An Interview with

DON VIETH

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 Robert McCracken talking to Don Vieth at his home in Los Alamos, New Mexico, September 13, 14, and 15, 2010.

CHAPTER ONE

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

DV: On my birth certificate my name is Donald Louis Vieth.

RM: When and where were you born?

DV: September 20, 1940, in Cincinnati, Ohio, at Christ Hospital.

RM: What was your mother's full maiden name, and when and where was she born?

DV: Mary Elizabeth Sorrell. She was born in Covington, Kentucky, on May 23, 1912.

RM: Covington is across the river from Cincinnati?

DV: Yes.

RM: What did her family do for a living?

DV: Her father was an engineer on the Louisville and Nashville Railroad, based in Butler, Kentucky. He had the run between Cincinnati and Corbin for a long time—fast freights, then eventually passenger trains with steam, and ended up with diesels.

RM: Where's Corbin?

DV: Corbin's in the southern part of the state of Kentucky, on Interstate 75.

RM: What was your father's name?

DV: My father was Edward Louis Vieth.

RM: When and where was he born?

DV: He was born on Isabella Street in Newport, Kentucky, in 1908. Newport's across the river from Cincinnati. It's also across the Licking River from Covington, Kentucky.

RM: What did his family do for a living?

DV: My grandfather was an insurance agent. He did a lot of things. He also was a timekeeper at the Wiedemann Brewery. He managed semi-professional baseball teams in the area.

RM: What did your family do to support the family?

DV: My father was good in science—physics and chemistry. In 1926, when he graduated from high school, he went to the University of Kentucky at Lexington and studied mechanical engineering. He graduated in 1930.

RM: Not the best time to come out on the job market.

DV: No, it wasn't.

RM: What kind of work did he do?

DV: He was a mechanical engineer for Alvey Ferguson—they made conveyor belts and things like that. When the Depression started they proposed having everybody take a 10 percent pay cut. My father thought he was a little bit better than that so he said wasn't going to take a pay cut; he quit and got another job. As the Depression got worse, his life sort of deteriorated as jobs went from bad to worse. It turned out that Alvey Ferguson never laid anybody off during the Depression and the 10 percent pay cut was the worst they took. My dad went from working as an engineer to eventually working for his brother—who quit high school in his sophomore year—in a handkerchief factory.

RM: Was Alvey Ferguson located in Cincinnati?

DV: In Oakley, which is a suburb of Cincinnati.

RM: So you grew up in Cincinnati?

DV: Yes, born and raised there.

RM: And you probably got some of your mathematical/science skills from your dad?

DV: Yes. There’s no doubt that those were my strong suits in high school. I got a lot of tutoring from my father, a lot of education. I didn’t do very well in high school in my freshman year of high school, but as time went on I did much, much better.

RM: When you look back, what stands out in your mind about growing up in Cincinnati?

DV: I though Cincinnati was a great town. The people there were very friendly. When I was growing up, in the ’40s and ’50s, it seemed to be a very safe place. We could walk eight or nine blocks from the house to the movie theater on a Friday night and never have to worry about anything happening to you. It had a good professional baseball team—I used to spend a lot of time on Sundays at double headers.

RM: Who were some of the stars in those days?

DV: In those days it was Roy McMillan and Johnny Temple. Bobby Adams played third base. A guy by the name of Ted Kluszewski played first base. Gus Bell, Wally Post were the outfielders. Smoky Burgess caught in those days. It was fun to go on a Sunday afternoon, take the trolley bus to the ballpark, which was in not the best part of town, and, for $1.50, get to see a double-header. You would take your lunch and sit up behind home plate and have a great time.

I went to Roger Bacon, a Catholic high school, and I got what I considered an extremely good education, well disciplined. The classes were taught by Franciscan priests and they were strong disciplinarians. No fooling around, you paid attention.

My parents were very active in the high school I went to. My father was an all-city football player in high school and was very instrumental in starting the boosters club for the high school’s athletic teams. The family was significantly involved in my education. I went on to college at the University of Cincinnati.

There's a story I tell about my father. We were sitting at the kitchen table in September of 1957, as I was starting my senior year, and he wanted to know what I wanted to study. I had a great general science teacher and he got me incredibly interested in geology. I used to do a lot of reading in the summertime, things from the library, especially crystallography. I told my father I wanted to be a geologist.

My father looked at me and said, "Like hell you are. You're going to be an engineer. It's my job to teach you how to feed a family." He was somebody who had gone through the Depression and found it very tough to get enough food to eat. He told stories about how during the Depression, they would get a lot of canned goods, but the canned goods had been in a flood and there were no labels on them. So the rule was, you opened two cans every night. No matter what it was, you ate it. He thought when he got his degree in engineering he was going to be pretty well set. His intention with me was if you're going to have a family—and to my father, family was incredibly important—you'd damn better learn how to feed them. It was very basic.

RM: And there was no inclination on your part to kind of rebel and say, "No, Dad, I don't want to do engineering, I want to do geology."

DV: No. My father gave me a grubstake to go to college—$1,500. That's what I had to get a college education. I thought, "Well, if I can't study geology, I'll study mining engineering," but all the mining engineering colleges were in the West. I started looking at them and discovered $1,500 would have covered one year of tuition. Obvious conclusion was, "Well, that's not going to work, so maybe we'll look at process engineering for minerals," where you convert the minerals into something useful. Well, that was taught in the West also, so I had the same problem.

Metallurgical engineering was the next one down on the list of options. I thought about that and I said, “That deals with refining materials and so on.” The University of Cincinnati had a metallurgical engineering program. I met some people in the metallurgy business and they got me interested in going there so that’s what I signed up for. At that time, from 1958 through 1963, the tuition at the University of Cincinnati, which was a city college then, was $500 a year. I had $1,500, so that covered three years.

In addition to that, the University of Cincinnati had started the Eng-Co-op program— Cooperative Engineering program—where, after your first year, you would get a job. You would work seven weeks and go to school seven weeks, work seven weeks and go to school seven weeks, and so on. You worked in an area that you were studying so you got some real hands-on experience. Instead of taking four years it took you five years to get through.

I got a great job and it paid me a starting rate of $1.50 per hour, or about $2,000 a year. I had to pay my first year out of the $1,500 so I had $1,000 left. Then I started to receive the $2,000 a year and it was enough to pay for a lot of things and I could start to save. I did that for the remaining four years.

RM: What was the job you had?

DV: I worked for a company called Metcut Research. I’ll send you a paper about it—they’re getting an award this very day as being an historic institution. Their main focus was how to machine metals and to do it in a way that would be effective. If you want to drill one or two holes into something, it’s not too difficult. But if you wanted to drill a thousand holes while manufacturing something, how long would it take before the drill would break? They did a lot of studies. The same kinds of issues applied to turning things on a lathe or a milling machine or grinders and so on. This information was critically important to companies that manufactured products for the Department of Defense.

RM: So there was a significant level of science involved?

DV: Oh, yes. The men I worked for were the three who started the company—Mike Field, Norm Zlatin, and a fellow named John Kahles. They started in 1950 and I showed up about 1959 so I was very early on in its development. It was a great opportunity because not only did I do machinability, I did mechanical testing, I designed stuff; I spent time on the drawing board designing things. I was also the artist who did the graphic arts for the reports. I worked in the metallography laboratory doing failure analysis. I sometimes had to work in the darkroom printing the photographs that would go in the final report. In all, it was a great opportunity to learn, to develop skills, and to know the importance of getting the job done correctly.

RM: It sounds like a college education in itself.

DV: It was a much broadened and practical education about how to work with people at different levels. The most important thing was, even though I was going to be a college graduate, I was working with guys that were machinists, technicians, and general office staff. They might not have been as formally educated as I was, but they were very smart and they knew how to do things. While I took a lot of hazing from them, they were very good at teaching me practical skills on how to make things work. That type of education, getting the academics along with the practical aspects of how to make life work, was incredibly important to me.

RM: It sounds like an ideal educational situation.

DV: Oh, yes. Not only did you learn how to do things, you learned how to get along with a lot of different kinds of people. You also got the sense that just because you have a college degree, that doesn't mean you know everything there is to know about getting the job done. That was possibly the most important part.

The thing that the university did for me was it taught me how to think. It was very demanding and the teachers I had always focused on really understanding the basic principles of what you were working on, and how to use your knowledge of those basic principles to understand the problems you were facing and what part of chemistry or physics or whatever was going to be the controlling factor in making sure that you were able to get something done properly.

My father was a very a smart engineer. He always said that only about 10 percent of engineers understand the chemistry and physics that underlie engineering, and it’s those 10 percent that really make the difference. The others can take the handbooks and are smart enough to make things work well, but they’re not going to be the ones on the cutting edge of developing things and finding new aspects of technology.

RM: What was the population in Cincinnati when you started college?

DV: Cincinnati, the city proper, was about 500,000 people. Its population is about 460,000 today, but the metropolitan area is about three million.

RM: You didn’t have that metropolitan area then, did you?

DV: Oh, you did, but it was little towns that surrounded the city. For example, Covington, Kentucky, and Newport, Kentucky, are considered part of the greater Cincinnati metropolitan area. The city of Cincinnati has a well-defined set of boundaries and you can’t change those boundaries. It’s landlocked, so to speak, so you’re not going to get a whole lot more people within those boundaries. As time went on, things deteriorated and people have left the city.

RM: Now it’s kind of hurting?

DV: Yes. Cincinnati was a big machine tool center at that time. Cincinnati Milacron is gone—it was Cincinnati Milling Machine and Tool at that time. R.K. LeBlond is gone;

they're now owned by Makino, a Japanese firm. Fosdick Drill Press, G.A. Gray—they made planers—there were a whole bunch of them. Ford and General Motors had good-sized plants there and there were a million small machine shops around the city that did support work for all the bigger companies.

And Cincinnati was big in the chemical industry. My father worked for a chemical company called Emery Industries that made a lot of really special organic chemicals. Proctor and Gamble was there, Formica, part of American Cyanamid—those are the ones I can think of off the top of my head. The chemical companies have been pretty stable but the machine tool industry, for the most part, is all gone.

RM: It's in Japan, China . . .

DV: And Germany and Hungary and other Eastern European countries.

RM: So the people who were in that industry were left with nothing. Then you graduated from college, and what did you do next?

DV: The company that I worked for, Metcut, wanted me to stay there and work for them and they would put me through graduate school. But my view at the time was that they were too limited. The head of the metallurgical engineering department at the university got an inquiry from the National Bureau of Standards. They were looking for a person to work in a new field called electron probe microanalysis.

I'd done a lot of work using a microscope when working on failure analysis and I could see things, but I could never figure out what they were chemically. This new technique allowed you to look at small things that you could see in a microscope, put a beam of electrons on it, and get X-rays out of it. Each element has a very specific spectrum and you could then do a qualitative analysis to find out what was in these various areas. If you had the right standards, you could then do a quantitative analysis of things as small as 10 microns in diameter.

That was the kind of thing that really interested me, and the opportunity to work for a much larger research and development organization. The Bureau of Standards at that time was at Connecticut Avenue and Van Ness in Washington, D.C.— basically downtown D.C. I began my professional career there. At that time the Bureau of Standards was 3,000-3,500 people. (They also had a significant facility in Boulder, Colorado.) I went there because it was a great opportunity to do some cutting-edge kind of research and go to school part time to get my doctorate.

RM: Where did you go?

DV: University of Maryland.

RM: So you could take a very small compound and put it under the microscope.

DV: Let's say it was a very small item.

RM: Okay. It's got a lot of different things in it.

DV: Right, a lot of different elements. You could detect the elements, determine which elements were present. And you could tell how much of the different elements, if you were clever about standards.

RM: How small were the particles you would study?

DV: They weren't necessarily particles. If you had a sample you cut and polished and were looking at in the microscope, you could see a lot of details, like inclusions or different phases. Inclusions might be 10 microns, 20 microns, 30 microns in diameters. If something was 10 microns in diameter, you could analyze it separately from everything else that was around it.

RM: That was a major advance in science, wasn't it?

DV: Oh, yes. It was basically the forerunner of the scanning microscope. I started out in the metallurgy division, and they wouldn't buy any new instruments; they were trying to build everything themselves. Then the analytical chemistry division hired a really great scientist by the name of Kurt Heinrich and he got first-rate instrumentation. When he first came, I went and sort of helped out and did support operations and learned from him. It got to the point where I decided I wanted to get out of the metallurgy division and go to work in the analytical chemistry division because basically, I would be doing analysis of metallurgical samples.

I got into using the techniques for characterizing standards that the National Bureau of Standards would sell. They had a program called the Standard Reference Material Program. They would make these standards and certify the quality of the chemistry of the standards.

RM: You mean, that something contained such-and-such of one thing and . . .

DV: Plus or minus so much. They did a lot of that for the metals industry—different amounts of steel and brass where people had to have chemistry of the alloys they were making. There are certain analytical chemistry techniques that you use and they made standards that would go along with those. So I got involved in that. And we were developing new instrumentation and practices for their use. Today, the scanning microscopes have what is called non-dispersive X-ray optics, in which one detector can measure all the X-rays that hit it and determine what the energy of each one of those X-rays is, and make you a chart of what those things are. Our laboratory was the first one to do that. We did a lot of things that were cutting edge.

RM: Just for my own edification, would that be considered engineering or was it more science?

DV: It was more science. But science is the basis of engineering and sometimes people had engineering problems and they would come to us and tell us, “Help us understand this,” because, if we understood the science and we could get the information they wanted, they could solve the engineering problem.

RM: Meanwhile, you’re going to the University of Maryland working on your doctorate.

DV: Yes, material science was my major and I took minors in nuclear engineering and physics.

RM: Exactly what does that entail?

DV: Basically, it’s understanding how materials are structured and the factors that contribute to and affect their various properties. For instance, steel has a certain strength and ductility, hardness, etc. And there are certain materials like polymers that have significant plasticity. You look at the different properties that a material has and understand the physics taking place inside of the materials to understand how they perform or behave in certain ways under different conditions. There’s a whole range of properties that materials have and the question becomes one of how you understand why they have those properties and how to make use of them. Like semi-conductors—why do semi-conductors do the things they do? That’s part of material science.

RM: And it all gets down to physics.

DV: And chemistry can be equally important.

RM: So you were in on the beginning of big advances in this field at the Bureau of Standards and also in your studies.

DV: Yes. And you have a major and you have minors. I took nuclear engineering because I thought nuclear engineering was going to be the focus of the future. Nuclear engineering is tied very strongly to the properties of materials. You have to understand what radioactivity means and which conditions are dangerous and which conditions are not. You also have to understand how to design reactors so that you can convert the fission process into usable energy; you have to understand the physics of what happens to the neutrons that come out of the fission process. You want to slow them down so that they can interact with the next uranium-235 atom and cause it to fission, so you usually put them in a water-moderated reactor. In fast breeder reactors, they found out that the stainless steel cladding they used in the fuel elements started to swell, resulting from large voids formed in the material. The question is, “Why did that happen?” It had to do with the process by which high-energy neutrons affected the atoms in the stainless steel.

RM: What were the neutrons doing?

DV: They were just banging the devil out of the material. You get a neutron that comes off a high-energy neutron. When those neutrons interact with other solids, they cause damage. Sometimes the damage is insignificant—it will go away—because of the temperatures at which things operate. Other times, it doesn’t.

RM: The reason you slow them down with the water is because they would knock the . . .

DV: You wanted to be able to slow it down so that when the neutron got close to an atom of U-235, the atom of U-235 would capture it and then it would fission again and produce, on the average, another two-and-a-half neutrons. You always had to be able to get extra neutrons out—more than one to get a sustainable chain reaction.

RM: What happens if you don’t slow it down?

DV: The physics of the process gets much more difficult to control. Remember how small the nucleus is. It’s like in this room there’s a small box and you’re trying to shoot at it and everything’s going all over. If you don’t create a situation where the thing is going slow . . .

RM: It won't suck the neutron in if it's going too fast.

DV: Right.

RM: And so you were studying and involved in fission-related work.

DV: The other part of it was nuclear-chemical engineering. How do you process nuclear materials? They're radioactive as the devil. For instance, if you wanted to separate plutonium created from uranium fuel, part of U-238 will be converted to plutonium-239. Because plutonium will fission, there is value in separating the plutonium from the uranium. How do you do that? You find there are chemical processes for doing that—some of them are efficient and some of them are not efficient. You're also going to have to do it in such a way that the process is shielded because there are high levels of radioactivity. It's got to be completely shielded so the workers operating the process are not exposed to that radiation.

RM: So you were studying these things as a minor in your graduate studies.

DV: Yes, and also physics. I had a double minor. Physics is what controls the nuclear process so you really want to be grounded in physics. Also, in the work I was doing on electron probe microanalysis, understanding modern physics and quantum theory and so forth was essential. The importance was to get a good grounding in the fundamentals of science and engineering.

RM: This would have been in the early '60s, right? Was a lot known then?

DV: Oh, yes. It wasn't as much as we know now, but it was good enough to be able to build the *Nautilus* submarine in 1955. In the early days nuclear submarines had to be cut open and refueled. Today, they can make fuel elements that will last the lifetime of the submarine.

RM: Which is how long?

DV: In the neighborhood of 30 years.

RM: How did they to achieve the ability to have it last so long without refueling?

DV: Basically, being able to define the materials the fuel elements were made out of that would withstand that radiation level for that level of time.

RM: How interesting; and so, knowledge increased.

DV: Yes. In my training I was working for very good scientists, and in my job they were pushing knowledge into my head. I was going to graduate school at the same time and they were pushing knowledge into my head. At that time the Bureau of Standards was one of the two or three best laboratories in the world in the field I was working in—microprobe analysis.

RM: Was that essential for nuclear engineers?

DV: No, that was different. Basically, if you're going to get a degree, you have to have a fairly broad spectrum of knowledge. I was not one that wanted to be coming out of my graduate education as an expert in one limited area. I wanted to be somebody who could deal with a broad spectrum of issues.

RM: At what point did you decide to get a Ph.D., as opposed to being satisfied with your engineering degree?

DV: That was clear when I graduated from engineering college at the University of Cincinnati. My father was very strong in urging me to get my doctorate.

RM: That resonated with you?

DV: Yes. You have to realize it was just after Sputnik was put up and they were pumping a great deal of the nation's resources into science and technology. If you were going to go into that field you'd better get on the cutting edge so you understood what you were going to do because things were changing fast. You didn't want to become a dinosaur by the time you were 30.

RM: Was your work at the Bureau of Standards a full-time job?

DV: Yes. I was working 40 hours a week.

RM: And then going to school at night? What did you do your dissertation on?

CHAPTER TWO

DV: Well, the University of Maryland and I had differences of opinion. I got to a point where I had completed all the coursework that I needed for my doctorate. I went to talk with them and they said, “You’ll have to come on campus for a year and you’re going to have to work here.” Most places allowed government employees in research and development to pick a thesis in the area where they were working, but they were changing that policy at the University of Maryland. I had a disagreement with the department head. It was like, no way. You’re either going to come on campus or you’re not going to get your degree.

By that time, I had a child. To go from a standard of living based on a professional-level job in D.C. to whatever a graduate student was making was not going to be a rational thought. The head of the department at the University of Cincinnati, who I stayed in touch with when I would return home on vacations, always considered I was going to become a high-class technician; he always chided me about coming back to the University of Cincinnati and working for him. I called him up and I said, “Here’s the situation. This is how many credits I have; what’s the deal?

He said, “I’ll take every one you’ve got and I’ll give you a decent stipend.” That way, I could live in my father-in-law’s apartment building. If I ran out of money in the middle of the month, I could go eat off my mom and dad. My wife JoAnn and I also got support in looking after our young son, Edward.

RM: So you went ahead and did a dissertation at the University of Cincinnati?

DV: They wanted me to do more coursework, which I was very happy to do. I thought it would take me two years to finish, but it took me three-and-a-half, mainly because I picked a thesis that was very challenging. My wife had an accident before I went back to the University of Cincinnati and she was in the hospital three times while I was in graduate school. So there were outside factors that impacted my ability to get everything done. But I eventually finished my thesis on high temperature thermodynamics.

RM: Could you talk about that?

DV: Basically, it was the understanding of how alloys were made. To do that, a certain process takes place. Thermodynamics drives certain aspects of how the alloys are formed. I developed some instrumentation that could allow measurements to be made that were never made before, because my thesis advisor wanted to try using liquid uranium as a solvent for this process called solution calorimetry. But that meant that it had to operate at above 1,100 degrees centigrade.

I designed and built all the equipment to make that happen. There were lots of problems in the process of making it work. As you ran into problems you had to overcome them. Eventually, I got the measurements made. The Department of the Army put up the funding for the research work, so they were the ones that paid for the final part of my graduate education.

RM: Where did this research lead?

DV: We were able to make these things happen, but nobody else wanted to follow up on it. Two or three other people used my equipment to finish their theses; it was a very complicated process and there were a couple of fundamental flaws in the concept that my thesis adviser had.

RM: But with your equipment, to use a really simple example, you could take chrome and iron and understand the energy release process when they would blend together?

DV: Basically, you wanted to know how much energy in the form of heat was given off when you made these alloys. One of the things you did was to take iron and drop it into uranium and you could measure how much heat it gave up when iron was done; then you could drop chromium in and do the same thing. Then you could take an iron-chromium alloy and drop it in and see what the temperature difference was. You'd get two things and you would normally think about a straight line, but usually the curve was like this or like this, so you could measure those things with some accuracy. But the uranium is a problem because it's so dense that things floated on top. Basically people were using my high-temperature instrumentation on materials different from iron or uranium.

RM: But you could measure those temperatures. How do you measure them?

DV: With thermocouples. A thermocouple is a device that takes two different metals, two wires, like one of iron and one of an iron and chromium alloy. You twist them together and take it back and you can measure the voltage is produced when the junction is heated up. The higher the temperature, the greater the voltage produced. Once you have them calibrated, if you know the voltage produced, you can determine the temperature. It's a very old technique for measuring temperature.

RM: What did you do after you finished your dissertation?

DV: I went back to work at the Bureau of Standards. I finally finished all my research work by September of '71 and I went back to work in October of '71 because they were not going to keep my job open for me anymore. I had taken a leave of absence from the Bureau of Standards. I eventually received my Ph.D. in June of '72.

RM: What did you do after you went back to the Bureau of Standards?

