EEG-22

EEG Review Comments on the Geotechnical Reports Provided by DOE to EEG Under the Stipulated Agreement Through March 1, 1983

Environmental Evaluation Group
Environmental Improvement Division
Health and Environment Department
P.O. Box 968
Santa Fe, New Mexico

April 1983

CONTENTS

FOREWORD

The purpose of the Environmental Evaluation Group (EEG) is to conduct an independent technical evaluation of the potential radiation exposure to people from the proposed Federal radioactive Waste Isolation Pilot Plant (WIPP) near Carlsbad, in order to protect the public health and safety and ensure that there is minimal environmental degradation. The EEG is part of the Environmental Improvement Division, a component of the New Mexico Health and Environment Department -- the agency charged with the primary responsibility for protecting the health of the citizens of New Mexico.

The Group is neither a proponent nor an opponent of WIPP.

Analyses are conducted of available data concerning the proposed site, the design of the repository, its planned operation, and its long-term stability. These analyses include assessments of reports issued by the U.S. Department of Energy (DOE) and its contractors, other Federal agencies and organizations, as they relate to the potential health, safety and environmental impacts from WIPP.

The project is funded entirely by the U.S. Department of Energy through Contract DE-AC04-79AL10752 with the New Mexico Health and Environment Department.

Robert H. Neill
Director

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INTRODUCTION

As part of the Stipulated Agreement signed on July 1, 1981 between the Department of Energy (DOE) and the State of New Mexico, the DOE agreed to provide the following reports to the State of New Mexico to help in the State's evaluation of the suitability of the WIPP site.

- Deep Dissolution
- Disturbed Zone
- Breccia Pipes
- DMG Hydrology
- Regional Hydrology
- Natural Resources
- Results of SPDV Site Validation Experiments
- Plans for SPDV Design Validation
- Results of SPDV Design Validation Experiments
- Plans for Simulated Wastes Experiments
- Brine Reservoir Report
- Horizontal Exploration of the Disturbed Zone
- Fracture Flow in the Rustler Aquifers
- Study of Aquifer Characteristics

The Environmental Evaluation Group had received the following reports in draft form by March 1, 1983:

- Deep Dissolution
- Breccia Pipes
- DMG Hydrology
- Natural Resources
- Plans for SPDV Design Validation
- Plans for Simulated Waste
- Brine Reservoir Report
- Disturbed Zone Exploration
- Fracture Flow in the Rustler Aquifers

This publication is a compilation of the written comments on each of these reports provided by EEG to DOE. Where DOE responded to EEG comments in writing, they are included here. On others, meetings were held between the appropriate EEG staff and the author(s) of the reports and the authors agreed to make changes in the final version of the reports, based on EEG comments. Only in the case of Breccia Pipe report prepared by the U.S. Geological Survey for DOE, no changes were made in the final version of the report.

EEG's conclusions on each of these issues will be provided in a forthcoming EEG report to be published in May, 1983.

NATURAL RESOURCES

TME-3156

Department of Energy
Albuquerque Operations Office
P.O. Box 5400
Albuquerque, New Mexico 87115

RECEIVED
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ENVIRONMENTAL EVALUATION GROUP

NOV 03 1981

Dr. George S. Goldstein
Chairman
Governor's Task Force on WIPP
P. O. Box 968
Santa Fe, NM 87503

Dear Dr. Goldstein:

Interim Policy Statement on Resource Recovery at the WIPP Site

Enclosed for your information is the Department of Energy's Interim Policy Statement on Resource Recovery at the Waste Isolation Pilot Plant (WIPP) Site. The interim policy will be used as the basis for the performance of dose consequence analyses related to resource recovery at the site as required by Item 6 of Appendix "B" of the Stipulated Agreement. This interim policy may be amended based upon the results of the analyses currently being performed and expected to be available in December, 1981.

Should you have any questions, please contact us.

Sincerely,

J. M. McGough
Project Manager
WIPP Project Office

WIPP:JMM 81-5046

Enclosure

cc w/enclosure:
See Page 2

INTERIM POLICY STATEMENT

By

U.S. Department of Energy

Resource Recovery at the Waste Isolation Pilot Plant Site

The primary concern of the U.S. Department of Energy (DOE) in the development of the Waste Isolation Pilot Plant (WIPP) in southeastern New Mexico is both the short and long-term protection of public health and safety. As a major element of this policy, the DOE has delineated buffer zones around the WIPP Site in which resource recovery will be strictly controlled or prohibited (Figure 1). The incorporation of multiple buffer zones in the present design is a conservative approach to maintaining the integrity of the site and ensuring that emplaced wastes remain isolated from the environment. The DOE recognizes, however, that the state of New Mexico relies upon the royalties generated from resource recovery as a significant source of revenue and that other adverse economic and social impact may result if access to the resources at the WIPP Site is permanently denied.

Accordingly, the DOE is issuing this Interim Policy Statement on resource recovery at the WIPP Site to clarify its present position relative to those resource extraction activities that the DOE believes may eventually be allowable within various portions of the WIPP Site. This interim position is based on the previously conducted long-term waste isolation assessment that utilized all available site geologic, hydrologic, and other pertinent data.

It is the policy of the DOE to maximize the opportunity for resource recovery at the WIPP Site, consistent with the requirements to isolate the emplaced radioactive wastes from the biosphere. The interim policy is temporary denial of all resource extraction within the four control zones of the WIPP Site until the decision is made relative to which, if any, of the emplaced waste will be retrieved. Within five years after the first emplacement of each type of waste (i.e., contact and remotely handled), separate decisions will be made about the retrieval of each kind of waste. Should DOE decide that all waste is to be retrieved, the site will be available for complete resource recovery. As an additional part of the interim policy, the DOE is currently undertaking analyses to identify the potential for possible radiation dose consequences of resource development within Control Zone IV of the WIPP Site. The interim and final Statements of Policy will be revised to include additional detail if the results of these studies indicate that the allowable extraction activities could cause an unacceptable hazard to the public health and safety.

The criteria for the final DOE policy is that permanent denial of resources should be limited to those areas in which extraction activities could potentially lead to radiation dose consequences or which are necessary to satisfy institutional requirements; with the exception of those areas required by insitutional considerations, all extraction activities that would not lead to unacceptable effects with the waste permanently emplaced will be defined as "allowable" under the anticipated final DOE policy. From a radiation dose consequence point of view, the timing of resource extraction activities is not critical; the radioactive wastes decay very slowly so that minor differences in waste age do not affect the potential radiation dose consequences of exposure.

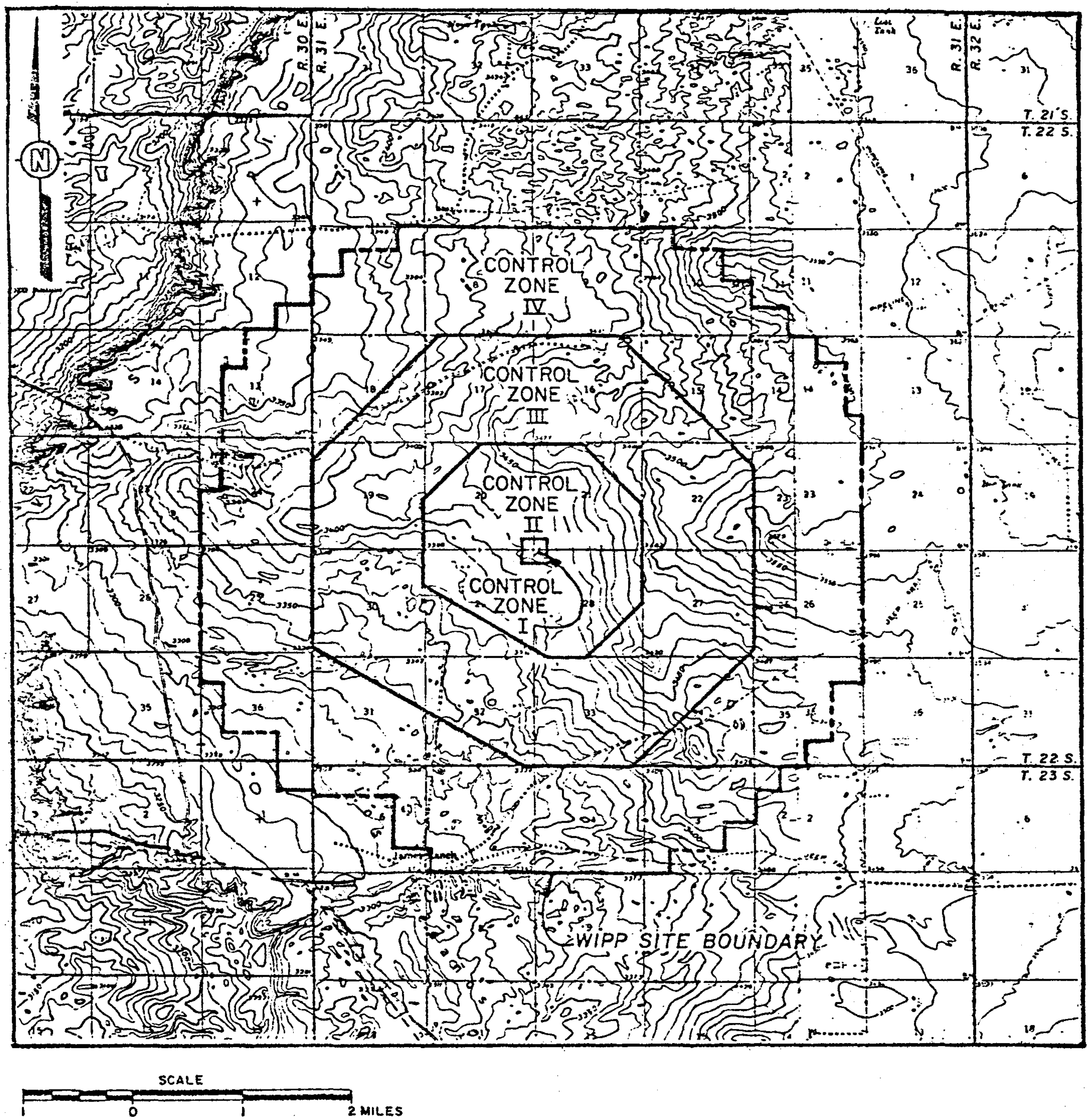
Potash (i.e., sylvite and langbeinite) and hydrocarbons (i.e., natural gas and distillate) comprise the resources present at the WIPP Site that are of interest, considering the technology and market conditions in the foreseeable future. These resources and the methods available to recover them are described in detail in the WIPP Final Environmental Impact Statement (Sections 7.3.7, 9.2.3, and 9.6.5).

The DOE anticipates that extraction of potash within Control Zone IV will be "allowable" if traditional underground mining methods are employed. Traditional methods include drill-and-blast, continuous mining, shortwall, and longwall techniques. If mining of potash is allowed, it is not reasonable to prohibit those mining techniques that make such an activity economically viable. To limit potash ore extraction ratios to low values is, in effect, to preclude such mining. Accordingly, it is anticipated that extraction ratios can be maximized in any mines developed within Control Zone IV of the WIPP Site consistent with mine safety considerations and other state and federal requirements. Solution mining is not now and will not be "allowable" within the limits of the WIPP Site. This restriction does not affect langbeinite recovery because langbeinite is less soluble than the surrounding minerals (i.e., halite, sylvite) so that solution mining for this material would be ineffective. The lack of existing solution mining for sylvite in the Carlsbad potash district confirms that this restriction does not place a significant economic hardship on the producers or significantly affect state revenues.

The DOE anticipates that recovery of hydrocarbon resources from Control Zone IV will be "allowable" following a final decision on waste retrival. This activity includes drilling, production stimulation, and, possibly, secondary recovery. Resources located beneath Zone IV may be accessed by vertical drilling; resources located beneath the inner three control zones may be accessed by drilling vertically in Zone IV to a depth of 6,000 feet and then deviating from vertical at the angle required to reach the target resource zone. It is not realistic to allow drilling for hydrocarbon resources and, if oil or gas is found, to prohibit those techniques available to the producer that maximize recovery. It is the anticipated postion of the DOE that analyses will confirm the acceptability of enhancing production from drilled wells by hydraulically fracturing the reservoir rock, acidizing the formation, or other applicable techniques. These types of production stimulation are used primarily to increase the permeability of the rock that contains the hydrocarbons. Secondary recovery methods (i.e., techniques used to enhance or replace the natural driving force that "pushes" the oil to the production well) may also be employed, but because the resources present are primarily natural gas and not oil, such techniques are not expected to be useful.

DOE intends to amend the present interim policy following completion of the impact analyses. This amendment will expand upon the present policy by identifying the extent to which resource recovery is anticipated to be "allowable" if radioactive waste is permanently emplaced. The amendment will not affect the interim policy that no resource recovery will be allowed until all retrieval decisions are made.

The final DOE policy on resource recovery will be based upon the interim policy, institutional requirements in effect at that time, and data obtained during development and operation of the facility.



REFERENCE:

USGS 15 MINUTE TOPOGRAPHIC QUADRANGLES: NASH DRAW, NEW MEXICO (1965) AND HAT MESA, NEW MEXICO (1972).
SCALE 1:62,500.

FIGURE 1

WIPP SITE
CONTROL ZONES

2-19-1-1 0

STATE OF NEW MEXICO
GOVERNOR'S CABINET
SANTA FE
87503

BRUCE KING
GOVERNOR

GEORGE S. GOLDSTEIN, PH.D.
SECRETARY FOR HEALTH & ENVIRONMENT

November 17, 1981

Mr. D. T. Schueler
Assistant Manager for Project
and Energy Programs
U. S. Department of Energy-ALO
P. O. Box 5400
Albuquerque, NM 87115

Dear Mr. Schueler:

This is in response to Mr. Joseph McGough's November 3, 1981 letter containing DOE's Interim Policy Statement on Resource Recovery at the WIPP Site to be used as the basis of dose consequence analyses as required by item 6 of Appendix B of the Stipulated Agreement.

The State is very interested in this Interim Policy as well as with other land withdrawal and land use aspects at the WIPP site. Since we believe that a resource recovery policy should only be adopted after a thorough consequence analysis, we will not comment in detail on your Interim Policy until we have had the opportunity to review your analysis scheduled for release in December, 1981. We have the following observations on the Interim Policy:

1. The policy statement implies that Zone IV will be withdrawn and under the control of DOE. It is our understanding that at the present time the Department of the Interior is not transferring Zone IV to DOE but only Zones I - III. The question of who will have control over activities in Zone IV needs to be answered.

2. The statement is silent on what resource recovery may be permitted in Zones I, II, and III and implies that the current analyses will not consider this. From a radiological health standpoint, resource recovery activity in these zones is more important than in Zone IV and should be evaluated in detail. At some date in the future, DOE will relinquish administrative control over Zones I-III and the consequences of resource recovery should be published.

3. The policy states that resource recovery will be controlled or prohibited in the buffer zones. How long a period is DOE using for planning purposes?

4. The statement that secondary recovery of hydrocarbons may be permitted in Zone IV is more permissive than the statement on page 9-27 of the FEIS: "Hydrocarbon exploration in control Zone IV would be permitted by DOE, but no

water flood recovery methods or extensive hydrofracture stimulation would be allowed."

5. The terms "unacceptable hazard" and "unacceptable effects" need to be defined. Is it based on exceeding a predetermined limit of exposure or on the presence of radiation effects in people?

Thank you for sending the Interim Policy Statement. While we look forward to the receipt of your analyses next month, we would appreciate your comments at this time.

Sincerely,

George S. Goldstein, Ph.D.
Secretary

GSG:teb

cc: Joseph M. McGough, Project Manager on WIPP, DOE
Larry kehoe, Secretary, Energy and Minerals Department
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EVALUATION GROUP

DEC 29 1981

Dr. George S. Goldstein
Secretary
Health and Environmental Department
P.O. Box 968
Santa Fe, NM 87503

Dear Dr. Goldstein:

Interim Policy Statement on Resource Recovery at the WIPP Site

This is in response to your November 17, 1981 letter providing observations on and requesting clarification of some of the points made by the subject document.

We are in complete agreement that a resource recovery policy should be adopted only after a thorough consequence analysis has been performed. The Interim Policy was not, and is not intended as, a DOE position on resource recovery that would allow resource extraction companies to begin planning for future mineral recovery at the WIPP Site. Instead, the Interim Policy provides a basis from which pertinent breach scenarios can be developed and analyzed; it will be modified before publication as a final Interim Policy, if necessary, to reflect the results of consequence studies presently underway.

Specific issues raised in your recent letter are addressed below:

1. At present, we have pending, an application for temporary withdrawal of lands in Zones I, II and III. This application was filed so that land could be withdrawn, thus, protected for the duration of the Site and Preliminary Design Validation (SPDV) Program. Zone IV is not included in this application but will be protected from some nonDOE activities (e.g., deep drilling) by a Cooperative Agreement between DOE and DOI. We have also filed a permanent land withdrawal application with the intent of obtaining DOE administrative control over the lands within all four zones of the WIPP Site.

2. Allowing resource recovery from Zones I, II and III is not currently planned and consequently was not addressed in the Interim Policy. The report, presently in preparation, discussing the effects of resource extraction activities in Zone IV will not provide analyses of short-term consequences of extraction activities in the other zones. The report will, however, address indirectly the potential long-term consequences of resource extraction at any location within the Zone IV boundary. No discussion of potential short-term consequences of extraction in Zones I, II and III was felt necessary because it is unlikely that DOE will allow such activities while it retains administrative control over the lands.

If DOE does desire, in the future, to allow some types of resource extraction in Zones I, II and III, the potential consequences will be evaluated prior to a final decision on whether to allow or disallow specific activities at the site. In any case, denial of resource extraction from Zones I, II and III would cause only a small percentage of the natural resources at the site to be lost. As reported in the FEIS, more than half the hydrocarbon resources and more than two-thirds of potash resources are located in Zone IV. Additionally, it is believed that deviated drilling from Zone IV would allow extraction of all hydrocarbon resources at the WIPP Site.

3. DOE plans to maintain its resource recovery policy at the WIPP Site for the duration of its administrative control over the site lands. In the FEIS, it was assumed to be reasonable that administrative control would exist for a minimum of 100 years.

4. At the time of publication of the FEIS, studies necessary to evaluate the effects of primary production stimulation and secondary recovery of hydrocarbons had not been conducted. Thus, to maintain the consistently conservative posture of the WIPP Project, DOE elected to preclude these hydrocarbon recovery methods until further study could be undertaken. As noted in the second paragraph of this letter, the Interim Policy was intended to provide direction relative to the types of breach scenarios that should be considered and analyzed. In development of the statement, it was felt unreasonable to allow an extraction company to complete an exploration well (at great cost) and to then not allow the operator to perform whatever techniques he deemed useful to regain his investment. Thus, it was felt essential that the potential consequences of all types of production enhancement be evaluated before general distribution of a policy on resource extraction.

5. Presently, the potential dose consequences of resource extraction at the WIPP Site are deemed unacceptable if the potential hazards to public health and safety exceed those of the breach events considered in the FEIS and SAR.

We are currently on schedule in the preparation of the report on the potential consequences of resource recovery at the WIPP Site and plan to submit a draft to the State in December. If you have further questions or comments on this matter, please contact me.

Sincerely,

J. M. McGough
Project Manager
WIPP Project Office

WIPP:JMM 81-5135/5190

cc:
D. T. Schueler, AMPEP, ALO
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STATE OF NEW MEXICO

ENVIRONMENT department

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February 23, 1982

Mr. Joseph McGough
Project Manager on WIPP
WIPP Project Office
U. S. Department of Energy
Albuquerque Operations Office
P. O. Box 5400
Albuquerque, NM 87115

Dear Mr. McGough:

The Environmental Evaluation Group has reviewed D'Appolonia's Draft Report, "Natural Resources Study" which you transmitted to us on January 29, 1982. There is attached a summary of our comments. We would appreciate your response to these comments and will be pleased to discuss them if you have any questions.

