YUCCA MOUNTAIN AS A NUCLEAR-WASTE REPOSITORY—NEITHER MYTH NOR MILLENNIUM

by Isaac J. Winograd

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FOREWORD

This paper was originally given as the keynote speech at a USGS workshop on Yucca mountain, held at Death Valley, October 23-27, 1989. Because Winograd presented a number of stimulating ideas that should be brought forward now rather than later, his paper is being released as an Open-File Report, in advance of a USGS Bulletin that will contain many other papers given at the workshop.

Dr. Isaac J. Winograd is a research hydrologist with over 30 years of experience in the Southern Great Basin. He first proposed the concept of placing a high-level nuclear waste repository in the thick unsaturated zones in the arid Southwest in 1972, but it was not seriously considered until 1982 when the USGS pointed out to the Department of Energy that Yucca Mountain, Nevada, which was being considered as a possible site for a repository below the water table, had good potential as a site for a repository above the water table.

Although Dr. Winograd has followed the DOE Yucca Mountain Project with great interest, he has never been funded by DOE or formally been a part of the USGS Yucca Mountain Project; the views and suggestions presented here are strictly his own - those of an astute and experienced scientist outside the Yucca Mountain Project who is keenly interested in both the technical and the societal aspects of nuclear waste disposal.

Eugene H. Roseboom, Jr.
Deputy Assistant Director
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YUCCA MOUNTAIN AS A NUCLEAR-WASTE REPOSITORY -- NEITHER MYTH NOR MILLENNIUM

by

Isaac J. Winograd

Many thanks, Bill Wilson, for your kind introduction. I, in turn, would like to extend well-deserved kudos to you and the Committee for Advancement of Science at Yucca Mountain (CASY) for encouraging an inter-disciplinary dialogue among U.S. Geological Survey scientists and engineers involved in the Yucca Mountain Project (YMP). If I worked in Denver, I can assure you I would have attended many of the CASY seminars held to date.

A word about the subtitle of my talk -- Neither myth nor millennium. It is borrowed from a subtitle used by Art Piper for his 1969 Circular on industrial liquid waste disposal into brine aquifers. And my intent is similar to Art's; just as liquid-waste disposal into brine aquifers was, and is, no panacea, so disposal of high-level radioactive waste (HLW) into Yucca Mountain (YM) is hardly a perfect solution to a complex problem. The subtitle can, of course, be interpreted in an even worse light; namely, that Winograd believes that disposal at YM may not even work for 1,000 years. Which of these possible interpretations I had in mind will, I hope, become clear in the next 40 or so minutes.

I will cover three topics in my talk this morning. I begin with a review of some major accomplishments of the Geological Survey in the radioactive waste arena. Next, I will heap some deserved praise on elements of the existing work. The bulk of my talk, however, will be devoted to what I view as several potentially major technical concerns in an otherwise very comprehensive program of studies.

I begin by outlining in chronological order major Geological Survey accomplishments in assisting the U.S. Department of Energy (DOE) and the Nation in radioactive waste disposal research. I do this in order to inform our younger scientists of the historical leadership role of the Survey in HLW disposal.

1955 - C.V. Theis, considered by many to be the father of modern ground-water hydrology, was one of a handful of world-class geologists invited to participate in the first National Academy of Sciences (NAS) conference on HLW disposal, held in Princeton in 1955. That conference, chaired by Harry Hess, endorsed bedded salt as a resting place for HLW (National Academy of Sciences, 1957).

1963 - C.V. Theis, utilizing ground-water data from Hanford, WA was the first to recognize that dispersion of dissolved contaminants in a field setting is at least an order-of-magnitude greater than widely reported from lab-column experiments. This was truly a landmark finding in its day (Theis, 1963).

1976 - Vincent E. McKelvey, Geological Survey Director, wrote to the DOE suggesting exploration of the Nevada Test Site (NTS) region for potential HLW disposal sites. He did this because of the wide variety of hydrogeologic environments at NTS, the aridity and deep water-table, and the then 900-man-years of knowledge acquired by Survey personnel at the NTS in support of DOE's weapons testing program.

1978 - Circular 779 (Bredehoeft and others, 1978), the Survey's first formal statement on the HLW problem, was published. This circular, which caused considerable discussion at the NAS--a matter which I'll return to later--basically reported the following: a) migration of fluid inclusions toward HLW canisters emplaced in salt deposits is likely to result in breaching of the waste packages by brine; b) other geologic media deserve consideration for HLW disposal besides salt; c) the repository temperature should be kept below $100^{\circ}C$; and d) there are severe limitations in prediction involving the earth sciences.

1978-80 - The Interagency Review Group. Two Geological Survey scientists served on an Interagency Review Group (IRG) convened by President Carter and chaired by Frank Press, then Chief of the Office of Science and Technology Policy. These geologists managed, with some difficulty, to have two major ideas incorporated into the IRG's final recommendations. These ideas sound like motherhood statements today, but Dave B. Stewart and I can assure you they were not welcomed by some members of the IRG 10 years ago. These, then heretical notions, were: a) future HLW disposal endeavors should not focus on rock type but rather on hydrogeologic, geochemical, and neotectonic environments; and b) the environments selected had to provide multiple natural barriers to radionuclide migration; that is, the engineering "defense-in-depth" approach was extended to geologic environments. Basically, we believed that far too much reliance was being given to the role of engineered barriers, at the expense of natural barriers to radionuclide transport.

1981-84 - The unsaturated zone (UZ) concept, first proposed in the early 1970's (Winograd, 1972, 1974), and ignored for nearly a decade, was endorsed by the DOE, the U.S. Nuclear Regulatory Commission (NRC), and by Lawrence Berkeley Laboratory (Wollenberg, Yang and Korbin, 1983). The NRC's rule making, legitimizing thick UZ's as potential HLW repositories, cited Survey Circular 903 by Roseboom (1983) 20 times (Ostrowski and others, 1984)! One should also note that some of the earliest Survey publications on the UZ (Winograd, 1974; Winograd and Doty, 1980; Winograd, 1981, Roseboom

1983) explicitly pointed out the need to evaluate water-table fluctuations during the Quaternary and the matter of ^{14}C outgassing; these are not new concerns.