DV: I went back to work doing the same thing: microprobe analysis. I actually wanted to do my doctoral work using microprobe analysis to work on alloy, so I just got another aspect of it in terms of a high-temperature thermodynamic approach. I went back to doing the same thing. In ’72 there were no jobs so they gave me my job back at the same pay grade at the Bureau of Standards. I said, “I paid for the last couple of years of my Ph.D.” I didn’t think getting the same pay grade was right.

They said, “Try someplace else.” I stayed until I was formally awarded my Ph.D. in June of ’72, and then I was looking for something else to do. The managers in the administration at the Bureau were interested in me coming to work for them to do program management and program oversight. I said, “Well, I’ll take a shot at that.” I got involved in that and did it for two years, until June of ’74.

A new director took over in the summer of 1973, a young physical chemist by the name of Richard Roberts. He was from the General Electric research laboratory. I was in meetings with him and he and I got along very well. We were on a couple of projects together and I did some things that helped make him successful; he offered me a job as his special assistant in June of 1974. Two years after I left the laboratory, I was assistant to the director of the Bureau of Standards, which was an incredible rise in status and influence.

RM: What was your job? What were the things you did and how did you exercise that power?

DV: The Director of the Bureau of Standards had lots of issues in managing a large and diverse R&D organization that he had to have resolved, and I was basically the bulldog that went and got the information necessary to get them fixed.

RM: What would an example of an issue be?

DV: For instance, trying to understand why a particular activity was important to commerce. A lot of the scientists were very interested in the science that they were doing and they weren't necessarily interested in why the science was important to commerce. Well, the Bureau of Standards was in the Department of Commerce and its mission was to facilitate commerce—one, by making standards like weights and measures, but it was also to understand the measurements that were important to science and industry and then be able to make products like the Standard Reference Materials (SRMs) that were useful in industry to keep them competitive and in good shape. The SRMs were important to controlling the chemistry of steels. When the Bureau scientists couldn't explain why the science mattered to industry, it was my job to go ferret out that information.

RM: Did you like being the bulldog?

DV: It was fine. I also did a lot of his presentations, I made all the slide shows, I wrote speeches for him. Sometimes he didn't know exactly what he wanted to say, so I had to tell him what he wanted to say. Things of that nature.

RM: You were living in D.C.?

DV: No, I lived outside, north of D.C., in Gaithersburg. Before we left Gaithersburg to go back to graduate school, the Bureau of Standards built a brand new facility out in Gaithersburg, Maryland, and moved the facility from downtown to Gaithersburg. It was 3,600 people, a huge campus of 500-plus acres, really a large place, a first-rate research facility. When I came back, that's where I went.

The Federal Council on Science and Technology was created in the early '60s by President Kennedy when we got into the Sputnik issues. The council was a very influential organization. The president's science advisor was part of the Federal Council on Science and Technology. The council had one standing committee, the Committee on Federal Laboratories. The director of the Bureau of Standards was always the chairman of that committee because the Bureau of Standards does more contract research for federal agencies than any other government laboratory. About a third of its budget at that time came from work for other agencies.

RM: What would be an example of a problem that would come in?

DV: One of the things that we got involved in, where I got my first security clearance, was AFTAC, which is the Air Force Technical Air Command. They were the ones that went out and collected the particles that came off foreign above-ground nuclear tests. They wanted somebody to analyze them and since we were the premier laboratory in electron probe microanalysis, they brought the material to us.

Or the EPA would have problems with lead paint and they wanted analytical chemistry to show certain things, and they came to us. I had, one time, a sample from the CIA—a piece of microelectronics from somewhere—and they wanted to know what it was made out of.

One thing we handled was counterfeit coins. If you tested a coin you could not deface it. Our technique never left any evidence of the coin's being analyzed. One time we had some 1950D nickels. There weren't that many 1950D nickels minted. Most of them were made at the Philadelphia Mint but didn't have a mint mark; the Denver Mint always had the D on it. They got a roll of these nickels from some coin dealer in New York City and they thought they were counterfeit so they brought them to us. If you were able to do a scanning picture of the D, you would find elements of silver solder around the edge of the D, but the forgery was so good that you couldn't tell it from a comparison microscope. Once we were able to demonstrate that the D had been silver-soldered on . . . How they managed to do that I have no idea. A 1950D nickel was worth about five bucks. I can't imagine a person putting that much money into making something that gave him a $4.95 increase in value, but they did. So they were able to heat up the counterfeit nickels and they could lift the D off and they were able to show hard evidence that they were counterfeit.

RM: Amazing. On the atmospheric nuclear tests, you could take particles coming from the explosion and tell characteristics of the device?

DV: You could tell what materials were in the device.

RM: That's fascinating. How long were you the assistant director?

DV: For a year. I started in June of '74, and in June of '75, the Energy Research and Development Administration, ERDA, had been created by the Energy Reorganization Act of 1974. They got rid of the Atomic Energy Commission and broke it up into Nuclear Regulatory Commission and the Energy Research and Development Administration. ERDA came into being in January of 1975 and they were looking for somebody to be the assistant administrator for nuclear energy. Bob Seamans, head of ERDA, met my boss, Richard Roberts, primarily because they wanted him to do the assistant administration work for energy conservation—the Bureau of Standards had the largest research program in energy conservation in the federal government. The Bureau did the work on a contract basis for the Department of Housing and Urban Development (HUD).

When Seamans met him and saw how good my boss was, and with his background with General Electric, which makes nuclear reactors, they offered the job of the assistant administrator for nuclear energy to him. Since part of my background and training was in nuclear engineering, I went along with him to ERDA as his technical advisor.

RM: Why did they split the AEC into two entities?

DV: The Atomic Energy Commission was, in the early days, mostly focused on nuclear weapons; it was the follow-on to the Manhattan Project. The Energy Research and Development Administration took with it all the manufacturing and production requirements and capabilities and facilities for research into and production of nuclear weapons. But by ’75, they had already gotten into supporting the development of nuclear power. We were into establishing what was the fast breeder reactor and light-water reactors and different kinds of means of producing nuclear power. Remember, the Atomic Energy Commission was the only agency allowed to deal with special nuclear materials. When they split it up, the Nuclear Regulatory Commission got the responsibility to be the independent agency to license and regulate nuclear power plants, but the control of special nuclear materials continued on with the Energy Research and Development Administration.

But when the Federal Power Commission was rolled into ERDA, the Office of Coal Research from the Department of the Interior was rolled into it as well. There were a lot of other research programs—the energy conservation programs that were sponsored by HUD were brought into it, and so on. A broad spectrum of bits and pieces of other government agencies got wrapped into the Energy Research and Development Administration, but the largest part of it still had to do with nuclear energy. That’s where I went.

RM: And you were . . . ?

DV: Technical advisor to the Assistant Administrator for Nuclear Energy.

RM: What kind of advice would you be giving him?

DV: I had the same responsibilities that I did in my previous job. There were four major divisions under him: the reactor research development. The people that ran the chemical processing facilities for the weapons program, like Savannah River and Hanford, all reported to him.

And there was something called space technology, which made the RTGs—Radioactive Thermoelectric Generators. They were units that had a sphere of plutonium-238 that would generate a source of high temperature inside. With various thermocouples back and forth between a hot junction and a cold junction, you could create a voltage and a power source that would provide electric power where solar panels would not. They were putting those units on space probes that were going further from the sun or for units that could not be easily detected. You remember the probes that went to Jupiter and Saturn—Galileo and Cassini. That's the kind of power source that they had—that division was dedicated to that.

There was a fourth division, the Division of Naval Reactors under Admiral Rickover. So when my boss needed something looked into, I looked into it. It's sort of like encouraging people to get things done. I still did the slide shows and worked with the graphics people. I would make sure all the content was right. I still wrote speeches for him and activities like that.

RM: What were your feelings about this new position?

DV: I thought it was a great opportunity to learn. When it became known where I was going, people told me I would see things that would knock my eyes out, and I did. Fascinating work was going on.

RM: At that point, what was the state of nuclear power in this country?

DV: It was in conflict. At that time—in 1975—there was a proposition in California to ban nuclear power because of the lack of means to dispose of the high level nuclear waste. There was great resistance to nuclear power. At one time the people who promoted nuclear power were looking at building a huge number of new power plants and there were a lot of them on the schedule. By the time 1978-'79 came along, when Three Mile

Island happened, everything seemed to collapse. A liquid metal breeder reactor was going to be built at Clinch River, Tennessee; that got cancelled. When I came into ERDA, nuclear power was on the rise and in a relatively short period of time, viewpoints and focus changed drastically.

RM: Why did they change drastically in that relatively brief period of time?

DV: Well, it was 1976. After Jimmy Carter got elected, organizations such as the Sierra Club and the National Resources Defense Council had significant impact. The lack of a commitment to nuclear waste disposal was a significant issue.

RM: Was Carter the linchpin of that shift?

DV: Basically, he was very concerned about nuclear proliferation. At that time, the Generic Environmental Impact Statement on Mixed Oxide Fuels (GEISMO) was being promoted because they wanted to make nuclear fuels for light water reactors that had both plutonium and uranium—mixed oxide.

RM: What was the advantage of mixed oxide fuels?

DV: Uranium, U-235, is in limited supply and eventually it was going to run out and we were making a lot of plutonium in light water reactors. The question was, how can we find a way to husband our resources and get the most out of them? That meant recovering the plutonium. Carter was dead set against that program and made it a major campaign issue. Ford reacted to Carter's threat and withdrew the environmental impact statement and put a hold on the use of mixed oxide fuels.

RM: Why did Ford do that?

DV: Because that was Carter's position and he was trying to find a way to take the thunder away from Carter. But Carter was elected, and he initiated a study called the International Nuclear Fuel Cycle Evaluation—INFCE. That was looking for ways to do nuclear power without creating a chemical processing system for producing fissile material that was potentially susceptible to diversion of materials to unauthorized users, just like we're dealing with in Iran today.

RM: Are there other avenues besides plutonium?

DV: Well, maybe thorium; there's a thorium cycle. But the question became one of how to avoid creating a nuclear fuel cycle that was not susceptible to diversion of the fissile material. There were, as I remember it, eight working groups in that INFCE study. I was executive secretary of Working Group 7, having to do with how you could guard against people processing the nuclear waste to recover plutonium to make bombs.

RM: As I recall, in 1976, in President Ford's State of the Union address, he called for the construction of 200 nuclear power plants in America. How did he get from that to canceling these things?

DV: The political framework changed.

RM: And much of that political framework was Carter?

DV: Yes.

RM: Carter was a nuclear engineer, wasn't he, under Rickover. How did he wind up holding a position that, essentially, damaged the industry?

DV: He was more concerned about nuclear proliferation. He was not concerned about the proper use of nuclear power, but about the improper use of nuclear materials to make weapons.

RM: In your opinion, what is the validity of his concern?

DV: Some countries have done it. India and Pakistan now have nuclear weapons and they didn't have any at that time. Pakistan's program is in plutonium weapons, not uranium, and so is India's. North Korea supposedly has plutonium weapons and a number of other countries have tried it, like Libya and South Africa, but they gave up. Iran's doing it, but they're going for the simpler way. The easier bomb to design is one based upon enriched U-235.

RM: Why would Pakistan go for plutonium?

DV: At that time they didn't have centrifuges; they were still in development. The centrifuge is used to enrich U-235. Natural uranium is only 0.711 percent U-235. You can't make a bomb out of that by itself. But if you can find a way to enrich it from 0.7 percent to 90-plus percent, you've got the basis for a bomb.

RM: If you know how to put it together.

DV: The uranium bomb with the gun design is the simplest one to make. Remember, in the development of the first nuclear weapons for the United States during the Second World War, they never tested the uranium bomb. They understood the basic science and knew it would work.

RM: You mean, Alamogordo was a plutonium bomb?

DV: Yes. There's a phenomenon associated with plutonium that's called delay neutrons that creates a serious problem if you want to use the gun design. I've never seen a weapon based on the gun design; apparently it shoots two subcritical parts together and the combined parts can fission as a bomb before they come apart. But if you use plutonium, as the two parts get close enough, the fission process begins and it starts coming apart before you get it assembled to a critical mass.

RM: What's the secret, then? Bring them together fast?

DV: That's what the implosion approach does—put all the explosives around the outside of the plutonium and blow it inward and compress the plutonium so it goes critical and explodes as a bomb.

RM: How far back did this fear of things nuclear go?

DV: Think about where Sierra Club was on this, and the National Resources Defense Council. There were numerous . . .

RM: What is the basis of that fear?

DV: It has to do with radiation and nuclear materials. People fear what they know can be dangerous and they can’t see. They last a long time and if they get out into the environment you can’t clean them up easily. They’re dirty and they’re deadly. There’s a great deal of misunderstanding about nuclear materials and radiation. A large number of individuals who are not necessarily knowledgeable have a lot of viewpoints, most of them inaccurate. I mean, there are a lot of knowledgeable organizations like the Union for Concerned Scientists. They have people who are knowledgeable and question proposals and actions, which they should. But it is possible to solve the problems. In a democratic political system, an uninformed vote has the same weight as an informed vote.

RM: There were other fearful things in the environment at that time—DDT and other things.

DV: Remember, in 1977, ’78, ’79, did we know about the greenhouse effect? So there was no counterbalancing argument. What you have to realize is that social and political systems are like mechanical and chemical systems. The equilibrium position is where the forces are balanced.

RM: Explain that a little further—that’s an important point.

DV: In chemistry you can get a chemical equilibrium. The push of a reaction in one direction is the same as the reverse reaction in the opposite direction. It’s in balance. If you put too much acid in the water, it will churn and boil, then eventually it will get uniform and nice. In mechanical systems, if you’ve got a bunch of things like springs pulling in different directions, you'll eventually get to a point where all the forces are balanced and something will stay in place. And social and political systems are like that. The pendulum goes this way and then something drives it back in the opposite direction. Eventually it may come back to the center because the forces on each side are pushing it into a uniform position. The question is, where is the Sierra Club today on nuclear power? They may not necessarily like it, but they like the greenhouse effect even less. They are taking a more even picture of nuclear power.

Basically, people understand what the forces are that you have to deal with in life. For a while, gasoline looked great, except that it put out too much CO_2. The question is, how do you get rid of the CO_2? It's the same way with fossil fuel power plants. The only really major generator of electrical energy that doesn't put out any CO_2 is a nuclear power plant. But it puts out a waste product that can be turned into a solid in a very small volume. If handled properly, the operation is safe and the material can be disposed of safely.

RM: Moreover, that waste product can be reprocessed and turned into more fuel.

DV: No, you don't process waste to get fuel. You process the fuel to recover the plutonium and uranium and to get the waste out. And then the materials, the plutonium and the uranium, can be recycled as nuclear fuel.

RM: Moreover, there's transmutation.

DV: Yes, that's happening with the uranium. But the waste is waste. It generates heat and radiation and you may want to find a way to use that . . . but there are problems associated with that. Such material, in the hands of incompetent individuals, can be deadly.

RM: When do you think this fear of nuclear began? Does it go back to Madame Curie getting cancer?

DV: The dominant place where radiation effects were experienced and discovered was the result of Roentgen developing the X-ray tube. Roentgen, in the late 1800s, discovered X-rays, and all the things that went with them. There was an expectation, a promise, that X-rays could solve all sorts of things. But by 1925, a large number of people in Germany and Europe had died from radiation poisoning brought about by extensive exposure to X-rays because they did not understand their nature. The people of Munich put up a statue in 1925 to all the people who died from radiation poisoning. So in 1925, they understood the impacts of radiation. They still had to learn how to use the technology safely.

And they were beginning, with Curie, to realize there were natural materials like radium that would give off gamma rays and alpha particles and beta particles that would cause deterioration of biological systems. Those materials were long lived, where with an X-ray tube you can turn it off and it's not a problem anymore. With radioactive materials, when you concentrate them, they're a problem because they're not going to go away by themselves quickly. The half-life of radium is about 1,600 years. If you've got a pound of radium, in 1,600 years you've got a half a pound. In 3,200 years you've got a quarter of a pound. It will eventually go away. The rate of disappearance is controlled by nature itself—the half-life of the radioactive isotope.

RM: This fear, then, beginning in the '20s, enters the public consciousness.

DV: Some of the most dramatic pictures of Hiroshima are of all the people that got radiation damage. And there was sloppiness with regard to handling materials and people got exposed to radiation. If I remember correctly, there have been six or seven people in the United States killed as a result of handling radioactive materials.

RM: That's a small number.

DV: Yes, but it happened. People started asking the question, “Can you guarantee that this won’t happen?”

And no scientist would say, “I can guarantee you. There are a lot of things you can do to prevent it, but it can’t be guaranteed.”

RM: Just to juxtapose that, when they mine coal in Wyoming or South Dakota or Montana and they take it by train clear to Atlanta, I’ve seen figures on how many people die in that transportation cycle. Those numbers are very acceptable to people. On the other hand, a few deaths in the radiation cycle becomes a problem in the public mind.

DV: How many people have died as a result of Chernobyl? Right now, we’re seeing a lot more of the people that were exposed dying of radiation poisoning.

RM: In contrast, I’m told nobody has died from Three Mile Island.

DV: Nobody, because they never did release a significant level of radioactivity. The reactor may have melted down internally, but it stayed within the containment vessel. In Chernobyl the Russians were so confident that they could control things that they never put a containment vessel around the reactor. This country never accepted that philosophy. With time, requirements got more and more restrictive and the power plants got more capable of containing large quantities of radioactive materials and the force of a significant internal explosion.

RM: I’ve been fascinated for a long time about the roots of the anti-nuclear movement, which eventually reared its head in Nevada, in Yucca Mountain. What are the antecedent conditions that helped generate that?

DV: In the ‘60s, people were concerned about radiation and radioactive materials and by the ’70s it just was bonkers, to the point where California, as I said, was the first state to put on the ballot an initiative to outlaw nuclear power plants.

RM: Did it pass?

DV: No.

RM: Now, what is the next step in your career?

DV: I worked for Richard Roberts for a year at the Bureau of Standards as a special assistant and then for a year and a half at ERDA as his technical advisor. One of the reasons for that was to give me experience at upper levels of management to see how important the dynamics of managing these kinds of operations were, and to understand the political framework that influenced the course of action. Then I was allowed to go get a real job.

RM: What were some of the things you learned in terms of management?

DV: Basically, how people have their own fiefdoms and they have certain paradigms about conditions and situations, and whether or not they're capable of stepping back and taking a look at them. Sometimes they get very myopic. And basically, how important good science is to conducting operations in a safe and effective manner.

Also, how important discipline is, and that the information and understanding are accurate. You can't just pull these things out of your ear and say this is how it is. Somebody else would point out to you that you're an idiot and that's not how it is. In the scientific community, they're always looking at what you're saying to make sure that it's accurate, that it reflects reality. That's another thing—the fact is, there is a broad spectrum of viewpoints out there. Again, it comes back to the issue—what's the real, rational position on these things? People often have a lot of conjectures and some of them are not necessarily related to reality. Sometimes the people in the government get so hung up on what policy that is they don't understand how off the mark it is.

RM: How does a manager deal with this basically human condition?

DV: It depends on how adroit he is, how sensitive he is, and what he wants to accomplish. You have to understand all the forces that are pushing on a particular project—you're going to do this and you want to accomplish this and go from here to there, but there may be a lot of people supporting it and there may be a group of people that are opposed to it. And sometimes the people that are opposed to it have a better understanding of the problem and are pointing out to you some of the things you're not paying attention to and making you look incredibly foolish. If you propose to do something foolish and it is recognized, the confidence in the person and the proposed solution lose all credibility.

RM: How easy is it to put good science into this cycle?

DV: It's fairly easy to do it, if you've got it. If you're dealing with conditions that are unknown, you'd better figure them out before you try to sell them.

RM: In terms of analyzing the forces that the manager's dealing with, does he ever do surveys and use quantitative data to try and understand it?

DV: Of course. If it's all qualitative, when somebody wants to know a hard answer, you're not in a position of being able to provide them.

RM: So then you went out into the "real world?"

CHAPTER THREE

DV: I got out of the real politicized world into a more politicized world. In ’76, we were talking about developing a repository program. We needed to be able to demonstrate that we could dispose of radioactive material. This goes back to a number of things. The NRDC (Natural Resources Defense Council) petitioned the Nuclear Regulatory Commission, I think in 1976, to stop licensing nuclear power plants until they could show there was a definite way of disposing of the waste. The NRC established an argument saying we wouldn’t continue to license if we didn’t think there was a way of disposing of the waste.

And then another case came up having to do with, I think, Northern States Power and a storage facility—you might want to look at Michael Voegele’s material on that. There was a lawsuit about that and the judge said, “You’d better reconsider this issue of disposing of radioactive waste.” So there were legal items that came up in the late ’70s that said, “You’d better be able to dispose of nuclear waste effectively, otherwise you might not be able to license any more of these power plants and you might have to shut down all the ones that are licensed right now.”

The NRC was able to write an argument that sort of got by the NRDC challenge, but then the Northern Power lawsuit came up, and the courts said that you need to reconsider things. It caused the repository program to get involved with something called the Confidence Rulemaking—a rulemaking that showed that there was a technical basis for having confidence that you could dispose of nuclear waste. It was initially tied to the position that the federal government was going to open a repository in 1998.

RM: This was before the Nuclear Waste Policy Act, right? They had already set the date at 1998?

DV: That is what we were planning on at the time because people wanted to know what the schedule was, how we were going to do this. The Nuclear Waste Policy Act legislative process was started in the 1978 time frame. In 1979, they were working on it and it was about to come out in 1980 when there was a huge flap over disposal of low-level waste from commercial sources like hospitals and power reactors. One of the proposals was to dispose of low-level waste at the Nevada Test Site because there was a disposal facility there already having to do with the weapons program's low-level nuclear waste. That caused a great deal of discomfort.

So the Nuclear Waste Policy Act, which was going to focus on high-level waste, was gutted of high-level waste and it dealt with low-level waste because they had to resolve a way to find a place to put low-level waste from hospitals, radioactive pharmaceuticals research facilities, power plants, etc. Disposal sites for commercial low-level waste were at places like Barnwell, South Carolina, or Beatty, Nevada. And there was one in Washington, on the Hanford Reservation.

RM: So that left high-level waste hanging.

DV: Yes. The low-level problem was immediate—facilities were going to start shutting down, health facilities and so on. Then they restarted the effort on the Nuclear Waste Policy Act for high-level waste. It was finally consummated in 1982.