Sincerely,

Robert H. Neill
Director

RHN:MSL:lgr

cc: George S. Goldstein, Ph.D., Secretary, Health & Environment Department
Larry Kehoe, Secretary, Energy and Minerals Department
Joe Hewett, Secretary, Highway Department
Thomas E. Baca, Director, Environmental Improvement Division
Joe Canepa, Attorney at Law
Wendell Weart, Manager, Sandia Laboratories
Chuck Little, Westinghouse Electric Corporation
TSC, IEA

REVIEW COMMENTS

CONCERNING

Draft Report on Natural Resources Study.

Waste Isolation Pilot Plat (WIPP) Project

Southeastern New Mexico

Comments by

Environmental Evaluation Group

Environmental Improvement Division

Health and Environment Department

P. O. box 968

Santa Fe, New Mexico

87503

February 22, 1982

General Comments

1. The report fails to comprehensively address the question of new exploration. Neither the DOE Interim Policy statement (Section 1.4) nor the balance of the report indicate that any restrictions will be imposed on new exploration for natural resources, only extraction. Since certain exploratory techniques may seriously affect the integrity of the repository, the Interim Policy Statement should impose restrictions similar to those for removal.

2. It is recognized that the construction of solution cavities for storage of hydrocarbons does not constitute exploration or mining for natural resources; however, it is an extraction technology; it is very relevant to gas and oil resources; it is becoming an increasingly popular practice; and it has not previously been examined by DOE. Therefore, EEG recommends that it be included and the impact analyzed in the Final Policy Statement and Report on Natural Resouces.

3. It is noted that this report addresses the extraction in Zone IV only, which is consistent with the present Interim Policy. It should be noted that if at some future date, DOE proposes to modify that Policy to allow extractions from Zones II or III, it would be necessary to make a new analysis of the impact of such change.

Specific Comments

1. <u>Section 1.4, DOE Interim, Policy Statement</u> -- In a letter dated November 17, 1981, Dr. George Goldstein, Secretary for Health and Environment of New

Mexico, submitted several comments with respect to the DOE Interim Policy Statement. Although a letter from J. M. McGough of the DOE Albuquerque Operations Office responded to Dr. Goldstein's comments, the Interim Policy Statement was not modified and the report failed to either clarify or amend the ambiguous passages of the Policy Statement. For example, the phrases "unacceptable effects" (p. 1-6) and "allowable effects" (p. 1-8) remain undefined. EEG recommends that the statement be revised to reflect Dr. Goldstein's previous comments and the comments contained herein.

2. Section 2.1.1. Page 2-2, second paragraph -- The report is inconsistent in statements about the depth at which potash may be found. This section states that it is not within 400 feet of the repository horizon. Table 1 indicates langbeinite as low as 1800 feet (350 feet above the repository horizon). However, Table 5 indicates depths as low as 2300 feet (150 feet below the horizon). It would be helpful if the report indicated where the potash mines listed in Table 5 are located, and what are the actual depths to potash within the site.

3. Section 3.1.1.1, Page 3-1, last paragraph -- It is emphasized that empirical data for the pressure arch theory is based on coal mining experience. The appropriateness and applicability for mines in salt needs to be addressed.

4. Section 3.1.1.2, Page 3-3, last paragraph -- The author should also include data from studies performed by Sandia using elaborate rock mechanics codes, as contained in the following reference:

Morgan, H. S., Krieg, R. D., Matalucci, R. V., "Comparative Analyses of Nine Structural Codes Used in the Second WIPP Benchmark Problem," Sand-81-1839, Sandia National Laboratories, Albuquerque, N.M.

5. Section 3.1.1.2, Page 3-5, first line -- This sentence assumes that the stress-relaxation process which occurs prior to establishing steady-state conditions, cannot be studied. This assumption seems to be incorrect since the problem of transient creep is treated in great detail in report ECN-89. (See reference provided in comment 4 above.) The report also contains a program for the HP-41C pocket calculator.

6. Section 3.1.1.2, Page 3-5 3rd paragraph -- This paragraph addresses the zone of influence of a potential potash mine when superimposed by the influence of the WIPP repository and concludes that a separation of 2,100 feet assures that interaction will not occur. This conclusion seems to be based on the assumption that the existing rock between the WIPP and the potential potash mine is reasonably stable. However, in Zone III, north of the repository, there is a zone of "anomalous seismic reflection" where the structure is presently unknown. Also the presence of brine at WIPP-12 suggests that fracturing within the Castile may be extensive. Further information is needed on this anomalous zone before it can be concluded that 2,100 feet is sufficient.

7. Section 3.1.1.2, Page 3-5, 3rd paragraph -- The conclusion that the effects of mining the WIPP repository will only extend to 200 feet beyond the perimeter of the repository, the effect of a single panel, is not convincing. A potash mining activity approaching the WIPP repository from the north faces

a 2560' x 33' room running east-west and crossed by 18 rooms running north-south. It may be better to model the WIPP facility by some effective cavity. Also, since potash mining may extend to depths from the McNutt zone to 2300 feet (see comment 2 above), it may be preferable to approximate the extent of WIPP influence on the potash mine by the extent of possible subsidence area over the WIPP.

8. Section 4.1, page 4-2, 3rd paragraph -- The assumption that plutonium would dissolve at the same rate as the Salado formation is undoubtedly conservative if the salt is actually all dissolved (this would give a concentration of ≈ 1.6 mg/ℓ Pu in the brine). However, leaching of Pu from the waste can occur in the absence of net salt dissolution. Studies at PNL* have shown that about 10^{-4} per year of plutonium oxide fuel pellets would leach into "WIPP-B" brine. At this leach rate it would take 500 years to get a concentration of 1.6 mg/ℓ and solubility limits would probably prevent this high a concentration from ever occurring. It is recommended that the final report recognize that some waste leaching will occur if brine is in contact with the waste. Also, leaching by brine is more plausible than scenarios that require large volumes of the Salado formation to be dissolved.

---

*Bradley, D.J., Harvey, C.O., and Turcotte, R.P. Leaching of Actinides and Technetium from Simulated High-Level Glass (PNL-3152), August 1979.

RESPONSES TO NATURAL RESOURCES STUDY COMMENTS

GENERAL COMMENTS

1. Comment:

"The report fails to comprehensively address the question of new exploration. Neither the DOE Interim Policy Statement (Section 1.4) nor the balance of the report indicate that any restrictions will be imposed on new exploration for natural resources, only extraction. Since certain exploratory techniques may seriously affect the integrity of the repository, the Interim Policy Statement should impose restrictions similar to those for removal."

Response:

The report indicates that exploration activities are an integral part of resource development for both potash and hydrocarbons. As exploration is an essential portion of resource development, it is implied that restrictions placed on extraction activities would also apply to new exploration. Additionally, it is highly unlikely that resource companies would spend money to explore areas that are closed to resource extraction; however, a phrase will be added to the introduction to address exploration activities.

2. Comment:

"It is recognized that the construction of solution cavities for storage of hydrocarbons does not constitute exploration or mining for natural resources; however, it is an extraction technology; it is very relevant to gas and oil resources; it is becoming an increasingly popular practice; and it has not previously been examined by DOE. Therefore, EEG recommends that it be included and the impact analyzed in the Final Policy Statement and Report on Natural Resources."

Response:

A statement will be added to the report prohibiting any extraction activities at the WIPP site other than those discussed in the report, without prior approval of the DOE. It is unlikely, however, that DOE would allow construction of storage cavities at the site because there are many other suitable locations in the Delaware Basin for construction of these cavities.

3. Comment:

"It is noted that this report addresses the extraction in Zone IV only, which is consistent with the present Interim Policy. It should be noted that if at some future date, DOE proposes to modify the Policy to allow extractions from Zones II and III, it would be necessary to make a new analysis of the impact of such change."

Response:

Substantial modification of portions of the report would likely be necessary should DOE choose to examine the impacts of resource recovery from Zone II or Zone III.

SPECIFIC COMMENTS

1. Comment:

"Section 1.4, DOE Interim, Policy Statement -- In a letter dated November 17, 1981, Dr. George Goldstein, Secretary for Health and Environment of New Mexico, submitted several comments with respect to the DOE Interim Policy Statement. Although a letter from J. M. McGough of the DOE Albuquerque Operations Office responded to Dr. Goldstein's comments, the Interim Policy Statement was not modified and the report failed to either clarify or amend the ambiguous passages of the Policy Statement. For example, the phrases "unacceptable effects" (p. 1-6) and "allowable effects" (p. 1-8) remain undefined. EEG recommends that the Statement be revised to reflect Dr. Goldstein's previous comments and the comments contained herein."

Response:

The DOE Interim Policy Statement was included in the report for historical perspective and to indicate that certain extraction activities would not be allowed at the site and thus would not be evaluated relative to their impact. The precise definitions of "unacceptable effects" and "allowable effects" are not germane to the technical content of the report, because no effects are expected to result from resource recovery in Zone IV.

It should be realized that the Interim Policy Statement was developed not as a legal document but to provide a basis with which the potential effects of resource recovery could be studied.

2. Comment:

"Section 2.1.1 Page 2-2, second paragraph -- The report is inconsistent in statements about the depth at which potash may be found. This section states that it is not within 400 feet of the repository horizon. Table 1 indicates langebinite as low as 1800 feet (350 feet above the repository horizon). However, Table 5 indicates depths as low as 2300 feet (150 feet below the horizon). It would be helpful if the report indicated where the potash mines listed in Table 5 are located, and what are the actual depths to potash within the site."

Response:

The depth to the base of the McNutt potash member of the Salado Formation is about 1740 feet at ERDA-9, or approximately 400 feet above the storage horizon. The depth at which potash is found in the basin varies as the depth to the McNutt potash member varies, which is dependent on the occurrence of strata dip, structural features and changes in topography. The base of the McNutt is at a depth of aproximately 1600 feet in the northern part of Zone IV; at a depth of about 1500 feet in the western part of Zone IV; at a depth of about 1550 feet in the southern part and at a depth of about 2000 feet in the eastern part. The depths to potash mineralization were included in the report primarily to indicate the depths at which potash mining experience exists. The location of the drillholes or mines from which depth measurements were obtained are available from the reference given in the tables or from maps prepared for WIPP or for other purposes and included in published project documents.

3. Comment:

"Section 3.1.1.1, Page 3-1, last paragraph -- It is emphasized that empirical data for the pressure arch theory is based on coal mining experience. The appropriateness and applicability for mines in salt needs to be addressed."

Response:

The pressure arch theory is a sound, proven concept and should be applicable to nearly any underground opening. As noted in the report, limited data exist for mines in salt compared to the extensive data base developed from coal mining experience. Further attempts, however, are underway to obtain more data on potash mining to strengthen the conclusions reached relative to mining effects. In any case, the report will be modified to demonstrate the appropriateness of the methodology used relative to the pressure arch theory.

4. Comment:

"Section 3.1.1.2, Page 3-3, last paragraph -- The author should also include data from studies performed by Sandia using elaborate rock mechanics codes, as contained in the following reference:

Morgan, H. S., Krieg, R. D., Matalucci, R. V., "Comparative Analyses of Nine Structural Codes Used in the Second WIPP Benchmark Problem." SAND-81-1839, Sandia National Laboratories, Albuquerque, N.M."

Response:

The Benchmark studies were directed at determining predicted stress-strain behavior of the WIPP underground workings in the near field. The effects that potash mining in Zone IV might produce on the WIPP facility are certainly not near-field. Thus, the applicability of the Benchmark analyses relative to this problem is not apparent.

In any case, more sophisticated techniques, such as geomechanical modeling using finite element techniques are not necessary. This is primarily because the results of first approximations as described in the report, using very conservative assumptions, demonstrate the integrity of a significant buffer zone between potash mines developed in Zone IV and the WIPP facility.

5. Comment:

"Section 3.1.1.2, Page 3-5, first line -- This sentence assumes that the stress-relaxation process which occurs prior to establishing steady-state conditions, cannot be studied. This assumption seems to be incorrect since the problem of transient creep is treated in great detail in report ECN-89. (See reference provided in comment 4 above.) The report also contains a program for the HP-41C pocket calculator."

Response:

The sentence referenced in the comment will be modified to indicate that the model used to determine zone of influence around underground openings cannot be used to address transient creep phenomena. The determination of the transient creep characteristics are unimportant in this instance because the cummulative effects of stress-relaxation on the surrounding strata, produced by a potash mine or the WIPP facility, are of primary concern.

6. Comment:

"Section 3.1.1.2, Page 3-5 3rd paragraph -- This paragraph addresses the zone of influence of a potential potash mine when superimposed by the influence of the WIPP repository and concludes that a separation of 2,100 feet assures that interaction will not occur. This conclusion seems to be based on the assumption that the existing rock between the WIPP and the potential potash mine is reasonably stable. However, in Zone III, north of the repository, there is a zone of "anomalous seismic reflection" where the structure is presently unknown. Also the presence of brine at WIPP-12 suggests that fracturing within the Castile may be extensive. Further information is needed on this anomalous zone before it can be concluded that 2,100 feet is sufficient."

Response:

The "zone of anomalous seismic reflection data" extends less than half a mile into Zone III at the Cowden Anhydrite horizon. No disturbance has been noted at WIPP storage horizon; in fact, there is very little steepening of strata at the storage horizon. At the McNutt potash zone horizon, the strata in the northern part of Zone IV are nearly horizontal and unaffected by the steepening of the

Castile Formation. Additionally, core obtained from boreholes located in the Salado Formation in the northern part of the Zones III and IV has not shown evidence of fracturing or deformation. Further, it is highly unlikely that any fracture created in the Salado Formation would remain open and permeable, due to the geomechnical properties of salt rock. It should be remembered that the fracture networks that are brine-containing in the Castile Formation are confined to thick anhydrite layers. Even in areas where fractures in Castile anhydrites are present, these fractures generally terminate at anhydrite-halite contacts. Thus, it is highly improbable that the "zone of anomalous seismic reflection data" represents any geologic condition that would impact the determination of the zones of influence of a potash mine or the WIPP underground workings.

7. Comment:

"Section 3.1.1.2, Page 3-5, 3rd paragraph -- The conclusion that the effects of mining the WIPP repository will only extend to 200 feet beyond the perimeter of the repository, the effect of a single panel, is not convincing. A potash mining activity approaching the WIPP repository from the north faces a 2560' x 33' room running east-west and crossed by 18 rooms running north-south. It may be better to model the WIPP facility by some effective cavity. Also, since potash mining may extend to depths from the McNutt zone to 2300 feet (see Comment 2 above), it may be preferable to approximate the extent of WIPP influence on the potash mine by the extent of possible subsidence area over the WIPP."

Response:

The statements that describe the independence of individual rooms within the WIPP underground facility will be better substantiated in the final report. It should be noted, however, that the length of the cavity is irrelevant as a cylinder of infinite length was assumed in the analysis. Relative to the expected depths of potash mining in Zone IV, see the Response to Comment 2 of the Specific Comments.

8. Comment:

"Section 4.1, page 4-2, 3rd paragraph -- The assumption that plutonium would dissolve at the same rate as the Salado Formation is undoubtedly conservative if the salt is actually all dissolved (this would give a concentration of 1.6 mg/l Pu in the brine). However, leaching of Pu from the waste can occur in the absence of net salt dissolution. Studies at PNL* have shown that about 10^{-4} per year of plutonium oxide fuel pellets would leach into "WIPP-B" brine. At this leach rate it would take 500 years to get a concentration of 1.6 mg/l and solubility limits would probably prevent this high a concentration from ever occurring. It is recommended that the final report recognize that some waste leaching will occur if brine is in

*Bradley, D. J., Harvey, C. O., and Turcotte, R. P. Leaching of Actinides and Technetium from Simulated High-Level Glass (PNL-3152), August 1979.

contact with the waste. Also, leaching by brine is more plausible than scenarios that require large volumes of the Salado formation to be dissolved."

Response:

The report simply restates the assumptions that were used in development of the WIPP long-term waste isolation assessment. The assumption that the waste is as soluble as the encapsulating salt is basic to the scenario analyses performed for this assessment. Although some leaching of plutonium oxide might occur under flowing conditions, some driving force must be present to cause brine to enter and leave the facility and pathways for brine to enter and leave must be established. The analyses and discussion in the report indicate that resource extraction activities in Zone IV would not significantly contribute to the creation of necessary pathways or driving forces.

9. Comment:

"Section 4.2, page 4-2 -- As previously discussed, the zone of influence in the northern sector of Zone III as a result of potash mining cannot be determined with reliability because the structure over the disturbed zone is unknown. It may contain significant fractures and be in direct communications with Zone II. See Comment 17 for recommended action."

Response:

See Response to Comment 6 of Specific Comments.

10. Comment:

"Section 4.2, page 4.2 -- It would seem that fresh water would not be required to move through a more permeable formation around the repository. Brine with some driving force should be able to flow through a permeable salt formation."

Response:

The permeability of undisturbed Salado salt is quite low and would allow very little fluid flow through the formation. The areal extent of mining-caused elevated permeability is very limited around both a hypothetical potash mine and the WIPP facility. No overlap of the zones of increased permeability would be expected, thus, a preferential flowpath for fluid travel would not be created.

If brine were to saturate the zone of increased permeability, no additional dissolution could occur. Additionally, the fracture network representing the zone would not be expected to remain open for any significant time interval based on the creep-closure properties of salt.

11. Comment:

"Section 4.2.1, page 4-3 -- The Interim Policy Statement does not appear to impose any restrictions on exploratory drilling in Zones II or III. If such restrictions were intended, it should be stated more clearly."

Response:

See Response to Comment 1 of General Comments

12. Comment:

"Section 4.2.1.1, page 4-3, first paragraph -- See Comment 2 above concerning the separation between the potash ore and the repository horizon. Also see Comment 11 above."

Response:

See Response to Comment 2 of Specific Comments and Comment 1 of General Comments.

13. Comment:

"Section 4.2.1.1. -- Why is it necessary to dissolve the (entire) one-mile-wide buffer strip to get to the repository? Could not fresh water follows fractures, (dissolving salt along the way) between potash mine and repository? The probability of dissolving in a straight line between the repository and mine may be low but subsidence (page 3-8) and zone of influence (page 3-3) effects could provide a preferential pathway for part of the distance between Zone II and III."

Response:

This section will be modified to clarify its intended meaning. It was not meant to imply that the entire one-mile-wide buffer must be dissolved for fluids to reach the WIPP facility. It is understood that fluids would seek preferential pathways (if such pathways exist). The primary purpose of Chapter 3 is to describe the expected extent of any preferential pathways created by resource extraction activities or by WIPP facility construction. As shown in this chapter, the effects on surrounding strata created by mining for potash in Zone IV and by construction of the WIPP facility do not overlap. Given the expected extent of effects from construction of these underground openings, millions of years would be required for fluids to dissolve a sufficient amount of halite to create a pathway from a mine in Zone IV to the WIPP storage facility.

14. Comment:

"Section 4.2.2., page 4-4 -- As previously indicated, mining of potash in the northern sector of Zone IV indeed may establish communications with Zone II at the repository horizon. The zone of anomalous seismic reflections encompasses virtually all of Zone III and portions of Zone II; there is a large brine reservoir at WIPP-12; thus there is considerable disturbance in Zone III which may extend to the repository horizon. Additional information is needed on this disturbed area before it can be known with confidence that potash mining in Zone IV can be allowed. See also Comment 13 above."

Response:

The analyses performed to evalute the effects of potash mining indicate that mining in Zone IV will not establish communications with Zone II.

Also, see Response to Comment 6 and Comment 13 of Specific Comments.

15. Comment:

"Section 4.3.1 -- There is a question concerning deviated drilling into the Ramsey sand. Since this formation is less than 6,000 feet deep zones I, II, III, it could not be developed under the policy stated on page 1-9. The question is whether this formation would not be developed under the inner zones or whether a variance would be permitted."

Response:

Deviated drilling into the Ramsey sand under Zones I, II and III will not be allowed. Disallowing this action will maintain the conservative approach taken by DOE on WIPP development. Additionally, denial of drilling into the Ramsey sand under the inner three Control Zones should not cause a substantial volume of hydrocarbons to remain untapped because the Ramsey sand is not an attractive exploration target in the site area.