1980-86 - The Geological Survey began and completed its study of the entire Basin and Range province for potential HLW disposal sites. This work, done in close cooperation with State geologists, has been published in Professional Papers 1370A-H (See Bedinger and others, 1989 for a listing of the titles of each of the seven chapters of this Professional Paper).

1988 - An article in EOS by John Bredehoeft (1988) pointed out that salt is not an impermeable medium and that radioactive waste repositories constructed in this rock will eventually fill with ground water even if leakproof shaft seals could somehow be developed. I wish that such a paper had been available in 1978 when we were doing battle with a certain engineer who could not comprehend that all natural media have some permeability and that, therefore, all repositories constructed below water table, even those in "dry" salt, would eventually fill with ground water.

The above cited work, done largely with Survey funds, was generic in nature. Its intent was to introduce new concepts while critiquing and augmenting older ones. It is a record we can be proud of.

When I agreed to present this talk last July at Bill Wilson's request, I knew that I would have to force myself to re-read key original studies as well as to plough through the DOE's voluminous Yucca Mountain Site Characterization Plan (DOE 1988) in an attempt to come up to speed. I have not yet finished reading the Site Characterization Plan (SCP) but am well along. As I read through Part A, that is, the first three volumes (Chapters 1-7) summarizing the state of knowledge about YM, I could not but be impressed with the breadth and depth of work done by you and your colleagues at the National Labs. How you did all this despite quality assurance requirements, stop-work orders, and other administrative add-ons is beyond my comprehension. All necessary studies appear to be under consideration.

Along with your impressive work has come the Survey's tradition of scientific integrity--a tradition we must not take for granted. Several times in the past few years I have come across a few blatantly biased studies by non-Survey scientists bearing on the unsuitability of YM. We must be careful never to relinquish our tradition of impartiality despite the press of external or internal milestones; it is what makes us special.

OK. I don't really believe that Bill Wilson asked me to speak today to hand out kudos. I'm assuming that, in keeping with the goals of CASY, he wants a critical overview of the Yucca

Mountain program from a clean-shaven graybeard, and such a critique fills the remainder of my talk.

As I worked my way through the SCP, I came away with several impressions that I believe constitute major technical weaknesses in an otherwise very impressive program. These perceived weaknesses are: a) a lack of synthesis and prioritization of studies; b) absence of a definitive study of the Paintbrush non-welded tuff; and c) an imbalance between earth science and engineering approaches to the HLW problem.

What you are about to hear are notions from a person 2,500 miles from the action, who is not in the DOE funded program, and who has just told you that he has not yet made it through the entire SCP, though I have scanned most of it. Consequently, if some of my notions are incorrect, unfair, or hopelessly naive, please let me know; I'll be here for the duration of your meeting. I begin with the potentially least serious of my concerns, the apparent lack of study synthesis and prioritization of efforts.

Somewhere in the SCP summary volume, I had hoped to find an in-depth critical analysis of what has been learned in the past 4 to 7 years. I sought answers to the following questions: How does a 1989 perception of the assets and liabilities of Yucca Mountain compare with our notions in 1982? Which of the 100 or so ongoing studies are the most critical and are they being tackled on a schedule commensurate with their perceived import? Are senior scientists working on YM generally optimistic concerning suitability of this site or should Congress be informed that alternative sites should be sought forthwith? Or, should the notion of HLW disposal in continental rocks be abandoned all together?

Perhaps such a critical overview does not belong in the SCP, but it is still necessary. The nature of the scientific endeavor is that each study begets other studies, each question addressed leads to new questions. That is, this knowledge-seeking endeavor of ours is always an expanding one. But, somewhere along the line, someone has to try periodically to synthesize existing knowledge so we know where we stand at a point in time. Such an exercise, in turn, forces one to assign priorities; it also can lead to identification of possible weaknesses in the program, such as those presented below. Preparing such a synthesis is clearly not an easy task, but I believe is worth doing. Basically, as I prepared for today's talk I would have greatly valued reading a thoughtful overview that provided a prognosis for YM as a potential HLW site. If such a document exists, please call it to my attention. If it does not exist, then I would suggest it be put together biannually by a small team of senior scientists from the Survey and the National Labs. I turn next to a second matter that I perceive needs more attention: insufficient study of the role of the Paintbrush nonwelded tuff in the distribution of percolation at Yucca Mountain.

In an internal Survey memorandum I wrote in the early 1980's, it was pointed out that the Paintbrush nonwelded tuff is a relatively unfractured friable porous medium that might, at best, serve as a capillary barrier to vadose flow into the underlying Topopah Spring Member of the Paintbrush Tuff and, at worst, as a buffer to prevent rapid fracture flow from the surface of YM direct to the repository horizon.

Montazer and Wilson (1984), in their frequently cited conceptual model of the hydrogeology of YM, greatly amplified these early ideas. They pointed out that depending on percolation amounts, the Paintbrush nonwelded tuff might be expected to: a) divert some fracture flow in the overlying Tiva Canyon Member downdip or eastward; and b) divert percolation that the nonwelded tuff receives downdip (via matrix flow) thereby precluding or reducing water entering open fractures in the Topopah Spring Member. Subsequently, numerical modeling by Rulon, Bodvarsson and Montazer (1986) confirmed the potentially key role that this non-welded unit might play in reducing percolation flux into the Topopah Spring Member. Since this paper was published, field studies in Illinois (Larson and others, 1988) and in the Netherlands (Anderson and Clausen, 1988) showed that stratified capillary barriers do work even in humid terrane. How much more effective must they be in the arid terrane that characterizes Yucca Mountain today and the semi-arid climate that existed in late Wisconsinan time (Spaulding, 1985).