RM: Were you involved in the process and thinking that led to that development?

DV: Not so much. I was not necessarily the highest-level person involved. There were other people like Shelley Myers and Colin Heath who were involved in that discussion; much above my pay grade at the time.

RM: Could you talk some about the thinking that went into the Nuclear Waste Policy

Act and maybe some of the politics and the push and pull involved in creating it?

DV: Basically it goes back to understanding the significance of the threat of radioactive materials and radiation. If you're going to get involved in the nuclear issue, safety's the most significant thing you're going to have to recognize and address. The Manhattan Project created a lot of nuclear waste at Hanford in the '40s and they put it in tanks, and they kept building more and more tanks. Eventually they found out the tanks had leaked.

In 1955, the Atomic Energy Commission asked the National Academy of Sciences for input on what would be the best way of disposing of the waste. That's when they looked at a lot of alternatives and decided that geologic disposal was the most effective way.

You have to understand a couple of things. First, radioactive material goes away because it has a half-life. The question is, how long do you have to have it? If you look at plutonium-239, the half life is 24,000 years. Let's just say, for argument's sake, 25,000 years. And you know if something goes ten half lives it decreases by a factor of 1,024, or 2^{10}—let's say, for argument's sake, by a factor of 1,000. So if you start out with 1,000 pounds of plutonium-239 and you go 10 ten half-lives, which is a quarter of a million years, you've got one pound left.

Now the question becomes one of how long do you want it? You can argue, well, if we go 10 half-lives and that 1,000 pounds of plutonium is reduced to one pound, and in another 10 half-lives it's one-thousandth of a pound, that's 20 half lives. Well, what about 30 half lives? That's three-quarters of a million years, but now it's down to a millionth of a pound. That's the kind of timetable you're looking at.

If you said, "Well, getting it down from 1,000 pounds to one pound is good enough," that means you have to have a structure in which to put waste that will last a quarter of a million years. You look around and see what structure there is on the earth that's a quarter of a million years old. The pyramids are 5,000 years old. You begin to say, well, man-made structures haven't made the grade yet. But if you look at the earth, there are geologic structures that are stable that have been around a long time. The ones that were interesting were salt. You know that salt is stable because it's still there. Like at the WIPP facility, the age of the salt is Permian, which means it's somewhere between 460 million years and 250 million years old.

RM: That's in Carlsbad, New Mexico, right?

DV: Yes. If it's been there, let's say on the low side 250 million years, do you think you can get an additional 250,000 years out of it? You start looking at natural structures because we know how to build facilities inside of natural structures. The only issue is, they have to be stable.

Now the question becomes, are those structures anywhere you look? The answer is, no, they're only in certain specific places. So that insight said, "Okay, geologic disposal looks to be the only significant way of managing nuclear waste that gives us a high probability of taking it out of man's environment for a very long period of time without a chance of it coming back and making a significant impact on people."

You also say, "Maybe we can process the stuff and we could take something that's got a half-life of 1,600 years, like radium, and reduce it down to five years." That's physics of the nucleus. Nobody knows how to do that. The thought is, maybe you can do transmutation. But the question is, "Can you do transmutation on the thousands of pounds of material?" and it doesn't work out very easily—it becomes cost prohibitive. Maybe somebody will figure it out in the future, but right now, in this decision-making time frame, it doesn't look like it's an opportunity. So geologic disposal is the rational thought about how to do it.

RM: And when did this become clear?

DV: In 1955. That's when the National Academy wrote its first report. That's what the scientists looked at and nobody since that time, except maybe Mr. Obama, thinks that there's a way of changing that. Most scientists don't see any easy way to get rid of the nuclear waste by some physical mechanism. The thing to do is to convert it into a stable solid that's not easily leachable and put it in a place where there's little chance of a significant amount of water interacting with it.

So some time in the '60s, the Atomic Energy Commission started to do some research in disposal of high-level waste in salt formations. People looked at salt and they thought that was potentially the most plausible candidate for a geologic medium, mainly because they knew how old it was, how long it had been around and you could evaluate the geologic structure it was in. They knew that it was easy to build underground facilities in. There was a lot of experience in terms of mining salt formations and the material mined had a value.

The question then was to begin to understand the phenomenology that might take place. They created a program called Project Salt Vault in the Carey Salt Mine in Lyons, Kansas, and they took spent fuel elements from an Idaho reactor and placed them in the holes in the ground there to see what would happen. They began to observe a lot of phenomena. One thing they found out is that there's a small amount of water trapped in salt, water inclusions. The water migrated to the heat, so water started to form in the hole where the spent fuel element was placed. And when you have an electrolyte like salt water, it's pretty corrosive. So that represented a potential problem.

Another thing is that the radiation created certain kinds of defects in salt—I think they're Wigner defects. If you got enough of them they potentially could recombine. And when they did that, they would give off a lot of heat and cause problems. So there were a number of issues raised. They began to understand the phenomenology of putting spent fuel elements into salt. But at the same time, they were enamored with this Lyons, Kansas, site and they didn't spend a lot of time characterizing the geology. They failed to evaluate the stability of the geologic structure.

Another situation arose—there was a fire at Rocky Flats, Colorado. So a lot of plutonium got burned and a lot of transuranic waste was generated so they had to clean it up. They started sending the transuranic waste from the fire to Idaho, where they kept their waste until they could get a facility to dispose of it. I think it was Governor Andrus who got very exercised about the fact that Idaho had become the permanent dumping ground for the waste from Rocky Flats so he began to pressure the Atomic Energy Commission to get that waste out of Idaho.

Glenn Seaborg, I believe, made a commitment to the governor to get that waste out of Idaho by 1980. And this was in the 1969 time frame. Now there was real pressure to get a facility up and operating. They had a facility in which they had done experiments, but it was flawed with regard to long-term safety because they were beginning to find out there were a lot more oil wells drilled in the area of Lyons, Kansas, than they had originally believed (note this was a belief, not hard knowledge). They weren't sure where all those oil wells were located and they weren't sure whether or not any wells might go through an area where they would build a repository.

Another facility in the vicinity was doing solution mining of salt—they would pump water down in, dissolve the salt, pump the brine back out, and evaporate the water.

They were losing about 100,000 gallons of water a week. And the question was, where is that water going? Did they know that this was a safe site? Did they understand the integrity? And the answer was, no, they didn't.

But because there was pressure from Governor Andrus, the Atomic Energy Commission proposed Lyons, Kansas, as the site for the first repository. A state geologist for Kansas by the name of Bill Hamilton found a significant amount about what was fundamentally wrong with the program. There were a number of early discussions about it in which the Atomic Energy Commission position was, "Well, we can engineer around it."

That didn't satisfy them. If you have a facility that you engineer that lasts 100 years or 200 years or 300 years, will your engineering solutions last a quarter of a million years? The answer is, "Don't think so." But it was the Atomic Energy Commission's position that they were going to solve it by engineering around it, which wasn't the brightest solution to the problem.

At this time, because of the Atomic Energy Commission's efforts regarding building nuclear weapons and the importance of the secrecy of it, there was only one committee that had oversight of the effort. That was the Joint Committee on Atomic Energy, which was made up of politicians from the House of Representatives and the Senate. They had jurisdiction over the budget of the Atomic Energy Commission and there was no outside review. So all the shortcomings of the Lyons, Kansas, site started to be outlined in front of Congress. And the question was, is Congress really prepared to deal with analyzing and resolving issues of a technical nature about something related to safety? Congress is not the body that's really ready or competent to do that.

RM: Now, what year was this?

DV: In 1971 and 1972. I'll go back over this document because I've written a lot about that in that same paper that Mike Voegele . . . In this process of doing these things, the project manager for the program at AEC was beginning to understand the snowballing effect of all these problems. He didn't think that this site was going to work technically. In February of '72, the Atomic Energy Commission under Schlesinger "abandoned this plan for a repository in Lyons, Kansas, citing technical uncertainties and problems in political and public acceptance."

So here's another reason why there's furor in the public about a nuclear waste repository: In the first attempt that the Atomic Energy Commission made, the experts blundered and it caused a political firestorm. They lost their credibility to manage a technically complex project, credibility with regard to protecting the safety of the public. Remember, waste isolation means waste isolation, out of man's environment. They had absolutely no strong technical argument to bolster the fact that they were, in fact, correct about the site.

Now, this comes back to the issue of are we going to solve this program with science or are we going to solve it with technical fantasy and politics? Everybody can agree politically that Yucca Mountain would be a great place to put the material, but unless you have science to confirm that it will work, it's not a very good solution because we've already got one example, a very high-quality example, of the stupidity of that approach. And that's the whole thing—the loss of credibility. The agency that was supposed to be expert in the field of nuclear power and nuclear energy and nuclear waste isolation was caught with its pants down. It was blatant.

RM: That was damaging in terms of the later outcome, wasn't it?

DV: It set the tone for everything that came afterwards. The agency asked the scientific community how to do it. They brought it back inside the executive branch of the government, they put a bunch of technical people on top of it that don't have a hell of a lot of understanding of the political world, and they went out and made a proposal that was blatantly bad. And now the question is, "Where's your credibility in front of the politicians?" And you don't have any credibility anymore.

RM: Did you say the National Academy of Sciences was involved in that one, too?

DV: Yes, they're the ones that said, "If you've got to do something, it's geologic disposal." Their proposal is fine.

But the next question is, "Can you find a site anywhere you want that's got the right characteristics to do that?" And you can't. The places that have the right characteristics are not a dime a dozen. We know there are certain kinds of geologic media that offer a good opportunity but you can't be guaranteed, without looking at it in great detail, that one particular site will, in fact, work. Lyons, Kansas was in salt, which is the medium that was preferred, but the site conditions didn't give you confidence that it would work as an isolation site.

Every time the politicians asked for reinforcement they got a lot of baloney out of the technical people, and it turned out that the technical people who were proposing the site were in error. As I said before, the single most significant aspect of Lyons, Kansas, was that the technical experts lost credibility. How could anybody have any confidence in them?

The thing was we knew the effects of radiation in 1925; it's deadly. As I said, in Munich, Germany, they built a statue in 1925 to memorialize all the people who had died from the radiation effect related to the investigation of X-rays. And radioactive materials had a half-life. If you're looking at plutonium-239, which is the major constituent in this thing, we’re talking a quarter of a million years, minimum. Now, where are you going to go with that?

Then the Atomic Energy Commission said, “Well, we’ll build a mausoleum. We’ll build a retrievable surface storage facility that will last for 300 years. We’ll replace it in 300 years with another one, because we’ll create a trust fund that gives it enough money to propagate the program.” And they eventually wrote an environmental impact statement to analyze that concept.

It was released for comment and the EPA commented on it, saying, “This is not the final solution to the problem, it’s just a construct that allows you to avoid solving the problem.” So in 1975, I believe in March, when Bob Seamans took over as head of ERDA, one of the first things he had to do was recall that environment impact statement. In 1974 or ’75, retrievable surface storage facilities were considered by a Republican administration—not only by the EPA—to be an unacceptable solution to the problem. Where do you go? The only option for long-term isolation was to go back to geologic repositories.

Now we come back to the “Wisdom and Will” of the Congress. What did Congress do in the midst of all of this? They were dealing with a nuclear power issue, and they were finding out the Atomic Energy Commission played a dual role because it was a promoter of atomic energy but it was also the regulator of atomic energy. People said, “We think there’s a conflict of interest there.” That’s when they created the two new entities in the Energy Reorganization Act of 1974. This move was driven in part in ’74 by the oil embargo and the need to separate the energy development part of the program from the energy regulation part of the program—nuclear energy regulation, that is. So they created the Nuclear Regulatory Commission to regulate nuclear power and the

Energy Research and Development Administration to continue to build nuclear weapons, to promote nuclear power, and to do energy conservation and other energy technologies like wind energy and solar energy. Now we have two agencies. We've got a new agency that only has one responsibility—safety in the use of nuclear materials.

They also said in the Energy Reorganization Act that any facility for the receipt and storage of high-level radioactive waste has to be licensed by the NRC. Now we have an independent review. Congress got itself off the hook for making the technical safety decision about Lyons, Kansas, only because the Atomic Energy Commission withdrew the proposed site. But they also said, "We don't want to be the decision-makers about the technical or safety aspect of the problem. We'll create an organization that has the scientific and technical capability to review it for us and tell us what the right answer is, but they will be independent." That's the role of the NRC. The first element in the wisdom and will of the Congress was to create an independent body to provide high-quality scientific review of a proposal for disposing of radioactive waste—temporary storage or long-term disposal.

Fiscal year 1976 was the year in which Congress decided, "We better get some competent people involved in this thing instead of the bozos that gave us the previous one." You know, the symbol of bozo with a line through it and a circle around it. No more bozos. They appropriated $36 million for the high-level waste management program. That was more money than was appropriated in all the years before 1976 for solving the long-term waste disposal issue. For the most part, the Atomic Energy Commission saw waste as the last thing you'd want to deal with—it was more interesting to build reprocessing plants or reactors or to deal with development of weapons or energy sources for space, and the disposal of the waste got short shrift. Congress said, "No, you guys have really got to get your act together and figure out how to solve this problem."

So now the executive branch of the government (ERDA was still an independent agency, it was not a cabinet-level organization) put an emphasis on the competent scientific and engineering resources necessary to solve the problem. One of the problems was that it picked intellectual resources that were scientifically oriented, it didn't pick any intellectual resources that were knowledgeable about how to solve the political problems that would arise from the need to dispose of "dirty, deadly radioactive waste" somewhere in the US.

So 1976 is when ERDA really got around to addressing the question of disposal, they hired Union Carbide, the Office of Waste Isolation, under a man by the name of Clay Zerby, to develop a program. They looked at it and said, "If you look at the projections of having 300 gigawatts of generating capacity online by the year 2000," because that was the projection in one of the AEC's planning documents, "it might take six repositories to handle the waste for all of those reactors." By the year 2000 there were supposed to be six repositories. The first two were supposed to be built in salt. The other two geologic materials they were looking at but didn't understand fully yet were argillite materials, which are shales and clays, and crystalline rock like granite or basalt.

RM: Why were they still considering salt after the experience in Kansas?

DV: Because that was the geologic material that was best understood and they still believed that was the best shot. One of the things you have to do is be able to model what goes on in the earth. From a geologic point of view, salt generally forms in what are called sedimentary basins, so the geology around salt formations is generally relatively simple. You need a geologic medium that you know you can build an underground facility in with relative ease. You know that you've got one that, when it's finished, will more than likely seal itself. You got one that's got long-term stability—250,000 years for isolation consideration versus 250 million years in terms of the existence of the formation.

RM: And this was in '75?

DV: It started in '75. That's one of the things I got involved in when working for the assistant administrator because sometimes things came out that were a little unusual and people hadn't thought through the political significance of it. One of the first sites in salt that we were interested in was the salt around Alpena, Michigan. That got out and the governor of Michigan, Governor Milliken, became quite upset. He strongly opposed any effort to explore the formation. So that was the beginning of the political resistance by the states to siting a high-level waste repository within their boundaries.

CHAPTER FOUR

DV: So those were early discussions that were going on. And now, we're getting into the time frame when things were changing politically—the referendum in California in '75 to prohibit nuclear power, the problems with GEISMO being retracted by President Ford because Carter was running for office and making nuclear proliferation an issue. There's a whole mixture of things, including confusion around what's going in the nuclear arena. This was a time when I left as the technical assistant to the Assistant Administrator for Nuclear Energy and went to work in the waste disposal business, the repository business.

RM: Was that by choice?

DV: Yes, it was by choice.

RM: What was your thinking?

DV: I believed, from an engineering point of view, building the first repository for disposing high level radioactive waste had to be one of the great engineering challenges of all time. As a person who grew up in the engineering arena with my father, I was very interested. And it's an opportunity to do something that's technically challenging and important to society. I thought it would be a very interesting challenge. I went to work for a manager by the name of Frank Baronowski and another manager by the name of Carl Kohlman.

One of my responsibilities was to write the letter announcing the program to find repository sites. It was the letter that would be sent out by the head of the agency, Bob Seamans, to all the governors and congressmen and senators in the states within which formations of interest were located to communicate with them so they would be aware of what the agency was planning. The pressure was on from Congress to solve this problem and get out there and start looking for sites. The federal government wanted a nice systematic solid technical plan for how we were going to do this.

We drew all the formations of argillite, all the formations of crystalline rock, and all the formations of salt on a map of the United States at the county level; I had a map in my office that showed all the counties. There were 36 states that had a potential for being investigated. Not all of them were going to work, but at least the formations that were of interest were in these 36 states. So the letter went to the 36 states—each governor and the congressman and senators from the states got a letter. It was important that the governor got the letter first because the governor is the person responsible for the health and safety of the public of that state. On the same day, the letters were delivered in D.C. to the congressmen and senators from those 36 states, so there were 36 letters to governors and 500 and some-odd letters to congressmen and senators. We had to deploy couriers with these letters so that everyone would get theirs at the same time. The idea was, how do you share this information with people so nobody could be one-upped? In order to get this letter out, I had to get 18 concurrences.

This was October of 1976, and what's in November of 1976? The election, Ford versus Carter. Now, can we put this letter out before the election? The Republicans wouldn't have been happy about it, the Democrats wouldn't have been happy about it. So we had to wait until the week after the election. We were ready to go, and then something happened so we had to cancel the delivery and everything had to happen the following week. I think the letters got delivered on Tuesday of the following week, the third week of November in 1976.

RM: Was there a caterwauling when it was delivered?

DV: No, because it was Thanksgiving. And December was kind of quiet because it was Christmas. Carter wins the election, and in January we start getting responses. The first person I had to go see was, I believe, the governor of Vermont. And I had to go see the governor of Mississippi and the governor of Washington state. So there was beginning to be interest in this thing.

There was a great deal of unhappiness—instead of realizing that it was 36 states and it was a diffuse thing, no decisions had been made, we were just starting to look, all hell broke loose. It was clear that one of the sites we wanted to look at first was Alpena, Michigan. The governor found that out because we had talked with some people in the state about permitting processes to drill holes and things like that. The governor, Governor Milliken, was very upset. It was like not only no, but “Hell no, over my dead body no.” There was no—“Well, why don’t you come out and look and see, and tell us what you found and what you think?” It was, “No way, you’re not going to get one permit to do anything in this state.”

I had to go talk to Governor Finch in Mississippi. He had me in his office and he had the head of the state senate in the chair right next to him, so the legislative branch and the executive branch were listening at the same time. After I talked to him, he looked at me and said, “Boy, where’s the white meat on this turkey?” So it was a pretty contentious kind of interaction.

All the efforts started out with great intentions of trying to consult with the people. The first thing they wanted to know was, “Do we have a veto?” I couldn’t tell them they had a veto—that was a policy decision that had to be made well above my pay grade. In all the time we were dealing with this issue, not one governor was positive about doing that, except maybe Governor List of Nevada.

RM: Was he a little open?

DV: Yes. This was in ’76. Basically he already had nuclear weapons being shot off in his state, with a lot of nuclear material underground.

RM: As you know, I’ve talked with Steve Bradhurst, who was List’s man on the MX missile project around that time. List had an open mind on MX. It seems like that it was a character trait of his, whereas subsequent people . . .

DV: Richard Bryan.

RM: For example.

DV: All the other Nevada governors took a hard “no.” In all of that time, no governor of any state ever invited us to come and work. They agreed to us doing site characterization work under protest. They acquiesced but they weren’t happy about it.

RM: Discuss that hard attitude by the governors from the beginning.

DV: First of all, it’s a politically divisive question. “Are you going to give these people an opportunity to turn our state into the nuclear waste dumping ground of the United States?” And that’s where the discussion ended.

RM: Even with enticements?

DV: Yes. The government was willing to put up a lot of benefits for the state that took this thing, but they said, “We don’t even want to talk about it. It’s toxic. The subject is toxic.” Because if the governor takes a moderate stand, his opposition takes it as, “This governor is out of his mind, and we’re going to demagogue him to death.”

RM: Steve Bradhurst’s take on MX is that Dick Bryan ran against List and was very tough on him for List’s open-minded attitude toward MX. And of course, Bryan was governor when the Nuclear Waste Policy Act passed. I was at the meeting that you chaired at UNLV, the first meeting about siting a repository at Yucca Mountain, in the spring of ’83, where Bryan marched in with his entourage. He was the first speaker, and announced he was unalterably opposed. Steve’s take on that is that Bryan knew that if he took an open-minded attitude—if, in fact, he had one—he would get beat up just like Bryan had beat up List on MX. Do you agree with that?

DV: Yes. There was no up side to the opportunity.

RM: In the way it was portrayed, politically.

DV: Yes. And that was what Governor Finch said in 1977—“Where’s the white meat on this turkey?”

RM: In fact there was profound upside, but because of ignorance they couldn’t see it.

DV: Well, in ten years there’s an up side, but for my immediate . . .

RM: For my immediate political needs, I’m going to get beat up.

DV: Yes, it’s a death knell. Remember, in fiscal year 1976 Congress was the one that said, “We’re going to put money into this thing so that you guys can get your butt out there and start doing research and finding out the information to make good decisions.” And the governors turned around immediately and said, “No way, Jose, not in my state.”

RM: What was your position in ’77?

DV: I was an adviser to the division director on waste management. I eventually took over one of the divisions that dealt with the repository. I had the projects at Hanford and the Nevada Test Site; there was another division that dealt with all the salt sites.

RM: What happened next?

DV: Schlesinger, Carter’s adviser on energy, called up one night in later March 1977 and told the head of the program, “Drop the 36 states, pick five or six sites, focus on them. The political pressure is too unbearable.”

RM: And this was ’77?

DV: March of ’77. You’ve got to look at the time frame. The letter goes out the third week of November of 1976. Thanksgiving and Christmas muted the response to it. January, Carter’s taking office; people are starting to ask questions. That’s when I have to go up and see the governor of Vermont, the first one, and then the governor of Mississippi.

RM: You talked to these governors yourself.

DV: Yes, I was the one that was on the point; I was the one that wrote the letter. I was the government person who was familiar with the details and had a sense of the political concerns that had to be addressed.

RM: And then comes their response.

DV: Yes. “This is disaster,” is what they’re thinking. “Why would the federal government want to look at my state? We’re not going to let that happen.”

RM: In your view was this a major tipping point in the disposal effort?