16. Comment:

"Section 4.3.2, Page 4-8, paragraph 2 -- What is the basis for the statement (on page 4-8) that the Capitan aquifer potentiometric gradient is high enough to reach the surface at the WIPP site? Hiss' data show the potentiometric surface to the north of the site as 2900-3200 feet (fresh water equivalent), compared to a surface elevation of 3400 feet at the site."

Response:

The sentence should have stated that the potentiometric gradient of the Capitan and the ground surface in the WIPP site area is insufficient to drive fresh water to the surface.

17. Comment:

"Section 5.0, page 5.1, last paragraph -- It is the conclusion of EEG that the mining of potash in Zone IV should not be allowed until after the completion of the analysis of the zone of anomalous seismic reflection in Zone III and of WIPP-12. Therefore, the decision on allowing extraction of natural resources within Zone IV should be delayed until after the State has commented on the DOE reports on Brine Reservoirs and the Disturbed Zone."

Response:

See Response to Comment 6 of Specific Comments

EDITORIAL COMMENTS

The report is in final editing and these as well as other editorial items will be corrected in the final report.

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HEALTH & ENVIRONMENT department

STATE OF NEW MEXICO

ENVIRONMENTAL EVALUATION GROUP
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October 26, 1982

Joseph M. McGough
WIPP Project Manager
U. S. Department of Energy
Albuquerque Operations Office
P. O. Box 5400
Albuquerque, NM 87115

Dear Mr. McGough:

Pursuant to our conversations over the past several weeks, I wanted to confirm the EEG's concern relative to DOE's issuance of a "Policy on Resource Recovery" without giving the State ample opportunity for review. We have a number of questions about the relinquishment of Zone IV control, a few of which are contained below. I request that you involve us at the earliest practical stage in the review process to assure that we can work together constructively on the resource recovery issues.

A few of our questions about the DOE policy follow:

1. Will solution mining be allowed in Zone IV?
2. If so, how will it be controlled?
3. Will storage cavities be allowed?
4. Will there be any restrictions on extraction or storage cavities in any of the evaporite formations in Zone IV?
5. Will directional drilling be allowed in Zone III?
6. Will extraction of brine reservoirs in the Castile from Zone IV permit drainage of brine from underneath the inner zones?
7. What will the Interim Policy Statement say regarding restrictons in zones?
8. Will the MOW between BLM and/or MMS allw DOE to have any input concerning future requests for activities in Zone IV? Or will it be completely out of DOE's hands?
9. What has become of the interim DOE policy to permit no exploration or extraction before 1993?
10. How will Zone IV be controlled if it is not under direct DOE control?
11. Will extraction of brine be authorized? Who will decide?

Please advise how we may best work together on resolution of not just these questions but others as well.

Sincerely,

Robert H. Neill

Robert H. Neill
Director

2-081AG2-19-1

RHN:JM:eg

cc: TSC, IEA

Department of Energy
Albuquerque Operations Office
P.O. Box 5400
Albuquerque, New Mexico 87115

RECEIVED

NOV 29 1982

ENVIRONMENTAL
EVALUATION GROUP

NOV 24 1982

Mr. Robert H. Neill
Director
State of New Mexico
Environmental Evaluation Group
320 Mary Street
P. O. Box 968
Santa Fe, NM 87504

Dear Mr. Neill:

DOE Policy on Resource Recovery at the WIPP Site

As you know, the DOE Interim Policy Statement on Resource Recovery at the WIPP Site, which was transmitted to the State of New Mexico on November 3, 1981, was developed to serve as the basis for performing the Natural Resources Study. And, as you also are aware, the study concludes that activities related to potash and hydrocarbon resource extraction and solution mining outside the Control Zone III boundary, using currently available and applicable technology, will not compromise the integrity of the WIPP waste emplacement facility.

It was my understanding that your concerns regarding the draft and final Natural Resources Study reports were discussed with representatives of our Technical Support Contractor and resolved to your satisfaction. The DOE is now considering the issuance of a revised interim policy statement on resource recovery which would reflect the conclusions of the Natural Resources Study and, possibly, permit access to potash and hydrocarbons that many believe are critical resources.

Although the revised interim policy statement is still undergoing DOE review, responses to your questions have been developed based on our current position and are included in the enclosure. The revised interim policy statement will be based on the results of the Natural Resources Study and will be transmitted to the State of New Mexico as soon as our internal review is completed.

Sincerely,

J. M. McGough
Project Manager
WIPP Project Office

WIPP:JMM 82-0801/6252A

Enclosure

cc w/o enclosure:
R. K. Brown, TSC
G. L. Hohmann, TSC
C. C. Little, TSC

cc w/enclosure:
C&C File, IEA, TSC

Question -- Will solution mining be allowed in Zone IV?

Response -- Unsuccessful experiences in solution mining of potash by the Potash Company of America and Continental Potash and the lack of suitable water supplies in the Carlsbad area suggest that the potential for application of currently available solution mining technology for the extraction of sylvite is remote; however, solution mining will be allowed in Control Zone IV.

Question -- If so, how will it be controlled?

Response -- The DOE will impose no controls over solution mining in Control Zone IV. The U.S. Department of the Interior Minerals Management Service and the New Mexico Energy and Minerals Department are responsible for reviewing proposed mining operations to assure compliance with regulations and prevent violation of adjacent leases.

Question -- Will storage cavities be allowed?

Response -- Although the development of storage cavities in the WIPP site area is considered unlikely, the DOE will exercise no controls over the development of such cavities outside Control Zone III provided that the WIPP site boundary is not violated.

Question -- Will there be any restrictions on extraction or storage cavities in any of the evaporite formations in Zone IV?

Response -- No, provided that the WIPP site boundary is not violated.

Question -- Will directional drilling be allowed in Zone III?

Response -- No drilling will be allowed from Control Zone III. However, drilling from outside Control Zone III to gain access to hydrocarbons beneath Control Zones I, II and III at depths greater than 6000 feet will be allowed if the planes formed by the downward vertical projections of the Control Zone III boundaries are not penetrated above a depth of 6000 feet.

Question -- Will extraction of brine reservoirs in the Castile from Zone IV permit drainage of brine from underneath the inner zones?

Response -- In general, the geographical extent of brine reservoirs in the Castile Formation has not been determined; however, it is possible that a brine reservoir encountered in Control Zone IV could extend beneath the inner zones. Consequently, extraction of brine from a reservoir beneath Control Zone IV could result in removal of brine from beneath the inner zones. Because of the characteristics of brine reservoirs found in the Castile Formation, extraction of brine will not result in subsidence of sufficient magnitude to fracture overlying strata. Thus, no effect on the underground facility is expected.

Question -- What will the Interim Policy Statement say regarding restrictions in zones?

Response -- The revised interim policy statement is expected to indicate the following:

- No potash or other commercial mining will be allowed in Control Zones I, II and III.
- No commercial drilling (hydrocarbon or other) will be allowed from Control Zones I, II and III.
- Drilling from outside Control Zone III to access locations beneath Control Zones I, II and III at depths greater than 6000 feet will be allowed.

Question -- Will the MOU between BLM and/or MMS allow DOE to have any input concerning future requests for activities in Zone IV? Or will it be completely out of DOE's hands?

Response -- The DOE does not expect to exercise any control over activities in Control Zone IV except that no permanent residences will be allowed by BLM in Zone IV. The DOE will, however, arrange with BLM and MMS to be kept apprised of all such activities in Zone 4 and provide recommendation or other assistance to Federal and State regulatory agencies upon request.

Question -- What has become of the interim DOE policy to permit no exploration or extraction before 1993?

Response -- The interim DOE policy which was transmitted to the State of New Mexico in November 1981, was developed to serve as the basis for performance of the Natural Resource Study. The results of this study demonstrate that activities related to potash and hydrocarbon resource extraction and solution mining from within (and outside of) Control Zone IV, using currently available technology, would not compromise the integrity of the WIPP waste emplacement facility. Because of these findings, the DOE is evaluating the desirability of revising the interim policy statement as indicated above.

Question -- How will Zone IV be controlled if it is not under direct DOE control?

Response -- The lands in Control Zone IV will be managed by the Bureau of Land Management in the same manner as other public lands. Commercial mining and hydrocarbon extraction activities will be controlled by the Minerals Management Service and the New Mexico Energy and Minerals Department in accordance with Federal and State regulations governing such activities.

Question -- Will extraction of brine be authorized? Who will decide?

Response -- As indicated above, no commercial mining or drilling (hydrocarbon or other) will be allowed in Control Zones I, II and III, however, no controls will be imposed on such activities outside the Zone III boundary. Although requests for approval of brine extraction activities in the WIPP site area are considered unlikely, such requests will be reviewed by the Minerals Management Service, New Mexico Energy and Minerals Department (for State lands) and the Bureau of Land Management to assure compliance with Federal and State regulations. The DOE will provide assistance to these agencies if requested.

HEALTH & ENVIRONMENT department

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December 6, 1982

Mr. Joseph M. McGough
Project Manager of WIPP
WIPP Project Office
U.S. Department of Energy
Albuquerque Operations Office
P. O. Box 5400
Albuquerque, New Mexico 87115

Dear Mr. McGough:

Thank you for your letter of November 24, 1982 (WIPP:JMM82-0801/6252A) enclosing a response to our questions concerning the DOE plans for the "Policy on Resource Recovery."

There is attached a summary of EEG's comments concerning the recent draft of the DOE "Revised Interim Policy Statement on Natural Resource Recovery at the WIPP Site."

Sincerely,

-for-

Robert H. Neill
Director

RHN:MSL:eg

2-92-AG2-19-1-1

cc: TSC, IEA

Providing an independent analysis for the New Mexico Health and Environment Department of the proposed Waste Isolation Pilot Plant (WIPP), a federal nuclear waste repository.

REVIEW COMMENTS
on
DOE REVISED INTERIM POLICY
STATEMENT ON NATURAL RESOURCES
RECOVERY AT WIPP SITE

by

Environmental Evaluation Group
Environmental Improvement Division
N. M. Health and Environment Department
P. O. Box 968
Santa Fe, NM 87503

December, 1982

A. GENERAL COMMENTS

1. The Statement implies that DOE plans to relinquish all control over Zone IV of the present WIPP site, and that DOE plans to rely on the New Mexico Energy and Minerals Department and the Minerals Management Service of the U. S. Dept. of Interior for reviewing plans in this zone in the future. Because of the remote possibility that activities in this zone may have an adverse impact on the long-term stability of the repository horizon, it is recommended that DOE maintain, in a memorandum of understanding with DOI, a provision for DOI to notify DOE of any proposal for solution mining, solution storage cavities, and mining of Castile brine in Zone IV prior to a decision on that proposal, and that DOE in cooperation with the appropriate State agencies will review these plans and submit comments to DOI for their consideration.

2. The Policy Statement applies only during facility construction and operation of the WIPP facility. The Policy should be expanded to reflect the post-operation controls, since the integrity of the repository and the consequence analyses are dependent upon some controls being maintained.

B. SPECIFIC COMMENTS

1. Page 1, line 6:

   The phrase "commercial drilling" should be defined so that it clearly excludes any drilling as well as mining not related to WIPP in Zones I, II, and III.

2. Page 1, line 14:

   As reflected in the preceding "General Comments," EEG recommends that the MOU between DOE and DOI request that DOE (and the appropriate State agencies) be notified of any proposal in Zone IV envolving solution mining, construction of solution storage cavities or mining of brine from brine reservoirs in the Castile.

3. Page 1, line 25:

   As recommended in the "General Comments," we believe that the controls which are to be maintained after decommissioning should be addressed. This would necessitate deletion of the sentence at line 25.

4. Page 2, line 6:

   The phrase "measurable effects" is ambiguous and may lead to misunderstandings. It should be deleted or more precisely defined.

5. Page 2, paragraph 2:

   This paragraph does not adequately state the potential for resources at the site. It should recognize that technology and market conditions are not predictable and some materials, such as may be found in Castile brine, or Salado salt could become of economic value if technology and market conditions are substantially modified.

6. Page 3, paragraph 4, line 7:

The D'Appolonia draft report "Natural Resources Study" indicates that "resource grade oil" is under the site.

Department of Energy
Albuquerque Operations Office
P.O. Box 5400
Albuquerque, New Mexico 87115

DEC 2 3 1982

Dr. George S. Goldstein
Chairman, Radioactive Task Force
Health and Environment Department
P.O. Box 968
Santa Fe, NM 87503

Dear Dr. Goldstein:

DOE Revised Interim Policy Statement on Natural Resource Recovery at the WIPP Site

Enclosed for your use and information is the DOE Revised Interim Policy Statement on Natural Resource Recovery at the WIPP Site. Under the terms of this policy statement no potash or other commercial mining in, or commercial drilling from, Control Zones I, II and III will be allowed; however, the DOE will exercise no control over mining or drilling outside Control Zone III. (Control Zone III is being redefined as the area withdrawn for SPDV which is a square containing 16 sections (10,240 acres) surrounding the center of the site.) Additionally, BLM will prohibit permanent inhabitation of Zone IV while the facility is in operation. Hydrocarbon resources below 6000 ft. beneath Control Zones I, II and III can be accessed by deviated drilling from outside the Control Zone III boundary. The DOE will rely on the review of State and Federal regulatory agencies, including the New Mexico Energy and Minerals Department and the U.S. Department of the Interior, Minerals Management Service, to protect the integrity of the WIPP Site boundaries from commercial exploration, mining or other extractive activities. So that the DOE can maintain information on resource recovery near the WIPP Site, the Bureau of Land Management will notify the DOE of any requests for resource recovery permits within one mile of the WIPP Site boundary.

The final DOE policy will be issued when the decision is made regarding retrieval of the waste. Should the DOE decide to retrieve all the radioactive waste, the WIPP Site will become available for complete resource recovery after retrieval and decommissioning are complete.

The initial Interim Policy Statement, which was transmitted to the State of New Mexico on November 3, 1981, was developed to serve as the basis for the performance of the Natural Resources Study. The initial DOE

Interim Policy, as indicated therein, was "temporary denial of all resource extraction within the four control zones of the WIPP Site until the decision is made relative to which, if any, of the emplaced waste will be retrieved." Based on the conclusions of the Natural Resources Study, which was transmitted to the State of New Mexico on October 5, 1982, we have determined that the initial Interim Policy can be revised as indicated above.

Not only does the DOE Revised Policy Statement reflect the conclusions of the Natural Resources Study but it also addresses comments provided by the New Mexico Environmental Evaluation Group on the Policy Statement.

If you require additional information or have questions on this matter, please contact me.

Sincerely,

J. M. McGough
Project Manager
WIPP Project Office

Enclosure

WIPP:JMM 82-0885/6366A

cc: w/encl:
J. K. Otts, Chairman, Radioactive Waste Consultation Committee, Santa Fe, NM
J. Bingaman, Attorney General, Santa Fe, NM
D. T. Schueler, AMPEP, AL
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J. Stout, OCC, AL
R. H. Neill, Director, EEG, Santa Fe, NM
C. W. Luscher, State Director, BLM, Santa Fe, NM
M. Wilson, OCC, AL

DOE REVISED INTERIM POLICY STATEMENT ON RESOURCE RECOVERY AT THE WIPP SITE

The policy of the Department of Energy (DOE) concerning resource recovery at the Waste Isolation Pilot Plant (WIPP) site during facility construction and operation is as follows:

- o No potash or other mining excluding that conducted for the WIPP Project will be allowed in WIPP Control Zones I, II, and III.

- o No drilling excluding that conducted for the WIPP Project will be allowed from Control Zones I, II, and III.

- o Drilling from outside Control Zone III to access locations beneath Control Zones I, II, and III at depths greater than 6,000 feet will be allowed if the planes formed by the downward vertical projections of the Control Zone III boundaries are not penetrated above a depth of 6,000 feet.

- o DOE will rely on the review of State and Federal regulatory agencies, including the New Mexico Energy and Minerals Department and the Minerals Management Service, U.S. Department of the Interior, to protect the integrity of the WIPP site boundaries from commercial exploration, mining, and other extractive activities.

- o If the DOE decides that all radioactive waste is to be retrieved, the WIPP site will become available for complete resource recovery once retrieval and facility decommissioning is accomplished.

This policy may be re-evaluated after facility decommissioning. The following paragraphs provide a measure of clarification of the rationale used to develop the resource recovery policy.

It is the policy of the DOE to maximize the opportunity for resource recovery at the WIPP site, consistent with the requirements to isolate the emplaced radioactive wastes from the biosphere. Within five years after the first emplacement of each type of TRU waste (i.e., contact and remotely handled), separate decisions will be made about the retrieval of each kind of waste. <u>If the DOE decides that all waste is to be retrieved, the WIPP site will become available for complete resource recovery once retrieval and facility decommissioning are accomplished.</u>

The criterion for the DOE policy is that permanent denial of resources should be limited to those areas in which extraction activities could potentially lead to measurable effects(1) on the WIPP facilities or whose protection is needed to satisfy institutional considerations, all extraction activities that would not lead to measurable effects on the WIPP site are defined as "allowable" under the DOE policy.

Potash (sylvite and langbeinite) and hydrocarbons (natural gas and distillate) comprise the resources present at the WIPP site that are of interest considering the technology and market conditions in the foreseeable future. These resources and the methods available to recover them are described in detail in the FEIS (U.S. Department of Energy, 1980).

---

1 Measureable effects are those influences from extraction activities that could cause the assumptions made in the breach scenario consequence analyses (U.S. Department of Energy, 1980) to be unconservative.

Due primarily to institutional considerations, no potash mining in or commercial exploratory drilling (hydrocarbon or other) from Control Zones I, II, and III will be permitted. A study was conducted to investigate the possible effects of resource recovery within Control Zone IV on the WIPP facility (Natural Resources Study, Brausch et al., 1982). The following paragraphs provide a brief summary of the results and conclusions of that study.

The extraction of potash outside Control Zone III is allowable. Potential methods of mining potash include drill-and-blast, continuous mining, solution mining, shortwall, and longwall techniques. Since mining of potash is allowable, it is not reasonable to prohibit those mining techniques that make such an activity economically viable. To prohibit such activities is, in effect, to preclude mining. Accordingly, extraction ratios can be maximized in any mines developed outside Control Zone III of the WIPP site, consistent with mine safety considerations and other state and federal requirements. Solution mining will be allowable outside Control Zone III. Resource extraction by solution mining may be applied to recovery of sylvite. Solution mining for recovery of langbeinite would be ineffective because langbeinite is less soluble than the surrounding minerals (e.g., halite, sylvite). However, the lack of existing solution mining for sylvite in the Carlsbad potash mining district suggests that solution mining for potash within Control Zone IV may not be feasible.

The recovery of hydrocarbon resources outside Control Zone III is allowable. This activity includes drilling, production stimulation, and, possibly, secondary recovery. Resources located outside Control Zone III may be accessed by vertical drilling; resources located beneath the inner three control zones at depths greater than 6,000 feet may be accessed by drilling vertically outside Control Zone III to a depth of 6,000 feet and then deviating from vertical at the angle required to reach the target resource zone.

If oil or gas is found, it is not reasonable to prohibit those techniques available to the producer that maximize recovery. Enhancing the production from drilled wells by hydraulically fracturing the reservoir rock, acidizing the formation, or other applicable techniques would not be expected to affect the WIPP facility.

These types of production stimulation are used primarily to increase the permeability of the rock that contains the hydrocarbons. Secondary recovery methods (techniques used to enhance or replace the natural driving force that "pushes" the oil to the production well) and tertiary methods (techniques used primarily to decrease the viscosity of heavy crude oils) may also be employed but, because the crude oil resources at the site are not reasonably or economically extractable, these techniques are not expected to be useful unless significant technological advances and adaptations are made.

State and federal regulatory agencies, including the New Mexico Energy and Minerals Department and the Minerals Management Service of the U.S. Department of Interior, are responsible for reviewing proposed mining and hydrocarbon exploration plans to prevent injury to adjacent leases or properties. The DOE will rely on this regulatory review process to protect the integrity of the WIPP site boundary from potash mining and hydrocarbon exploration on adjacent properties. The DOE will provide assistance to these agencies during the review process upon request. In addition, the BLM will notify the DOE of any requests for permits for resource recovery activities within one mile of the WIPP site boundary.

