alamos program here is totally different. It's not a test management program, it's a waste certification, characterization, and transportation program. But because of my love of the underground and my enthusiasm for the program that I'm in, over time, the Department of Energy and the other organizations have said, "There's a good spokesperson; there's a good person to lead those tours." It's the same reason you have been that voice for Yucca Mountain for so many years.

I use shock value a lot in my life to get people to listen or to tweak them, and then I'll back off. In the thousand times since I've been here in salt that I've had people come who were familiar with or at least knowledgeable about Yucca Mountain, almost invariably they ask about that comparison. As recently as last weekend, Representative Clyburn from South Carolina was here (DOE asked me to lead that tour, and that was an honor).

I tell people when they ask the invariable question that I might be (this might be wrong, but no one has challenged me yet) the only person on the face of the earth who has spent more than a decade at both of the United States repository programs.

And that's important, because people love the comparison. And when they ask me, just for the shock value, I often tell them the similarity is they're both holes in the ground. I mean, it's much easier to talk differences than similarities between these two. I felt that was important because right out of the chute, the missions of these two programs are extremely different.

WIPP in the '70s was looked at as a possibility for housing both of these programs. But the will of the land, the policy of the land, the law of the land said "We're going to separate them." And the mission of Yucca Mountain was very different than the mission of WIPP. So that shock value gets people at least not trying to find too many correlations.

I was always a little disappointed, both in my years in Yucca, and I have been here, that we stovepiped these two things. I don't think there was the synergy and interaction between the two repository programs that I would have liked to have seen. I felt that Yucca could learn from WIPP and WIPP could learn from Yucca. There were no real good cross-pollinating programs for lessons learned. I've had many people say, "Well, you've told me there's very little similarity, so why would it have been important?" It's a good point, it's a valid point.

But the missions were different. The stage, or the level of development, of the two programs was entirely different. We were in a site characterization phase at Yucca when I was there with the license application coming up. Here, it wasn't licensed at all—this was not an NRC license, this was actually a self-regulated facility—DOE self-regulating, with EPA in the oversight role. It didn't require licensing—it required certification and recertification. They had an oversight lead regulator, but in a different role than NRC was going to perform for Yucca Mountain.

The medium is different at WIPP. The operational standard that you have to meet was entirely different. When I get down to it and people really ask me what is that main difference, the main difference, in my opinion, Mike, is because of a lot that we've talked about and a lot we haven't. Yucca Mountain always had a very heavy dependence on an engineered system. WIPP is almost 180 degrees the other way—it has almost a total dependence on the natural system. I don't consider shaft seals and things like that to be engineered barriers in the sense that we're asked to implement an engineered barrier. The only engineered system for repository enhancement and performance here is the addition of magnesium oxide—that's it. It's an addition of a prill on top of a stack of waste.

You compare that to what we have gone through in the development of Yucca Mountain, in the canisters and the drip shields—a very different degree of dependence on natural versus engineered systems. The performance timeframe is very different—10,000 years here, with up to a one-million-year standard at Yucca Mountain.

I think that I have always spent more time on the differences. When people talk about the similarities, I've always struggled a little more—what can you really talk about? They're both deep geologic repository systems so there are certainly similarities, but I've always felt it better to dwell on why they're not one and the same than trying to bring them together and show that they're similar.

MV: I also think there are significant differences between the two of them in the political aspect.

NE: Absolutely. Maybe I would ask you to help me understand those because I'm sure you have a more in-depth understanding. You certainly understand the political side of Yucca Mountain better. I'm not sure that I even understand the political side of WIPP that well.

MV: I'll give you two things that just drive me crazy. There were issues with WIPP early on, political issues.

NE: Absolutely.

MV: They found a way through those. Yes, there's local community support, but there's a lot of resistance in the state of New Mexico. They found a way to get a consultation and cooperation agreement worked out. They found a way to make it go forward. They've never been able to get the state of Nevada to come to the table to talk about a cooperative agreement. One of the reasons is because when the Nuclear Waste Policy Act was promulgated, there was a commitment that no one state would have to take all that waste. That commitment was broken when they amended the act.