DV: This was the major tipping point. Basically the federal government had done very little about disposal of high-level nuclear waste up until that point. For fiscal year ’76, the Congress put in a lot of money. The fiscal year of ’76 started in July of ’75, and that’s the year they had a transition quarter and moved the start of the federal fiscal year from July to October. So ’75 is when they got the first money to really focus on this thing. So it goes along for a year, and now we’re in fiscal year ’77; but it’s taking place in calendar year ’76, in November. And four months later March 1977, the thing is turned upside down.

RM: Was waste storage an issue at all in the election of ’76?

DV: No, because the letter hadn’t gone out. The issue in ’76 had to do with nuclear non-proliferation and controlling the fuel cycle, not disposing of the waste.

RM: But Gerry Ford had a different view than Carter on the controlling the . . . ?

DV: He was trying to buy time. It was a ploy to blunt Carter's attack by making it a nonissue because we weren't going to do plutonium in mixed oxide fuels. But Carter had a lot of people supporting him, a lot of grassroots—the National Resources Defense Council, the Sierra Club . . .

RM: Was this a tipping point in the world's movement toward nuclear power? I'm trying to tie this into global warming.

DV: No, that's not even a consideration then because nobody in Europe was doing anything on disposal of waste. The United States was always out there first in terms of making a proposal and actually taking on the job of doing site characterization, actually identifying a site on a map. Where in France are they looking for a site today? They don't have a potential location.

RM: I thought they were disposing of it.

DV: No, they're processing it and they're storing it in a facility like a monitored retrievable storage facility at La Havre, but they haven't started any disposal effort. Maybe the Swedes have gotten something done in terms of finding a site, but this is basically 30 plus years after the United States started its process.

RM: I'm trying to understand the sweep of history. I believe that the stalling out of nuclear power in the United States, and maybe one could argue the rest of the world, since Chernobyl and Three Mile Island has had a very significant effect on the future of the earth from global warming. Because if we had built more nuclear power plants, we would have less carbon dioxide in the air.

DV: Another one of my philosophical things . . . do you remember the Paul Masson commercial with Orson Wells? Orson Wells is sitting there with a Paul Masson wine and says, “No wine will be drunk before its time.” Well, in the government, no action will be taken, no decision will be made, before its time. There’s a joke that in Washington, D.C., there’s only two times—not now and now. It goes along not now, not now, not now—now, now! [Laughs] That comes back to my point—no decision will be made before its time. There are confluences of events that occur that will cause something to happen.

And in our particular case, one of the tipping points is the 1976 NRDC proposal for the NRC to discontinue licensing reactors and maybe to shut down operating reactors. As I told you, the NRC got by that proposal, but then there was the Northern States Power lawsuit in which the judge said the NRC ought to reexamine its position on waste management, and that pushed the NRC into the rulemaking process. There are a lot of confluences of events that make the federal government react.

That’s what I’m dealing with in this concept of the “Wisdom and Will” of the Congress. Had the executive branch distinguished itself in solving the social and political aspect of the problem? The answer is no, because it’s become a political problem. The executive branch has a difficult time addressing grassroots-based political issues. Congress said, “Get out there, get the required information, and lay the stuff on the table.” First of all, we will establish the technical referee that will judge the quality of this proposal, and that is the NRC. Then we’ll give the Energy Research and Development Administration enough resources to go out and hire competent people that are good in science and project management and all of the operational things, not necessarily good politicians who can understand the political process.” We had a lot of those people try to help us, but the people on the technical side had a strong feeling about the science. Remember the primary reasons for the failure of Lyons, Kansas. They didn’t necessarily understand why the local people were so upset about this because for the technical people, a site evaluation is still in the science investigation phase—no serious conclusions would have been reached.

Now that the politicians are lining up saying, “No, no way,” the question is, from a congressional point of view, are we going to have nuclear power? It becomes clear that it is important for Congress to step in—the purpose of the Congress is to deal with political problems. Congress knows that it’s important to the country to have nuclear power; that it’s important to be able to properly dispose of the waste. The question is, “How can they find a process that will allow the political issues to be presented, discussed, resolved, and move forward?” That’s where they were in 1977.

So they start looking at creating the Nuclear Waste Policy Act in the 1978 time frame. The NRC is beginning to prepare its regulations, 10 CFR 60, in terms how to dispose of waste and how to conduct the licensing process. We’ve got the confidence rulemaking that says this is the reason why you can have confidence that the waste management problem will be resolved and there will be a facility in place in 1998 to accept the waste. These things are all going on simultaneously.

Now you’ve got Carter coming in, and he doesn’t like the situation as it stands. So he starts creating a review group—the Interagency Review Group (IRG) on Nuclear Waste Management, in March of 1978. Do you remember a guy by the name of John Deutch? He ran the CIA for a while and he also was Secretary of Energy for a while. When he came out of MIT, he came down as an environmental guy, and he was put in charge of this IRG. There were 14 agencies involved in this thing and they were trying to figure out the strategy for siting a high-level radioactive waste repository.

There’s another thing going on simultaneously. When they wrote the environment impact statement for Lyons, Kansas, that facility was going to handle two types of waste.

It was going to dispose of high-level waste and it was also going to dispose of the transuranic waste from Rocky Flats that was temporarily stored at the Idaho National Laboratory—there were going to be two disposal levels. The EIS got a lot of comment about the high-level waste and the issues around that but didn't get many comments at all about the low-level transuranic waste.

The weapons people had made a commitment to the Idaho governor to get the waste out of Idaho by 1980. Now the basic site to meet that commitment is overthrown, so their commitment to the governor is not very good. And now it turns out that the things that the high-level waste people were going to do at the monitored retrievable storage facility—that was called the RSSF, the Retrieval Surface Storage Facility—was eliminated in 1975. So when all this was going on, the agency continued to consider what they called the alpha waste repository (alpha waste is the transuranic material).

They found a site in Los Medaños in southern New Mexico. It had good salt underneath it. Eventually, as they were planning the facility, they said, "Well, since we got all these comments about high-level waste and salt, we ought to create a developmental facility where we could run all of these experiments to show that the phenomena that commenters were concerned about regarding salt were not that significant. We'll call it the Waste Isolation Pilot Plant. And then we would license the Waste Isolation Pilot Plant to become the repository, once we showed that the waste was in there and there were no physical effects that would make it unacceptable."

RM: So we're back to salt.

DV: Yes, we're back to salt, and we're still interested in salt. Remember this is 1976. One of the things John Deutch, as head of the IRG and a program manager at ERDA, wanted to do was, "Maybe we'll put a thousand spent fuel elements in WIPP." When this is proposed at a public meeting in Albuquerque, the wheels come off the scooter. Nobody wants to hear that. While New Mexico might entertain a transuranic (low-level) waste repository, the state and public become terribly upset. Mr. King, I think was the governor, or Apodaca, and he was not happy. There were all sorts of political issues associated with the idea.

I'll use the words that are in this document to lay out that particular issue. The DOE, in a letter, asked the Nuclear Regulatory Commission, "If we did this and wanted to convert the Waste Isolation Pilot Plant to a high-level waste repository, would you be prepared to license it?" And the NRC said, "Unless we're involved in that process from day one, we won't license it."

Now, enter the armed services committees—the House Armed Services Committee and the Senate Armed Services Committee. They are interested in fulfilling their obligation to Idaho but do not want the delays that go with the licensing process. They don't want any part of the Nuclear Regulatory Commission licensing the facility in which they're going to dispose of their transuranic waste. At that point their position was that high-level waste will not go into WIPP because there were going to be two different levels of waste, low-level for transuranic waste and a level higher up, the high-level waste, and that was not going to happen. So they were adamant about that so they took control of it. They funded it, they controlled it.

By this time Dixie Lee Ray had come in as the head of the Atomic Energy Commission (1973 to 1975), she made the commitment to Idaho Governor Cecil Andrus (1971 to 1977) to have the waste out of Idaho by 1985. This new commitment reflected the problem with Lyons, Kansas. She took Glenn Seaborg's commitment of 1980 and extended it to 1985. And if you're going to license this thing, for which there are no license regulations yet, and get it done by 1985, it's not going to happen. So the Armed Services Committee said, "No way" to licensing by the NRC—no high-level radioactive waste.

RM: What happened next?

DV: The problem with the waste in Idaho sort of went away. Now let's come back to March 1977. In March 1977, it was, "Pick five or six sites. Also, we're getting pressure to make sure that you consider DOE sites that have already been contaminated with radioactive material." The issue was not to overlook property and facilities the DOE already owned. There was the potential for a facility at Savannah River—we looked at Savannah River as a potential location back in '68 and '69, but the then-governor of Georgia was upset about that idea. And the governor of Georgia at that time was Jimmy Carter. So now, he's president. You think we can propose Savannah River? Well, the Nevada Test Site's been contaminated.

They said, "Consider the Nevada Test Site." That was not in our plan because it didn't have any of the three proposed structures underneath it—no significant argillite, no significant crystalline rock (aside from some minor amounts), and no salt. They had looked at Hanford previously because of the basalt rock up there and there were about four salt sites— Mississippi, Louisiana, Texas, and Utah. Those were the ones that were looked at. On April 10, 1977, I had the first meeting with the Nevada Test Site to consider the process to evaluate potential locations for a repository.

RM: Do you recall who was at that meeting?

DV: I know the people from Richland, people from the Office of Waste Isolation in Columbus, Ohio, myself, a guy by the name of Critz George, Carl Kuhlman, and I suspect Colin Heath. I might be able to find a roster somewhere but that was basically it.

And the question was, are there any geologic formations on the Test Site that are of interest? There was some argillite under a place called Syncline Ridge, where if you stood on it, you could look at the nuclear weapons testing area of Yucca Flats. The thought of putting a repository that close to where they were testing nuclear weapons was not a rational thought. And there was the Gold Meadow Stock and the Climax Stock. There may have been one other one.

But the people at the weapons laboratories, the weapons testing people, were not strong supporters and one or two were dead set against it. A woman by the name of Barbara Killian was the ringleader against it. She prepared a paper that said a repository was going to interfere with the weapons testing program and relegated the repository site review to the southwest corner of the Test Site, the portion of the site reserved for nuclear rocket engines and the Plowshare Program, also known as Area 25.

That brought the program to a formation called Calico Hills. It was of interest because the aeromagnetics study indicated that there was a pluton of granite about 1,500 or maybe 2,500 feet below the surface. We were saying, "Well, that sounds like a reasonable place. Let's look at that." But at 1,500 feet it was blue goo and at 2,500 feet it was blue goo and there was no evidence of granite. We eventually found out that the shale that we were drilling through was filled with iron oxide, magnetite, which impacted the results of the aeromagnetic surveys.

RM: Oh, and that's why they interpreted it as granite?

DV: Yes. When they took that into account, the granite was more likely about 6,000 feet below the surface, which was too deep. The last place considered at the Test Site in 1977-'78 was Yucca Mountain. The U.S. Geologic Survey thought that that was potentially a good place.

RM: And what was their thinking in looking at Yucca Mountain, that it was a good place?

DV: They knew the water table was very deep. And there was a strong proponent in the USGS to do disposal in the vados zone. That means the unsaturated zone. If you're in the saturated zone and you dig a hole and let it sit for a while, it will fill up with water. If you're in the unsaturated zone and you dig a hole, it won't fill up with water because you are above the water table. And again, moving groundwater is the major mechanism for carrying radioactive waste away from the repository.

If you do a systems analysis of a repository and ask, "What's the purpose of the geology?" The geology, first of all, is to provide a barrier between the waste and man's environment. Second, it's to provide the geologic material in which you can build an underground structure that's going to be stable for the necessary period of time necessary.

Then you ask, "What's the purpose of the hydrologic system?" Well, the purpose of a hydrologic system is to carry the waste away from the repository. The systems engineer would say, "Why would you do such a stupid thing to put a hydrologic system in there?" If you are using what nature gives you, you have to figure out how to deal with the hydrologic system.

And you say, "That's what's there. You've got to take it into account." From a systems analysis point of view, you have to be able to say, "That's what it's going to do." You have to know the purpose of it, even though you don't want it to carry the waste away. If you've got a hydrologic system that doesn't have any water in it, is there any chance of it carrying it away? Not very likely. People argue that there's a small amount of water there, and we know that, but it's not water that's readily moveable. It's going to move a little bit but it's not like an underground river running through the repository.

RM: Was there any discussion initially of the welded versus the unwelded tuff as a part of that initial discussion?

DV: Yes. The issue was the unwelded tuff is chemically reactive and if you put a heat source into it, it can change it. And it's much further down and it's closer to the water table. So what we were looking for was a suitable horizon that was about 1,000 feet below the surface, which was a good rock for underground structures, but one that was separated from the water by a significant distance. And the fact is, the unwelded tuff is zeolitic so it would chemically react with chemicals in the water . . . anything that would want to percolate through the formation, it would be like a water softener and would take the minerals out of the water, or the waste out of water, temporarily.

So those were the thoughts in those early days. And there was another formation about 3,500 feet below the surface that was very nice, but it was below the water table.

To back up, the program was changed in the spring of 1977 to focus on six sites and get them done so we could deal with six states politically—the Carter Administration couldn't deal with 36 states. One of the political issues was to consider regionalism. Since the reactors were in the eastern part of the US, the political view was that a repository should be in the East. You had to try to find different places. They also wanted multi-media, so we had four salt sites, we had one in some other kind of geologic medium we didn't know yet at the Test Site, and we had the basalt at Hanford, which is a crystalline rock.

So now, we've sort of done those things. The pressure was regionalism and to look at DOE sites that have already been contaminated. Consider salt, which is your geologic medium of primary interest. Consider these other two sites that have different geologic media. But they're also contaminated, the Nevada Test Site from atmospheric and underground weapons testing and Hanford from leaking tanks, which was the political pressure that we were getting from people saying, “Why in the hell don’t you use a site DOE already owns that’s contaminated, so we’re not going to screw up another site?”

RM: So in ’77, DOE or whoever started looking at these six sites.

DV: DOE comes into existence on October 1, 1978. The Energy Research and Development Administration went away because energy was so important now to the nation that it had to become a cabinet-level position, not an independent agency. Remember, every US nuclear weapon that’s out there was manufactured by the Atomic Energy Commission, the Energy Research and Development Administration, the Department of Energy. So there’s pressure, for a number of reasons, to turn this operation into a cabinet-level position. And Schlesinger becomes the first Secretary of Energy.

RM: So ERDA became the Department of Energy?

DV: Yes. And more things were thrown in because then you got the Energy Information Agency and the Federal Energy Regulatory Commission, which used to be the Federal Power Commission. I don’t remember all the elements that went into it. I think this is when solar and wind power came into the department.

RM: Did that dilute the mission of finding a repository or disposal of spent fuel or waste?

DV: No. There were a lot of different interests, but it was still focused on that problem.

RM: So what’s the next thing?

DV: In this whole process, this interagency review group under John Deutch that’s looking at waste management, the U.S. Geological Survey issues a paper that’s somewhat critical of DOE’s approach to solving a problem; but eventually, the interagency review group comes to a conclusion. It puts out a report, and it put out a significant number of recommendations.

Let's see, [reading] "IRG prepared a draft report to the President which it released for public comment, along with a subgroup report on alternative strategies for isolation of nuclear waste. A major philosophical point in the conception of the approach was to solve the waste disposal problem within the generation's lifetime." They're saying, don't push this thing off to another generation. "The draft presidential report draws heavily on the analysis of the subgroup report and its appendix, which assessed the state of knowledge with regard to geologic repositories. . . . The discussion led to general agreement that the permanent disposal should be pursued." What Obama's doing now with the blue ribbon committee was done, in a sense, by Carter, focusing more on waste disposal rather than all the stuff that Obama's thrown in.

RM: What has Obama thrown in besides disposal?

DV: Alternative reactor technologies and waste reprocessing, things like that. "The IRG recommends proceeding with the geologic repository and left it to President Carter to resolve how many sites and geologic media to study before selecting the first site. . . . And the draft presidential report agreed that waste disposal programs should proceed on the assumption that the first disposal facility for high-level waste will be in a mined repository. The site characterization work in a variety of geologic environments should be accelerated. Funding should be increased for near-term technical alternatives to geologic disposal." In other words, find something else. Not highly probable, but spend some money on it.

"Initial placement of waste in a repository should be done on a technically conservative basis and should permit retrievability. The opportunities should be pursued if available to site a licensed intermediate-scale facility in which as many as 1,000 spent fuel elements, fuel rods, or waste canisters would be in place with the possibility, but not necessarily with the expectation of their removal." That was John Deutch's problem with WIPP. I mean, put the 1,000 spent fuel elements in it. "The IRG disagreed with the strategy to be employed in choosing a site to be submitted for licensing and on the future proposal of the Waste Isolation Pilot Plant."

Now, this is another place where Carter decided that he didn't want WIPP to be built. "In 1980, Carter announced the administration comprehensive waste management program. He ratified all the unanimous IRG recommendations. He resolved the two controversial issues of site selection strategy and WIPP. He decided to adopt a siting approach in which four or five sites in a variety of environments would be characterized extensively before a license application for one of them would be submitted to the NRC.

"The president also decided the recommendation to Congress to terminate the WIPP project, despite the fact that the 1980 date was committed to by Glenn Seaborg to Governor Cecil Andrus of Idaho and subsequently revised, reinforced by Dixie Lee Ray to Governor Andrus, to a 1985 date to remove the transuranic waste from Idaho." So basically, you had the president saying to the governor of Idaho, "Drop dead." Now, what happened was in June of 1980, Congress overruled President Carter with the WIPP budget and the WIPP project would be continued.

RM: And Carter signed it, obviously.

DV: "But Congress was more sensitive to the federal government's commitment to the state issues than was the executive branch." This is a third element of the "Wisdom and Will" of the Congress: "Don't stick it in the eye of the governor of a state. If you made a commitment to solve a problem, solve the problem." In other words, stay the course.

RM: Just for the sake of clarity here, the first two items were?

DV: The first item was creating the Nuclear Regulatory Commission with the requirements to license the repository for high-level waste, to be the technical referee in the review of the proposed site.

RM: That was the "Wisdom and Will" of Congress.

DV: Yes. The second one was putting the financial resources into the agency (ERDA) to get the intellectual and technical capability to solve the problem and the resources to go out and start investigating these things. What was wrong with Lyons, Kansas? They didn't do any competent site characterization. They made a blunder because the site was highly questionable.

RM: So this is, again, the "Wisdom and Will" of Congress. And the third part was stay the course for WIPP, to meet the executive agency's commitment to the state governor.

DV: Stay the course and don't screw the governor of the state, once you made a commitment to him. I mean, how are you going to get the governors to agree to something if you say you're going to remove something from the governor's state and then the president tells him, "No way."

RM: Yes, your word's no good.

DV: Right. The credibility of the Atomic Energy Commission went to hell with Lyons, Kansas, and now, with Carter's approach, the credibility of the executive branch would go to hell. This was really unusual political sensitivity considering Carter was a state governor. And Congress says no, you can't do that. People don't understand that history. That's why, in this document, I tried to lay out the logic of how we got to where we are. And how the Congress had taken a number of actions to facilitate a process and an infrastructure to solve a problem. The executive branch did some good things, but they made a lot of blunders. The Congress was the one that tried to deal with the political framework in which a significant technical issue could be resolved. And they tried to put in the elements—independent review, the resources to get competent people to do the job, the commitment to honor their word.

CHAPTER FIVE

DV: The next advancement was the Nuclear Waste Policy Act of 1982. What I have just described were the activities that were going on. And you've got to have a timeline. Several timelines are going on and you could see the interrelationship of all of these things.

RM: Do you have a lot to say about how the '82 law was put together?

DV: I thought it was a marvel. Congress really wanted the issue resolved. I mean, the political people took it on themselves to find a way to resolve this issue. They dealt with a lot of testy issues. The states wanted to have an outright veto; however, the Congress kept for the final decision for themselves, in the Nuclear Waste Policy Act, the ability to override the states' veto. And that's what exactly happened. They made a proposal for Yucca Mountain to be the repository, they allowed the governor of Nevada to review the proposal and offer a veto, which he did, and they overrode the veto.

The only person that gave up that right, usurped it, was Harry Reid. He said, "Now that I'm running this whole place, we're going to override the Wisdom and Will of the collective Congress by this maneuver." I think that's the issue—how can one senator be much better than the other 99 in this decision when, in all of this proposal and review process, the entire Congress voted on it, both houses?

RM: Do you think his actions may ultimately prove futile?

DV: I don't know what's going to happen. I can't predict what a political body is going to do. Right now the balance of Congress appears to be a NATO organization on the issue – No Action Talk Only!

RM: It seems so contrary to the trajectory of this story.

DV: That's the whole thing—the collective body of the Congress solved the problem.

RM: And then he comes and throws a monkey wrench in it.

DV: Right, because the collective Congress overrode the veto of the state.

RM: I have been told by people who have been at Obama's blue ribbon committee meetings that Secretary Chu put together that it's just a rehash of the same old, same old. These issues have all been dealt with. So where are they going to go after they're done rehashing it? We're going to have to go back to a repository.

DV: Well, considering we have a country that is capable of throwing away $8 billion or $10 billion of work and requiring that we start over again—and that $8 billion, in today's term of reference, is going to be $40 billion, $50 billion . . . But we're so rich and we don't have a debt problem or anything at all like that.

RM: And meanwhile, we have the greenhouse effect breathing down our neck.

DV: I can't predict what's going to happen.

RM: I believe the action on Yucca Mountain won't hold.

DV: There are a lot of different irons in the fire. But if Harry Reid's reelected, it's going to be hard to get anything through him.

RM: A lot of people are saying that he's not going to be a majority leader next time around, even if he is elected. He's certainly not an effective majority leader for his party.

DV: I don't know about that, but I find it fascinating because at one time we were beating to solve a problem and now they're beating to ignore the problem. That's typical of politics; as I said before, where are the forces balanced? What we saw for a long time was the forces were balanced under the Nuclear Waste Policy Act—all the way from 1982 to 2008.

DV: In the 1978 to 1980 time frame we had the interagency review group, and Carter's proposal to shut down WIPP, which the Congress did not agree with. Other things were going on at the same time. The Nuclear Regulatory Commission was in the process of writing its regulations, 10 CFR 60, that would govern the licensing of a nuclear waste disposal facility.