This policy will be modified if changes in institutional requirements occur or if significant new data relevant to the policy are obtained during development and operation of the WIPP facility.

References Cited

Brausch, L. M., Kuhn, A. K., Register, J. K., 1982, Natural Resources Study, Waste Isolation Pilot Plant (WIPP) Project, Southeastern New Mexico, TME 3156, Prepared for U.S. Department of Energy.

U.S. Department of Energy, 1980, Final Environmental Impact Statement, Waste Isolation Pilot Plant, DOE/EIS-0026, Assistant Secretary for Defense Programs, Washington, D.C.

Department of Energy
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Albuquerque, New Mexico 87115

RECEIVED

JAN 12 1983

ENVIRON.
EVALUATION GROUP

JAN 6 1983

Mr. Robert H. Neill
Director
State of New Mexico
Environmental Evaluation Group
P. O. Box 968
Santa Fe, NM 87503

Dear Mr. Neill:

DOE Responses to EEG Comments on DOE Revised Interim Policy Statement on Natural Resource Recovery at the WIPP Site (Letter, Neill/McGough, dated December 6, 1982)

This is a response to your review comments on our Revised Interim Policy Statement on Natural Resource Recovery at the WIPP Site. Enclosed is our response to your comments and a revised Interim Policy Statement which incorporates changes resulting from your comments.

Based on the conclusions of the Natural Resource Study and the DOE's desire to mitigate any adverse effect of the WIPP Project, we believe that this policy provides the maximum opportunity for resource recovery without any degradation of public health and safety or facility integrity.

Sincerely,

J M McGough

J. M. McGough
Project Manager
WIPP Project Office

WIPP:JMM 82-0899/6363A

2 Enclosures

cc w/o enclosures:
G. L. Hohmann, TSC, AL
C. C. Little, TSC, AL

cc w/enclosures:
C&C File, IEA, TSC, AL

General Comment No. 1

"The Statement implies that DOE plans to relinquish all control over Zone IV of the present WIPP Site, and that DOE plans to rely on the New Mexico Energy and Minerals Department and the Minerals Management Service of the U.S. Department of Interior for reviewing plans in this zone in the future. Because of the remote possibility that activities in this zone may have an adverse impact on the long-term stability of the repository horizon, it is recommended that DOE maintain, in a Memorandum of Understanding with DOI, a provision for DOI to notify DOE of any proposal for solution mining, solution storage cavities, and mining of Castile brine in Zone IV prior to a decision on that proposal, and that DOE in cooperation with the appropriate State agencies will review these plans and submit comments to DOI for their consideration."

Response:

The WIPP Site is now defined as the lands withdrawn by Public Land Order 6232, March 30, 1982, for the purpose of performing the Site and Preliminary Design Validation (SPDV) Program and protecting the integrity of the Site. The administrative land withdrawal application for full WIPP facility construction and the legislation land withdrawal, which is being requested by DOE, will encompass these same lands, i.e., 8960 acres of public lands and 1280 acres of State land which will be subject to the withdrawal if they pass to Federal ownership.

The lands withdrawn will be managed in accordance with a DOE/BLM Memorandum of Understanding which will, among other things, withdraw DOE objection to leasing, drilling and mining outside this withdrawal area; require BLM to notify DOE of any requests for resource recovery permits within one mile of the site boundary; and prohibit habitation within one mile of the site boundary. While the DOE does not plan to exercise any control over resource extraction activities outside the WIPP Site boundary, if required, the DOE will take any actions necessary based upon our review of resource recovery permits which may include State involvement if adverse impacts could result from the proposed resource recovery permits.

This position is consistent with the conclusions of the Natural Resources Study (Brausch, et al., 1982) which indicates that resource exploration and extraction activities more than one mile from the limits of the underground facility will not compromise the integrity of the facility.

General Comment No. 2

The Policy Statement applies only during facility construction and operation of the WIPP facility. The Policy Statement should be expanded to reflect the post-operation controls, since the integrity of the repository and the consequence analyses are dependent upon some controls being maintained.

Response:

The Policy Statement does apply only during WIPP construction and initial operation, a period of about 10 to 15 years. During that period, advances in drilling and mining technology and other pertinent information can be evaluated to determine whether more or less stringent resource recovery restrictions are required to protect the facility. Early (5-10 years) in the operating life of the facility, DOE will decide whether to retrieve all emplaced waste and return the land to BLM for releasing or to finalize the present interim policy. At this time, no revisions to the interim policy are planned if WIPP becomes a permanent disposal facility.

Specific Comment No. 1

The phrase "commercial drilling" should be defined so that it clearly excludes any drilling as well as mining not related to WIPP in Zones I, II, and III.

Response

The sentence has been changed to read: "No drilling excluding that conducted for the WIPP Project will be allowed from Control Zones I, II and II."

Specific Comment No. 2

As reflected in the preceding "General Comments," EEG recommends that the MOU between DOE and DOI request that DOE (and the appropriate State agencies) be notified of any proposal in Zone IV involving solution mining, construction of solution storage cavities or mining of brine from brine reservoirs in the Castile.

Response

As indicated in our response to your first general comment above, the DOE/BLM Memorandum of Understanding, under which the lands will be managed, will require the BLM to notify the DOE of any requests for permits for resource recovery activities within one mile of the site boundary. The following sentence has been added to the third paragraph on page 4: "In addition, the BLM will notify the DOE of any requests for permits for resource recovery activities within one mile of the WIPP Site boundary."

Specific Comment No. 3

As recommended in the "General Comments" we believe that the controls which are to be maintained after decommissioning should be addressed. This would necessitate deletion of the sentence at line 25.

Response

As indicated in our response to your second general comment, the 10 to 15 year duration of remaining construction and initial operation allows adequate time to review and assess changes in technology and develop detailed plans for controls following decommissioning.

Specific Comment No. 4

The phrase "measurable effects" is ambiguous and may lead to misunderstandings. It should be deleted or more precisely defined.

Response

A footnote has been added to clarify the phrase. The footnote reads as follows: "Measurable effects are those influences from extraction activities that could cause the assumptions made in the breach scenario consequence analyses (U.S. Department of Energy, 1980) to be unconservative."

Specific Comment No. 5

This paragraph does not adequately state the potential for resources at the site. It should recognize that technology and market conditions are not predictable and some materials, such as may be found in Castile brine, or Salado salt could become of economic value if technology and market conditions are substantially modified.

Response

The brief paragraph on resources present at the site is not intended to describe all possible resources which could become of economic value. It does indicate the two resources that are of current interest and whose recovery could potentially affect the underground facility. In the event that other resources at the WIPP Site become of economic value in the future, exploration and extraction technology for such resources will be evaluated and, if required, appropriate changes will be made in the Policy Statement.

Specific Comment No. 6

The D'Appolonia draft report "Natural Resource Study" indicates that "resource grade oil" is under the site.

Response

The hydrocarbon resource evaluations of the WIPP Site are based on known resources of natural gas and crude oil in the region and the probability of discovering new reservoirs. The fundamental assumption is, therefore, that the WIPP Site has the same potential for containing hydrocarbons as the much larger area for which exploration data are available. Although the New Mexico Bureau of Mines and Mineral Resources (NMBM & MR) study shows that minor deposits of crude oil are statistically probable at the WIPP Site, later studies have discounted the existence of economically attractive quantities of crude oil at the site.

H E A L T H & ENVIRONMENT department

"Equal Opportunity Employer"

STATE OF NEW MEXICO

ENVIRONMENTAL EVALUATION GROUP
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January 21, 1983

Mr. Joseph M. McGough
Project Manager on WIPP
WIPP Project Office
U.S. Department of Energy
P.O. Box 5400
Albuquerque, New Mexico 87115

Dear Mr. McGough:

This is in reply to your letter (WIPP:JMM 82-0899/6363A) and enclosures of January 6, 1983 concerning the DOE Revised Interim Policy Statement on Natural Resource Recovery at the WIPP Site. While we are generally satisfied with the revised Policy Statement, as provided in your recent letter, there remains one concern which needs to be resolved.

The Policy Statement and the DOE/BLM Memorandum of Understanding include the requirement that BLM notify DOE of any requests for resource recovery permits within one mile of the new site boundary. While the DOE does not plan to exercise control over resource extraction activities outside the new WIPP site boundary, we note that, "if required, the DOE will take actions necessary... which may include State involvement if adverse impacts could result from the resource recovery permits." So that the State may continue to make its own independent analysis of potential adverse impacts, we would like DOE's assurance that EEG will be notified of any request for resource recovery permits within one mile outside of the site boundary. And further, we would like to be advised of the planned Federal action on such requests prior to the initiation of the resource recovery.

The statements accompanying the Policy Statement implying no possibility of objection to solution mining of potash and secondary recovery for hydrocarbons outside the Control Zone III should be revised to indicate that any proposals for fluid injection underground within one mile of the WIPP site will be evaluated by DOE and EEG to determine if such proposals will adversely affect the integrity of the repository.

Sincerely,

Robert H. Neill

Robert H. Neill
Director

RHN:jdc

2-100AG2-19-1-2

cc: TSC, IEA

Department of Energy
Albuquerque Operations Office
P.O. Box 5400
Albuquerque, New Mexico 87115

RECEIVED

FEB 28 1983

ENVIRONMENTAL
EVALUATION GROUP

FEB 24 1983

Mr. Robert H. Neill
Director
State of New Mexico
Environmental Evaluation Group
P. O. Box 968
Santa Fe, NM 87503

Dear Mr. Neill:

As you know, the DOE Revised Interim Policy Statement on Resource Recovery at the WIPP Site is based on the Natural Resources Study which concludes that resource recovery outside the Site boundary (Zone III) using current technology, will not compromise the integrity of the WIPP underground facility. Accordingly, the DOE does not plan to exercise any control over resource recovery activities outside the Site boundary and will rely, primarily, on other Federal and State regulatory agencies to assure that the WIPP boundaries are not violated. As an additional protection measure, the BLM will notify the DOE of any requests for resource recovery permits within one mile of the WIPP Site boundary so that the DOE will be aware of resource recovery activities near the Site.

We do not expect to perform a comprehensive review of resource recovery plans utilizing conventional technology; however, any plans which employ unusual methods or advanced technology will be evaluated to determine possible effects on the underground facility. Upon receipt of notification of unusual or advanced technology planned resource recovery activities from the BLM, we will forward the information to the EEG.

Sincerely,

J. M. McGough
Project Manager
WIPP Project Office

WIPP:JMM 83-0171

cc:
G. L. Hohmann, TSC, AL
C. C. Little, TSC, AL
C&C File, IEA, TSC, AL
M. Wilson, OCC, AL

DEEP DISSOLUTION

SAND 82-0461

HEALTH ENVIRONMENT department

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August 13, 1982

Joseph M. McGough
WIPP Project Manager
U. S. Department of Energy
Albuquerque Operations Office
P. O. Box 5400
Albuquerque, NM 87115

Dear Mr. McGough:

Enclosed is a summary of our comments on the Draft "Interim Report: Dissolution of Evaporites in and Around the Delaware Basin, Southeastern New Mexico and West Texas," by Stephen J. Lambert.

We would appreciate your consideration of these comments in the preparation of the final report. As with the other formal reports required by the Stipulated Agreement, we would like to have our staff and yours meet to discuss the final version of the document before its publication.

Sincerely,

Jack M. Mobley
-for-

Robert H. Neill
Director

RHN:eg
2-047AG2-15-1-1
Enclosure

cc with attachment:
- George S. Goldstein, Ph.D., Secretary, Health & Environment Department
- Joe Hewitt, Secretary, Highway Department
- Charles Turpen, Secretary, Energy and Minerals
- Jeff Bingaman, District Attorney
- Russell F. Rhoades, Director, Environmental Improvement Division
- Joe Canepa, Attorney at Law
- James K. Otts, Chairman, Rad-Waste Consultation Committee
- D. T. Schueler, Assistant Manager for Project of Energy Programs
- Wendell Weart, Sandia Laboratories
- TSC, IEA

REVIEW COMMENTS

CONCERNING

Interim Report: Dissolution of Evaporites in and Around the Delaware Basin, Southeastern New Mexico and West Texas, by Stephen J. Lambert

Sandia National Laboratories

Printed February, 1982

Comments by

Environmental Evaluation Group

Environmental Improvement Division

Health and Environment Department

P. O. Box 968

Santa Fe, New Mexico 87504-0968

August 12, 1982

INTRODUCTION

These comments are based upon a critical reading of the report and many of the references cited in the report. A meeting with the author of the report, Steven Lambert, was held on July 21, 1982 to clarify the important points discussed in the report. Since a major part of the report deals with the ideas of salt dissolution at depth as postulated by Roger Anderson, he was also invited to attend this meeting. Some of the following comments resulted from this and subsequent discussions with Lambert and Anderson.

The report is a major effort to include in one document the various components of the theory, experimental and field work bearing on the partial removal of evaporite beds in the Delaware Basin. The work and scholarship displayed in the report is most commendable.

GENERAL COMMENTS

The purpose of the State's request for this document was to get DOE's most current thinking on the matter of the dissolution of evaporites in the Delaware Basin. The main area of concern in this matter is the possibility of removal of salt from the repository horizon in lower Salado by circulating waters, in the recent geologic past and the possibility of such dissolution being ongoing. Roger Anderson has published several papers during the past 10 years developing this hypothesis. This report was expected to provide DOE's views on the feasibility of such dissolution having taken place and the consequences and threat, if any, to the integrity of the proposed waste repository.

Although the report has presented data on the geologic, geomorphic, geohydrologic and geochemical aspects of the question of dissolution and removal of salt and has presented a review of various models of dissolution, it has not addressed the possibility of "deep dissolution" in sufficient depth. It is hoped that the document in its final form will either accept or reject the idea of active dissolution of salt beds in the lower Salado formation in the vicinity of WIPP site. Of course, detailed reinterpretation of the existing data and argumentation will have to be provided for a conclusion.

To help the author accomplish the objective of a more definitive conclusion of the deep dissolution controversy, the following is a discussion of the important aspects of this question, with reference to their treatment in the draft report.

Non-existence of Lower Salado Salt

Anderson has shown that a large amount of salt from the lower part of Salado formation is missing from the Delaware Basin and has attributed this to the removal by dissolution at depth in geologically recent times. A number of acoustic logs from oil and gas wells have been used by Anderson as hard data to advance his hypothesis. The report has acknowledged that the salt is missing from the Lower Salado, but does not acknowledge that it is missing due to dissolution. Other ideas advanced are non-deposition or erosion shortly after deposition (p. 98). No arguments are advanced, however, for statements such as, "...removal of some halite (if originally deposited) near the basin margin must have occurred during Castile time (not Pleistoene)" -p. 90.

The argument of non-deposition could easily be settled with a statistical study of regional thickness trends in acoustic logs. However, according to Anderson, (written communication) there is no reason to do this because lateral changes into depressions are dramatic and associated with collapse and therefore not the result of original deposition (or non-deposition). Also, according to Anderson, the argument of dissolution at some earlier than late Cenozoic time is not tenable because of the age of the fill in the collapse.

According to Lambert, "A complete review of raw geophysical log data, and possible interpretations of them, will be undertaken, together with an independent determination of thickness variations in the Castile and Salado formations" (p. 98). EEG looks forward to the results of such a review. In fact, this should have been completed before issuing the draft report.

An important point raised in the report with regard to the use of acoustic logs for dissolution studies is that "the sections of anhydrite which are postulated to be dissolution residue by virtue of no interlayered halite, show no log signature of chaotic dissolution residue, but are nearly pure anhydrite" (p. 97). Also such zones "bear the signatures of anhydrite, not gypsum as would be present in abundance if large quantities of fresh water were circulating in subsurface open space" (p. 98). It appears that the best way to resolve whether there is a distinct signature for the dissolution residue would be to run an acoustic log in one of the WIPP holes in Nash Draw (WIPP-25 to WIPP-30) where dissolution residue is known to occur and compare the signatures thus obtained with those used by Anderson where salt sections have changed to Anhydrite.

Other specific instances of "errors" (p. 91) in Anderson's data and interpretations are addressed under Specific Comments (for pages 88-100).

Connection of Evaporite Beds and the Basin Aquifer

In his deep-dissolution model, Anderson (1978, 80, 81) invoked the Delaware Mountain Group aquifer as a pathway for supply (i.e., source) of unsaturated water to the evaporite beds for salt dissolution as well as for removal (i.e., sink) of saturated brines. Contrary to the findings of Hiss (1975) and Anderson, the report rules out the DMG aquifer as a potential supplier of water for dissolution in the evaporite beds, for the following reasons.

a. "The entire Delaware Mountain Group is probably not a single, vertically interconnected hydrostratigraphic unit" (p. 37).
b. In the Bell Canyon formation, as encountered in AEC No. 8 "Static water levels were 615 and 560 feet below land surface as supported by the lower and upper sands, respectively. This conspicuous difference (in levels of water of similar density) attests to the strata-bound, vertically-isolated nature of water occurrences in the Bell Canyon formation." (p. 38)
c. "The deposition-controlled porosity containing natural gas in isolated lens-shaped sandstone reservoirs is also an indication of but small degrees of vertical and horizontal connected porosity in the Bell Canyon formation. Thus, in the upper 700 feet of the Bell Canyon formation, the total saturated thickness is less than 30 feet."
d. "Much of the Bell Canyon water is highly saline, but not completely saturated with sodium chloride under the evaporites" (p. 39).
e. "The salinity does not abruptly rise from west to east as evaporites appear in the overlying section" (p. 39).

f. "The water contains solutes in combinations not found in the evaporites" (p. 39). "Bell Canyon waters from AEC-7, AEC-8 and ERDA-10 have Na/Cl ratios of 0.50, 0.46 and 0.56 respectively" (p. 150). "Na/Cl (weight) ratio of brines formed by dissolution of salt in western Oklahoma is remarkably close to 0.64, regardless of whether the water is a low-salinity or a saturated brine" (p. 150). "Oil-field brines consistently have Na/Cl ratios of 0.55 or less, and the ratio decreases well below 0.50 as the salinity increases" (p. 150). "Thus we see that the Bell Canyon waters clearly have closer affinity with oil-field brines than with dissolution brines (and therefore) it has not been a sink for dissolution brines" (p. 150).

The reasons cited above can be disputed individually, but together they do present a formidable challenge to the idea of an active interconnection of the DMG aquifer with Castile and Salado beds.

In addition, the report also rules out any interconnection of the Bell Canyon aquifer with the Capitan aquifer on the basis of the Bell Canyon potentiometric heads being higher than the juxtaposed Capitan, "at all locations along the Basin margin....even after corrections are made for salinities" (p. 44). This, by itself, does not eliminate the possibility of the Bell Canyon water moving into the Capitan aquifer in the eastern part of the basin.

Replacement Limestone as Basis for Past DMG-Evaporite Bed Connection

Anderson (1980) has cited replacement limestone masses - "Castiles" and "limestone dikes" as evidence for past movement of water from the DMG aquifer to the evaporite beds. Konrad B. Krauskopf (EEG-7, p. 85) commented on these features as follows:

> "The fact that Bell Canyon water has invaded the overlying evaporite sequence at places other than the immediate vicinity of the Capitan reef seems clearly demonstrated by the replacement of Castile Anhydrite by biogenic limestone, locally accompanied by sulfur, in the western part of the Delaware Basin. The reduction of sulfate and its replacement by porous carbonate with a high carbon 12/13 ratio is clearly the work of bacteria, and the organic matter needed for bacteria to flourish could have come only from the Bell Canyon."

Lambert (in the report under review) suggests that "A more likely source for the water to supply the formational process for Castiles is in the nearby solution-subsidence troughs of the type described by Olive (1957), which are both near-surface and recently have contained water" (p. 74). There are two serious errors in this suggestion. Firstly, it does not take into account the brecciation seen in the Castiles, and more importantly, it is contrary to conclusions reached by all the serious investigators (Kirkland and Evans, 1976; Smith, 1978) of these features, without presenting any analysis or reasons.