The SCP calls for two 30-foot-long radial holes to be drilled into the Paintbrush nonwelded tuff from the Exploratory Shaft Facility-1 (ESF-1). The holes are to be drilled for rock sampling and long-term monitoring of matric potential, gases, etc. I believe much more needs to be done in the study of this key hydrogeologic unit. I propose that this nonwelded unit be studied in three dimensions in a 100-foot-long drift off of the ESF-1 shaft. After all, this hydrogeologic unit is the closest thing we have at YM to a classical porous medium, a medium which permits use of standard soil physics techniques to measure the flux of vadose water. I suggest that if we really want to get a handle on percolation amounts and on the potential role of this nonwelded unit as a capillary barrier to flow into the Topopah Spring Member, we need to be able to study it in great detail in three dimensions within an instrumented drift.

I envision such studies continuing for a decade or more. Please reconsider the importance of such a drift to significantly improve our knowledge of site hydrogeology.

I turn next to the third and, in my view, the most critical weakness in the YM program as presented in the SCP:a disconnect exists between earth-science and engineering studies at Yucca Mountain.

It is ironic that an earth scientist, who in the late 1970's complained that the HLW program was dominated by an engineering mentality and pitifully underrepresented by earth science, should in the late 1980's partially reverse himself. I do so today. To say this does not mean that continued earth-science input to YM is not essential, only that considerably more attention, in my opinion, must be given to certain engineering measures of HLW disposal at YM.

Thanks to Chapter 7 of the SCP, I now am aware of the major materials-science efforts devoted to waste-package design intended to assure radionuclide containment for several thousands of years. But, in two other areas, engineering measures, in furtherance of HLW containment, must be re-examined and (or) bolstered. These areas are first, in the way the waste package is emplaced into the Topopah Spring Member and second, the need to keep repository temperatures below 100°C. I discuss these in turn.

The ubiquitous fractures and faults in the Topopah Spring Member are routinely considered to be a major site liability by ground-water modelers and by others, including the NRC, and State of Nevada critics of YM. But are they? To a few of us in the early 1980's, the large fracture transmissivity of this densely welded tuff was, and is, viewed as a major asset of the site. Were we then unaware that unsaturated, or even saturated, flow through fractures could not readily be modeled? No. Then, why our optimism? Simply because we believed that the large fracture transmissivity could, with simple common sense placement of canisters, preclude or greatly reduce contact of vadose fracture water with the canisters. For example, in Circular 903 (Roseboom, 1983), you will see the following canister placement strategy illustrated. Figure 1 depicts vertical emplacement of HLW canisters in a dry well. Note especially that the dry hole extends well beneath the canister and contains gravel and sorbers (zeolites, iron oxides, etc.) underlying the gravel. The intent here was, of course, to permit any fracture or matrix flow entering the portion of the emplacement hole, opposite the canister, to drain to lower levels precluding contact of the canister with standing water; the sorbers would presumably retard any nuclides somehow dissolved after canister failure.

Figure 2 is the DOE's SCP version (Chapter 6, p. 158) of vertical canister emplacement. Note the absence of a dry hole beneath the canister; sorbers are also missing. The SCP text does note the presence of an air gap, that is, the annulus between the canister and the emplacement hole, as a capillary barrier to <u>matrix flow</u> into the emplacement hole. But, the SCP makes no allowance for possible long-term shifting of the canister or

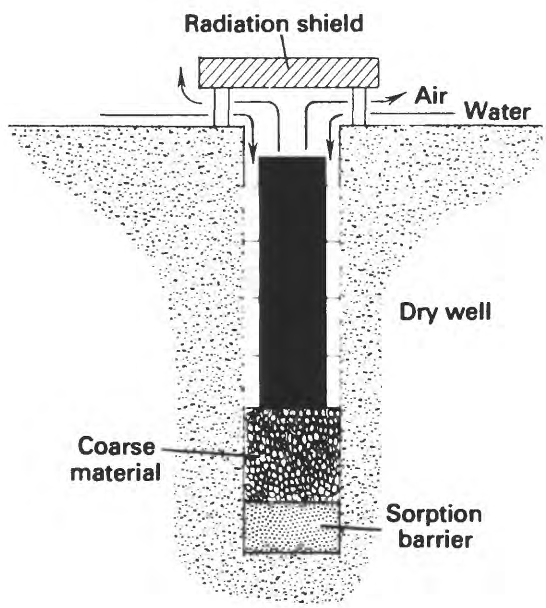

Figure 1. Vertical emplacement of an HLW canister to allow circulation of air for heat removal and to allow drainage of water (from Roseboom, 1983, fig. 2)