And there was also some upheaval in the Department of Energy because the Office of Waste Isolation from Oak Ridge, Tennessee, was not exactly happy with the way the Department of Energy was taking over its program. I believe in March of 1978, they told the Department of Energy they did not want to stay in the program. Oak Ridge was selected for this work primarily because they were the ones that had run Project Salt Vault in Lyons, Kansas. They had extensive knowledge of underground facilities and nuclear waste so they were asked to organize this effort and develop the concepts for it, but they tended not to take into consideration a lot of the political factors. And some in the organization thought Lyons, Kansas, was still a viable site. So there was some disagreement on a number of issues, and Oak Ridge decided that they did not want to stay involved any longer.

Woody Cunningham was, I believe, the assistant secretary for the area that covered waste isolation at the time. DOE contracted quickly with the Battelle Institute, in Columbus, Ohio, to take over the program, which became the Office of Nuclear Waste Isolation, ONWI. So there were some significant changes in the technical support that the Department of Energy was going to get.

At this time also, in 1980, after the Nuclear Waste Policy Act had been refurbished to focus on low-level waste, the Congress, again, started to reexamine the issue of high-level waste, with the intent of issuing a nuclear waste policy act for high-level waste by the end of 1982. So Congress proceeded, and by the end of 1982, they issued the Nuclear Waste Policy Act of 1982. It gave the responsibility to the EPA to set a safety standard and to the NRC to review and license the operation and to DOE to search for sites for nuclear waste disposal. The act provided for two repositories—one in the East and one in the West. That is, regionalism.

It also created a funding mechanism to pay for the program. At that time it was called a tax, but it's actually a fee that the users of nuclear generated electricity would have to pay, like one mill per kilowatt hour. That would generate a significant amount of money to cover the expense of doing the site characterization and developing the depository. It gave DOE the opportunity to dispose of defense-related high-level nuclear waste in a civilian repository, rather than having to design and build their own. It allowed for the establishment of a monitored retrievable storage facility, if it was required.

We had looked at the issue of monitored retrievable storage several times. I think in 1976, there was a proposal to put waste in the tunnels used for weapons testing at the Test Site. It was brought up again in 1978, and a report was written and sent to Congress on monitored retrievable storage facilities. Basically, they were something that would be, like the EPA thought with the RSSF, counterproductive with regard to solving the problem of permanent disposal of high-level waste. The DOE was focused on finding a site and assuring that geologic disposal would, in fact, be capable of isolating the waste.

As I said, the Nuclear Waste Policy Act allowed the governor of the state in which the facility was sited to disapprove the project but it maintained for Congress the authority to override the governor's veto, if the veto was not substantial in its nature. The Congress kept to themselves the ultimate responsibility for making the decision about going forward with a repository for waste disposal. The act required that an environmental assessment (EA) be prepared to describe the impact of the thing, but it was not a traditional environmental impact study, or EIS. This was a specific thing called an environmental assessment, and it was not going to be subject to all the hassles that go with the conventional EISs.

RM: Like following the rules of NEPA (National Environmental Policy Act) and all of that?

DV: That's right. It was, basically, to provide a substantial technical analysis about what the potential environmental impacts were for the purposes of comparison. It also required a standard contract to be negotiated with the utilities for the disposal services. The utilities with the oldest waste would receive the earliest allocations for spent fuel. The idea was to take the fuel that's been in the fuel pools the longest time and put those things into the repository first, because they had the least thermal energy.

By the end of 1982, the Congress had dealt with the fourth, and possibly most important element, of what they call the "Wisdom and Will" of the Congress, in terms of laying out the specifics of a process to move forward that was considered politically balanced and acceptable. And it was accepted by the Congress as a whole. There were dissenters, certainly, but in this democratic society, the Congress had done its job in terms of trying to find a way to resolve the issues. The most important aspect of the process, it was structured to allow the Federal Government to solve the problem, not short-stopped by a state. I think this is incredibly important because the executive agency was so focused on solving the technical problems, and making sure that there was a significant basis for safety, that they were not capable of figuring out how to resolve all the political issues. Congress, being a political body, had a fair amount of experience in figuring out how to deal with contentious issues and find a reasonable path forward. So by the end of 1982, there was a reasonable framework on which to move forward and implement a workable solution.

I was still in Washington at that time. I believe I was managing the division related to repository development. I still had the responsibilities for the projects that were being done at the Nevada Test Site and at the Hanford Reservation.

Let me go back, now, to 1978. At that time, the Deputy Secretary of Energy, Jack O'Leary, was concerned that if there was no spent fuel in the ground by 1980 there would be a significant disruption in the nuclear industry. He wanted us to look at ways to conduct a demonstration project to show that we could take spent fuel elements and put them in the ground in a configuration that was similar to a repository, and one that would potentially provide scientific data with regard to that.

It fell upon two of us at the headquarters to sort of pursue that. One was a fellow by the name of Critz George, who was responsible for the evaluation of the salt sites. They were trying to find a place that they could store the waste in salt. I had the Hanford Reservation and the Nevada Test Site. There were proposals forthcoming from both Hanford and the Nevada Test Site for demonstration facilities. Since the Department of Energy owned no property that had salt under it, Critz was not able to establish a situation that would allow us to put spent fuel in salt sites. So it came to me to sort of husband the work on these two sites—to get the Nevada Test Site and the Hanford office to send us proposals on those things. And as a result of that, both sites were considered reasonable approaches to solving the problem.

We started the Near-Surface Test Facility at Hanford around April 1978, and what was called the Climax Facility at the Nevada Test Site in May 1978. Climax utilized the shaft that went underground about 1,400 feet in which they had done a weapons test called, I believe, Hardhat. So while that weapons test was off in one direction, we went in the other direction and built tunnels and so on. We acquired spent fuel elements from the Turkey Point reactor in Florida, north of Miami, and shipped them to the Test Site. Some of them went to Columbus, Ohio, to the West Jefferson facility, for characterization by the people at Battelle. I think we acquired 13 spent fuel elements at that time and some of them were used in demonstration and surface storage facilities and 11 of them were used in the demonstration of placement of spent fuel underground in the Climax facility.

In my mind, Hanford lagged, in terms of its technical development, compared to the project at the Nevada Test Site, so we canceled the one at Hanford in 1980 and continued on with the one at the Test Site. Lawrence Livermore Laboratory had developed six very interesting experiments to gather scientific and engineering data about what was going on if you replaced the spent fuel elements, or thermal heaters, and found out whether or not there was any difference in the data.

RM: So you compared the two?

DV: Yes. And basically, we were able to build the transport equipment and other support equipment. We stored the spent fuel elements at EMAD on the southwest corner of the Test Site and transported them up to the Climax facility, which is on the northern side. We were able to do everything remotely, in terms of getting the spent fuel elements lowered 1,400 feet into the underground and then having them received by a huge cask that was radiologically safe. It was all controlled from the surface. We would place the spent fuel elements in the containers, then put a covering back over them. That project went on for about three years, until the spent fuel elements cooled off to the point where you weren't getting any more useful data.

RM: Just for the record, what is a spent fuel element?

DV: A fuel element's a bundle of 20 or 40, I can't remember the number, of rods.

They're in a block about 10" by 10" or 14" by 14". They were very radioactive. The test took us 24 months and 21 days from the time we said "go" until the last spent fuel elements went in the ground. And the experiments continued on until 1983 or early 1984. They pulled them out and then kept them for a while and then shipped them off to Idaho to wait for disposal.

That was one of the significant experiments, and it also provided a significant opportunity for people to come and see what a repository looked like. It gave us a substantial edge, in terms of communicating with people, of what they might look like and what it was like to be able to walk in and stand on top of them and not have any risk or radiation exposure.

RM: What are some of the things you found out in the experiment?

DV: We found out that the heat transfer by radiation was more effective in rock than it was by conduction. We found out certain things about the heat transfer characteristics, about removing heat from the tunnel through the ventilation systems. And we found out there was no significant difference in whether or not to use the spent fuel element or a heating element, with regard to getting data in crystalline rock. I don't remember all the details.

RM: And the fact that heat transfer by the spent fuel rod was faster than by conventional means was a plus for the repository, wasn't it?

DV: Well, it has to do with the fact that if you've got an open tunnel, and you've got heat coming up from the floor, it transfers to the wall faster than it does by going through the rock to the wall. And we found out that there were certain things about when you cut and open rock, how much the rock deforms. And did the models, the rock mechanics, predict the nature of the deformation that was going to take place?

RM: When you cut a hole in a rock, the rock that's remaining deforms?

DV: Oh, yes. If you cut a tunnel, you're cutting it in rock, so you're taking out the middle, where the supports are, and there's a tendency to come down a half a centimeter or so. You know, the world is dynamic, it's not static. But by 1984, 1985, we closed out Climax as a test facility. It was very instrumental in terms of communicating to people what the nature of a repository might look like.

RM: And it was pioneering, leading-edge science, wasn't it?

DV: Well, it was a good demonstration of science. You always have to see the practical application of the science, and does it reflect the fact that you know what you're doing? I think those are the things that came out of it.

RM: I remember early on in the Yucca Mountain program, they showed films of the casks and all that. Were they developing those in parallel or they had already been developed?

DV: The cask had already been developed. The requirements for shipping spent fuel elements was an issue long before the repository ever came to be an issue because we had to ship the elements from a reactor to a storage facility. You always had to be able to move radioactive material in a safe manner. Sandia Laboratories took a very strong lead in developing that and in looking at experimental methods and demonstrating that the kinds of possible accidents you would expect to occur were more than likely not going to compromise the safety of the public because of damage to a cask. They were built with a great deal of robustness so that they could withstand the forces that they were going to experience.

RM: And now you hear critics, mainly Senator Reid, saying that the real problem was the transportation of the waste. Is that issue really a canard, or does it demonstrate ignorance?

DV: I think that if you look at the things, that may be the highest risk there is, but it's not significant. The question is, is someone going to die from the radiation or is he going to die from the truck running him over? There's a greater possibility of loss of life due to a conventional accident than there is from any radiation that might leak out of a cask and then cause somebody to be exposed to a level where their health would be damaged.

RM: So the safety issue associated with the casks, and the transportation, was a solved problem?

DV: Basically, the technical people believed that they could put the casks on trucks and trains and could move them. And if there were accidents along the way, there was very little likelihood of any significant health damage to people. The question becomes one of what's the nature of the risk?

I mean, if you've got ten cars of chlorine moving back and forth, they can do a lot more damage, quickly, than you might expect. And we've seen that sort of thing happen. The issue with radioactive material is that it's long-lasting and it's very difficult to clean up. The way that you avoid having to clean it up is not to let it be disbursed. That's why building robust containers is important.

So there's a group of people that are making decisions based upon some intuitive sense, as opposed to understanding the physical reality of what's occurring. That's a viewpoint, an opinion. They hold those things and they will argue. And there are a lot of people that are not trustful of the government. We saw that the Atomic Energy Commission lost their credibility regarding some things with Lyons, Kansas. And we saw that some other people in government lost credibility by proposing ideas that were not very rational.

So the question becomes one of who do you trust? It's always easy to demagogue an issue by taking the easy road and saying that it's not going to work. Just think of the discussions with Governors Bryan and Bob List. Bob List had kept an open mind. Bryan took a strident position that it was very risky to do this, and Bryan dominated in that discussion. But his position is not based upon fact; it's based on people's intuitive sense and the lack of understanding by the populous.

RM: What could the government have done to make the populous better understand the issue? In this country, maybe vis-à-vis Western Europe, there's a distrust of science that is not appropriate to the capabilities of science. What could the government have done to help alleviate that?

DV: I don't know. This was an issue that we dealt with in Nevada when I came to take over the Yucca Mountain Project—what capability do you have to use the mass media, as the politicians have the capability of, of getting your point across? The difficulty for the government—and this is weighed out in policies by the Office of Management and Budget—has to do with propagandizing: Is the government giving you the truth or are they giving you propaganda for their own benefit?

As a result, there was a very strong reluctance by the federal government to get into an aggressive program to demonstrate the positions that would be supportive. And you had a lot of freelancers at that time. I can remember seeing programs on television where people were misusing facts about nuclear energy, radiation, and so on to create interesting stories on the public networks on how these things were terrible, and they were going to be responsible for lots of serious injuries to the public. The government didn't do anything to respond. My viewpoint was that you had to have a very strong effort to communicate the facts; but on the other hand, you had better have your facts straight. If it becomes clear that you are inaccurate in what you state, then your credibility is lost and you're even in worse shape.

RM: I've always wondered why the nuclear industry didn't do their best to educate the public and to get proper information in their hands. They did very little, as near as I can tell.

DV: That may be an accurate representation of what took place.

RM: Yes, but why? Wasn't it in their interest to have the populous know the reality of nuclear energy?

DV: Well, who were they going to get to do that? The experts tend to work for the government.

RM: True, but, they could assemble the information and do documentary films and press releases saying, "It's been shown at Sandia Laboratories, and it's been shown at Lawrence Livermore," and so on. You don't have to have the government in that advocacy position.

DV: Remember, the industry at that time was under severe attack by a lot of people. And just keeping their reactors running was a full-time job because there was a group of people that wanted to shut them all down. It's hard to remember all the details, the nuances of things that were going on, but it was not a friendly time for the nuclear power business. Three Mile Island was in '79 and Chernobyl was four or five years later. And Chernobyl was the far more significant one. So you had pretty good demonstrations that the systems were not foolproof, but could fail. The ones that the Russians had were far more demonstrative of sloppiness in management than the Americans had. But it was one of those things where everybody was willing to point the finger, and say this is not supportive.

RM: But again, America, one could argue, overreacted to all of this, whereas France didn't.

DV: France is a special case. The French needed electrical energy and they did not have a good supply of coal and they did not have any resources in terms of oil. The question is, what do you want to do in terms of your national debt in finding something you're going to burn up? They were very pleased to jump on the nuclear bandwagon. They were heavy into the nuclear weapons business—they had developed their own nuclear weapons. And they had gotten into an agreement with Westinghouse for developing commercial nuclear power plants. I think it was spring of '76 that the French company Framatome abrogated their contract with Westinghouse and threw them out. They said, "We're going to take all the technology you developed, and we're going to start our own business." And you can see where the French are today, in terms of a company called Areva.

As a matter of fact, I was at one of their facilities the day that takeover happened. I was with Bob Seamans, who was the head of the agency, and with Dick Roberts, my boss, who was the Assistant Administrator for Nuclear Energy, and we were caught in one of their facilities by an onslaught of the Communist workers who thought we were there to reverse the thing. It was a rather testy moment. They blocked all the entrances and so we had to go out the back way, down the canyons.

France is a very aggressive country in terms of commercialization of technology, and in selling their services anywhere they can in the world. When I worked for the Assistant Secretary, I was responsible for negotiations with the French on the exchange of scientific and technical information. Essentially, they were very strong on us giving them most things for nothing. And the things we wanted, they wanted us to pay for. So we said, “I don’t think we’re going to negotiate under those circumstances.”

But for their own economic benefit, the French realized that they were going to need nearly all of their electricity generated by nuclear power. They set on that course, and they were steadfast in making it happen and they did a lot of great development of the technologies for those reactors.

They also were going to export the thing. They were willing to sell fuel for reactors to countries like Japan, and so on. They would take the fuel back after it came out of the reactor, when it was radioactive, and reprocess it at Le Havre, extract the radioactive materials, recapture the plutonium and uranium that had not been burned, and then turn their waste into glass logs and store them in a surface facility, until such time as the other country could either build their own repository or buy space in the French one, if the French ever develop one.

But to my knowledge, the French have not been very far along in finding a site for a repository. Again, in Europe, people have talked about it. The only people that seem to have taken the bit in their teeth and attacked the problem have been the Swedes. They’re very lucky in the fact that they do not have a strong tradition of state’s rights.

They do not have an intermediate level of government between the federal government and the local government, which is different from the United States; we have very strong state governments that have significant resources and make their own decisions. When it comes to solving a problem of this nature in the United States, the federal government is not lucky enough to work directly with some local community in deciding the matter. They always have to work through the state government. And the state governments, which are sovereign political entities here, have been steadfastly 100 percent against any thought of putting a repository in their state. So there’s no comparison between the United States and Sweden.

RM: A couple of people I've talked to have suggested that, were the Yucca Mountain repository program to be revived and get back on track, one strategy would be to take the state out of it and not give them veto power. What's your take on that?

DV: Under the Constitution of the United States, you cannot take the state out of the picture. That comes down to the phrase "state's rights." What authority is not given to the federal government is given to the states and the states are responsible for the safety of the residents of the state.

RM: Did the original legislation, or at least the way it was interpreted, give Nevada too much say?

DV: No, I don't think it did. It's just that the federal government was tardy in terms of moving on with the problem because the people did not want to deal with the issue at the time and let it slide another year. Remember, I said there are two times in Washington, D.C., "now" and "not now." Most of the time it was "not now." It wasn't until 2000 that George Bush said, "Let's go ahead with it. Let's put in the proposal." But we knew by 1990 that the place was good.

The question was, how much more do you want to do to be able to convince us? So you sat around another 12 years, and in the meantime, the state was very progressive in terms of hiring a lot of people that were good in the areas of risk and public concerns and so on. The state paid them to do research that would point out that they were terribly concerned—the repository was so risky that nobody would want to do it. If they put it in the state, people would move out of the state. There was a great deal of sociological research done, sponsored by the state of Nevada, that built up almost a grassroots objection to the repository.

RM: And it was encountered by the nuclear industry and everybody else.

DV: Yes, there was nothing done to the contrary.

RM: I'm probably asking you the same question again, but is that a flaw in the original legislation? How could that be prevented, if we were going to cycle back on the thing again?

DV: The legislation was not the problem. It's a question of the imagination of the people who were responsible for the effort, and what they were going to do. They had lots of flexibility.

RM: In what way?

DV: You have internal resources; you have the ability to make presentations. Instead of being reluctant to do that, you should have been out there finding a way to aggressively make the case for the government. But you had to do it in a way that wasn't propagandizing and you had to do it in a way you weren't giving people misinformation. Remember, again, this is a confidence game. It's, more than likely, the toughest confidence game there is. If you're going to maintain confidence, you have to be accurate all the time.

CHAPTER SIX

RM: Yes. You basically can't make any error. So now I guess we've arrived at the Nuclear Waste Policy Act of 1982. Do you want to say any more about the behind-the-scenes passage of the act or about the kind of politics that went on?

DV: I wasn't close enough to the politics. If you want to find out about the politics, you can call up Critz George and talk to him. He was more involved in things like the National Governors' Association and their committee on waste disposal.

RM: At the time of the passage of the act, where were you?

DV: In 1982, I had moved to Las Vegas.

RM: Because you were involved in the experiments at Climax?

DV: That's not the reason. I mean, I did a lot of that work from Washington, D.C., but the person on the ground was a guy by the name of Bob Nelson. He was the project manager who was hired to run the program in the early days.

From 1977, a fellow by the name of Mitch Kunich, who worked at the Test Site, and myself were the ones who were responsible for organizing and managing the program at the Test Site. It took a long time; I don't think Bob Nelson came on board until 1979. The project got started in those early days in terms of looking at the site—with the U.S. Geological Survey, the Sandia Laboratory, Livermore Laboratory, and Las Alamos Laboratories, mainly because those people had the technical expertise, they had been on the Test Site before, and they had all the security clearances to work at the Test Site.

RM: At what point did Yucca Mountain, itself, come into focus?

DV: In '78. When we finished with Calico Hills and decided there was nothing workable there, the USGS said, “This is the last place that we can look at on the Test Site.” Because if that area didn’t pan out, the Test Site was history.

RM: Were they looking at any other places in Nevada, like around Fallon?

DV: There was a study done by the U.S. Geological Survey called the Basin and Range Study. They were into the concept of the geologic systems and hydrologic systems. The Basin and Range was very attractive to the geologists because it was a closed basin—anything released into that basin will never go to the ocean. They did some studies in those areas, and we never paid much attention to them. Our responsibility was to determine if there was a viable repository site on the Nevada Test Site.

The mission at the Test Site was to evaluate the Test Site, seeing if there was anything there that was reasonable and, if so, that we could develop. When we started the project, Yucca Mountain didn’t look like the most favorable place because it was a complex geology. And if you stand on top of Yucca Mountain and look westward you see cinder cones, which are evidence of basaltic volcanism. So it wasn’t something that looked like it was going to be very fruitful. But first blush can sometimes be misleading.

Our job was to turn over all the big rocks first, to see if there was anything significant that said, “This site is no good. Let’s not waste any more time here.” And we did that. That was the strategy—look at the thing and say, okay . . . We still thought salt was the reasonable geologic medium. If there was a valid reason to say Yucca Mountain wasn’t an acceptable site, we would close it out and move on. I always joked that I did not want the epitaph on my tombstone to say, “He picked the wrong spot.” We were very conscious about the potential of getting a negative answer and accepting a negative answer—we had no motive to force a positive answer.

RM: Can you talk a little bit about the evolution of your consideration of Yucca

Mountain at that time?

DV: The guidance I had at that time was, “You’ve got two years to do some experiments and get some information.”

RM: This was when?

DV: The ’78, ’79 time frame is when we started. We were examining the site and trying to identify a place for an exploratory shaft. We could go underground if we were lucky enough to find that there was potential.

RM: And you were involved in that, right?

DV: From headquarters, yes. Bob Nelson was hired, as I said, at the beginning of 1979. He took over the work on the ground, and he was heavily involved in getting the Climax facility up and running. So we looked at those things at Yucca Mountain, and we didn’t find anything immediately that said, “This is so disastrous that it’s not going to work.”

The thing that’s attractive about salt is that the geology of salt formations is relatively simple. They’re in sedimentary basins with not a lot of turmoil at the sides of the formation. If you’re dealing with something like Yucca Mountain, you’re obviously in an area that has been thrust up and subjected to tectonic forces. The phenomenon that is nice about salt is that the salt seals itself automatically. In hard rock you get a lot of fractures and so on. That means that the modeling that you have to do regarding the migration of water and radioactive materials is a lot more difficult. It’s not as easy to analyze and to build a convincing case that would work as it would be for salt, especially if you’re below the water table. But if you’re above the water table and there is no significant flow of water, the analysis is easier.

We were beginning to look at those considerations, and about 1981, there was a new group of people that came in with the Reagan Administration and they had some different ideas. It was suggested to Colin Heath, the head of the office in Washington, D.C., that he do something else in life. He was a person I had a great deal of confidence in. He and I had significant disagreements, but he was a competent person. I don't remember what the title of his position was, but he was responsible for the repository project. The Republican administration felt that he wasn't aggressive enough.