I will never tell you Yucca was picked fairly, but I will tell you I still think Yucca was a good site. We got there because the USGS wrote a lot of papers on the advantages of the unsaturated zone, a very different approach to geologic disposal from what people have historically thought about salt.

And today, we have a senate majority leader who is adamantly opposed to Yucca Mountain. If you listen to him talk, you will conclude he believes there are technical problems with the site. I just don't understand how we were able to get one group to the table to talk about it and work on the differences and not the other.

NE: That's the big question here. I admit that the political side was never my focus, but when I rotated from Yucca Mountain and came here to New Mexico, one thing that I saw as different was a demographic flip-flop between Nevada and here. This site was geographically and demographically fairly isolated in New Mexico.

The resistance that came was in the major population centers in the Rio Grande corridor in the northern part of the state. It helped, to some degree, that they realized that

it's 300-plus miles away down there. Whereas at Yucca Mountain, we were literally in the back door of Clark County. It became apparent to me that that played a role.

The other thing that I can't answer, because I wasn't in Yucca Mountain in the formative years, is the Department of Energy was very open to working with New Mexico, and what can we do to help you here? Last year, $28 million of the money that came to this program was immediately turned into highway work and infrastructure in this state.

I never saw the equivalent of Nevada being able to stand up and say at least here's something that we are gaining that helps this state—that helps it economically, that helps it provide a future. I was watching the threat game of, "We'll pull that bomber wing out of Nellis." It was an ugly deal.

I wasn't here in the beginning of WIPP—I wasn't there at the beginning of Yucca—but in the end result, at least there are programs where the state and local community are shown to be benefiting very directly from WIPP.

MV: I couldn't agree with you more, Ned. That's that consultation cooperation agreement—money is given to the communities here because they ask for it. The state of Nevada won't even ask for it. They use that to say, "See, the government won't give us anything." I don't know how to get past that.

NE: I don't either, and I don't know how it developed. I don't know if, in the early days, it was a different model and something went south.

MV: It was the Nuclear Waste Policy Act amendment breaking the fundamental commitment that no one state would have to take all of the waste that really started Richard Bryan on the pathway that's being followed by the current politicians in Nevada today. You can't argue against that—it was not fair.

NE: I understand. WIPP has stayed extremely true to the original. It hasn't changed dramatically from what the Department of Energy came in here and said WIPP was going to be. I think that the state of New Mexico would engage very aggressively with the Department of Energy if that mission was considered for expansion, or a new mission involving high-level waste was proposed. I am not saying the state would oppose an expanded role in waste disposal in New Mexico, but they would visibly and actively engage the process to ensure the plan was safe and scientifically sound, and they would want an enduring say in the process. I won't conjecture too much on it, but it is different. The mission here was defense transuranics when it started, and the mission here is still defense transuranics today.

MV: What do you think about the political situation in Nevada right now? I don't want to ask you if Yucca Mountain is dead, but you've watched both of these programs—is there hope for the high-level waste, spent nuclear fuel programs, in this country?

NE: Well, absolutely. And I say that coming from both perspectives. No. 1, I don't believe that I'm out of the box or making a politically sensitive statement when I say that politics is a large part of the decision we've ended up with at Yucca Mountain. Well, politics shift, administrations change, and ruling parties in Congress change. I cannot sit here and say with certainty that walking away from Yucca Mountain is a decision that this nation will continue with. I think there is an opportunity to reverse some of those, at least the political drivers.

I don't know how successful that process will be in terms of how well received in the state of Nevada or nationally it would be. I will say that I see real awareness of the importance of nuclear power, and the fact that you have to have a back-end solution.

Tragically, I think world events in the last few months, what we have watched

happen in Fukushima, Japan—I tell people that I'm afraid that we may see a real increase in solving that back-end problem now at the cost of being less willing to advance the front end of that process. Nuclear power, I'm afraid, may suffer mightily. And yet, programs like WIPP, and ultimately maybe Yucca Mountain, may actually be more on the radar screen because Fukushima showed us that we can't, as a society, keep mortgaging or putting those answers off to a future generation. Beyond that, Mike, I'm not much of a political thinker.