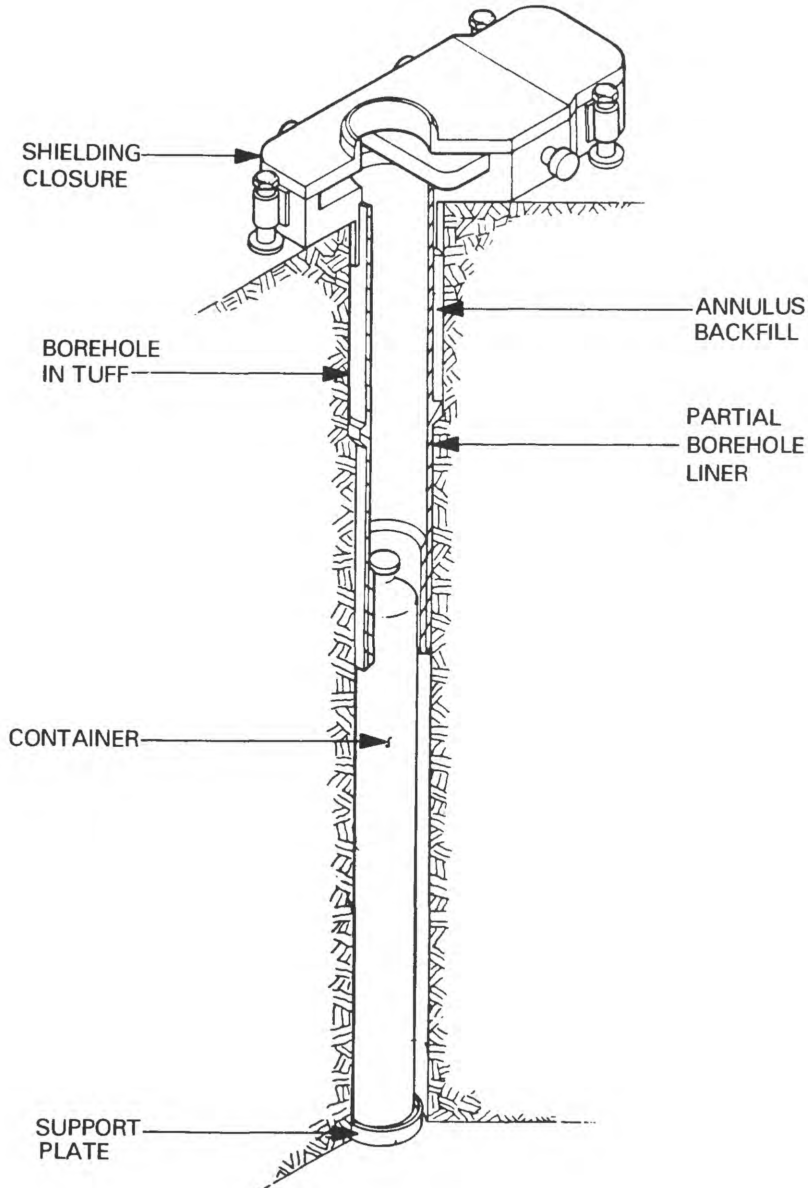

Figure 2. Vertical emplacement of an HLW canister as portrayed in the SCP (USDOE, 1988, fig. 6-68, p. 6-158)

bedrock to eliminate the air gap, nor for fracture flow. Unless an open fracture fortuitously occurs at the base of the emplacement hole, the SCP's vertical design creates a potential bathtub should water enter the hole via fractures intersected by it.

Figure 3, also from Circular 903 (Roseboom, 1983), depicts an alternative horizontal emplacement of HLW canisters. Note first that the hole is subhorizontal and sloping toward the drift. Note an opening at the base of the radiation shield to permit drainage of any fracture water, and note the sorbers on the drift floor.

Figure 4 is the SCP version of horizontal canister emplacement, again taken directly from Chapter 6 of the SCP (p. 159). Although you cannot tell it from this illustration, the hole is not sloping. Second, even if it were sloping, fracture water could not drain out because the radiation shield fits snugly into the mouth of the hole. Third, cement grout placed at the base of the hole would seal any open fractures that could allow natural drainage. Again, the SCP design potentially creates a bathtub for waste immersion in the event of fracture (or matrix?) flow into the emplacement hole.

Now I acknowledge that the SCP design wisely has the entire repository sloping downdip, which facilitates drainage. Also, it is clear that the tunnel designers realized that they could take advantage of the ubiquitous fractures in the Topopah Spring Member to get rid of unwanted water during construction, as seen in figure 5. This figure, also from Chapter 6 of the SCP (1988, p. 187), shows a scheme for diverting water encountered during construction by using the fractured nature of the welded tuff. Yet, both of the SCP designs for canister placement (figs. 2 and 4) could trap fracture water in the emplacement holes rather than permit it to drain. The tunnel designers were apparently unaware of Geological Survey Circular 903 (Roseboom, 1983); it is not cited in the references for Chapter 6 of the SCP, the chapter that presents the waste-emplacement plans!

Mind you, the designs in Circular 903 are hardly a compendium of waste-emplacement possibilities. Consider the following design for those of you desiring even more conservatism. Figure 6 shows subhorizontal canister emplacement with a ceramic or aluminum umbrella at the top of the waste package to divert fracture water around the canister. Many other simple designs are possible.

The point of the canister-emplacement discussion in the preceding paragraphs is simply that I have not seen in the SCP (or elsewhere in the vast YM literature) an attempt to imaginatively put to use the natural very high fracture transmissivity of the Topopah Spring Member in order to keep fracture water from contacting the wastes, or if contact occurs, to minimize the residence time. Indeed, the SCP design has taken a major step

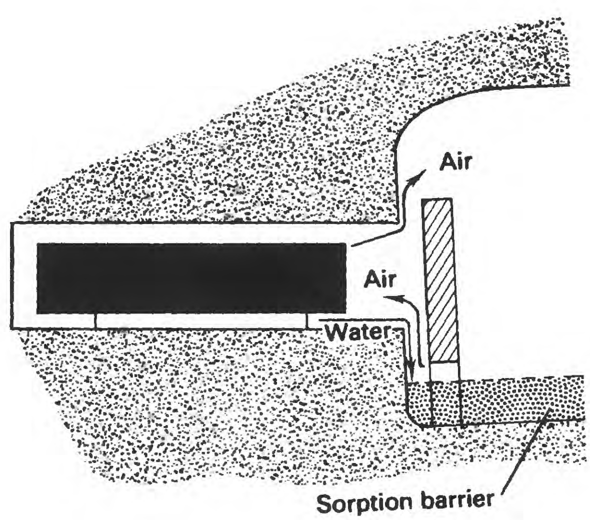

Figure 3. Subhorizontal emplacement of an HLW canister to allow circulation of air for heat removal and to allow drainage of water (from Roseboom, 1983, fig. 2)