Castile Formation Brine and Bell Canyon Aquifer

The report rules out the possibility of interconnection between the Bell Canyon waters and the brine encountered at several locations in the Castile Formation. "The (ERDA-6) water's unique stable isotope relationships isolate it from any active source in the Capitan or Bell Canyon. The solutes in the Castile water also make it incompatible with the Bell Canyon; a high sulfate (Castile) water and a high calcium water (Bell Canyon) cannot be in connection lest gypsum precipitate (p. 120)." The proposed deepening of ERDA-6 through the upper Bell Canyon formation should help in answering this question.

Continuity of Strata in Castile

The report (pp. 91-92) criticizes Anderson (1978) for implying that thickening and thinning of Halite Units in Castile are related to deep dissolution and, by comparing the Halite-I "sinks" of Anderson with the isopach map of Anhydrite-I (Snider, 1966) (Fig. VII-2), concludes that "several of these thinings actually represent non-deposition, due to a local elevation of substrate above base level" (p. 92). According to Anderson (personal communication, 1982), the mounds shown on Snider's (1966) map (Fig. VII-2 of Lambert's report) probably resulted from salt tectonics and are not due to non-deposition.

Clearly, there are possible errors in Anderson's estimate of the amount of salt removal through dissolution at depth, due to the apparent assumptions (p. 89) made by him in calculating the percentage of removal. On the other hand, non-deposition or pre-Cenozoic subaerial erosion does not satisfactorily explain the amount and pattern of salt missing.

Geomorphic Evidence against Deep Dissolution

The report cites (p. 82) Bachman's interpretation of the Gatuna stream deposits as evidence of the westward migration of the Pecos river; and also that this is "contrary to a monotonically-eastward progression of postulated evaporite dissolution, if the river is said to keep pace with the dissolution front". Possibly this argument is in reference to the possibility of eastward migration of the Pecos which was suggested in an early EEG report (EEG-2 pp. 16-17). However that possibility was based upon the subsidence which has occurred east of the Pecos, and such subsidence may continue to move eastward if dissolution is occurring at depth. In spite of the assertion made in the report that, "Scarp formation appears to be insensitive to depth of halite removal and appears to keep pace with halite removal" (p. 139), slow removal of salt at depths of approximately 2000 ft. below the surface may not result in the formation of a scarp prominent enough for the river to follow. Erosion by water or by wind would most likely "keep pace to obliterate scarps as they form" (p. 140), if they are formed by lateral sapping of salt at depth.

The Timing of Deep Dissolution

Chapter IX of the report presents the evidence for dissolution in Triassic, Jurassic and Tertiary times. Anderson (1981) presented detailed arguments against Bachman's (1980) interpretation of the Cretaceous outcrops resting on the Castile formation as signifying a profound episode of dissolution down into the Castile during the Jurassic period. Anderson (1981) has referred to several published instances of Cretaceous rocks resting ("implaced by collapse") on rocks of various other ages. The main evidence cited by Anderson (1981) in favor of the more recent age of much of the deep dissolution is the age of the fill in Maley and Huffington's (1953) depressions; the depressions and the fill are clearly post-tilting and the Salado salt is absent in the center of these depressions.

The report acknowledges that, "San Simon Sink, at least, is a collapse feature" (p. 75) but has not commented on Anderson's postulation that the collapse has resulted from the dissolution of lower Salado Salt by waters derived from the Capitan reef aquifer (Anderson, 1981). The question has been left open with remarks such as, "Subsidence in San Simon Sink might (also) be attributable to collapse into a phreatic cavity in the Capitan" (p. 148), "It is likely that these depressions (as well as such features as San Simon Swale) are in part erosional features" (p. 152-153), "The Cenozoic-filled depressions could simply be pre-Gatuna deeply-developed equivalents of Nash Draw" (p. 154) and "The ultimate nature of San Simon Sink, however, remains unresolved." (p. 76).

Stratabound Dissolution

A model of deep-dissolution called "Stratabound Dissolution" has been presented in the report as an alternate hypothesis to that of Anderson's. The essential features of this model are outlined on pages 153-154 and in Figure X-2.

According to the report, "The fatal weakness of the Anderson (1981) model of dissolution is the identification of the Bell Canyon formation as a sink" (p. 150). On the other hand, "One limiting factor in the stratabound dissolution model is as yet the inability to identify an efficient sink for the disposal of saturated brine of dissolution origin" (p. 161). One might say that this is a fatal weakness of thé stratabound model also. The other weakness of this model is its ambiguity with respect to the geologic horizons involved and the time of occurrence.

SPECIFIC COMMENTS

Page 24, paragraph 1 - Jones et al. (1960) refers to drill hole logging in potash deposits which are in the Salado formation. This comment is therefore applicable to the Salado marker beds, which at any rate, "are traceable in the subsurface over horizontal distances of several kilometers to tens of kilometers" (p. 26, 4-6 lines). Anderson et al. (1978) have shown that Castile units can be correlated in wells as much as 113 km. apart.

Page 44 (last 2 lines) - The water from Bell Canyon could flow into Capitan?

Page 83 (First 2 lines) - But the line of cross-section in Figure x-1 does not pass through San Simon Swale!

Page 90 (Bottom) - "... removal of some halite (if originally deposited) near the basin margin must have occurred during Castile time (not Pleistocene)." What is the basis for this statement?

Page 100 - With regard to the quotation from Bachman (1980), Anderson points out that the correct interpretation is that of Castile-Salado unconformity and not "pan" deposition. At any rate, it does not address dissolution and collapse in lower Salado. With respect to the quotation from Jones (1972), Anderson points out that there are lateral variations in Salado, but not enough to explain the correlatable salt zones and missing salt as seen in logs.

Page 103, Fig. VII - 1-B - According to Anderson (personal communication), his interpretation of these logs was wrong. Cowden Anhydrite should be at the top of UNM Phillips No. 1 and higher in all other logs. The discussion of this figure on p. 97 is therefore irrelevant.

Page 144 - There should be detailed discussion of Maley and Huffington's depressions and Pleistocene dissolution and fill exhibited by them in this section.

Page 160 (3rd para) - Anderson's (1980) Brine Density Flow works laterally as well as vertically, driven by density gradient.

Page 161 (Mid Para) - Maley and Huffington's Cenozoic filled depressions provides the evidence that much of the salt removal at depth occurred in late Cenozoic, according to Anderson (1981).

SUMMARY AND RECOMMENDATIONS

Looking at Figure X-2 and the discussion of the Stratabound Dissolution concept, one gets the impression that the author of this report accepts the central basis for the idea of deep dissolution i.e., the preferential removal of salt at depth in the lower Salado beneath the Maley and Huffington depressions. The stratabound explanation is a fairly strong commitment to the idea of removal of salt by dissolution. The admission to the lack of a "sink" for the brine makes the concept difficult to accept. Not completing a thorough review of the geophysical logs and cores from wells to examine the causes for the absence of salt in the lower Salado horizon is a serious omission on the part of DOE which should be corrected as soon as possible.

It appears that one could skirt the issue of deep dissolution in the Delaware Basin altogether Vis-a-Vis the safety and integrity of the WIPP site, if an area of stable platform into which dissolution at depth has not made a dent, could be established around the WIPP site. This would, of course, require a fairly accurate estimate of the rate of advance of the "dissolution front", which could then be used to estimate a minimum period of safety. However, such an effort cannot be undertaken unless the mechanism for salt removal at depth is fairly clearly understood. The lower Salado salt is seen to be missing on accoustic log of Perry Federal #1 well, which is less than 2 miles east of the Zone IV boundary of WIPP. And of course, the encounter of brine at WIPP-12 has opened the entire question of the relationship of brine in Castile and the mechanism of salt removal in lower Salado formation. These questions must be answered in the best possible manner if confidence is to be placed on the geological integrity of the WIPP site.

EDITORIAL COMMENTS

1. Page 3, line 4 - delete "as."
2. Page 5, line 7 - "dendritic" is misspelled.
3. Page 9, equation in middle of page - The formula for polyhalite is incorrect. It should be

   $$Ca_2K_2Mg\ (So_4)_4.H_2O$$

4. Page 22, line 2 - The word "with" or "to" should be inserted between "proximity" and "the Ochoan."
5. Page 36, line 5 - The word "water" should be "salt" or "evaporites."
6. Page 49, 2nd paragraph, line 1 - The word "Rustler" is misspelled.
7. Page 72, line 1 - The word "completion" should be "completeness."
8. Page 94 - The first sentence on this page needs rewording....something like, "Anderson (1978) did not entertain the possibility that infra-Cowden thinning over the reef was due to reef-controlled non-deposition rather than reef related dissolution from below."
9. Page 163, 3rd line - "Origin" should be "original".
10. Page 164, para 4, line 4 - "features" is misspelled.

Department of Energy
Albuquerque Operations Office
P.O. Box 5400
Albuquerque, New Mexico 87115

RECEIVED
NOV 8 1982
ENVIRONMENTAL
EVALUATION GROUP

NOV 0 5 1982

Mr. Robert H. Neill
Director
State of New Mexico
Environmental Evaluation Group
P. O. Box 968
Santa Fe, NM 87503

Dear Mr. Neill:

Reply to Review Comments by EEG on Deep Dissolution Report (Neill to McGough, August 13, 1982)

Your comments on the draft Deep Dissolution Report have been taken into consideration, and the enclosed response has been prepared by the authoring organization.

It is necessary to keep in mind that it was the intent of this work to show that:

(1) there is no direct evidence of present or past preferential removal of lower Salado halite;

(2) previous (Anderson's) hypotheses <u>have</u> been tested and found wanting;

(3) a potentially efficient mechanism for stratabound dissolution (more efficient than "brine density flow" involving Bell Canyon, Capitan or Castile) <u>had</u> been identified;

(4) there is little evidence for stratabound dissolution anywhere save in the Rustler;

(5) if an efficient sink for brine disposal cannot be identified, there is <u>no</u> active dissolution, regardless of the postulated mechanism, and;

(6) if greater confidence in these conclusions is required, specific tests of the stratabound hypothesis could be made, for it <u>is</u> a testable hypothesis.

If after reviewing these responses you feel that further discussion of your comments is warranted, please contact my office to arrange for a meeting with the authors prior to printing of the final report.

Sincerely,

J. M. McGough
J. M. McGough
Project Manager
WIPP Project Office

WIPP:JMM 82-0759/6217

Enclosure

cc w/o enclosure:
R. K. Brown, TSC
G. L. Hohmann, TSC
C. C. Little, TSC
W. Weart, Sandia
C&C File, TC

ATTACHMENT TO MCGOUGH LETTER
NUMBER 0759/6217 DATED 11/5/82

Non-existence of Lower Salado Salt

Anderson's "hard data" for absence of salt are really his interpretations of acoustic logs, not the logs themselves. We disagree in numerous instances with his stratigraphic picks. A rereading of p. 90 will reveal that the page is replete with arguments for the statement about some halite removal (if any) being Permian and not Pleistocene.

Use of the word "missing" forces specific conclusions not appropriate with a sense of healthy skepticism toward any hypothesis. Non-deposition, for example, means "not missing." The whole discussion on p. 92 says the thickness of anhydrite near the basin margin is strong evidence that no halite was deposited, or if originally deposited, "removal . . . must have occurred during Castile time."

Despite your assurance from Prof. Anderson that thickness trends are not worth reanalyzing, we shall continue our re-interpretation as we have proposed and you have urged. As to the age of fill, Maley and Huffington called it Cenozoic. There is no evidence that we are aware of that assures us that broad definition (Cenozoic) has any precision greater than 65 my to present. No faunal or radiometric data are known on which to base any maximum age. Gatuna age deposits are known as part of the fill, but that does not demonstrate anything other than a minimum age. The age of the fill in the collapse areas is not well documented as all Pleistocene; therefore, Anderson's speculation of constraint of all dissolution to the Pleistocene on this basis alone is not tenable.

SNL proposed log correlations early in this process, but the preparation of the dissolution and deformation reports is what convinced us of the necessity. Activity was deferred until the specific goals of such a reinterpretation could be defined, and additional resources could be allocated, since the activity will serve many endeavors (structure, deformation, dissolution, etc.).

The acoustic logs have been run in Nash Draw holes (WIPP 25 through 30) and compared with Anderson's logs. That comparison was the basis for saying that the Anderson logs showed no signature of either gypsum or dissolution residue. Specific examples will be included to demonstrate the point.

Connection of Evaporite Beds and the Basin Aquifer

The potentiometric (static) differences between Bell Canyon and Capitan do not eliminate the possibility of interconnection. A re-reading of p. 45 will uncover a further, more powerful, argument against interconnection in the immense difference in osmotic potentials. The report says "there is no tendency" for water movement across the Capitan/Bell Canyon interface, and also explains the basis for this statement. Page 45 explains the high osmotic gradient between Capitan and Bell Canyon waters, which must be taken into account as well as the static potentiometric gradient in predicting degree of water movement. The discussion will be augmented to clarify that in view of these opposing conditions, flow in either direction is considered unlikely.

Replacement Limestone as Basis for Past DMG-Evaporite Bed Connection

It was stated that the hypothesis of Kirkland and Evans was not the primary point of review; perhaps a more orderly discussion in the text is necessary so that the point about castiles not necessarily indicating deep dissolution will be considered "serious."

There are several serious flaws in the Kirkland and Evans suggestions and the Anderson speculation; chief among these is the innate (thermodynamic) incompatibility between (oxygenated) meteoric water (postulated to have moved freely in the Bell Canyon) and highly reduced hydrocarbons in the Bell Canyon.

By "errors," it is assumed that you mean the discussion is incomplete. It isn't what might normally be considered an "error" to suggest explanations, no matter how "serious" the investigation. We fail to comprehend how brecciation in the castiles is inconsistent with a deeper development of Olive's solution - subsidence troughs in the Gypsum Plain.

Castile Formation Brine and Bell Canyon Aquifer

There remains no "question" in our minds of Castile/Bell Canyon isolation in view of fundamental thermodynamics. Deepening ERDA-6 just to "answer" the "question" (which we consider non-existent) is not expected to help much. We feel that this comment recommends no useful change to the original text.

Continuity of Strata in Castile

It is Lambert's opinion expressed in the report that thinned halite overlying thickened anhydrite represents a syndepositional phenomenon. Apparently, Anderson considers it deformation. It is difficult to imagine how salt tectonics can give rise to anhydrite mounds, long after sedimentary consolidation. Anhydrite does not flow. The deformation report (Barrows et al., in preparation) considers that compensating variations in thicknesses of Castile anhydrite and halite beds are due to deformation (rather than late Cenozoic dissolution), despite cursory geological reasoning for such a situation.

It can be agreed that non-deposition or pre-Cenozoic subaerial erosion does not explain all things concerning the amount and pattern of salt "missing." The professional opinion is expressed that these are very important factors which can explain the basic amount and pattern. Certainly a very strong case is made that a pronouncement of "deep dissolution" through the last few million years has very serious shortcomings whether the Anderson or Lambert models are applied. We are unable to make specific revisions based on the comment.

Geomophic Evidence Against Deep Dissolution (sic)

The results of serious investigations of dissolution in the Permian Basin of west (and northern) Texas shows the presence of scarps and dissolution as a front (Gustavson et al., 1980). If erosion destroys such a scarp or prevents it from forming, the dissolution rate is reasonably slow. If scarp formation due to "deep dissolution" is so slow that erosion obliterates scarps, (and the river keeps pace), the dissolution is so slow as to be inconsequential on the time scale appropriate to WIPP. Simultaneous blanket dissolution to prevent scarp formation is a difficult process to envision as "fresh" water has to be distributed over large areas before dissolving salt. There is no revision apparent for responding to the comment.

The Timing of Deep Dissolution

Rereading of p. 142 will reveal that Bachman's Cretaceous occurrences were not used as timers, and the report considers some of Anderson's objections to them valid. The absence of Jurassic rocks (Bachman, 1980) still stands as evidence of Jurassic or post-Jurassic exposure, since no Jurassic has been found associated with the Cretaceous collapsed rock.

The comment has been made (p. 141) that not all of these times when the area was above (and below) sea level are separable. However, there is consistent evidence for dissolution during earlier times, no matter how the Cretaceous rocks are interpreted. We have to disagree over the precision of the age of fill in the troughs -- see comment above, same subject.

No comment was intended to be made on Anderson's postulation of lower Salado dissolution by Capitan water under San Simon Sink. Overnight collapse occurs, however, only in brittle rock, and that is strong evidence for collapse into a phreatic cavity in the Capitan. A large natural open cavity in halite at 2000 ft depth has not been found: such cavities are not preserved so as to collapse catastrophically. See p. 147 (quote from Brokaw, et al, 1972). It was intended that the question of San Simon Sink be left open. The origins of Capitan-associated features (such as San Simon Sink, Carlsbad Caverns, and "breccia pipes") were not intended to be ultimately resolved by this report, unless they are shown by consequence analysis to be worthy of consideration as direct threats to the integrity of the WIPP horizon. This will be made explicit in the revision.

Stratabound Dissolution

The identification of the Bell Canyon Formation as a sink in Anderson's (1981) model is a fatal flaw. The lack of a demonstrated sink for stratabound dissolution is a problem, but not fatal in the same sense. This lack of a good candidate is a major reason why all schemes for dissolution at depth that are postulated to be very active now must be considered suspect. The parenthetical remark on p. 152 will be made more explicit in this regard.

Discussion of the stratabound model will be clarified to indicate that the only evidence of its occurrence is in the lower Rustler/upper Salado of Nash Draw.

Specific Comments

p. 24, para. 1: It is recognized that C. L. Jones can correlate the Salado marker beds as well and as far as R. Y. Anderson can correlate the Castile anhydrites.

p. 44 (last 2 lines): Yes. No tendency for Capitan water to flow into the Bell Canyon. See p. 45, middle. No tendency is indicated for flow in the other direction, either.

p. 83 (first 2 lines): Indeed the cross-section does not pass through San Simone Swale. A transition phrase will be added to make the reasoning smoother. The discussion concerns a general category of depressions to which San Simon Swale may belong. It is not necessary that Figure X-1 show San Simone Swale, since the relationship is clear along the line of Section A-A.

p. 90 (bottom): The basis for the statement is that the total thickness of Castile halite/anhydrite paired units remains the same. The only time that anhydrite could have formed "mounds" over which halite was thinned was the Permian, not the Pleistocene. Words will be added to that effect.

p. 100: The quotation is accurate and it does concern the evolution of evaporites within the basin. An unconformity at the Castile/Salado boundary would be an acceptable way of having Castile halite beds in the western part of the basin disappear, but we doubt this is the inference Anderson means to have drawn. The known Salado variations are evidence that beds may have disappeared during Salado times. No claim is made that it accounts for all "missing" halite.

Anderson has totally ignored the greatly thickened anhydrite sections that occur in the same boreholes with the thinned halite sections. This neglect makes his interpretations suspect. Thus, variations in the Salado can be explained by processes other than Pleistocene dissolution. This statement will be made explicit.

p. 103: The discussion is not irrelevant just because Anderson found an error in his interpretation. The interval nomenclature will need to be annotated or clarified. Final revision will probably await reinterpretation of the log data. This is an example of why the interpretation should be redone.

p. 144: It is not the intent of this work to reproduce all the source works within it. It will be clarified that the "Cenozoic" age of all the fill is not well established, and therefore cannot be used as a strong argument in favor of dominantly Pleistocene dissolution.

The basis of ages (e.g., lack thereof) for fill in the depressions will be more explicitly stated. The chapters on recommendations will be more explicit concerning some followup investigations of information on the fill.

p. 160 (3rd para.): There is geological evidence that Brine Density Flow does not work at all in this geologic setting. There is no salinity stratification observed in any subevaporite aquifer in the basin (excluding the Capitan). See p. 161, 1st para.

p. 161 (mid-para.): There is no evidence that all the Maley and Huffington fill is "late Cenozoic." Anderson's speculation is unwarranted solely on this basis. Again, it will be re-emphasized that the age of the fill is largely unknown.