But I know that in the years I was committed to Yucca Mountain with the Department of Energy, I believed every day that we had a good site. There is nothing that has changed that, in my opinion. But I do believe—and maybe this is where I begin to stray from the fold to some degree—that there are other options that could be looked at. I never said, even when I was there, that Yucca Mountain is the only answer there could ever be, or is the complete answer by itself.

Watching and being a part of this success story here at WIPP, I believe that salt is a medium that ought to be looked at. There may be some time and cost advantages to exploring that option as opposed to starting cold and going out and saying, "Where in the hell could I find a site to even look at the amenability?"

As you and I have talked many times, I never saw either salt or Yucca Mountain as the only possible answer, and I certainly have never seen them as mutually exclusive, one or the other. I think that we need to embark on a very robust program. If Yucca Mountain were to come back, if that was the Department's decision and they were to say let's go that way, I would have no problem with that and would have the same confidence I had in the years that I was there that it's a good answer. I never saw a great technical flaw at Yucca Mountain.

MV: Anything else you'd like to say?

NE: Going back to the beginning of this, I feel blessed that, with all the wrong background, and probably all the wrong motives, I was led by circumstance and personal desire to engage in what is now 25 years in this nation's programs to look at safe and secure ways to close that back end of the fuel cycle. I couldn't have asked for a better career. I will always look back and say it wasn't a wasted career. I believe that the trials, the tribulations, the struggles were worth it in the beginning and they are worth it today. I truly believe that these are nationally important programs and you don't have to say, "Why do we really care about that?" It's been a great ride. I wouldn't have taken anything for the Yucca Mountain program and the science program that we did there.

I have huge pride in what we've done here in defense transuranics in salt, and I'm a big proponent of looking at the ability to work that into the solutions for future answers, but not at the exclusivity of saying it's the only answer, and certainly not saying it's an answer that you ought to do instead of Yucca Mountain. The right answer is in a combination of solutions for different parts of the problem. We require diversity.

MV: I've often said to people that I can count on one hand, maybe two hands, the people that made significant contributions to advancing this program, and you're one of them. Cindy is one of them too, for letting you do this.

NE: [Laughs] Yes, you know how that goes. It's a commitment of your family. There have been a lot of times when I know I probably have gone over the top in terms of the time that I've spent and the amount I will sacrifice in my private life and family life. But I'd do it again. I guess that's what I'm saying—I wouldn't have changed that.

I might be stepping on some toes, but if I had to pick the one most important person in that program up there, I'm looking at him—that was you.

MV: We need to turn this tape recorder off, Ned. [Laughs]

NE: If nothing else Mike, the longevity of it. You started, when, in '81?

MV: I worked on the review of the near-surface test facility up at Hanford in 1975.

NE: I'm talking about Yucca.

MV: Yucca, '81.

NE: Well, '81, and you're still Yucca today. You've got a Nye County focus, but you're still that go-to resource. No one can touch that longevity. For someone who did it, and it meant as much to, to put me in a group of 10 or 12, that's just as great a compliment as I could have. I thought that my role was very narrow; it was extremely focused. But it happened to be in the focus area that I think it was all about.

MV: This program moved forward because of the underground test program, and that was you. If you say underground test program at Yucca Mountain, your picture will be on that book, there's just no two ways about it.

NE: I appreciate that, and to some degree I am arrogant enough to say that, looking back, I believe the combination of personality and drive, and the ability to get people to work together, the ability to be articulate enough to sell a point to an audience of Ph.D. scientists and to a bunch of people from Nye County there who cared about it—I have always been able to gauge a discussion that's meaningful for the group I am talking to and give them what I thought was right.

Given all of that, I do look back and say I don't think there was a better person at the right place at the right time to implement that test program than I turned out to be. I had nothing that prepared me for it, but I've never said, "Jeez, it could've been done better if someone else. . ." I have that kind of ego to say it couldn't have been done any better, and I'm proud of the role that I had there, and the great team of scientists and

underground constructors, and I do believe it was important to the nation.

MV: You should be, Ned. Thank you for taking the time to sit down and talk with us.

NE: Well, thank you for including me.

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