One of the things that happened was that the Nuclear Waste Policy Act created the Office of the Civilian Radioactive Waste Management in that time frame. We were getting a new head of the agency, a person by the name of Ben Rusche. Other people were coming in and they had different ideas about how to manage the program. For about three years, I was the U.S. Representative to the IAEA Committee on Underground Disposal. And when I came back from my last trip in November of 1981, I quit and took a job in the Department of Energy fusion program. The politics of it was, in my mind, so crazy and uncomfortable that I quit and I went to work for the Office of Magnetic Fusion, running the materials R&D program. It was another branch and another assistant secretary. That was in November of 1981.

RM: Was magnetic fusion what we would call nuclear fusion?

DV: At that time there was still belief that fusion would work. One of the things you found when you got into it is that there are so many technical problems, it's a snowball's chance.

RM: The whole fusion approach is improbable?

DV: If I'm a betting man in Las Vegas, I'm going down to the sports book and bet the mortgage money on No—that fusion won't work.

RM: Ever? Or do you mean for the foreseeable future?

DV: For the foreseeable future.

RM: So the technical problems are just too great?

DV: Yes.

RM: Was the work in fusion a positive experience?

DV: Yes, it was. I like the technical work. It was something that I was interested in and it gave me an opportunity to do things that were different and I didn't have to deal with the political hassle every morning.

RM: Give me an example of the political hassles you had to deal with before you quit.

DV: It was like getting the letter out to the governors. You couldn't do anything until after the election season. When we first started Yucca Mountain Project, it was called Nevada Nuclear Waste Storage Investigation, NNWSI. Which means "Next November We Start Investigating." [RM laughs] There are all sorts of political people. You get letters from congressmen and you've got to explain why you're doing what you're doing and all of this kind of stuff.

It just gets to be a complete hassle. The people from the states are calling up and being pretty obnoxious about things. For instance, when we started looking at the salt situation in Michigan, we were dealing with the state of Michigan and they didn't even want to let us consider doing our job. They didn't want us to have any permits to do drilling and so on. If you're focused on trying to get answers to problems, and people ask you, "Tell me the answer to this problem," but they won't let you do the work necessary to get the answers, you're caught in a Catch-22.

RM: So you were in that office for three years.

DV: The three years I just talked about were the three years in which I was the US representative to the IAEA. I was in that office from 1976. I had an assignment to go to

Vienna for a week once a year to work with the international committee on underground disposal in repositories. And after I did my last trip, I came back and resigned.

There were lots of issues about how the project was going to be run. We had issues in the state of Nevada that were always difficult. We started out with a fairly friendly person, Bob List, and then Richard Bryan came along and it was, like, no way. And then we had Bob Loux, who worked for the state, to deal with, and so on. You saw these things getting more and more aggressive.

RM: So you just bailed out and went to nuclear fusion?

DV: There's something else in life I can do that's more interesting, from a professional perspective.

RM: And then how long were you in fusion?

DV: I started that new job in mid-November of 1981 and in January of 1982, Bob Nelson, who was running the project in Nevada, got moved up to be the Assistant Manager for Nuclear Weapons Testing at the Nevada Test Site. I got a call from him about the 28th or 29th of January saying, "You've got to come out here and take over this project." I always had a very good relationship with the people in the field office in Nevada, and I had very good relationships with the people at the laboratories. And Nelson was saying, "You're the right person to come out here and take over this because you've been involved with it from Day 1. And you've worked well with Mitch" (Kunich, the guy that was his deputy).

I said, "I don't think so, Bob. I've just changed jobs. I've only been in this job two months, and you're telling me I've got to quit it and come out there? I don't think so." Time went on, and there were still people who were suggesting that I take the job. Eventually, a person in the Reagan Administration by the name of Frank Coffman, who was a deputy assistant secretary, called up and wanted to talk to me. He made a very strong case that I was the person that really needed to go to Nevada and take over the project.

RM: And the Nuclear Waste Policy Act had not yet been passed by Congress. It was getting close.

DV: Right, it was close; it was going on. The argument was that "A lot of people on the project, in the laboratories, think you're the one with the greatest credibility who could come and take over this thing. You have had the best understanding of the problems and, therefore, would be the one that they would really want to work with." It took until the end of March of '82 to make the decision that I would take the job.

Of course, I was in a bad position because I had to tell the person I was working for, who had gone out on a limb to hire me, that I was going to quit on him after not even six months. That's not good on your resume. But I decided to do it. Then it was time to decide to move to Las Vegas.

RM: Where were you living at the time you made the decision?

DV: I was living in Gaithersburg, Maryland, and we had bought a new house about three years earlier. At this particular time the economy was in the dumpster. The interest rates on mortgages were, like, 14 and 16 percent. And trying to sell a house in D.C. and not lose my shirt was very tough.

One of the things that happened was, I came home for the Fourth of July. I'd actually moved into Nevada about the last week in May of 1982. My father and I drove out there—I stopped in Cincinnati and picked him up and we drove the rest of the way. I was on my own trying to find a house and all that kind of stuff. My father stayed a week and I put him on an airplane and sent him back to Ohio. I went home for the Fourth of

July to see my wife. After we went to church, she was fixing breakfast and dropped a knife. And somehow, even though she had slacks on, and leather shoes, as she turned the knife was knocked off the kitchen counter and while falling to the floor hit her leg and cut her Achilles tendon in half.

RM: Oh my God!

DV: So now I had a wife in a hospital bed in the living room with a cast from her toes to her tush and no mobility, trying to sell a house.

RM: With a new job.

DV: With a new job. Not only that, it was a contentious job. So there were a lot of things that were very tough. On the other hand, we had a very dynamic crew that was very positive. And it was at a time when they wanted to see progress made in investigating the sites. Jim Santini made a proposal . . .

RM: Jim Santini, who was a congressman.

DV: And got passed in one of the laws that if the primary bore hole for the exploratory shaft wasn't drilled before August 1st, it wasn't grandfathered; it couldn't be drilled. If you had started the exploratory shaft by drilling the first bore hole where the shaft was going to be located, you were fine. If you're starting your drilling operations after August second, you're not grandfathered. You can't do anything. We started our hole on August the second so he put a real twist on us. That's an example of some of the political stuff that you run into.

RM: Did you have to get the state's permission?

DV: No, it was prohibited.

RM: Was that just obstructionism on his part?

DV: You can make a judgment of what it was. I mean, it was something that limited us from making the progress that we wanted to make.

RM: So that was one of the early examples of throwing monkey wrenches into the program.

DV: It was one of the things that made it difficult to do our job.

RM: And this was when?

DV: August of 1982.

RM: Oh my lord. At that time, it seemed that people were pretty sure the Nuclear Waste Policy Act was going to pass. It wasn't nip and tuck on whether or not that the act was going to pass, was it?

DV: It was going to pass. You had to bet that something was going to happen. And the other things that we were dealing with at the time were water rights and other issues.

We also had internal issues about who was going to run the program. At the Test Site, the federal laboratories (Sandia, Los Alamos, and Livermore) were, in fact, in charge of the weapons testing program. There were a lot of people in the laboratories were working on this project out at Yucca Mountain who thought they were going to run it. And I had a significantly different viewpoint on that.

RM: Right, you believed you had been hired to run it.

DV: Yes. Basically, they were telling me that it was my job to go out there and turn that thing into a real project. If you went to a meeting in those early days, USGS got up and gave a talk about what they were doing, and the presentation had the USGS logo on it. Then Sandia did something and got up, and its presentation had the Sandia logo. And so on. There was no sense of project cohesion. It appeared to the management in D.C. that it was a collection of activities, not a project with a well-defined objective.

I believed Bob Nelson was doing a great job in getting a lot of stuff done, but from the optics of it, that was one of those things that said, “This has got to be a much more coherent operation.” My job was to go out and turn that operation into a real project. I always joked that Yucca Mountain was tenth on a list of three when I started. Five years later, we were really the only site standing.

RM: Now, Yucca Mountain was one of a number of sites around the country that were being seriously characterized.

DV: Yes. There were four salt sites—Texas, Utah, Mississippi, Louisiana.

RM: Washington was still on the table, or was it?

DV: And the basalt project was the Hanford Reservation. Eventually, the number was narrowed down to five sites, and then to three.

RM: Do you want to talk about your rise to director of the project? Was that your title?

DV: I was basically the project manager.

RM: Do you want to talk in any detail about what that was like those first few years?

DV: It was difficult. The first year I was there, I was on the road 156 days of the 250 work days. I was making a transition. I had a son who was going to become a sophomore in high school so I had to get him into school. And I had a wife who had a problem with a leg that was trying to heal. We were trying to get a house up and running—I finally found one and we moved into it. But I still had to rent the house back in D.C. because I couldn’t sell it.

It was interesting—the Nevada operations office is a weapons program field office and I was the most significant non-weapons program there. We weren’t necessarily appreciated. I told you about the efforts of the people from the weapons program to try to get us thrown off the Test Site because we were bringing other people in and we were going to disrupt the conduct of the nuclear weapons testing.

So it was contentious. Also, I think in the early days my budget was $60-some million, and I knew it was going to get much, much larger than that. There was a rule in the Atomic Energy Commission, and the follow-on, the Department of Energy, that a prime contractor could not report in through another prime contractor. Livermore, Sandia, Los Alamos, Reynolds Electric, Fenix and Scisson, and Holmes and Narver were all prime contractors and they couldn't report in through another contractor.

RM: They had to submit their own reports?

DV: Directly to my office. And I had five people. The question becomes one of, how do you manage a $60 million program with five people? I had a layout for what the office was going to look like. The personnel people didn't like the idea that I wanted to hire the managers of the divisions first and let them hire the people. They said, "You have to have so many people on board before you can hire a manager."

I said, "That ain't the way it's going to work." It took me until March of '83 to get the personnel people to agree that I could hire the managers first and let them hire the staff rather than having me hiring everybody, and telling the managers, "Here's what you've got to work with." That was another of the issues that we had to deal with.

RM: It was along about March or April of '83 that your office held the first public meeting regarding Yucca Mountain at UNLV. That was when Governor Bryan came in and said he was unalterably opposed to the project. Do you want to talk about that meeting, and the political context, as it existed at that time?

DV: There was nothing spectacular about it. We knew that was going to be the state's position. It was our job to facilitate people expressing their viewpoints and getting them on the table. It was basically evidence that the state was going to be completely uncooperative with the federal government in trying to solve a problem. The state's viewpoint was that they had done enough for the country already. They had contributed the Nevada Test Site and this and that and the other thing, and they were not going to be the nation's dumping ground for dirty, deadly radioactive waste. I think my favorite picture was one of Mr. Greenspun saying it's not a repository, it's a dump; it's a dirty dump.

Those were the positions and that's what you had to work with. You had to be able to stand up in front of that conflict and express the federal government's position with as much detail and strength from technical basis as you could. And that's what we were doing. So the question became one of whether or not they would hold sway, or the federal government would.

Over time, the effort of the project was to really focus on getting high-quality scientific and engineering work done. The scientists and engineers at the national laboratories were very competent. They wanted to do their own thing and they didn't necessarily like how we did it.

I used to have a management meeting every month for two days and the state was invited to come to that meeting; it was open. Eventually the NRC, the Nuclear Regulatory Commission, sent a site representative to the meetings. Whatever we did was completely open. But it was basically trying to really get the operation organized. We had a very strong supporting organization.

We had a young lady who was a facilitator, and we would map out every minute of the meeting as to what we were going to do. People who were going to make presentations knew what they had to present, what material they were going to distribute, and how many minutes they had. And we knew what kind of outcome we thought we were going to have after spending time discussing/debating. Were we going to make a decision? Was it just for informational purposes? Were assignments going to be given? We made sure the meeting was focused and the points of concern were addressed and that assignments and decisions were recorded.

For three or four years, that's how we ran the thing. I had some very good help. SAIC was a contractor that the project hired and they provided very strong technical people to provide support to my office so that we could do the management of this project with a limited number of people. And we saw, with time, that the products that we were putting out were getting to be very good.

When the Nuclear Waste Policy Act passed in '82, one of the first things we had to do was write the environmental assessments and create a site characterization plan. And we had to deliver those things on very aggressive schedules, all of which we did. The consensus in Washington was that the work coming out of Nevada was superior to the other two sites.

By 1987 the other two sites were eliminated. I mean, they had to go through the process. There had been nine sites that were being looked at. There were EAs, environmental assessments, written. Then they had to narrow them down to five. And of those five, Yucca Mountain was considered the best and right behind it was the salt dome in Mississippi. The Hanford site finished last of those five, but they selected Hanford as one of the final three because it was in a different geologic medium. They wanted geologic diversity.

But in the end, in 1987, the Congress was aware of what we were doing. I had testified before Congress, along with the other project managers, on, I think it was July 2, 1987. We presented our cases and they asked whether or not we thought there were any fundamental flaws in the site. I said that, based upon what I knew at the time, I couldn't see any. And by November, Congress had made the decision that they were going to go forward with Yucca Mountain.

RM: Do you think that your testimony was influential in them making that decision?

DV: There was a whole body of work. It's not rational to think about the testimony of one person being a basis for a decision of that nature. There were a lot of people who were looking at it and could tell them about Yucca Mountain. I mean, the Nuclear Regulatory Commission was living with us at that time.

The thing that made Yucca Mountain attractive for me was the data that was starting to come back from the bore holes. One was the information that showed that the water table was, like, 1,800 feet below the surface, and that maybe there was another formation down about 3,600 feet—in hard rock. It might be a good thing, but it was underwater. If you're looking at something like 1,800 feet as where the water table is, and 3,600 feet . . . that means you've got 3,000 feet of water over you. Which means you've got a lot of pressure.

I don't remember what the temperature of the rock was down there. But at Hanford, they had 3,000 feet of water over them, which means 1,500 psi. If you go into an aquifer at 1,500 psi, a stream of water coming out of that would cut you in half.

The scientists at the USGS, Gene Roseboom and Ike Winograd, had the idea that you could possibly build a repository in the unsaturated zone. That's the thing that made it unusual and, from an engineering point of view, significantly different than the other locations. You were going to be way above the water table and there was little or no chance that the water table would ever rise to inundate the repository. So the primary mechanism by which radioactive materials would be taken out of the repository—flow of water—was a potentially very limited factor. The temperatures of liquid water open to the atmosphere was limital to the boiling point. At Yucca Mountain it was almost 95^{o}C, or 200^{o}F. A repository below the water table with 3,000 feet of water above it increases the boiling point significantly—it is possible to get liquid water well above the boiling point of water open to atmospheric pressure.

RM: So the water was hot?

DV: It could get very hot. You had to be very careful about that, especially if the rock temperature was 140 degrees to begin with.

RM: And that would affect the whole system.

DV: Yes, because you have waste packages and the question is, if you have an electrolyte with water, what will the corrosion characteristics of that thing be? There was a regulation that the waste package was supposed to last 1,000 years, or a minimum of 300.

Those were the kinds of technical issues you were trying to figure out. So we were going to be above the water table. And Yucca Mountain is about 3,000 feet above sea level, where water boils at 95 degrees centigrade, so under 100 degrees centigrade. Water’s not going to get any hotter than that because it’s going to evaporate and it’s going to go away. The heat will drive the water away while it’s still warm. It will eventually come back, but it’s not going to come back in great quantities. It’s not going to be standing water.

CHAPTER SEVEN

RM: Now, talk about what happened when the Amendment to the Nuclear Waste Policy Act of 1982 was passed in November of 1987.

DV: I wasn't project manager then.

RM: When did you leave?

DV: About July seventh. I had a significant disagreement with the management in Washington. They brought in a guy by the name of Carl Gertz. The first thing that Nick Aquilina did when he came into the office was tell me that I was going to do something else in life.

RM: And what had Nick Aquilina been before?

DV: He was in the Idaho field office before that and he was down at Savannah River for a short period of time. Tom Clark and Bob Taft were also asked to leave.

RM: And who was asking them?

DV: The people in Washington, DC. The secretary and the undersecretary.

RM: Was this a new administration?

DV: This was still under Reagan. I don’t remember who the Secretary of Energy was at the time.

RM: So they made massive changes in personnel? What was their reasoning for that?

DV: It could have been that I was strong in expressing my viewpoints and viewed as contentious. My position was that the science regarding the site evaluation was primary; I was not going to repeat the mistake the AEC made in the early 1970s with Lyons, Kansas.

RM: But Tom Clark?

DV: Tom Clark, Bob Taft, and I got along fine. He often remarked that I didn't suffer fools graciously but he never had any problem with the technical work or the public and political interactions that I did. I never embarrassed the office, I never compromised the Department of Energy, and I was reasonably good on television and in the newspaper.

RM: So before the '87 law was passed, you had been replace by Carl Gertz? Do you have any more you want to say about that? To me, that's very interesting and it's a part of the chemistry of the thing.

DV: The management of the project took a significant turn at that time. But by then, most of the significant technical work was already in place. We'd already done the review with the environmental assessments, and we had the site characterization plans written and submitted and so on. So basically, it was kind of on autopilot at that time. It's just that I had oftentimes challenged some of the people in D.C. in terms of their vision of how this thing was going to be done.

RM: And they didn't take that lightly, in the end?

DV: They don't take lightly to challenges by people from the field. It wasn't a desirable transition, on my part. For an engineer that was really interested in the scientific and technical work, being assigned to manage environment, safety and health programs was being put on punishment detail. But it was now my career and I had to learn and adapt and do the best possible job I could. Because of my technical background, I go assigned to solve other problems that the DOE was having.

RM: Was DOE noted for making those kinds of changes in other contracts?

DV: You saw a significant change that happened with the Department of Energy. The Energy Research and Development Administration was what's known as an exempt agency. Generally they paid one grade higher for people than cabinet-level organizations because they tended to be much more technical. At the time the Department of Energy was created, the Office of Personnel Management changed the structure of some of the positions—when they created the Senior Executive Service. They were looking for people that would come in and would be paid at a higher level and would be sensitive to the political needs of the country.

Starting in, I think, '78, there was a significant loss of technical talent from the agency because they couldn't support all the high GS-16s and GS-17s that were doing technical work as opposed to management work. Over time, the level of technical competence in the management of the organization suffered. There were people who were more interested in paying attention to the political regimes than to what it was going to take to make the things work technically.

RM: I've heard a suggestion that if the project were to be revived or come back, it would be better to run it as an independent agency like the TVA. In other words, take it out of the political arena and turn it over to some real pros. What's you take on that?

DV: As a matter of fact, there was a commission set up under Reagan, I think about 1983, to look at how to deal with things in the Department of Energy. They were on a tour of the Test Site and they wanted to know what I thought was important for being able to be successful. I told them I thought the most important thing for them to do would be take the project out of the Department of Energy.

RM: You've already talked about it, but, specifically, what was the problem with the Department of Energy?

DV: It just has to do with the competence of the people that are involved in doing the work. I had significant disagreements with some of the people in D.C.—sometimes people would be trying to tell me how to run the project. The first question I would ask them is, “How many papers have you ever published in a refereed journal?”

And the answer was, “What’s a refereed journal?”

“How many design drawings have you ever signed as a designer? How many design drawings have you ever signed as a checker? How many pieces of equipment or facilities have you ever accepted for performance basis?” There were a lot of things that these people had no experience with.

RM: Would you agree, then, with the idea of setting up an independent agency like the TVA or the Manhattan Project?

DV: There’s got to be some basis of being able to solve the problem technically. Again, we come back to the single most important element, which I’ve said time and again in this discussion—safety is the most important thing. And it’s safety based upon analysis, and understanding of physical phenomena that take place in the earth. That means you’ve got to have some fairly sharp people. You need people that are recognized as experts in their field. The people that aren’t at that level aren’t going to contribute.

RM: Yes, and can actually impede an effective solution.

DV: One other problem is that the real work is done in the field. Sometimes people, even though they are smart, like to manage from a desk in Washington, DC. That’s always a limitation.

RM: To reiterate, within DOE there was a kind of an attrition of qualified people and a promotion of people who were a little marginal, so that you wound up with less confidence in the agency as a whole.

DV: Yes, that’s what I said.

RM: One of the things I’d like to get your take on is: I did an interview with Senator Chic Hecht not too long before he died. He was very disappointed that there hadn’t been more progress on Yucca Mountain. He told me when he was in the Senate he was called in to Secretary Herrington's office one time, and the offer was made, by the secretary, under Reagan, that if Nevada would accept Yucca Mountain, the federal government would build a multibillion dollar nuclear medical research facility on the Test Site associated with UNLV, and that the support would be such that in a few years there would be more Nobel Prize winners working at that institution than any place on earth.

And Steve Bradhurst said one time when he was Nye County's representative for things related to Yucca Mountain he was talking to somebody from the NEI; it may have been the head of it. And the guy said, "What will Nevada take to accept the repository?"

Steve said, "Well, how about the superconductor/supercollider?"

And the guy said, "Okay. What else do you want?"

Steve said, "Well, how about the super train from Vegas to L.A.?"

And the guy said, "Okay. What else do you want?"

And Steve said, "Well, I'll have to get back to you on that one."

That wasn't an official offer in the same sense that Herrington's was. But it shows that there was the possibility for Nevada to receive huge federal projects if they accepted the repository.

DV: As far as the offer from Herrington, Troy Wade was there and he said it happened; I would believe it. Those are the kinds of things they were trying to do. But those things were way above my pay grade. I mean right now, why isn't the utility industry complaining bitterly about Yucca Mountain being closed down?

RM: Yes, why aren't they?

DV: Because it's in their best interest to get Harry Reid to give them loan guarantees. They're looking at other things. And the utilities never were strongly on board.

RM: Why?

DV: I don’t know why. I’m just telling you my experience with them.

RM: One would think they would be. Nuclear power is a cheap source of energy, relatively, isn’t it?

DV: No, it’s still fairly expensive, but it’s competitive. And it’s got a benefit—what good is it to have cheap coal if you’re going to ruin the atmosphere? When you take into account all of the factors, nuclear power is the best opportunity you have to solve the problems you’ve got.

RM: I heard the other day that one offer was to double the lanes on I-15 from Vegas to L.A. I don’t know if you’ve driven that lately, but it’s considered one of the most dangerous sections of highway in the United States.