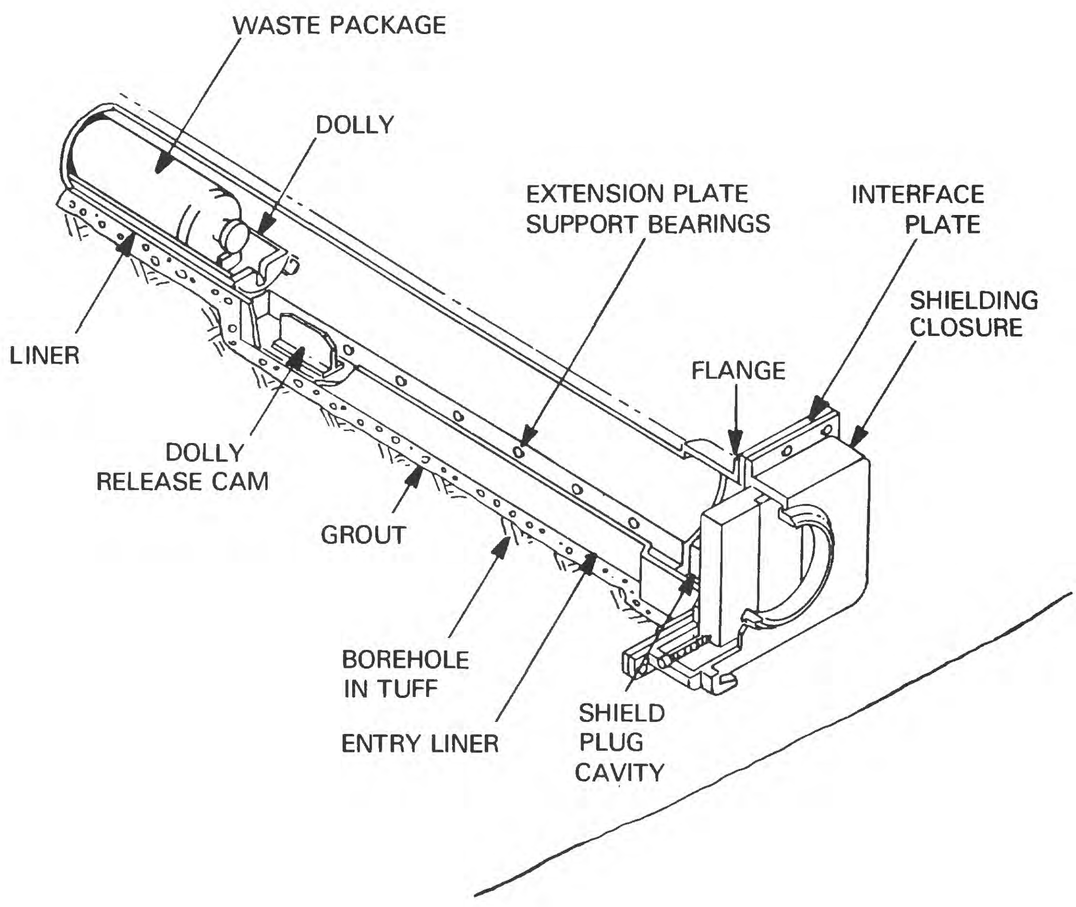

Figure 4. Horizontal emplacement of an HLW canister as portrayed in the SCP (USDOE, 1988, fig. 6-69, p. 6-159)

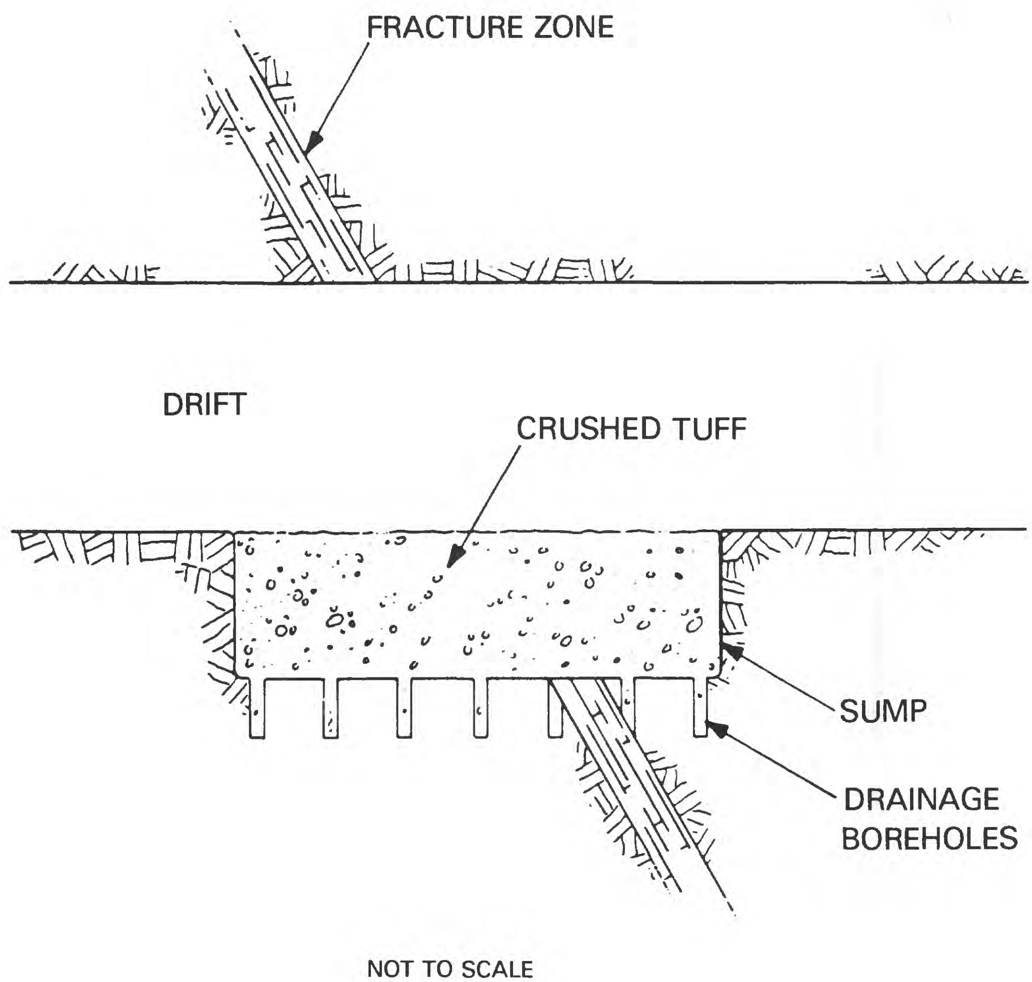

Figure 5. Utilization of fractures in the Topopah Spring Member to augment drainage of water encountered during mining (USDOE, 1988, fig. 6-81, p. 6-187)