SUMMARY AND RECOMMENDATIONS

The comments do not recognize that the Figure X-2 is diagrammatic and hypothetical. Looking at the text, we find it difficult to get the impression that the report accepts "deep dissolution." In addition, to our knowledge this report is the first instance of the detailed examination of the immense difficulty of brine disposal in any proposed dissolution mechanism, given the geohydrologic conditions of the Delaware Basin. If the concept of dissolution is made difficult to accept for want of a sink, there can be no active dissolution.

Stratabound dissolution is a hypothesis to explain how dissolution is occurring (Nash Draw); the lack of sink makes any hypothesis regarding active dissolution at great depths suspect. See similar earlier comment.

The review of geophysical logs had been previously done by Anderson under contract. The drafting of dissolution and deformation reports demonstrated the necessity of re-examining and updating the interpretation.

The problems with an operating, active method of "deep dissolution" will have to be more explicitly summarized. No estimate of rate of advance is germane without an operating mechanism. A revision to express the threat from stratabound dissolution (vis a vis Nash Draw) will be included.

There is no relationship apparent between brine reservoirs and salt removal.

The example of Perry Federal #1-31 cannot be used to illustrate preferential (active?) dissolution in the lower Salado. Note the greatly thickened section of Castile Anydrite III underlying the "thinned" halite between "Cowden"(?) and MB136. The total thickness of this section does not differ markedly from regional thickness, thus supporting depositional thinning of halite over an anhydrite "mound."

At the risk of repetition, we say that it has been scientifically established that WIPP-12 brine is not active dissolution brine. We do not consider the "relationship of brine in Castile and the mechanism of salt removal in the lower Salado Formation" an "open question." Such an issue has been addressed in detail on pages 114 through 121.

HEALTH ENVIRONMENT department

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January 3, 1983

Mr. Joseph M. McGough
Project Manager on WIPP
WIPP Project Office
U. S. Department of Energy
P. O. Box 5400
Albuquerque, NM 87115

Dear Mr. McGough:

RE: Deep Dissolution Report (SAND 82-0461)

Your reply to EEG's comments on the subject report (McGough to Neill, 11/5/82) and the attachment to your letter, indicate that the author of this report has accepted a number of EEG suggestions for improvement of the report. We understand that the final version of the report will include the following major changes.

a. A reinterpretation of geophysical logs of Salado and Castile strata in the northern part of the Delaware Basin will be completed and the results clearly presented to prove your assertion that the absence of halite in Salado is due to non-deposition or erosion shortly after deposition.

b. Specific examples of accoustic logs from WIPP 25 through 30 will be used to show that the zones of missing salt in Salado, as interpreted from accoustic logs of deeper boreholes do not show signature of either gypsum or dissolution residue.

c. Clear examples and explanations will be provided to justify the contention that the total thickness of Castile halite/anhydrite paired units remain essentially the same in the basin. Figure V11-2 (p. 105) and the discussion on page 92 show some examples of errors in Anderson's argument but do not bring out the point being made convincingly.

There are other points in your reply to our comments which could be argued from an academic point of view. However, the changes listed above will satisfy our major concerns in this area.

Sincerely,

Robert H. Neill
Director

RHN:eg

2-096-AG2-15-1-1

cc: TSC, IEA

DMG HYDROLOGY

TME-3166

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HEALTH & ENVIRONMENT department

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August 25, 1982

Mr. Joseph McGough
Project Manager of WIPP
U.S. Department of Energy
Albuquerque Operations Office
P.O. Box 5400
Albuquerque, NM 87115

Dear Mr. McGough:

Enclosed are our review comments regarding the draft of "Delaware Mountain Group (DMG) Hydrology - Salt Removal Potential NM78-648-813B. April 1982" by D'Appolonia Consulting Engineers. We shall be looking forward to hearing your response.

Sincerely,

Robert H. Neill

Robert H. Neill
Director
2-050AG2-18-3
RHN:du:lgr

cc: TSC, IEA

Comments on Draft DMG Hydrology Report

INTRODUCTION

These comments are based upon a critical reading of the report and many of the references cited in the report. The calculations, assumptions and data in the report were checked for accuracy and reasonableness. Most of the calculations are correct - a few specific errors or discrepancies are noted under Specific Comments. Construction of some of the graphs presented in Chapter 5 is unclear - these are also discussed under Specific Comments. The section on General Comments contain our suggestions for improvement of the report on a thematic basis.

EXPECTATIONS FROM THE REPORT

The "Cost and Merits Evaluation for Stipulated Agreement Activities" attached to the 8.31.81 letter from Schueler to Goldstein contained details of the proposed work and expected results. In our judgement, the following two items have not been included in the draft report.

a. "Possible communication (of DMG) with other aquifers e.g. reef aquifer, San Andres limestone aquifer and shallow aquifers" has not been treated in this report. Although the details on this theme are expected in the regional hydrology report, a brief treatment of this subject will be desirable in this report for completeness. We recommend adding a subsection under section 2.2 of the report for such discussion.

b. The "Expected Results" section of the Costs and Merits document for DMG Hydrology states, "Additionally, the relative merits of various dissolution theories will be discussed." We understand that the primary document for such a discussion is the Deep Dissolution report, but the DMG Hydrology report should at least discuss the possibility of proposed mechanisms, other than Anderson's deep dissolution, explaining the observed features of DMG and Reef hydrology and hydrogeochemistry. A discussion of Bachman's "Solution and fill," subaerial erosion during Jurassic time and Lambert's "Stratabound Dissolution" in this context would be very appropriate and useful.

GENERAL COMMENTS

Acceptance of Deep Dissolution

On the basis of geological and geochemical evidence and observed rates of mass transport, the report has accepted the possibility of deep-seated dissolution at the margins of the Capitan reef and above it. The following quotations from the report indicate such acceptance.

> "Based on observed mass transport rates in the Capitan Reef and existing deep-seated dissolution features (breccia pipes), convective mechanisms such as brine density flow may be occurring at the reef margin." (p. 4-16, para 3)
>
> "...there is evidence to suggest that active convective dissolution of the overlying Salado Formation together with the diffusion from halite zones can result in the observed mass transport rate in the Capitan Reef aquifer". (p. 4-9, para 2)
>
> "....dissolution at the Capitan Reef margin in the Castile and Salado may be associated with the convective mechanism and is consistent with observed deep dissolution features which suggest a more vigorous dissolution process than diffusion." (p. 5-4, 3rd para)

In addition, the report has accepted the possibility of some deep-seated dissolution throughout the basin.

"Additionally, the presence of saline waters to the Bell Canyon and Capitan Reef aquifers which underlie or are adjacent halite units suggests that some form of deep-seated dissolution may be present throughout the basin." (p. 3-3, 4th para).

Omission of Salado Salt Removal

Having accepted the possibility of the mechanism of salt removal through convective flow at depth, the report has not addressed a major contention of Anderson (1981), i.e., the removal of salt from the Salado formation. All the analytical models considered in the report deal with the "salt dissolution in the Castile Formation and mass transfer to the Bell Canyon Aquifer" (p. 6-1, para 2).

The reasons for this omission are not clear, since according to the report, "it seems likely that the groundwaters have dissolved some salt from the Salado and Castile formations" (p. 3-2, 2nd line). In fact, task no. 3 (p. 1-4) for the study is to, "Assess the potential for dissolution in the Castile and Salado formations." It is hoped that the final version of this report will correct this omission.

DMG Aquifer Parameters

The conclusion that the rate of dissolution of salt is so slow that, "this would have an insignificant effect on the integrity of the facility" (p. 1-7) is based upon the salt transporting capacity of the DMG aquifer. The report accepts that, "Removal rates from the Castile Formation based on convective transport mechanisms are estimated to be significantly greater than the salt transport capacity of the Bell Canyon aquifer" (p 4-11, line 3 to 6). Therefore, it becomes critically important to examine the raw data which has been used to estimate the salt transporting capacity of the DMG aquifer.

The input parameters for numerical computations are listed in Table 4-1 of the report. These parameters are identified from the hydrogeological characteristics of the Bell Canyon formation listed in Table 2-1. The data for Bell Canyon hydrogeology are taken from Hiss (1975a). The only other source of data is Lynes Inc. (1979) which reports on a drill-stem test in AEC-7 drillhole.

The most important input parameter is the hydraulic conductivity of Bell Canyon aquifer. Hiss (1975a) compiled laboratory determinations of permeability and porosity, made by oil companies on cores collected from the lower Bell Canyon and Upper Cherry Canyon formations. The cores were collected from sections most promising for hydrocarbon production. On the basis of these compiled values, Hiss (1975a) computed an "average" permeability for the DMG formation as 6.70 millidarcies which is equal to about 0.005 m/day of hydraulic conductivity. The hydraulic conductivity of 1.8 m/yr. listed in Table 4-1 is simply this value computed by Hiss (1975) from reported laboratory values (0.005 m/day x 365 = 1.8 m/yr.). Similarly, the effective porosity value of 0.16 is also the "average" porosity value reported by Hiss (1975a) from his compilation of oil companies data. Hiss (1975a) did not report any aquifer performance tests for Bell Canyon. In fact, the only aquifer data are that collected from a drill stem test in AEC-7 (Lynes, Inc. 1979).

While the report treats several mechanisms for flow in the Castile formation, it only treats porous media flow in the DMG aquifer. The hydrologic data available for porous media flow in the DMG indicates that the DMG is not capable of transmitting significant amounts of salt. This in turn keeps dissolution of salt to a minimum even when considering the "implausible worst case scenarios" described in the report (Sec. 5.2).

In view of a possible fault connecting Bell Canyon with Castile formation (U.S. DOE, 1980b, Figs. 2.7-20 and 2.7-21) and the existence of a joint system (p. 2-7, para. 2), it is surprising that no consideration is given to salt transport through fractures which may exist in DMG aquifer. It is recommended that the final version of the report includes calculations based on assumptions of joints in DMG and at least one fault connecting DMG and Castile formations.

Capitan Aquifer as a Deep Dissolution Sink

The report states that the, "Geochemical evidence of salt dissolution is provided by the composition of groundwater from the Bell Canyon and Capitan aquifers" (p. 3-1, 3rd para) and that "it seems likely that the groundwaters have dissolved some salt from the Salado and Castile formations" (p. 3-2, first para). While the report rules out DMG as a carrier of the dissolved salt, it suggests that the Capitan aquifer may directly participate in the salt dissolution at depth. The Chapter on "Conclusions" (Chapter 6) states,

> "As is evident from this study, brine density flow or convective dissolution is a potential mechanism for removal of halite and its occurrence in the Delaware Basin is possible in areas overlying and at the Capitan Reef aquifer margin." (p. 6-3)

This is an important statement and raises several questions concerning the mechanism of salt removal without DMG aquifer participation, directly to the Capitan aquifer. The existence of decreasing chloride concentration down gradient in the Capitan Reef aquifer is one example of the problems to be resolved and understood. The report, in its final form, should try to present a mechanism of salt removal from Castile and Salado into the Capitan Reef aquifer without involving the DMG aquifer.

Brine in Castile

The report has disregarded the importance of pressurized brine in the Castile formation vis-a-vis the question of salt removal from Castile and Salado (p. 2-12). Even though these brine occurrences do not seem to be connected to the DMG aquifer, they may not be completely isolated. Also, the brine is found in large volumes. The most recent estimate of the volume of the brine reservoir encountered by WIPP-12 is 30 million barrels (Popielak, NAS-WIPP panel presentation, Aug. 1982) which would occupy 170 million cu. ft of space. The significance of such large volumes of pressurized brine to the question of removal of salt through dissolution in the same formation should be discussed more thoroughly in the report.

Use of Mathematical Models and Equations

The report has quantified the rates of dissolution for mechanisms of diffusion and density induced convection by using the equations for density induced flow effects, Rayleigh numbers and Nusselt numbers; use of a steady state analytical mass balance model; and the use of a numerical mass transport model.

Many of these approaches and assumptions appear to have possibilities of errors. Thus, for example it seems possible that brine density flow could occur in fractures much smaller than the 0.5 and 1.5 mm calculated in the report; the value of Rs used for the equation $N_s \approx 0.1\ R_s^{1/3}$ may not exceed 5×10^{10} whereas the report uses $R_s = 1.2 \times 10^{21}$; and the comparison of the dispersion coefficients calculated from the Knapp and Podio (1979) experiments to the diffusion coefficient could be erroneous. All these points are discussed in detail under Specific Comments.

SPECIFIC COMMENTS

page 1-3, 8th and 9th line from top and page 2-7, 2nd paragraph:
The WIPP Safety Analysis Report (page 2.7-33, Figures 2.7-20 and 2.7-21) indicates that a northwest-southeast trending fault may exist on the interface between the Delaware Mountain Group and the Castile Formation. The fault is located approximately 9 km northeast of ERDA-9 and would be within 1 km of the repository as presently planned. The potential existence of a fault and its consequent hydrologic effects on the repository should be addressed in this report.

page 1-4, 1st paragraph:
The statement, "When placed in salt beds which have remained generally stable since deposition in the Permian time (more than 230 million years ago), the waste buried in the WIPP facility may reasonably be expected to remain isolated from the biosphere for thousands of years" ignores everything that has happened to the salt beds since their deposition, viz. uplift, tilting, folding, salt tectonics, intrusion by a dike, collapse along breccia chimneys, dissolution, formation of cavities filled with huge reservoirs of brine, erosion, etc. It is clearly misleading, detracts from a satisfactory resolution of the question of future stability and isolation of the WIPP repository, and should therefore be removed from this report.

Similarly the sentence following the above mentioned one states that radioactive decay will reduce the hazard to "negligible levels" in a few thousand years. The Pu-239 inventory will be essentially the same as at closure and the statement as incorrect.

page 1-7 3rd, and 4th bulletted conclusions:
The terms "insignificant", "no significance," and "not greatly increase" are qualitative. They should be either replaced by or appear with the respective quantifiable number from Chapter 5 or Appendix B.

page 2-7, section on "Fracturing":
In which formations are the two sets of joints located?

Does the joint set extend into or is it located within the transmitting sandstones of the DMG? These joints may be capable of transmitting water and thus have a high dissolution potential. The dissolution effects of convection along a joint or fracture in the DMG should be addressed in the report.

page 2-8, 19th through 29th line from top:
The values of permeability presented here appear to be taken from Table 6 of Hiss's (1975a) report and are average permeabilities on a county by county basis. Figure 21 of Hiss's (1975a) report indicates that permeabilities near the WIPP site range from less than 1 md to 59 md (<0.3 m/year to 18 m/year for pure water at 20°C).

page 2-11, last paragraph
The report should indicate that the hydraulic conductivity of the Castile anhydrites is not limited to porous flow. At WIPP-12 a fracture in Anhydrite III-IV of the Castile is capable of producing over 300 gallons per minute of brine (D'Appolonia Consulting Engineers, Inc., "Data File Report, ERDA-6 and WIPP-12 Testing," Volume IV A, Activity WIPP-12.2, Feb., 1982). At ERDA-6 a fractured zone in Anhydrite II of the Castile is capable of producing over 20 gallons per minute of brine (D'Appolonia Consulting Engineers, Inc., "Data File Report, ERDA-6 and WIPP-12 Testing, Volume II A, Activity ERDA-6.7, Feb., 1982) Preliminary calculations by EEG staff members indicate hydraulic conductivities of 2000 m/day for the fractured zone at WIPP-12 and 5 m/day for the fractured zone at ERDA-6. These values of hydraulic conductivity are at least six orders of magnitude greater than the values presented here.

page 2-12, 2nd paragraph:
Does the source of salt in the Castile brines come from dissolution of halite overlying or underlying the anhydrite layers?

page 2-12, 2nd paragraph:
The statement is made that Castile brine "pockets exhibit different (mostly higher) potentiometric surfaces than the Bell Canyon". It appears that all the potentiometric surfaces for the brine pockets are higher than those for the Bell Canyon.

page 2-13, 2nd paragraph:
Are the contours on Figure 2-7 "averaged" over the various water bearing units of the Rustler Formation or are they the contours for the Culebra only? They look like they represent water levels in the Culebra. A recent draft report (Gonzalez, D.D., "Fracture Flow in the rustler Formation: Waste Isolation Pilot Plant (WIPP) Southeast New Mexico (Draft Interim Report)," SAND 82-1012, May, 1982) has changed the Culebra contours from those presented in previous reference works (Mercer, J. W., and B. W. Orr, 1977; Mercer, J. W. and B.R. Orr, 1979; Mercer, J. W. and D. D. Gonzalez, 1981). Figure 1 through Figure 4 indicate how conceptions of the head in the Rustler Formation and the Culebra Dolomite have changed with time.

page 3-4, section 3.2.1
This section deals with possible mechanisms for salt dissolution. This section appears to put forth only the ideas developed by Anderson (1978) and Anderson and Kirtland (1980). If any other ideas exist, they are not presented. No additional ideas for potential deep dissolution mechanisms are put forth. The possibility of dissolution from flow in joints or fractures in the Delaware Mountain Group and Castile anhydrite rocks should be addressed.

page 3-5, section 3.2.2
This section quantifies the amount of salt that can be diffused through the lower anhydrite of the Castile Formation by means of either a fracture or a porous medium. The results indicate that the fracture will propagate upward at a rate of 3×10^{-5} meters per year and that, in the porous medium case, a dissolution front would propagate upward at a rate of 3×10^{-6} meter per year.

The analysis is based on the assumption that steady state is reached. This approach is probably correct for the porous medium approach because the porous medium has been in place for more than 200,000,000 years. On the other hand, fractures can form at any time. In a fracture the initial unsteady state rates of dissolution and diffusion of salt should be very large compared to those of the steady state because of the steep concentration gradient which forms at the top of the fracture. The amount of salt that can be dissolved at unsteady state by a fracture should be quantified here.

Both the fracture and porous medium rate of diffusion calculations should include the range of Delaware Mountain Group NaCl concentrations because the amount and rate of dissolution are dependent on this. These calculations should show that dissolution of halite will occur faster at the up gradient parts of the Delaware Mountain Group than at the down-gradient parts.

page 3-8 to 3-10, "Threshold of Convection in Fractures and Porous Media"

This section is used to estimate the width of a fracture required to initiate brine density flow. This is done by approximating the width of a fracture with the radius of a tube. A study performed by Wooding (Wooding, R. A., "Instability of a Viscous Fluid of Variable Density in a Vertical Hele-Shaw Cell," Journal of Fluid Mechanics, vol. 7, Jan. - Apr., 1960, pp. 501-515) tends to indicate that this is not the correct approach. Using a mathematical model of water and mass transport between two parallel plates, he found that the width required to initiate density flow was dependent on the length of the parallel plates. Wooding (1960) verified his results with a Hele-Shaw analog model. The results of his study indicated that brine density flow could occur in fractures much smaller than the 0.5 and 1.5 millimeters indicated in this report. If a fracture is assumed to have smooth parallel sides, then a fracture with a width of 1 mm has a high hydraulic conductivity (0.7 m/s) and is capable of transmitting significant amounts of salt.

page 3-10, 2nd paragraph:
A basis or reference for the statement "It is doubtful whether single fractures of one millimeter or more in aperture could remain open and continuous in Anhydrite I" should be provided. While the drilling in the Castile Formation has not indicated any significant fluid producing fractures in Anhydrite I, they have been observed in the higher anhydrites of the Castile. The most notable example of a fracture occurs at WIPP-12 about 3010

feet below land surface. This fracture is capable of producing several hundred gallons per minute of flow and it could be classed as open and continuous.

page 3-10, last line:
The validity of the equation $Ns \approx 0.1\ R_s{}^{1/3}$ should be examined. It appears that this relationship was originally derived by Elder (1967) although this report attributes it to Golitsyn (1979). Elder (1967) presented data which indicates that the above equation is valid for $\approx 5x10^8 < R_s < \approx 5x10^{10}$. Elder (1967) has other relationships for $R_s < \approx 5x10^8$, but none for $R_s > \approx 5x10^{10}$. The value of R_s used in the calculation involving the above equation is $1.2x10^{21}$, which is many orders of magnitude higher than the known range of valid R_s values for that equation ($\approx 5x10^8 < R_s < \approx 5x10^{10}$).

page 3-11, 2nd paragraph:
The comparison of the dispersion coefficients calculated from the Knapp and Podio (1979) experiments to the diffusion coefficient could be erroneous. This comparison is made on page 3-11 of the report as support for the contention that convective mass flux is 10^6 times higher than diffusive mass flux. Knapp and Podio (1979) treated the salt transport as a purely dispersive process. Wooding (1959), who studied the same phenomenon, included both a convection term and a diffusive term in his analysis. The large value of the Knapp and Podio (1979) dispersivity estimates tend to indicate that convection is occurring. Essentially the dispersion coefficient determined by Knapp and Podio (1979) approximates the convection of brine as a dispersive process.