DV: When I lived there, I used to go down to L.A. and San Diego all the time. I know the road. But the question becomes maybe Nevada’s not in a very good position now if the administration changes in Washington because the state has an economy that’s not very strong right now. They don’t have a vision of the future that reflects a diversity in the economy; they’ve continued to put all their eggs in one basket. And now they’re seeing, because of the economic situation in the country, that gambling is not going to be the goose that’s laying the golden egg anymore.

RM: And my take is, there’s a good chance it’s not coming back like it was.

DV: No, I do not believe that it will. Certainly, with regard to mortgages and resale of property, it is my understanding that Las Vegas is the worst situation in the country. So the question is, what has the political leadership brought them?

RM: Do you think if Yucca Mountain were offered up today, Nevada would take a different attitude toward it given the economic problems in the state?

DV: Yes. Certainly Nye County has a different perspective than the state does.

RM: I do, too. Again, it's a question of timing, as you've been saying.

DV: Timing, and location. But from a technical point of view, to me Yucca Mountain was the best option in the country. Suppose they had said, "Okay, you've got the environmental assessments, and we'll build it in Mississippi at the salt dome near Hattiesburg." That's basically land that's owned fee simple by a lot of people. What do you think it would cost them just to buy the land? And the fight over that would be incredibly difficult. The Army Corps of Engineers, the real estate agency for the federal government, would have to condemn the land. There would a great deal of time consumed in litigation.

RM: Was that true of a lot of sites considered? Like Deaf Smith County, Texas?

DV: I don't know about Deaf Smith County. Remember, everybody said put it where the waste is, and the waste is, for the most part, in the East. From a point of view of potential risk in the future, I don't think there's going to be too many people wanting to build housing around the Death Valley area, or other areas where the discharge point is—where the water that would come out of. And there's not a lot of people living in Amargosa Valley. So in the long term, Yucca Mountain is the site that has the least potential for causing harm in the future.

One of the things that was most significant for me, when I got into the repository program as an engineer who's used to dealing with things in the short term, was to recognize that you were going to have to think in terms of hundreds of thousands of years for a solution to a problem, not in terms of a hundred years. Things that I can make work in a hundred years don't necessarily work in a hundred thousand years.

RM: What is your take on the time frame—they upped it from 10,000 years, when you were there, to a million years, I believe.

DV: It's certainly a fascinating thought, because we're really not good at making projections for periods up to 10,000 years. And we're even less capable of making projections for a million years. I think the expectation that we can make accurate projections for a million years is extremely questionable.

RM: Did the state promulgate that?

DV: No, it was the National Academy of Sciences that promoted the idea. I'm pretty sure that the person behind it was an individual by the name of Tom Pigford. I thought it was a very bad idea. I thought the collective dose rate was more reflective of things. It smoothes things out. It gives you a better picture of what the overall risk is, rather than having the fencepost person as the basis for your decision.

RM: Do you have anything more that you would like to say on any topic?

DV: I want to say something about the people who were involved in making the Yucca Mountain project a success. There were a couple of people who were incredibly important. Like Mike Voegele—the work that he did in terms of keeping the technical program on course was phenomenal. And then, a person like Tom Hunter, who just retired as president of Sandia Laboratories, worked on the project. He was also incredibly important. Who is the guy that worked for the *RJ* (*Review- Journal*)? Ed Vogel? I always was, I thought, treated very fairly by the *RJ*.

RM: Yes. And I feel the *RJ* was not that opposed to Yucca Mountain.

DV: Did you know a reporter by the name of Myram Borders?

RM: No.

DV: She wrote an incredibly good article on me in 1985. I've got a copy of it in Cincinnati.

RM: I think we should include it in your oral history. And on the other side, you had Mary Manning, who was the reporter for the *Sun*. She could say nothing good about the Yucca Mountain project.

DV: That's because she was doing what her boss wanted. I never was in a situation where I said anything bad about governors or other opposition leaders. People used to try to bait me to get me to say those things and I always knew that that was not a rational approach. And with the newspapers, there's always a certain logic that says, never get into a pissing contest with a guy that buys his ink by the tank car. [RM laughs]

We left off yesterday, I think, talking about the things that went on at the Test Site after I arrived there in 1982. In the early part of 1982 there was a lot of pressure to demonstrate progress. There was a desire to build the exploratory shaft until we found out about the limitation on the time when the principal bore hole had to be drilled. We had spent a fair amount of time acquiring equipment for a shaft—head frames and mining equipment and so forth. Our primary focus at the time was to continue the characterization work that was started by Bob Nelson and his team.

And again, the thing that was of primary interest, or concern, to myself and a number of others was the quality of the scientific research. We always said that in justifying a repository that had to have a lifetime of 250,000 years, or even longer—a half million, three quarters of a million, and when the revision in the environmental standard pushed it all the way out, to a million years—the focus on the work was knowing that whatever analysis was going to be done, as speculative as it might be, extending it to time frames that went way, way beyond anything man had ever done had to be accurate in terms of analysis. And there was an expectation that it would be quite accurate. That was very interesting, trying to match the expectations of politicians and the public against the physical reality of what could actually be done. The primary objective, again, was to focus on acquiring high-quality scientific information and then perform high-quality analysis and interpretation of the data.

That was what most concerned myself and the scientists and engineers from the laboratories in those years. We were very fortunate to have some very talented people from the laboratories. They were first-rate scientists and they contributed significantly to understanding of the site and surrounding environs.

One of the most important problems, in the early days, was to resolve the issue of whether or not volcanism was going to be significant for us. The cinder cones did lie right off to the west of the site. A scientist by the name of Bruce Crowe from Los Alamos Laboratory had a very significant role in answering those questions very early on. We were all uneasy about it when we first started the project, but we knew that any volcanic activity had been dormant for a tremendous length of time. The only question is whether or not there was any indication that there would be a recurrence of the phenomenon. Bruce did an excellent job of understanding the chemistry of the lava and so on and provided an early indication that volcanism was not likely going to be a problem. But it would still take a long time to really put the nails in the coffin in that issue.

The other issue had to deal with understanding the hydrology of the site. Where was the water? Where was it coming from? How fast was it going? Where was it going to discharge back to man's environment? The USGS were the scientists responsible for doing that research. They did an excellent job of gathering that information and giving us a very clear picture, early on, that gave us some confidence that we could really count on the site. It's not a place where there was a risk of significant infiltration of water and dissolution of waste and then carrying the radionuclides back to man's environment.

RM: In terms of the volcanism, what were some of the concepts that really sealed the coffin for that issue?

DV: Basically, it was basaltic volcanism. Silicic volcanism is the type where you get mountains created, and basaltic volcanism is a very limited kind of phenomenon. The fact that there were just small fissures through which the stuff came out was an indication that there weren't any significant underlying forces that were going to push the earth upward. And the fact that Yucca Mountain is a million or two million years old, or something of that nature, and it all came from volcanic eruptions that deposited the volcanic ash there, those phenomena were clearly dead. Bruce focused on all the technical details that would support that kind of understanding.

That wasn't an endpoint that we proposed, it was an endpoint that we got from the work. When you do scientific research, you have a lot of thoughts about what the endpoint could be, but the idea is to let the science tell you what the endpoint will be, not to force the data to demonstrate that there was an endpoint that we desired. Those were always the philosophical aspects of managing the program.

RM: In the same vein, what were the nails in the coffin on the water issue?

DV: The simple fact is the water table, which is defined by nature, was so far below the surface. And there was no evidence that that water table had fluctuated significantly. That was, eventually, one of the things that was an issue for Jerry Szymanski. He was arguing that the deposits in the cracks were from water that was coming up from beneath, and other scientist were arguing that it was water that was coming down from the surface.

RM: And in the end, his argument didn't hold up, did it?

DV: No, it didn't hold up. It was an interesting confrontation over the issues but the science community, the peer review, said the nature of the deposits was that they came from water on the surface, not from water coming up from beneath.

The nature of our project was always very open, allowing people to see what was going on. As I said, our monthly meetings were always open to the state. A representative from the state attended almost every meeting. The state always had a full view of everything that was going on. I don't think there was a reciprocity; they didn't share, as far as I remember, any of the information they got out of their research with us.

RM: Unless they considered it to be negative.

DV: Right. I think the NRC representative, Paul Prestholt, came around 1983 or '84, and he always had full access to my office, and full access to our meetings. We had a lot of significant discussions with him. We had a philosophy about no surprises. If something came up that was unusual, we would not hold back on sharing that information with the regulator. The important thing had to be that they had confidence in our integrity about the things we did, and in the honesty with which we presented the information.

That was always an issue because the scientists wanted to keep the data to themselves, primarily because they wanted to publish information. Their publications are the basis on which they get their acknowledgement as a scientist and rewards as a professional in terms of salary and advancement to leadership roles. There was always a dynamic tension in terms of people wanting to know as much as they could as early as they could and basically not allowing other people to steal the information and publish it first. In my lifetime, work that I have done was taken and published by a collaborator before I could get it done because we gave access to it and somebody chose to use it in an inappropriate way. They didn't even bother to tell us they published the work independently.

RM: In my interviews with people who are knowledgeable on Yucca Mountain, I have come to the view that the whole study of Yucca Mountain as a waste repository site was of extremely high quality; and the effort was, as you say, so open. And it was so big in the amount of money spent, that it was kind of a watershed in human history. I don't know if there was an effort that was comparable to that in human history. What is your take on that?

DV: Well, there were three projects that were vying for money to conduct their research. And of the three projects—the salt projects, the project at Hanford, and the NTS—the Yucca Mountain Project was the one that got the least money.

RM: Really? And why was that?

DV: People had preferences for other things. We were very diligent. I was able to motivate a lot of people that worked for me to work very, very hard. People put in a lot of extra hours without ever charging anything, mainly because they felt committed to the project and to the significance of what it meant to the country.

We were very fortunate. We were always in a situation where we would comment to headquarters that some of their schedules for getting things done were way too aggressive, and that if we wanted to do a good job, they needed to give us a little bit more time. The other two projects would say, "Well, we can do it in that time frame." So the timing issues were always a two-to-one vote against me. But we were always able to find a way to get the work done and we always delivered the product to the Department of Energy in Washington, D.C., on time, while the other people usually asked for additional time to finish. And the work we did was usually considered to be the most thorough.

RM: But on the whole, the work for all three sites was good, wasn't it?

DV: Oh, yes.

RM: In terms of the macro view, I think, in human history, there has seldom if ever been an effort with this quality of science and investigation used to solve a problem of culture.

DV: There are a lot of people out there who would challenge you on that comment, saying it wasn’t all that good. But from the point of view of the people that were involved in making judgments about it, I don’t know of any time that we’ve had any position overturned by work done outside. That’s always the test.

And the philosophical things were important. One of mine was that I always wanted the scientists to publish their work in refereed journals so that it would be out in the open and would have critical peer review by people other than those who were in the program. That way, if there were flaws in an argument, they would be identified early on. You don’t want it to be two weeks before you’re going to say you’re going to do something, and find out that you’ve got a major problem.

That was the thing that really came out of the experience at Lyons, Kansas—to not have basic technical or scientific surprises when you were in the middle of the congressional budget hearings. It was your job to be on top of everything that was going on in everything that was really important, to demonstrate that the piece of land that you had was really capable of long-term isolation capability.

CHAPTER EIGHT

RM: I'm interested in the history of science, in science as a human endeavor. Personally, I think science is the glory of the human race. In your view, would the general repository effort—including the salt, the basalt, and the effort at Yucca Mountain—be a watershed scientific effort to solve a problem and solve it in terms of high-quality science?

DV: I think that the problem they were solving was a critically important one. I mean, nobody's going to get a Nobel Prize for it. If you're talking about the grandeur of science, then you find it in people that are on the cutting edge. We were in a different regime, one in which you had a practical problem to resolve. You had to have a very strong scientific and technical basis to demonstrate that the engineering work that you were going to do would function correctly.

RM: And it was a huge science/engineering problem, right?

DV: Right. That's the most important thing about it. And that's one of the things that an awful lot of people did not understand, including the Nuclear Regulatory Commission. I always had significant uneasiness about the Nuclear Regulatory Commission because when they established requirements for the quality assurance program, they established it using the quality assurance requirements for the design and building of surface facilities. People don't understand that once you say you're going to use geologic disposal as the mechanism for disposing of waste, you've made the fundamental decision that the facility is already built. And that facility is the earth. Man will have had absolutely no involvement in putting that piece of earth in place.

A quality assurance program that is designed to focus on putting the facility in place is not appropriate on the front end of the effort, when you are gathering scientific information to determine if the site is suitable. It's clearly appropriate when you start building the surface and underground facilities, but when you have a facility that's already built, your job is to describe it accurately in terms of all the physical phenomena that are taking place there—the structure that nature gave you. What you're looking at is the scientific aspects of the local conditions. If you read 10 CFR 60, Appendix B, and NQA-1 carefully, they don't provide a lot of guidance to the scientists to tell them how people can judge the quality of their work as a basis for making a comparison to performance standards. The fundamental question is how good does the data and analyses have to be to make a decision that is recognized as authoritative and correct?

There was always a significant conflict about quality assurance over that fundamental starting point. And it led to a lot of significant unhappiness by the scientists because you're saying, "You're making me work to a set of standards that have absolutely no relationship to the nature of the work that I'm doing." Eventually you work through it, but it causes a significant amount of unhappiness and results in lots of findings that say you aren't doing things right, which is an inaccurate representation regarding the work being done.

RM: That is such a key point, and I want to make sure that the readers understand it—this conflict between what the scientists are doing and apparently the NRC and others are doing in setting standards. Could you rephrase that?

DV: Well, it's very clear that the Nuclear Regulatory Commission's standard for quality assurance is tied to the building of a facility, and how you control the design, the calculations of the design, all of the things that you're going to create and put the structures, systems and components in place. How you specify the materials that are going to go into it, the procurement of those materials, how you test them when they come in, how you control the actual design of systems that go inside the facility, in addition to the concrete and steel. There are ventilation systems, piping systems, electrical systems—all sorts of things that go inside that you want to control. A reactor is a dynamic system which has large structures and systems necessary to control its operations and its functions. The quality assurance standard is designed for a very active facility. By comparison a repository is a completely static facility. In the end configuration it has no moving parts. It depends upon not macroscopic things to control it, it depends upon microscopic things to control it, like the interaction between the water in the rock and the waste, in terms of what might take the radioactivity out of the repository.

So there's a fundamental difference. Another thing to know is that when you build a facility on the surface, you put it in place by adding things to it. When you build something underground, you build it by taking material away. In order to get underground, you have to mine the rock and you have to be able to stabilize openings so they don't collapse.

A lot of people that were big on the quality assurance went into, let's say, "I need a quality Level 1 rock bolt." And the question was, what does that have to do with nuclear or radiological safety? A lot of people who were familiar with the normal operations of nuclear quality got into sort of a funny area when we started to go underground. There were a lot of differences. What you really want to know is, how good are your measurements of phenomena taking place? How good are your hydrologic measurements? How good are your predictive capabilities?

You're dealing with things that have never been done before. How good is a model that's going to project something for a million years? The standard quality assurance programs that the NRC had didn't provide criteria that were very focused on answering those kinds of questions. So you spent a lot of time on the front end working on things that were not critical to the issues at hand.

You also know that when you build the underground facilities, you can't damage the earth in a way that would cause it to lose whatever isolation capability it had. You always had to take those things into consideration. And we were on the cutting edges of those questions in the '82, '83, '84, '85 time frame. I don't think they changed significantly. They found a way to make it work, but it was a very difficult voyage across those issues when it didn't have to be.

RM: What you were doing in the early '80s time frame was something new in human history, on this level. So in that sense, it was a seminal effort, and maybe in the future, historians will single it out for that reason.

DV: Maybe. If they catch on. Yes, those were significant issues. We had open discussions about those issues. The question is, who really judges the quality of your science? It's usually the scientific community as a whole. That's why it was important to get research work published in open journals. The NRC staff and contractors involved in the review are capable scientists and engineers. They represent the intellectual basis for the quality technical review that the Congress envisioned when they wrote the Energy Reorganization Act of 1974.

RM: On the whole, the scientific community was very supportive of your science, right?

DV: Yes. There are certain people, even today, who say that what we've done was wrong. But that's a viewpoint that's not necessarily shared with the majority of knowledgeable people.

RM: And yet you have an effort by people like Senator Reid to say the science was flawed. DV: That's an assertion by somebody who has absolutely no qualification to make that judgment. He has no background in science; that's a vacuous assertion on his part. He has never been able to put on the table a written document that lays out the significance of his assertion. What he is saying now falls in the category of political "shuck and jive." Right now the NRC is reviewing the license application, and they're not finding any significant issues with what has been presented as scientific justification by the project. It is my personal belief, based on what I have seen so far, that the NRC will find that the site is suitable.

RM: So if the NRC goes thumbs down, it will be, essentially, for political reasons?

DV: Under the current leadership, I would say that is an accurate evaluation. They can't put it down for scientific reasons. They're just about finished with Volume 3 of one of the documents they're reviewing, which is the one that looks at the long-term projections. And the State of Nevada has asked that that not be allowed to go public.

RM: What are the reasons they give?

DV: Suppose it comes up and says there's no flaws in the site? And it's very important that that not come out before the elections this November. Next November they start investigating, remember?

I'm just talking about what the philosophy was, and the focus. There was always a significant number of activities going on simultaneously. One is to have all the resources, the drilling equipment and so forth, available. There was technology that had to be developed so that you could drill holes dry because you did not want to introduce water into the rock. One of the earliest issues we had, in 1982, was how the exploratory shaft at the Test Site would be built.

Workers could drill big diameter holes very deep on the Test Site for the weapons program. There was a contingent of people that wanted to be able to drill a hole like that. But if you had an eight- or ten-foot diameter drill bit in a hole, and it had 300 feet of water/drill mud over the top of the drilling bit, the issue was, what was that going to do to the rock, in terms of water content? When you got to the bottom of the hole, if the first thing you wanted to do was measure how much water was there in nature, you would have an experimental setup that was completely flawed. There was a significant discussion about that and I was able to argue the case that we had to do it by mining.

RM: You mean drilling and blasting?

DV: Drilling and blasting.

RM: Foot by foot. And that's what they did?

DV: No, eventually they drilled the ramp with a tunnel-boring machine which could be done dry.

RM: And that's water free?

DV: It's got a little bit of water, but it's not like being on a vertical drill that's got 300 feet of mud standing over the drill. That mud is also in there to stabilize the rock formation so it doesn't collapse on you.

The regulation talked about a "shaft." So we were going to stay with a shaft until we knew about a lot of things. There was a contingent from Livermore early on that wanted to commit to building the ramp. Our viewpoint was, "No, we'll stay within the language of the regulations because we're not going to build this thing tomorrow." There was time to get that issue on the table, which they eventually did. So you were able to build the ramp in a way that didn't cause significant damage to rock from the drill and blasting. And you were able to build a shaft, or an opening, that was significantly larger than you would normally have with the drill and blast method.

That was typical of issues that came up. In the early days, the regulation was written in a way so that you would build a small exploratory facility. As time went on, in 1981-’82, people were talking about an exploratory facility that would include a thousand spent fuel elements. You went from an underground exploratory facility that was very small to an underground facility that was significantly larger. And the question was, “What’s the right size facility for the information you want or need to get out of it?”

Oftentimes people wanted to do experiments that would be interesting to the scientists, but you would always have to ask the question, “Suppose you made the decision that the site was very good based upon surface measurements. Then you go underground and you’re looking for information regarding a phenomenon, what measurement could you make underground that would change your viewpoint on what you believe about the site?” Then you would ask the scientist, “If you were going to make this measurement, what issue are you going to resolve that changes your viewpoint?” Or, “If this is critical to the design, when you’re finished with this measurement program, who specifically do you give your information to? Who specifically is going to use it? You’re arguing that it’s a critical piece of information, but when you’re finished, what critical issue about the site is answered?”

Those were all concerns you had to address to ensure money was being spent effectively. There was always significant interest in the program and at that time, we had a tremendous amount of money in the science area. People saw us as a potential source of funding for doing research that wasn’t necessarily related to answering the questions that I had to address. I had only one question to answer: “Is this site any good? Now, tell me how your information is going to help me make this decision."

There was always a sense of stewardship. When I started, my budget was around $60-some million, as I told you. When I left the program, it was in the $130, $140 million range. I was asking the government to give me that kind of money and the question was, "What was I doing for the government in return?" While I'm very interested in many many interesting issues in science, that's not what I was being paid to answer. They always gave you some leeway to look at a new way of solving a problem, and to do things that were not necessarily on the beaten path, but most of the reason was to answer the question about whether the site could be used as a repository. So that was the focus of the operation, and to be a reasonable steward of the resources that you were given to answer this particular question.

We were very interested in the site being acceptable. But if we came upon a fatal flaw, then we would have done our job in finding that fatal flaw and showing how it would result in some unacceptable risk to the people of the state of Nevada, and then close it down. To find that out would not be as rewarding to me, professionally and personally, as it would be to do something that was significant in terms of solving a problem. But on the other hand, if I gave them a site that didn't solve the problem, what credit would that bring to me? Nothing. Those were the things that were in the back of your mind and were the major concerns for those five years I was responsible for the effort.

RM: In looking back, what were some of the obstacles? You've already talked about some of them, but were there other obstacles that you had to deal with? I'm particularly thinking of your relationship with the state, and also with the counties—if you had one at that point.

DV: The relationship with the state was always contentious. We tried to have reasonable relationships. I thought I had a reasonable working relationship with Bob Loux. We appeared on TV shows together and at public meetings. He would argue that the site was no good, and I would argue that we were still in the process of gathering information and analyzing the data, and here are some of the things that are attractive about the site, that make it look like it's going to be a very positive place for the disposal of waste. But, as you know, the governor was always dead set against it.

We would always acknowledge that the governor had the ultimate responsibility for the health and safety of the people of the state of Nevada, and that it is his job to challenge us to do the best job that we could. And if that was by taking a very extreme position, that was okay. But on the other hand, if the site turned out to be good, he would eventually have to accept that basic fact. The Congress established the NRC as the federal government agency that would review the data and arguments and make the formal decision on acceptability of the site.

RM: Do you feel that the governors played a cynical obstructionist role?

DV: They didn't start to become obstructionist until they got into the issues of groundwater—how much water would be available to us. We eventually had to truck in water from other places.

RM: They were trying to discredit the effort almost from the beginning, weren't they?