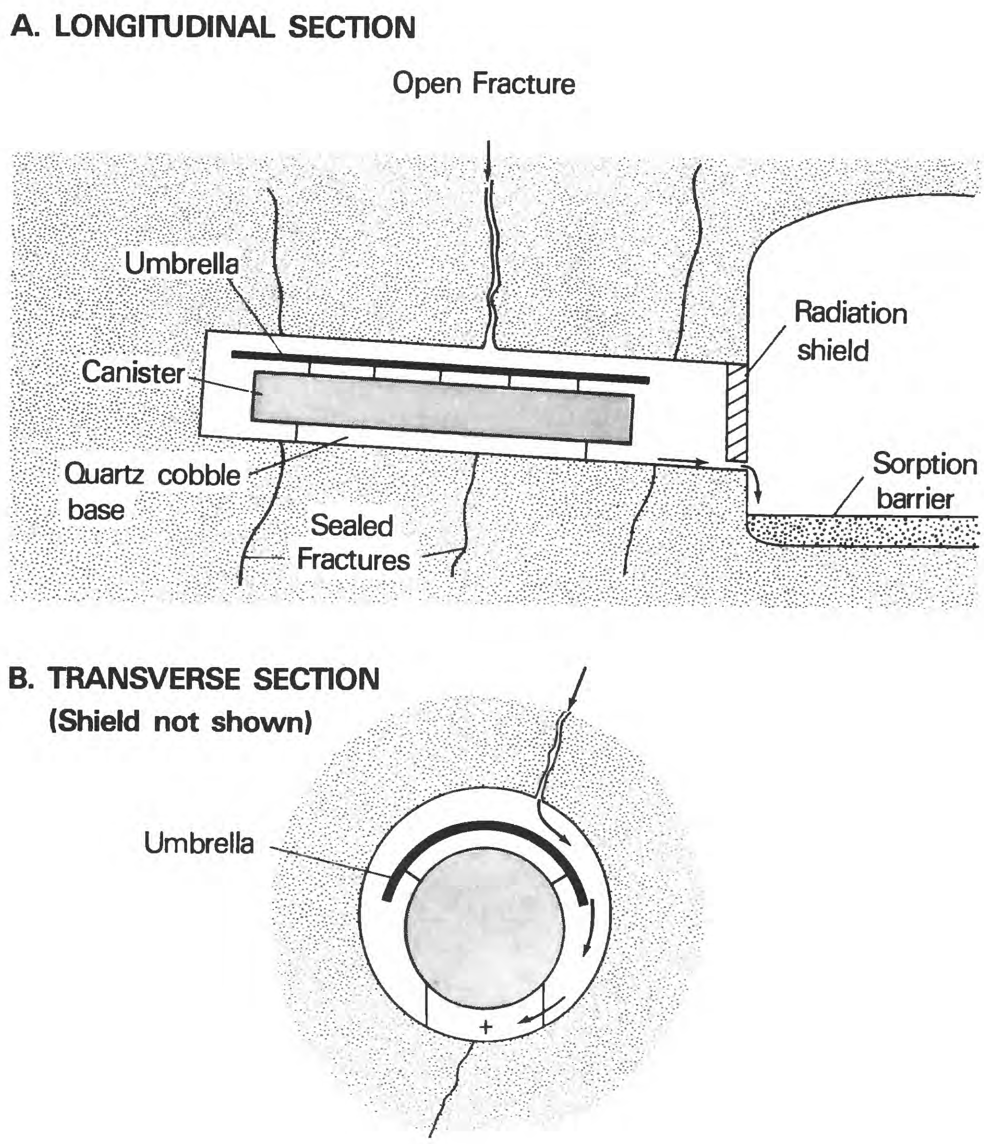

Figure 6. Sub-horizontal HLW canister emplacement using ceramic or aluminum umbrella to divert recharge

backwards from that suggested in Survey Circular 903, which was published in 1983. This is unfortunate because we know, from a vast archeological record, that thick UZ's, even in humid terrane, can provide amazing preservation of even delicate organic objects for millennia, provided they are kept relatively dry; more on this later. Last week I stumbled across an intriguing paper by J.S. Devgun (1989) that further strengthens my point. For decades the prevailing philosophy of low-level radioactive waste disposal has been to emplace such wastes in aquitards, such as glacial till. In a major break with tradition, hydrogeologists at the Canadian Chalk River Nuclear Laboratories are now proposing waste burial in a sand dune at Chalk River in order to permit recharge to drain past their solid wastes. The base of their burial trench is to be just above the historic highest water table.

So, let's return to the question I asked initially. Is our inability to model the flow of vadose water in fractures really a major flaw in the suitability of YM as a repository? Or, might simple engineering measures compensate for, and even overshadow, our modeling deficiencies? One of the State of Nevada's hydrogeologic consultants is fond of pointing out that the Topopah Spring Member within the repository block contains billions of fractures, the implication being, of course, one of gloom-and-doom. To me these fractures, though obviously not all water bearing and not all interconnected, constitute a major asset of this arid-zone site.

Those of you who are concerned with the thermal aspects of waste emplacement on site geochemistry will rightly ask how can I be certain that silica dissolved by hot vadose water near the canisters might not reprecipitate elsewhere in the Topopah Spring Member to reduce the ambient fracture transmissivity. Maybe so. I will return to this possibility in a few minutes.

To sum up the canister placement discussion, I think there has been a disconnect between the waste-package and tunnel designers, on the one hand, and the hydrogeologists on the other. This disconnect results in our not putting to work for us a major natural asset of this arid-zone site, namely, its very high fracture transmissivity.