Knapp and Podio (1979) performed four experiments in their study of salt transport in boreholes. Three tests were run in a bore tube with a diameter of four inches. Two of these tests were run with an induced velocity in the borehole; one was run with no induced velocity. The fourth test was run in a two inch diameter borehole and had no induced velocity. The calculated dispersivities ranged from 45 cm^2/sec to 48 cm^2/sec for experiments run in the four inch bore tube and was 12 cm^2/sec in the two inch bore tube. Knapp and Podio (1979) concluded that the dispersivity depends on the cross-sectional area of the bore tubes. If these dispersivities are corrected for the "radius of a fracture" of 0.001 meter, the dispersivity would be very small, say on the order of 10^{-6} m^2/sec. This would yield a Nusselt number of about 10^3 instead of 10^6 and would dispute the contention that convective mass transport is 10^6 times higher than diffusive mass transport.

page 3-12, 5th line from top:
The reason for believing that fracture of 1mm or more are unlikely to exist should be given. Wooding's (1960) results indicate that convection in a fracture of less than 1mm width can exist.

page 3-14, 3rd paragraph:
If a fracture were to propogate itself, (i.e. dissolve only the salt directly above it) it would reach the repository in less than 20 years at a rate of 28 cubic meters per square meter per year.

It seems very unlikely for a front to propogate as a square tunnel. Does any literature exist or has any been reviewed to indicate what shape forms when salt dissolves?

page 3-15, 1st line to 6th line from top:
This calculation assumes that there is no flow or dispersive flux through the DMG. What is the effect of flow and dispersion through the DMG on the time for salinity buildup to saturation? It is possible that a fracture extending into the DMG could transport the salt away toward the reef at a high rate and saturation would never be reached. It is highly probable that, due to the sparse drilling activity in the DMG, vertical fractures were missed during drilling.

page 4-4, "Hydraulic Conductivity" section:
The range of hydraulic conductivity should be extended from 1 md to 59 md (0.3 m/year to 18 m/year). See comment regarding page 2-8.

page 4-5, "Chloride Concentrations" section
The chloride data on Hiss's (1975a) Figure 26 tends to confirm the existence of the 100 kg/m^3 contour on the upgradient end of the Bell Canyon aquifer.

page 4-6, section 4.3.1

This is a good approach to use as a first approximation because the method is insensitive to whether the source of salt is by diffusion from above, convection from above or from some other source. In essence, the amount of calculated salt input by this method can be assumed to be from dissolution of overlying halite. Thus the amount of halite dissolved is probably overestimated. However the basic assumption in this model as applied to the DMG is porous media flow in the DMG aquifer. In addition the model presented here does not include longitudinal dispersion, which would tend to increase the amount of salt dissolved. Is dispersion insignificant in this case?

page 4-7, 8th line from top:

The mass of salt dissolved per year or 10,000 years should be presented here. Also the mass flux and rate of salt being dissolved from underneath the WIPP site should be presented for comparison purposes. An EEG calculation indicates these values are 4.1×10^{-4} kg/yr/m^2 and 0.31 cm/10,000 years, respectively.

page 4-9, 2nd paragraph:

It would be interesting to see the amount of salt that can be dissolved by the mechanism described in this paragraph. Would it be large enough to dissolve Salado salt laterally from the reef to the repository? Would it also be large enough to account for the amount of salt being transported by the Capitan Reef aquifer? However, the decreasing concentration of chloride downgradient along the eastern side of the Capitan Reef (see page 4-5) tends to indicate that convective dissolution of the overlying Salado is not occurring in this part of the aquifer. Active convective dissolution would tend to increase the chloride.

Could salt transport through fractures in either the DMG or Castile be enough to account for the estimated salt transport in the Capitan Reef aquifer?

page 4-10, 2nd and 3rd paragraphs:

The mass balance model described here should have a longitudinal dispersive term included.

page 4-13, 8th line from bottom:
The average value of vertical removal of 0.34 cm per 10,000 years obtained from the numerical approach agrees very well with the 0.31 cm per 10,000 years obtained from the analytical approach. What is the range of vertical salt removal over the 16,500 m long line underneath the repository?

page 4-14, section 4.4.3
The sensitivity analysis with respect to hydraulic conductivity should be extended to 18 m/yr. See comment regarding page 2-8.

page 4-16, 3rd to 6th line from top:
One of the reasons the numerical approach concluded diffusion as the source of salt to the DMG is that the model assumed diffusion as the source to start with. The model was then calibrated to determine the diffusion coefficient, which happened to be in the range of acceptable values. It can only be concluded that diffusion is a possible explanation but by no means the only one.

page 4-16, last paragraph:
Has an estimate of the rate of salt dissolution from the reef toward the repository been obtained? Page 4-9 indicates that the reef transports 20×10^6 to 440×10^6 kg/year of chloride, of which only about 3×10^6 kg/year is accounted for. If the remainder of the chloride transported by the reef comes from the brine density flow indicated here, how large a cavity would form in the Salado? What is the structural integrity of such a cavity? How fast would a cavity advance toward the repository? No sound basis is provided for the argument that the salt removal potential of the reef will not affect the repository.

page 5-2, 6th line from bottom:
"0.34 centimeter" should read "0.31 centimeter".
It appears that this paragraph is discussing the rates of dissolution determined from the analytical model. Page 4-7, third paragraph, indicates that the amount of salt dissolved is 0.3 centimeters in 10,000 years.

page 5-3, 1st line:
"0.7" should be "0.6". The dissolution process described on page 5-2 has a linear relationship between flow thickness and dissolution height. If the aquifer thickness is doubled, the dissolution height should double.

page 5-4, 5th line from top, ref. Figure 5-2:
The "dissolution controlled by diffusion" curve on Figure 5-2 does not pass through the point defined by flow rate= .135 m^3/yr/m and height = 0.34 cm. The results of the numerical modelling as presented on page 4-13 indicate that flow rate= .135 m^3/yr/m and height = 0.34 cm is the solution to the numerical modelling problem. Is the "dissolution" curve on Figure 5-2 correct?

page 5-6:
Going back to the analytical model; for an aquifer flow rate of .135 m^3/yr/m and a chloride concentration change of 50 kg/m^3 in 16,500m, underneath the repository, the amount of salt dissolved over the 16,500 m line is 4.1×10^{-4} kg/yr/$m.^2$ If this amount of chloride all dissolved from one fracture, the amount of chloride passing through this fracture is 6.8 kg/yr/m, which is slightly less than the 10 kg/yr/m being used here. Therefore the results presented in Figure 5-2 may be slightly higher than what can actually occur,subject to any sensitivity analysis and the assumption of porous flow in the DMG. An approach to maximize salt dissolution would be to use a chloride concentration change of 150 kg/m^3, which is the change from one end of the DMG to the other. Using this approach, one gets about 20 kg/yr/m of slat dissolution through a fracture.

page 5-6 5th line from bottom:
As mentioned earlier, work by Wooding (1960) indicates that convection can occur in fractures smaller than 1.5 mm.

page 5-6, 2nd para.:
The calculation of fracture width is not quite clear. If a fracture is capable of transporting 6×10^4 kg/m^2/yr (page 3-11) of salt, it is capable of transporting 3.64×10^4 kg/m^2/yr of chloride. If fracture width is calculated by $Q\Delta C/3.64 \times 10^4$ kg/m^2/year, then the fracture width curve on Figure 5-4B

should be lowered. Again, Wooding (1960) indicates convection can occur in fractures with an aperture smaller than 1.5 mm.

page 5.12, bottom paragraph:
The simple statement that 400 m thickness over a 1 m cavity with a 94 m diameter should be enough structural support is weak and not convincing. Some more justification of this idea should be provided.

Table 4-1
What is the basis for the dispersivity of 3.048 meters shown on Table 4-1?

TYPOGRAPHIC ERRORS

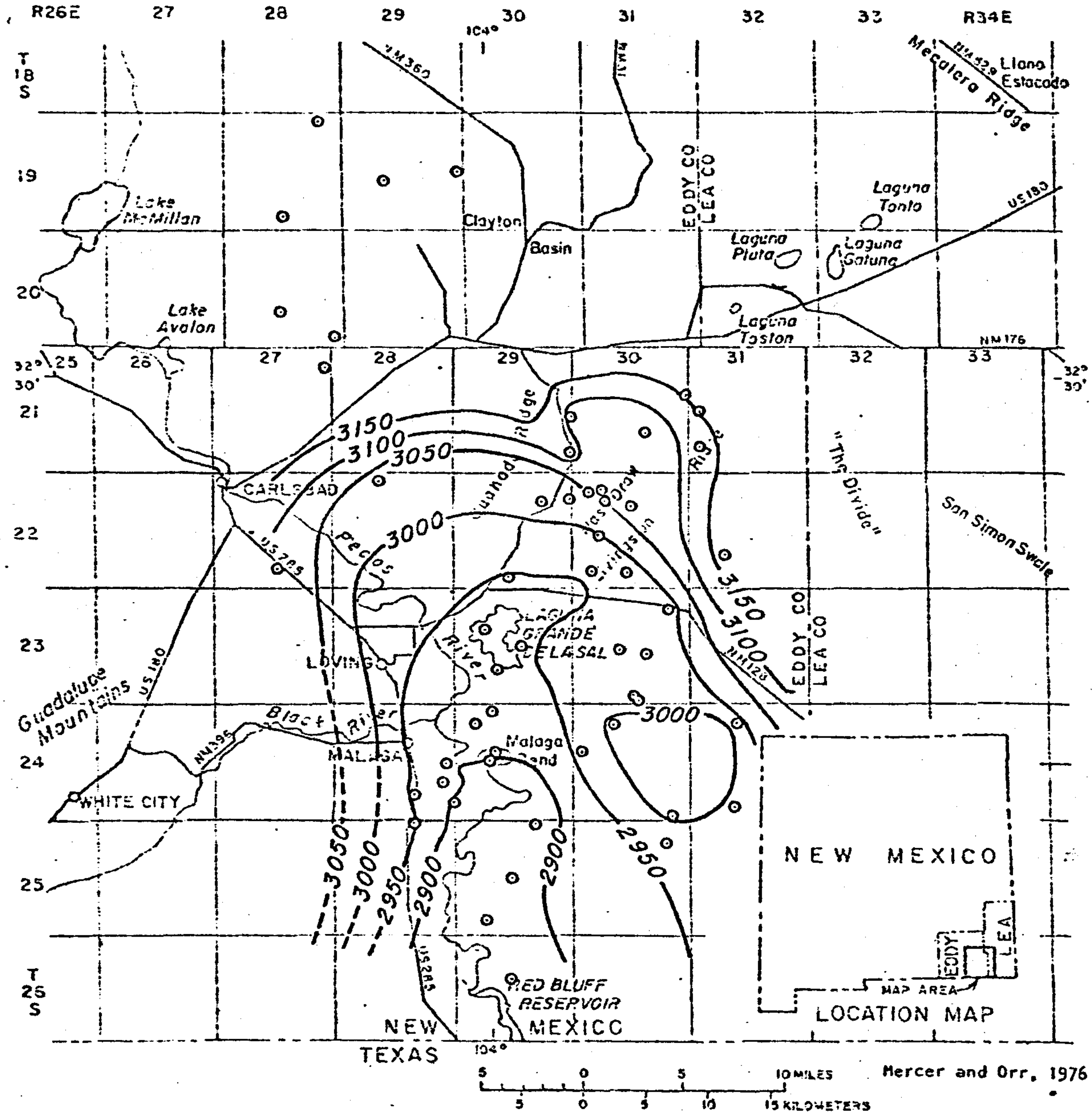
page 2-2, 6th line from bottom, page 2-12, bottom line and "Bibliography," page 6:
"Gonzales" should be "Gonzalez"

page 3-6, bottom line and "Bibliography," page 3:
"Bear, (1975)" should be "Bear (1972)." This typographic error occurs several places in the report.

Table 3-1, footnote 5:
"Figure 3-8" should be "Figure 3-7"

Figure 3-7:
An "H" should be placed after "Dissolution Cavity Width" at the top of the figure.

Figure 5-1:
"Cayon" should be "Canyon" in footnote 2.



EXPLANATION

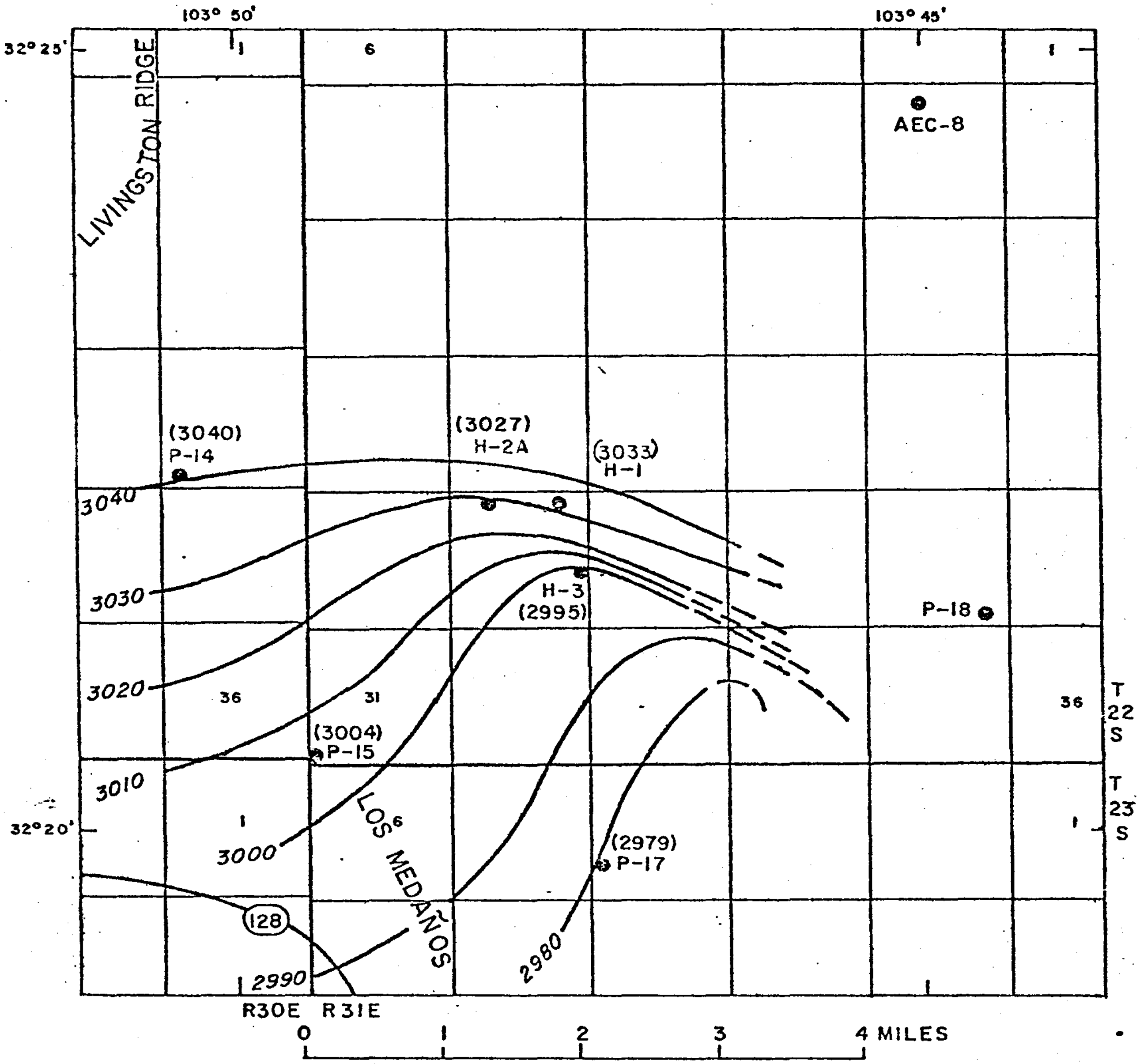
⊙ Well producing water from Rustler Formation

3000 — Potentiometric contour showing altitude at which water level would have stood in tightly cased wells. Dashed where approximately located. Contour interval 50 feet. Datum is mean sea level.

Sources of data:
Hendrickson and Jones, 1952; Hale and others, 1954;
Cooper, J. B., 1962; Jones and others, 1973

Figure 1.--Potentiometric surface of the Rustler Formation, 1952 through 1973.

From: Mercer, J. W. and B. R. Orr, 1977



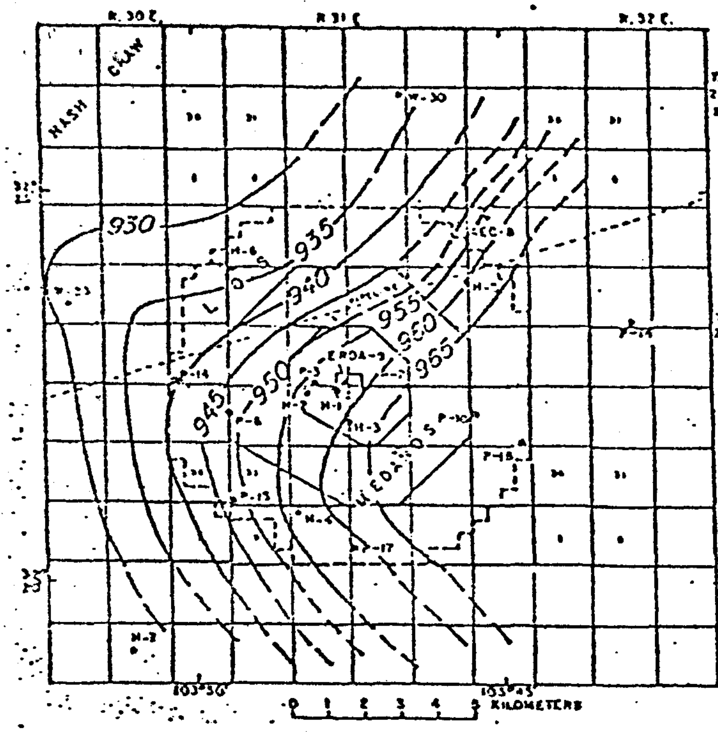
EXPLANATION

● P-17 (2979) — WELL--P-17 is well identification. (2979) is altitude of water level, in feet (expressed as freshwater with a density of 1.00 gram per centimeter). Datum is mean sea level

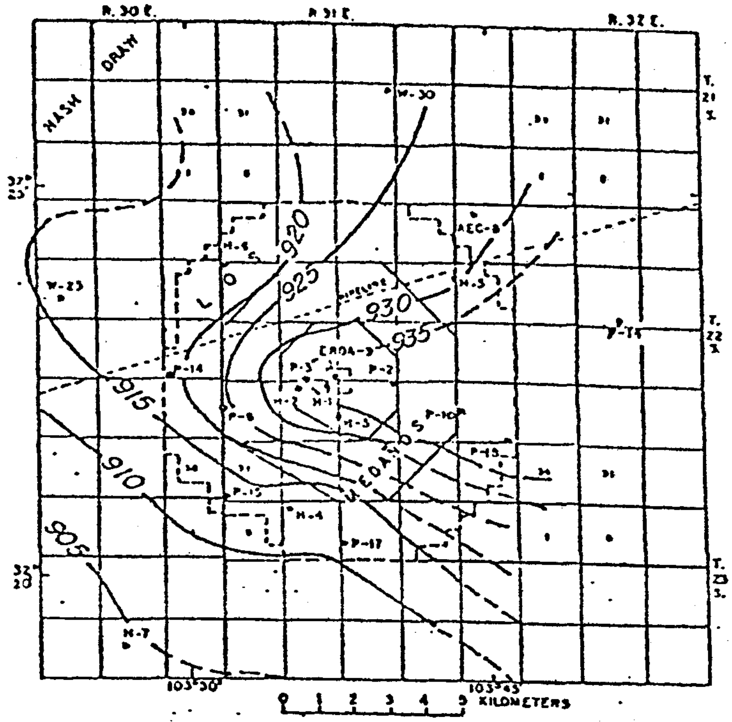
——— 2990 — — POTENTIOMETRIC CONTOUR--Shows altitude at which fresh water having a density of 1.00 gram per centimeter would have stood in a tightly cased well, October 1977. Dashed where approximately located. Contour interval 10 feet. Datum is mean sea level

Figure 2. --Potentiometric surface of the Culebra Dolomite Member of Rustler Formation.