DV: They always would make technical challenges and have a different viewpoint about the significance of the data, but I don't think any of it held water. Take the state's support for Jerry Szymanski's issue. And they could always demagogue us, and they would use the standard approach: "Can you guarantee the site's safety?" Early in a study, no good technical manager would make a guarantee until he had significant data. If you make a guarantee and it goes bad, where's your credibility? This always became the most important thing, in my mind, for the government. I was the government's representative on that project and it was my job to make sure that the government's credibility was never impugned in terms of accomplishing the objective.

So you were always very cautious about the claims you could make about the site. I mean, you had the concept of no water, zeolites, the water table 800 feet below the horizon of the repository. The slow groundwater travel times, low infiltration rates, conditions of that nature. You always had the ability to draw a picture of why the site was reasonable for a repository. But if anybody said, "Can you guarantee that you can meet the million-year standard?" until you really work the issues through and obtain sufficient data, you can't make a significant comment because you just don't have the knowledge and understanding, and you don't have the scientific horsepower to refute things.

I would say that in the five years that I was there, nothing that we proposed was overturned or shown to be implausible. Now, some people argue that putting the repository in the unsaturated zone, where there's an oxidizing environment, is a problem. But the Nuclear Regulatory Commission never considered that as a significant issue important to safety.

RM: And the consensus in the scientific community didn't either, right?

DV: Right. As I said, there were a few people that had that viewpoint but they don't have much traction in the scientific community.

RM: What about your relationship with the counties? They didn't have an official position at that time you were there, did they?

DV: They were trying to achieve some official standing. Steve Bradhurst of Nye County was the person I interacted with. I thought the relationship was very good. There were a number of people that were involved. I can't remember the guy's name, Bob. The county commissioner threw a dinner party for me one night in Pahrump and gave me a baseball.

RM: Was it Bobby Revert? He was a commissioner.

DV: Yes, but there was another fellow involved. Revert was involved with us significantly. He'd sit and listen to various things. I felt that I was treated incredibly well by Nye County; I never had any unhappiness with what they did. They felt that this represented a significant benefit for them, but they were careful about how they approached the issue. They weren't going to give an individual carte blanche to do whatever he wanted. And I don't think we would have done anything like that. The fact that we kept everything open—there was never any indication that we were doing something behind their back. I always felt that the relationship with the county, on a social and political level, was very good.

RM: Do you remember any Nye County names besides Steve Bradhurst and Bobby Revert that you might want to mention?

DV: It's been so long. Those are the ones that stand out. Bob Revert was on the first panel we created, and he is quite memorable.

RM: What would have happened if the state had taken an approach more like the county did?

DV: I can't speculate on that, but the fact that the state was not supportive of the project didn't significantly impact our ability to get the job done.

RM: Yes, but I think it did impact the eventual outcome.

DV: Yes, and it's only impacting it because one senator has a specific position of power. If he wasn't in that position, none of the things that have happened since Obama became president would have taken place. But that was more likely the result of some internal issues with the Department of Energy, to get the secretary to send the proposal to the president to say this was the site. It didn't happen until the George W. Bush Administration in 2003. They may have been in a position to have done that in 1995. If that was the timing, we would be well underway.

CHAPTER NINE

RM: Could you talk a little bit about some of those internal issues that have had the effect of delaying the site?

DV: A lot of it had to do with the quality assurance issues. Again, people would say the science is no good because you didn't have the quality assurance program. But this was another case of people focusing on the wrong things.

RM: If the country had it to do over again, what would you recommend, in terms of making the process more efficient in terms of quality assurance?

DV: The important thing is to ask the question, "Quality of what?" The repository is a two-part program. The first part is science focused—understanding that the facility (that was built within the earth's geologic structure and hydrologic system) has the capability to provide the required isolation. If you've already got a facility in place—that is, the geologic formation—then you've got to understand what nature gave you in terms of how the geology is structured and what phenomenology is ongoing within the geology that could change the conditions, such as short-circuiting a path back to man's environment, and that there are no volcanoes ready to pop up from underneath. You have to understand that with a significant level of confidence.

The question becomes, "What are the most important features of the information and the analysis and knowledge that you gain that give you the credibility to make a decision that this site and its geology are okay?" And that wasn't done. As I said earlier, they just took the existing regulations for controlling buildings that were surface facilities and said, "You will follow these criteria." There are no criteria that address the elements of the scientific method. The single most important element of the scientific method is peer review, which I strongly encouraged.

RM: What features should they have been attending to?

DV: What's the quality of the geologic description of the site? How many bore holes are you going to drill? You can't drill bore holes every two feet and you've got to drill them strategically. Then the question becomes, what information comes out of a bore hole that's really critical? And how do you make sure that the information you get from those things is accurate?

The other thing you want to know is, what's the chemistry of the earth there? And you want to understand the hydrology. How much water is there? Where is it coming from? Where is it going? How fast is it moving? What's the chemistry of the thing? And so on. And there are other phenomena. One of the things we discovered was the chlorine-37 phenomenon. When nuclear weapons testing started, there was no chlorine-37 in the world. Nuclear weapons testing created chlorine-37, and now that it was in the rainwater, when it landed on the earth, how far down did it go?

RM: And how far down did it go at Yucca Mountain?

DV: I don't remember, but it was more than we expected. So those are those kinds of things you do. And how do you calibrate making measurements? There's always a requirement that people think about calibration. Well, there are certain methods that you don't calibrate very well. There's no standard that you can show that you have to deal with the concept of internal consistencies of your measurements. But remember, in the early days there was significant pressure to get on with it.

RM: Yes, it was a push/pull.

DV: Yes. They wanted to have the repository in place by 1998. Remember the issues we had with WIPP? The chairman of the Atomic Energy Commission, Glenn Seaborg, promised the governor of Idaho that he would have the waste out of there by 1980. Everybody got this ten-year thing from Kennedy, who said, “We’ll go to the moon in ten years.”

We did that in less than 10 years, but that problem was a lot different, and far more tractable to short-term issues. We’re talking about, you’re going to do this program that’s going to operate over a 30-day period. A missile going to the moon doesn’t take longer than 30 days. Whatever you’re going to build has got to be able to last 30 days. In building a repository, you’re dealing with something that’s going to be ten half-lives of the radioactive material. Plutonium-239 is 25,000 years, so ten half-lives is a quarter of a million.

And the question is, do you understand all the elements necessary to effectively address the issues? Making predictions of where the moon’s going to be in two weeks is not very difficult. Making a prediction of where the geology’s going to be, or the hydrology’s going to be, on a site in 250,000 years is not the same problem. People in the political realm said, “Well, if they can put a man on the moon, which was the most difficult thing there is in the world, in ten years, we ought to be able to solve this problem here on earth.” Then Dixie Lee Ray said, “We’ll move the date for disposing of transuarnic waste out to 1985.” And when did WIPP start? I didn’t think it started until 1992, or somewhere in that neighborhood.

RM: And would you argue that the whole waste storage challenge was a greater challenge than the challenge of going to the moon scientifically?

DV: Use the right term: disposal. Isolation. Storage means you put it over here. We’re not talking about storage; we’re talking about permanent isolation. We’re going to put it in the ground with no intention of taking it back unless there’s a catastrophe that we don’t understand. But we'll make it so that we can go back in there and get it if we have to. I mean, we're smarter than the average bear, right?

So those are the kinds of things that we're capable of doing. That's the nature of the problem. And we wanted to do it. The NRC even suggested, when we were doing the confidence rulemaking, that a commitment for 1998 was possibly not the most strategic action you could take. Because there are always things that come up that will keep you from making those dates. But there was a competition to get the job done in that 1980 time frame.

RM: How could they make the disposal effort more efficient on another run?

DV: This kind of thing is a learning experience. Somebody's going to write a regulation on how you're going to make judgments. They have judgments that the groundwater travel time had to be 1,000 years. The waste package had to last 1,000 years. A minimum lifetime of 300. And there were two other criteria that were out there—I don't remember what they were.

At least one of the judgments—speed of water movement in the earth—is not the most important thing. What you're looking for is the flux—how fast is the mass of water moving? I can pick out one stream that might move very fast, but it's so minute that it's like water dropping from the ceiling. It can happen very fast, but the amount of water that's dropping from the ceiling is very small comparing to the cubic foot of water that's flowing through the sand. I've pointed out to the NRC that they used the wrong term. It should not have been "velocity of water." They did that because it was an easy concept. Determining flux is somewhat more difficult, but it's the more important parameter.

And with regard to the waste package lifetime, the question was, what measurements would you make in the five years, or ten years, that showed that some of the material would last for a thousand? And you also had to deal with the statistics. Are you going to make the assumption that all the packages are identical and they would all fail in the same number of days after emplacement in the repository? Or do you want to consider this as a statistical ensemble? Then the question is, what does a thousand years mean? Is that the mean lifetime or the average lifetime? Or is it the mean lifetime minus two sigma? And so on. Those are the questions we asked the first time. I went to a friend of mine who was a good statistician at the Bureau of Standards, and I asked, “How do we approach this problem?”

He said, “It’s very simple. Let’s take the manufacturing process we understand best.” He said that manufacturing process that was understood best was the manufacture of light bulbs, and the testing of how long light bulbs last. He said, “Suppose you had a thousand light bulbs, and you were going to guarantee that they all would last ten years. You don’t have enough people in the United States to put on that job.” And that’s for the best-known manufacturing process. “And the fact is, we’ve got statistical data for that. You’re not going to have any statistical data for the waste containers so don’t go down that path. You cannot use that line of argument.” Again, this comes back to the science of understanding how you’re going to make your case.

RM: What you’re really talking about here—which is really fascinating, and interesting to me—is, to a great extent, epistemology, how you know what you know.

DV: Right. If you’re a technical person managing a technical project, you’ve got to come to grips with, how are you going to measure the right parameters to give you the information you need? I think that’s one of the things I brought to bear in managing the project—I had a pretty good background in science and engineering, starting with my career at the Bureau of Standards, and I wasn’t going to be tricked by a lot of superfluous arguments in terms of answering the critical questions.

You always had to come back and say, “How is this experiment, this measurement, going to help me answer the question? And here are the five or ten areas—the stratigraphy of the geology, the nature of the geology, the quality of the rock. What’s the water formation? What’s the geophysics of the site? Are there any massive phenomena like tectonics? Are we going to have to deal with them? Are we going to have to deal with earthquakes that would shake the thing? Are we going to have to deal with volcanism?” There were big issues that you had to be able to eliminate as risks. The focus was on answering questions about the mechanisms that could possibly cause the site to not be suitable, that would cause it to fail.

RM: Yes, it seems to be, at its root, a big epistemological problem. How do you know what you know, and how are you going to use that knowledge?

DV: Right, that’s the whole nature of what we were doing. But most people didn’t want to know anything about that. Bob List would. Richard Bryan didn’t, and Guinn didn’t. But if you had that knowledge, and that approach, your ability to stand up in a contentious environment and answer the questions gave you a lot more leverage than if you were just shot-gunning it.

RM: And on the whole, the program did that successfully?

DV: Like I said, when I took it over, it wasn’t a very well-focused program. By the time I left, it was pretty damn tight.

RM: And a lot of the scientific data was collected while you were the manager.

DV: Most of the scientific data—all that was necessary to make the decision.

RM: What were they doing for the years after you left? Mainly trying to address regulations and bureaucratic things?

DV: Ask Mike Voegele—he was the one who was involved in that. As I told you, they said, “It’s time for you to do something else. Your talents are not appreciated, and they’re not needed. Go do something else because you’re hard to deal with.”

I said, “Okay.” The Department of Energy, in its infinite wisdom, gave me a job in something that I knew very little about. I did bring to the table the ability to [be] organized and get things done. I also learned quickly.

RM: So in ’87 the program had collected a good percentage of the data that was necessary to make a decision?

DV: We already had put out the environmental assessment. And the multivariable analysis that compared the five sites was done. It was done by independent people of significant competence. Yucca Mountain was the first on the list and the close second was the site in Mississippi. Then there were the sites in the states of Texas and Utah and Washington.

When I took the job, I figured I could get enough information in four years to do the site assessment, and it took me five. I had a lot of help. I was able to accomplish this because I had a significant amount of great help from Sandia, Los Alamos, Livermore and SAIC.

RM: So in terms of the necessary data, the program was at a place where it could have made an appropriate decision. “Yes, we can confine the waste here.”

DV: Yes. At the end of five years we were in a position to say, with pretty good certainty, that the site would work.

RM: So then the time from ’87 to . . .

DV: To 2002, which is 15 years. That additional time gave the scientists more time to refine their work and focus on answering questions raised by NRC.

RM: That that money and time could've been spent more profitably?

DV: Right. But, remember how the politicians phrased it. They did not pick it as the real site; they picked it as the single site for continued study.

RM: And that's what the '87 legislation did?

DV: Yes, and the entire Congress voted on the question to support that course of action.

RM: And in your view, that wasn't necessarily appropriate?

DV: It all depends upon how certain you want to be when you go in front of the Nuclear Regulatory Commission. But there were a lot of other people that had their fingers in the pie.

RM: Let me ask you a question you may not want to answer. How much of that delay, that subsequent 15 years, was just milking down the government?

DV: Well, they had to do the tunnel-boring operation; that took a fair length of time. They had to deal with Szymanski's arguments.

RM: And that cost the government a lot of money, right?

DV: Yes, and time. And they had to deal with all the quality assurance issues, and whether or not you checked all the boxes when you went into the regulatory framework, and so on.

RM: So some of that time had to have been spent, given the realities of our system.

DV: Right. But the fact was, the decision was made in 2002, or 2003, and the Nuclear Waste Policy Act worked. Again, the "Will and Wisdom" of the Congress was shown when they supported the proposal to build the repository at Yucca Mountain. They passed it on to the state of Nevada for their review and comment and the governor said, "No way." And then the Congress, in its totality, overrode the governor's position. It rejected the comment of the governor because there was no significant basis for his objection from a safety point of view. It was just political statement.

RM: And then it turns out that they reject the thing at this point, in 2010.

DV: Who rejects it? Two people.

RM: Yes, Harry Reid, and his influence over Obama.

DV: Obama bought his future as president with Reid by saying that he would cancel the program.

RM: And I would like to know what happened to Steven Chu. I mean, the guy won a Nobel Prize and apparently he was a real scientist. How could he do it, in conscience?

DV: The laboratory that Dr. Chu managed, Lawrence Berkeley Lab, was critically influential in coming to the understanding of the hydrology and the unsaturated zone. Dr. Paul Witherspoon was the person who established that program, put it into operation, and the program continued under Sally Benson and Chin Fu Tsang. This program made a major scientific contribution to understanding the hydrology of Yucca Mountain.

RM: Fascinating. To me, that's getting right down into the belly of the beast, of how our political system works.

DV: Well, my contribution to this history is to get the effort from 1982 to 1987, when I disappeared; I don't have really much to contribute about the operation after 1987. I brought the project, which was in somewhat disarray in 1982, to a point where the product was considered to be the best, and we did that in five years. Based on what my effort contributed in five years facilitated a decision.

RM: The product was considered to be the best what?

DV: The best science, the best justified, the best organized. Why would they have picked Yucca Mountain for the site if it was based on shoddy work? That's one of the lessons that people have to remember—a politician can make any decision at any time. The question is whether or not that decision will stand the test of time especially in a contentious environment.

Again, we saw what happened in Lyons, Kansas. They made that decision, and they had it shoved back in their ear within two years. I mean, the Atomic Energy Commission makes the proposal to build the repository at Lyons, Kansas, in 1970, and by 1972, they have to withdraw their budget request because the site is no longer considered technically viable. We were not going to make that same stupid mistake; we were going to learn a little from history.

Now, the question was, were the people, the politicians, aware of the quality of what was going on in the work on Yucca Mountain? Remember, Congress established the NRC to license the repository. And they weren't getting any vibes from the NRC, that was on site from 1982, that said, "This is a terrible place." The Hanford site was not a viable site because of the hydrology and the NRC staff was about to tell the DOE that.

RM: Were any of the other sites, like Texas and Mississippi, good enough, in retrospect?

DV: Well, Texas, yes. There were some disadvantages to the Texas site, but it would have worked technically; whether or not it would have worked politically is another matter.

RM: How about Mississippi?

DV: Mississippi, in the judgment of that analysis, was second to Yucca Mountain.

RM: What is your take on people who said, "They chose Yucca Mountain because Nevada didn't have much political influence?

DV: The same could be said about Mississippi. However, there were many other positives. We already owned the land. We were told to pick a site that was already contaminated. And we were in a closed hydrologic basin, so if anything ever got out of the repository it would not go to the sea, it would go to some other place inside Nevada or California. There was no significant population around the site. It was not like trying to go to Hattiesburg, Mississippi, and withdraw three or five square miles of land.

RM: Do you think Chu's decision is going to stand the test of time?

DV: I'm not going to get into that issue. I did what I was asked to do. I was sent to Nevada with a mission, which was take the operation that was considered in organization disarray and turn it into a real, coherent project. I suspect people didn't think that the Nevada Test Site was going to work; as I said, it was tenth on a list of three. And five years later, it was the one that was standing. That was because of a lot of knowledge and experience and imagination and the ability to motivate people to work hard. We had a site that was good that we were capable of defining. The individuals that did the work had scientific and technical credibility in their fields.

I had a lot of good people working for me. I couldn't have done it without the guy that was my deputy, Mitch Kunich. He was a longtime Test Site worker, and he knew how to get things done there. You had Reynolds Electric, Holmes and Narver, Fenix and Scisson—you had a whole lot of people that were doing things. It was a weapons site that had its own ways of doing business and he was a person on the inside who could make things happen.

RM: Was Mitch Kunich there for your full term?

DV: Yes, he was there when we first started this thing in 1977—April tenth was the first meeting. Alan Roberts was the budget man. He really was very knowledgeable and helpful. Mitch was the kind of guy who could really get things done. And if he didn't like something, he would come in and tell you your head was in the wrong location.

One of the things you always do is stop and listen to people when they get frustrated. When they start telling you something, you've got an opportunity to learn something, so don't get frustrated with them. Sit down and listen.

I had other people, like Max Blanchard and Larry Skousen. Those were the two guys I hired to run the branches that I had. Max had to worry about the geology of the site and Skousen had to worry about the engineering aspects, like the waste package and things of that nature. He also had to worry about facilities, getting bulldozers out, building pads for drilling, and so on. He was worried about the exploratory shaft. How were we going to build that?

So those were the guys in my office who were very good. Then there were people who came with SAIC. Mike Spaeth was the guy who ran it. He certainly was capable of getting really good people to come and help. They had a difficult situation because all the laboratories were suspicious of SAIC, thinking they might want to take over everything. Because Mike had, definitely, the entrepreneurial spirit to see how much money he could make for his project and for his company.

He brought some very good people along, and one was Mike Voegele. Mike was a superior technical person, and an easy guy to get along with. He was extremely knowledgeable, with great experience, and very smart. He wasn't afraid to challenge the things I might think about. He always knew that if he got the facts out in front of me, he had a good chance of making his arguments stick.

There were a whole lot of people—the staffs that worked for them. The graphics art people were always great, like Marilyn Kamna. Whenever you had a problem, those people got on it right away. They got you what you needed for whatever presentation you had to make, and so on. Joy Fiori was a person who helped out with a wide variety of operational activities. We always had a tremendous amount of tours, and there was a young lady who worked for REECo who was very good at taking care of getting all the buses, and stuff like that. There were just a number of people. A woman who was most influential, a facilitator who controlled the planning and management of our monthly meeting, was Mary Gelinas.

Sandia had a dual role when I went there. A small group supported my office directly and there was a larger Sandia group that conducted repository design work and test programs. We brought in SAIC, another contractor, to take over the support to my office. Livermore and Los Alamos thought the program was giving too much work to Sandia, and they didn’t like that. The first person that was involved there was a fellow by the name of Dick Lynch, a very good engineer; I believe he was from Illinois. He eventually left and then we got Tom Hunter. He was the other guy who was a real sparkplug—his imagination and ability to get things done were incredible.

RM: And what was his job?

DV: We always had what they called a technical project officer, the head of the organization. REECo had a technical project officer—he was the one that I went to if I needed something from REECo. And the same was true for Fenix and Scisson and Holmes and Narver. Livermore had a technical project officer named Larry Ramspott and Los Alamos had a technical project officer by the name of Bruce Erdle, but he was eventually replaced by a man named Don Oakley.

Those were the guys that helped me run this thing. They were the primary people who would sit in the monthly meeting with me and have things all worked out. They got the laboratories focused on what they were supposed to do and made sure that the things that I wanted to get done, got done. I would say that the two key people in that whole bunch were Mike Voegele and Tom Hunter.

RM: Can you give a quick overview of the roles of each of the labs?

DV: Sure. Livermore was primarily responsible for certain kinds of experimental facilities, like the Climax facility. They were the ones that were working on the waste package. And they did some things in risk assessment and rock mechanics and things having to do with building the technical basis for underground structures.

Los Alamos was primarily concerned with the geochemistry of the site and how well the site retards the movement of radionuclides. They were also involved in organizing the plan for the exploratory shaft. The guy that works here, Wes Meyer, and Paul Amodt did an excellent job of getting the Exploratory Shaft Test Plan document.

Sandia was primarily oriented to the engineering of the facility—how to actually design the facility, how to do the performance assessment, how to evaluate if the water's going through the site, and so forth.

I didn't mention the U.S. Geological Survey. The USGS was responsible for doing the evaluation of the geology—the hydrology, the geology, the tectonics, and those things. The initial technical project officer was a fellow by the name Bill Dudley. A great guy, a great hydrologist. But he was working for an organization that was so independent that they felt they didn't even have to send in their timesheets to get paid. We were sort of harsh on the Survey because I was getting beat up from the program management point of view because I didn't get any estimates of how much money was being spent. I got on the GS, and the GS took it out on Bill Dudley and brought another guy in to take his place.

They blamed Bill for the issues, but he wasn't the problem. It was because the director of the geologic survey would not do his job and get the people who were running the things to learn how to do simple accounting. So they brought in Larry Hayes, and he was a great guy. He was very good at getting things done.

It was a very strong team intellectually. The question is whether they were a strong team at getting something done. What I thought I brought to the table was to get them motivated, and push them to get things done.

RM: And succeeded?

DV: Yes.

RM: Thank you so much for talking to me. It's been fascinating.

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