As suggested by the subtitle of my talk, waste disposal, like life, is not simple. The same fractures that should enable properly emplaced canisters to remain dry, provide avenues for migration of gaseous nuclides (for example, ^{14}C, ^{85}Kr, and others) to the surface after canister failure. Whether the concentration of such releases are of environmental concern is not clear from what I have read. Study of this matter ranks at the top of my list of critical studies of YM.

I turn next to a second example of an overlooked engineering measure, one that may be controversial but which warrants debate at an early date by senior Survey, National Lab and DOE personnel that is, the need to keep repository temperatures below 100°C.

In reading the SCP, I was surprised to find that under present designs, using 10-year-old spent fuel, repository rock temperatures will be well above 100°C for hundreds of years, with peak temperatures on the order of 250°C. I had naively assumed that the argument for keeping repository temperatures below 100°C had been won in the early 1980's. I must digress once again for a bit of history.

In the mid-1970's, an internal Survey memorandum forcefully pointed out that if repository temperatures were kept below 100°C, a variety of problems arising from the coupling of thermal-mechanical-hydraulic and geochemical processes could be eliminated. And, in Circular 779, the Survey recommended in print (Bredehoeft and others, 1978) that repository temperatures be kept below 100°C. As Dave B. Stewart and I can attest, this recommendation led to spirited discussion one night in 1978 at a National Academy of Sciences radioactive waste committee meeting.

By the early 1980's, the Swedes did us one better. Their final HLW plans call for keeping repository temperatures at or below 80°C (Swedish Nuclear Fuel Supply Co., 1983).

In 1981, Frank Parker, a long-time member of the NAS radwaste disposal panel, went on record in print for flexibility in repository design. He stated (1981, p. 279, 281, and 287): "It should be stressed here that there are an infinite number of combinations of aging of wastes and spacing of canisters so that one can achieve almost any temperature at any time that one wishes within a waste repository." He concludes, "One should look for a guaranteed safe system where problems of disposal would be minimized. Consequently, the temperature level should be reduced to one to two hundred degrees <u>Fahrenheit</u>; the design of the mine should be extremely conservative; the waste should be aged 30 to 50 years before they are placed in the repository ....". (Underscoring in preceding quote is mine.) In 1984, the International Council of Scientific Unions (ICSU) recommended surface storage of HLW for even longer periods (Fyfe, and others, 1984).

So, here we are in 1989 planning to load the repository so as to generate repository rock temperatures well above 100°C. Why am I concerned? Well, for starters, if I believe what's in the SCP, temperatures above 100°C might reduce fracture transmissivity and might alter favorable zeolite mineralogy in the near field. And, in the event of canister failure, it would certainly enhance convective air flow within the Topopah Spring Member and concomitant movement of outgassed ^{14}C and other gaseous radionuclides.

Keeping repository temperature under 100°C is something in our power to engineer. We can do this by storing the wastes in a Monitored Retrievable Storage (MRS) facility, which incidentally, was recommended for study by the Nuclear Waste Policy Amendments Act of 1987. Storing the wastes in an MRS for several decades coupled with a lower loading density, should enable us to keep the repository temperature below 100°C (See for example the discussion in Wollenberg, Yang, and Korbin, 1983).

Clearly, there are also major reasons not to cool the wastes prior to disposal. These reasons are: first, extremists within the environmental movement--the anti-nukes--will claim, as they have in the past, that such a postponement of disposal shows that there is no solution to the HLW problem and that therefore the nation must abandon nuclear power. (As an aside, more-moderate environmental groups are now saying, "We better take another look at the nuclear option in view of the 'greenhouse' problem.") Second, the nuclear industry will be concerned because it means that unless MRS facilities are built, the spent fuel must remain at reactors. There also is, incidentally, one good technical reason for keeping repository temperatures above 100°C, namely, by so doing, vadose water purportedly will be vaporized and thus kept from touching the wastes for several hundred years which, if correct, is clearly a plus.

But, now let's consider the advantages of multi-decade interim storage of spent fuel prior to geologic disposal; my list contains six items.

1. The numerous unknowns involved in the coupling of thermal to mechanical, hydraulic, and geochemical processes in the repository are greatly reduced or eliminated.

2. The exploratory shafts and contained experiments can proceed on a less-than-crisis schedule.

3. Forty-year-old spent fuel already exists in small quantities and might be used in prototype testing in short tunnels driven into YM expressly for such testing.

4. Lower thermal loadings might permit design of two or even three disposal horizons in the 300-meter-thick Topopah Spring Member.

5. Retrievability can be demonstrated in the test tunnels. Incidentally, the likely ease of retrievability in the unsaturated zone--in contrast to disposal in salt or below water table in any medium--is another major attribute of YM that surprisingly has not received adequate notice.

6. Adequate time would exist to fully address volcanism, ^{14}C outgassing, and other key technical issues identified to date.

I recognize that a decision for extended interim storage of spent fuel could be viewed as a cop-out. Yet, it is dictated solely by a desire to eliminate a whole cadre of technical unknowns and, moreover, it entails additional benefits. I want to make it clear that I believe it is critical that shaft construction and surface and subsurface work continue as planned because some answers to the suitability of YM can only come from underground workings.

To begin to wrap this up. Why am I pushing for additional engineering measures, specifically utilization of the large fracture transmissivity of the Topopah Spring Member in order to keep the canisters dry, and keeping the temperatures below 100°C? The basic reason is that prediction in the earth sciences is fraught with difficulty (Winograd, 1977; Bredehoeft and others, 1978; Winograd, 1986); additionally, every geologic or hydrologic study begets subsidiary studies and nothing ever gets simpler in the scientific endeavor. Engineering approaches, in contrast, better lend themselves to lab and field testing, to quality assurance, and to licensing, though clearly engineering cannot address issues such as the probability of a basaltic dike swarm permeating the repository, or the magnitude of water-table fluctuations beneath YM during the past 100,000 years. At the same time, we should not rely solely on the longevity of the waste package when other simple engineering measures are readily available to us for keeping the canisters dry and for reducing other uncertainties.

I can't resist in my few remaining minutes giving you a micro-sampling of the types of preservation of delicate objects provided by thick, well-drained unsaturated zones in semi-arid to arid terrane, as shown by archaeological and paleoecological records (figures 7-10). The excellent preservation of packrat middens (figs. 9, 10) for tens of millenia in caves and fissures throughout the Southwest is especially instructive when you consider that they are easily disaggregated by soaking in water--the first step used by paleoecologists to separate out the fossil plants that compose the middens. Most amazing to me is the extent of preservation of delicate objects emplaced in humid zone caves (i.e., unsaturated zones) by Upper Paleolithic man. I am referring, of course, to the remarkable "ice-age" paintings and clay statuettes, dating back to 20,000 years, and found in over 150 caves in southern France and northern Spain. I refer you especially to the works of Ruspoli, 1986, White, 1986, and Bahn and Vertut, 1988.

I will confess that when I examine such archaeological and paleoecological records, a recent hobby of mine, I at times can't

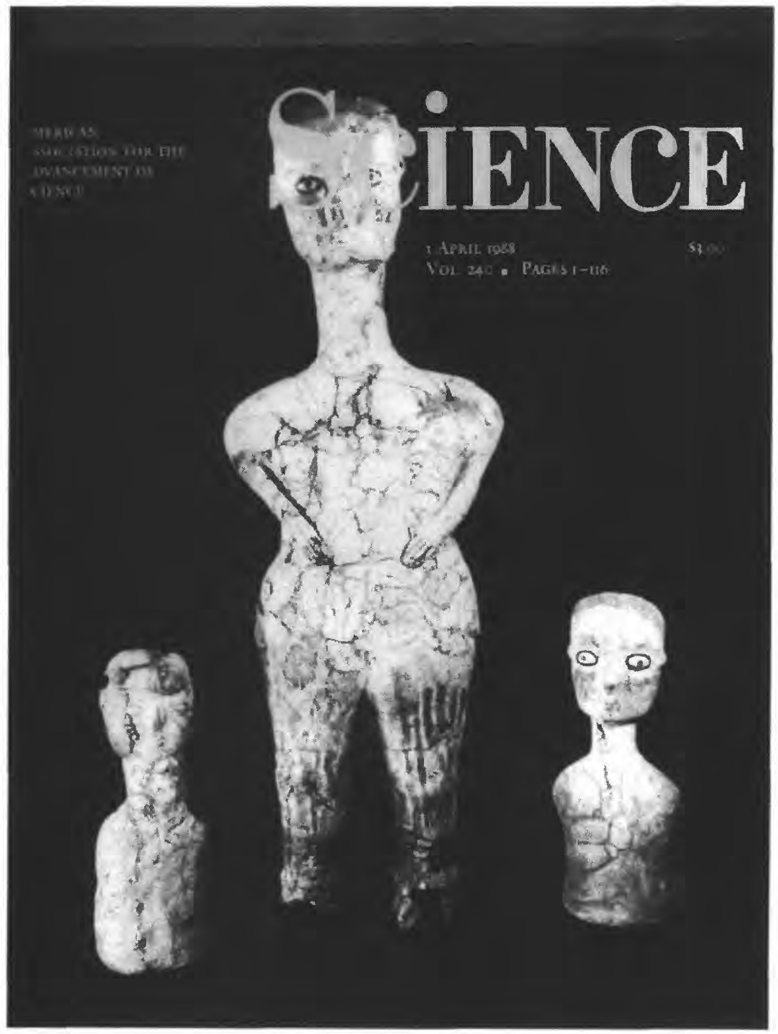

Figure 7. Neolithic plaster statuary from pre-historic village of Ain Ghazal, Jordan (8,000-9,000 years old; from Simmons, et al, 1988, with permission of American Association for the Advancement of Science; scale not given, but text states that statuary are near life size)

Figure 8. Gypsum statuette from Tell Hariri, Mari, Syria (4,500 years old, 34 cm height; from Weiss, 1985, p. 159; with permission from SITES Division of Smithsonian Press)

Figure 9. Packrat midden, Eleana Range, Nevada Test Site (Midden is about 1 m tall and ranges in age from 11,000 to 17,000 years; from Spaulding, 1985)

Figure 10. Close-up of packrat midden from Sheep Range, Nevada (Midden is about 12,000 years old; photo courtesy of W.G. Spaulding)

help but ask myself if we, in the HLW game, aren't, in our quest for certitude, making a scientific mountain out of an engineering mole hill. We should be able to improve on the amazing preservation obtained for valued objects by Upper Paleolithic man, by Neolithic man, and by the Egyptians (See especially D'Auria Lacovara, and Roehrig, 1988) all done, incidentally, without quality assurance, without study plans, without administrative "regs" and, in most cases, even without intent. Those of you wanting an introduction to the relevance of archaeology to toxic-waste disposal in the unsaturated zone may wish to read Geological Survey Circular 990 (Winograd, 1986); those desiring further information on the excellent preservation of late Pleistocene animal and plant remains in arid western U.S. caves are referred to Davis (1990) and Betancourt, Van Devender, and Martin (1990).

In summary, I am greatly impressed--actually, overwhelmed--by the depth and breadth of knowledge developed by the Survey and the National Labs in the past half-dozen years. I believe, however, that the Yucca Mountain endeavors can be strengthened by more attention to engineered barriers for HLW isolation. The advantages of cooling the wastes for several decades prior to emplacement and their emplacement at lower power densities, appear significant to me and to the others whom I cited. I urge you to carefully examine this matter with your colleagues in the National Labs and the DOE at an early date. I also urge you to give additional attention to putting the high fracture transmissivity of the Topopah Spring Member to work as a principal way to keep the waste canisters dry in the event of fracture flow during future pluvial climates; to the extent that they can be kept relatively dry, our main concern shifts to transport of gaseous radionuclides to the surface. Finally, construction, in the exploratory shaft, of a 100-foot drift, in which to study vadose flux through the Paintbrush nonwelded tuff, could provide a better understanding of the distribution of percolation at Yucca Mountain.

Thank you for your patience and have an exciting technical meeting.

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