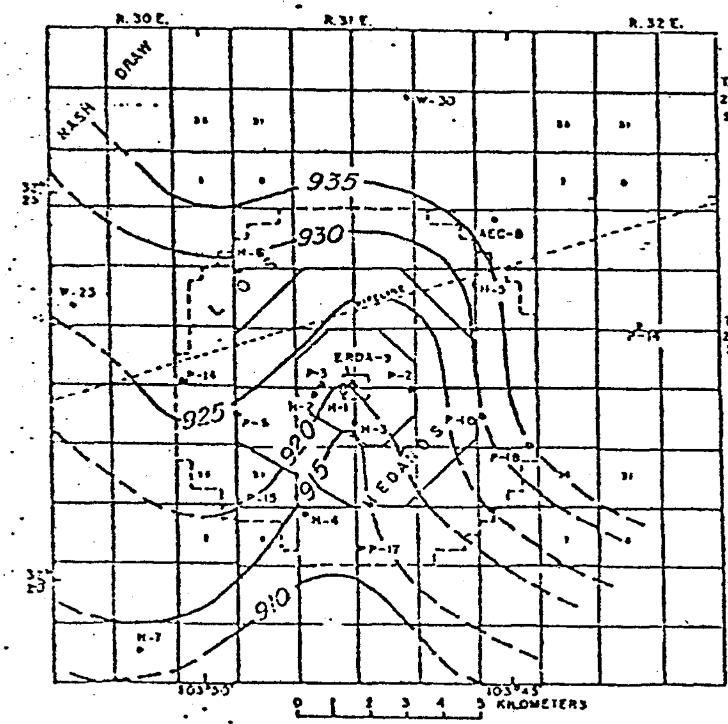
From: Mercer J. W. and B. R. Orr, 1979



MAGENTA DOLOMITE MEMBER
OF THE RUSTLER FORMATION



RUSTLER SALADO CONTACT



CULEBRA DOLOMITE MEMBER
OF THE RUSTLER FORMATION

EXPLANATION

—925— POTENTIOMETRIC CONTOUR--Shows altitude at which freshwater having a density of 1.00 gram per cubic centimetr would stand in a tightly cased well. Dashed where approximately located. Contour interval 5 meters. National Geodetic Vertical Datum of 1929

P-17 WELL AND NUMBER

Figure 3.--Preliminary potentiometric surface maps of water-bearing zones at WIPP in fresh-water equivalent head.

From: Mercer, J. W. and D. D. Gonzalez, 1981

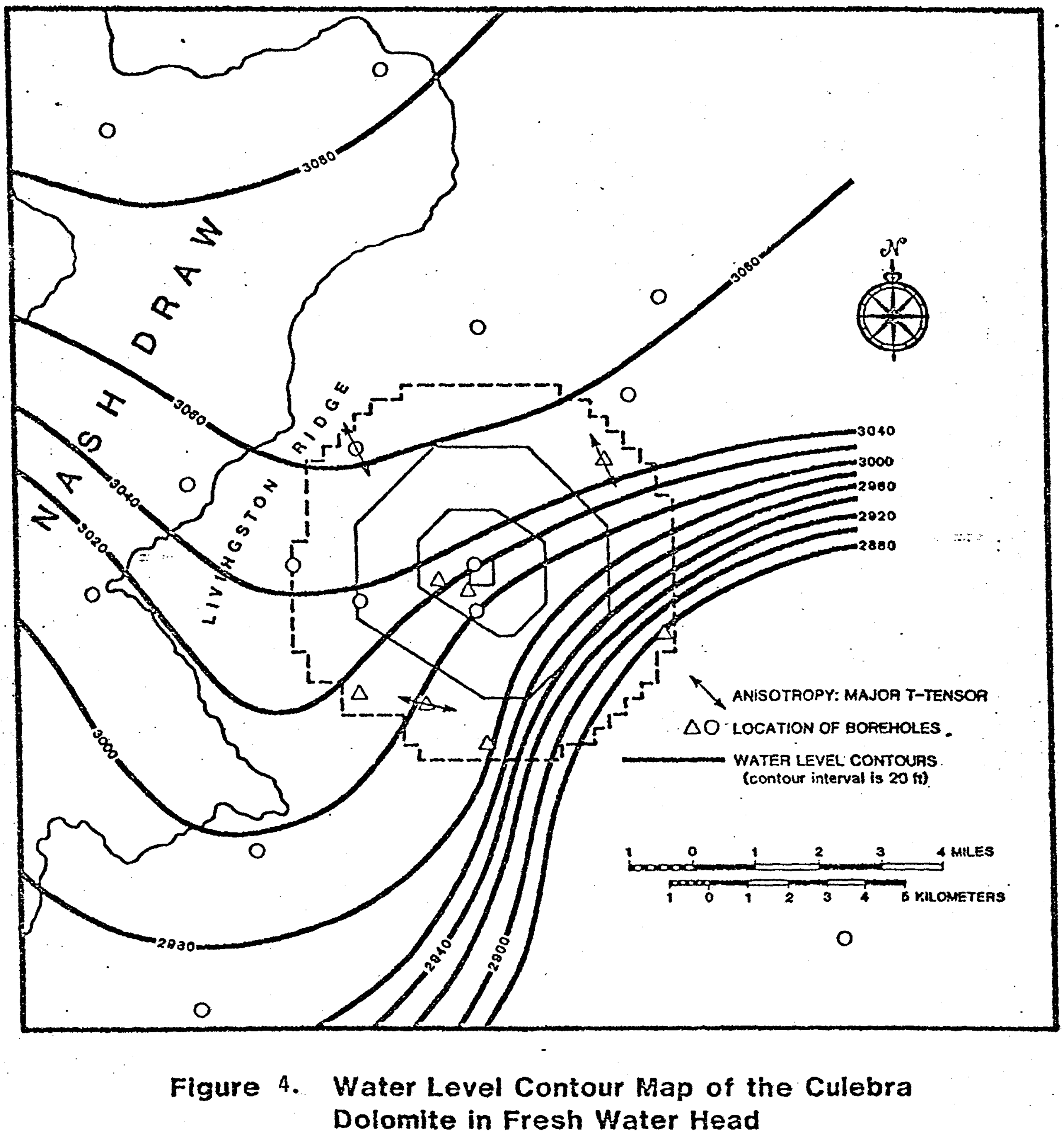


Figure 4. Water Level Contour Map of the Culebra Dolomite in Fresh Water Head

From: Gonzalez, 1982

Department of Energy
Albuquerque Operations Office
P.O. Box 5400
Albuquerque, New Mexico 87115

RECEIVED
NOV 29 1982
ENVIRONMENTAL EVALUATION GROUP

NOV 24 1982

Mr. Robert H. Neill
Director
State of New Mexico
Environmental Evaluation Group
P. O. Box 968
Santa Fe, NM 87504

Dear Mr. Neill:

Reply to EEG Comments on Draft Report, "Delaware Mountain Group (DMG) Hydrology - Salt Removal Potential"

Responses to your comments on the subject document have been prepared by the authoring organization. Your comments have led the authors to propose changes in the report which are given with the enclosed responses. It should be noted that the primary purpose of the report is to determine the solute-transport capacity of the Delaware Mountain Group (DMG), specifically, the Bell Canyon Formation. An understanding of the solute-transport capacity of the DMG is essential to any proposed model for dissolution that invokes the DMG as a source of unsaturated fluids or a sink for saturated fluids. Regardless of the specific model proposed for dissolution, transport in the Bell Canyon is the rate-determining mechanism.

After you have had an opportunity to review the responses to your comments, if you feel that a meeting with the authors of the report is necessary, please contact us as soon as possible so that arrangements can be made.

Sincerely,

J. M. McGough
J. M. McGough
Project Manager
WIPP Project Office

WIPP:JMM 82-0829

Enclosure

cc w/o enclosure:
G. L. Hohmann, TSC
C. C. Little, TSC
C&C File, IEA, TSC

General Comment No. I

"a. Possible communication (of DMG) with other aquifers e.g. reef aquifer, San Andres limestone aquifer and shallow aquifers" has not been treated in this report. Although the details on this theme are expected in the regional hydrology report, a brief treatment of this subject will be desirable in this report for completeness. We recommend adding a subsection under section 2.2 of the report for such discussion."

Response:

The USGS regional hydrology report currently in preparation includes an assessment of the aquifers of the Delaware Basin and surrounding areas and possible communication between aquifers. As part of the subject study for the draft report, "DMG Hydrology-Salt Removal Potential," the regional hydrogeology, and specifically communication of aquifers, has been reviewed. Chapter 2.0 provides the necessary hydrogeologic background for the study and indicates the communication of ground water between the Bell Canyon and Capitan Reef. To elaborate further on the communication of the DMG with other aquifers, the following discussion summarizing hydraulic communication characteristics of the DMG with the Capitan, San Andres, and shallow aquifers will be added in Section 2.2 after the third paragraph on Page 2-10:

> "Communication between the DMG and the Capitan, San Andres, and shallow aquifers is determined by hydrogeologic parameters. Hiss (1975a) has compiled stratigraphic cross sections, potentiometric surface maps, and hydraulic characteristics of the DMG and Guadalupian age rocks in the Delaware Basin. Hiss' work indicates that the Capitan is immediately underlain by members of the DMG and that the potentiometric surface in the DMG is greater than the Capitan Reef aquifer and some discharge from the DMG to the Capitan is expected.
>
> Stratigraphic cross sections in Hiss' work show that sandstone tongues of the Cherry Canyon Formation interfinger the San Andres limestone. Thus, hydraulic communication between the two units is likely. Hiss, however, reports that the average hydraulic conductivity of the shelf aquifers, including the San Andres, is about 4.8 meters per year compared with the DMG average hydraulic conductivity of 1.8 meters per year. The low conductivity of both units restricts the transfer of large quantities of ground water. The head differential between the DMG and San Andres is difficult to determine from literature sources, but it appears to be similar or less than the differential at the Capitan-DMG interface.

A relatively small amount of literature information is available on the degree of hydraulic communication between shallow aquifers and the DMG. Major dissolution or fracture zones are the most probable areas where hydraulic communication between shallow aquifers and the DMG could occur."

General Comment No. II

"b. The "Expected Results" section of the Costs and Merits document for DMG Hydrology states, "Additionally, the relative merits of various dissolution theories will be discussed." We understand that the primary document for such a discussion is the Deep Dissolution report, but the DMG Hydrology report should at least discuss the possibility of proposed mechanisms, other than Anderson's deep dissolution, explaining the observed features of DMG and Reef hydrology and hydrogeochemistry. A discussion of Bachman's "Solution and fill," subaerial erosion during Jurassic time and Lambert's "Stratabound Dissolution" in this context would be very appropriate and useful."

Response:

This subject report focuses on the DMG and addresses dissolution data and hypotheses only as far as they relate to salt removal associated with the DMG aquifer. Sandia National Laboratories is preparing a report on deep dissolution which sufficiently addresses the dissolution hypotheses which have been referenced with respect to the DMG. The major hypothesis which involves the DMG is Anderson and Kirkland's (1980) brine density flow model which uses the Bell Canyon Formation as the source of unsaturated ground water and as the sink for saturated brine. Section 3.2 of the report addresses the Anderson and Kirkland hypothesis as the primary model for salt removal associated with the DMG aquifer and discusses the mechanisms (diffusion and convection) for salt removal by ground water. Additional hypotheses which could involve the Bell Canyon as a sink for dissolved halite include downward percolation of meteoric water and Lambert's "stratabound dissolution" hypothesis. Both of these hypotheses employ the mechanisms of diffusion and/or convection for salt removal by ground water which have been studied in the report. Additionally, the results of such dissolution hypotheses have already been implicitly discussed in the report for it was shown that under most circumstances the maximum amount of salt which can be removed is controlled by the mass transport rate of the Bell Canyon aquifer. Further discussion of alternative dissolution hypotheses is given in the response to Comment No. 11.

General Comment No. III

"Omission of Salado Salt Removal

Having accepted the possibility of the mechanism of salt removal through convective flow at depth, the report has not addressed a major contention of Anderson (1981), i.e., the removal of salt from the Salado formation. All the analytical models considered in the report deal with the "salt dissolution in the Castile Formation and mass transfer to the Bell Canyon Aquifer." (page 6-1, paragraph two)

The reasons for this omission are not clear, since according to the report, "it seems likely that the groundwaters have dissolved some salt from the Salado and Castile formations" (page 3-2, second line). In fact, Task No. 3 (page 1-4) for the study is to "assess the potential for dissolution in the Castile and Salado formations." It is hoped that the final version of this report will correct this omission."

Response:

The analytical and numerical modeling which was performed focused on the potential of dissolution of halite immediately overlying the DMG because the mechanisms for salt removal are dependent on the distance of the dissolving media from the aquifer. As a result, dissolution of the lowest halite unit in the Castile Formation was specifically addressed in the report. The potential for dissolution by ground water from the DMG would be less for the upper halite zone and Salado Formation. This is a conservative approach for determining potential dissolution in both Castile and Salado halite deposits because it maximizes the concentration gradient between saturated brine at the dissolution front and unsaturated Bell Canyon ground water. The calculation overestimates the rate of salt removal from the Salado Formation, which is located approximately 300 meters above the lowermost halite unit of the Castile. Thus, the dissolution rates calculated for the lowermost Castile are conservative estimates of salt removal from the Salado Formation.

The report will be clarified at several locations to emphasize that the calculated dissolution rates can be applied to both the Castile and Salado formations.

The last sentence of the first paragraph on Page 1-4 will be reworded as follows:

> "Review of the geologic and hydrogeologic evidence and potential salt dissolution mechanisms has been undertaken to assess the possible extent of the dissolution in the Castile and Salado formations and evaluate the potential impact of such processes on the facility integrity."

The fourth bullet on Page 1-4 will be revised to read:

- "Establish hydrogeologic models for evaluation of the potential for salt removal from the Castile and Salado formations by fluids in the underlying DMG units."

The first sentence on Page 1-6 will be reworded:

> "In addition, their application for salt removal from the Castile and Salado formations by the DMG for site hydrogeologic and geologic conditions was investigated."

The last sentence of the second paragraph on Page 3-4 will be changed to:

> "It is the purpose of this section to review the physical and chemical mechanisms by which salt may be removed from the Castile and Salado formations by solution from below and to estimate the relative efficiency of these processes in removing salt from halite units within the Delaware Basin. In order to simplify the discussion in the remainder of this chapter, salt dissolution only in the Castile Formation will be used to illustrate the mechanisms. Salt dissolution in the Salado Formation could occur due to similar mechanisms but, as will be shown, the dissolution rates would be smaller because of the larger distance between a dissolution front and the Bell Canyon aquifer."

The following sentence will be added at the end of the first paragraph on Page 4-1:

> "As discussed in Section 3.2.1, calculated dissolution rates in the Castile Formation provide conservative estimates of Salado salt removal."

The second sentence on Page 5-1 will be revised to read:

> "An assessment of the impact of salt dissolution on the site integrity must include consideration of the potential solution cavity that may form in the Castile or Salado formations due to the anticipated dissolution rates."

The last sentence on Page 6-3 will be reworded as follows:

> "Furthermore, the very low flow rate of the Bell Canyon aquifer and the associated salt transport rate indicate that significant convective dissolu-

tion of halite in the overlying Castile and Salado formations would be prevented due to the inability of the aquifer to maintain the density gradient for any significant time period."

General Comment No. IV

"In view of a possible fault connecting Bell Canyon with Castile formation (U.S. DOE, 1980b, Figs. 2.7-20 and 2.7-21) and the existence of a joint system (p. 2-7, para. 2), it is surprising that no consideration is given to salt transport through fractures which may exist in DMG aquifer. It is recommended that the final version of the report includes calculations based on assumptions of joints in DMG and at least one fault connecting DMG and Castile formations."

Response:

As discussed in the response to Comment No. 1 (Specific Comments), the existence of the fault cited above, which was based on seismic reflection data, has been recently reevaluated. The existence of a joint system has been identified by Anderson (1978) although no other investigations have identified fracturing and jointing in the central basin area. The joint system identified by Anderson (1978) is exposed near Carlsbad Caverns and has reportedly fractured the lower anhydrite of the Castile Formation along the western basin margin, more than 40 kilometers from the WIPP facility. Available permeability measurements and drill stem tests throughout the site area indicate very low permeabilities and the potentiometric surface in the DMG dips gently northeastward with no apparent discontinuities or steep gradients. These data all strongly suggest that large-scale fracture flow is absent in the DMG aquifer. If localized fracturing of the aquifer was present, the net ground water flow and salt transport rates upgradient and downgradient of the fracture zone would control the flow. As a result, the report calculations have been based on measured permeabilities of the aquifer with the assumption of porous media flow. Because of the importance of the parameters governing flow in the Bell Canyon aquifer, a sensitivity analysis was performed to determine the potential dissolution for a range of aquifer flow rates varying over more than one order of magnitude.

General Comment No. V

"Brine Aquifer as a Deep Dissolution Sink

The report states that the, "Geochemical evidence of salt dissolution is provided by the composition of groundwater from the Bell Canyon and Capitan aquifers" (p. 3-1, 3rd para) and that "it seems likely that the groundwaters have dissolved some salt from the Salado and Castile formations" (p. 3-2, first para). While the report rules out a DMG as a carrier of the dissolved salt, it suggests that the Capitan aquifer may directly participate in the salt dissolution at depth. The Chapter on

"Conclusions" (Chapter 6) states, "As is evident from this study, brine density flow or convective dissolution is a potential mechanism for removal of halite and its occurrence in the Delaware Basin is possible in areas overlying and at the Capitan reef aquifer margin." (p. 6-3)

This is an important statement and raises questions concerning the mechanism of salt removal without DMG aquifer participation, directly to the Capitan aquifer. The existence of decreasing chloride concentration down gradient in the Capitan reef aquifer is one example of the problems to be resolved and understood. The report, in its final form, should try to present a mechanism of salt removal from Castile and Salado into the Capitan reef aquifer without involving the DMG aquifer."

Response:

The purpose of the study on salt dissolution was to evaluate the salt removal potential of the DMG and the associated effects on the WIPP facility. The Capitan reef is not a part of the DMG and is located more than 16 kilometers from the WIPP facility. Nevertheless, a preliminary analysis is presented herein to assess the possible rates and hypothetical cavity sizes associated with dissolution from the Capitan reef. Based on the relative isolation of the WIPP facility from the reef aquifer and the brief analysis presented herein, it is believed that dissolution associated with the Capitan reef will not affect the structural integrity of the WIPP facility in 10,000 years and that the last paragraph on Page 4-16 of the report sufficiently addresses this concern.

Section 4.3.2 addressed potential chloride transport rates in the Capitan aquifer and the possible origins of the dissolved salt. A general decrease in chloride concentration is identified downgradient based on recent data (Hiss, 1975a). Evidence of flow reversal in the Capitan due to pumping associated with the petroleum industry has also been reported by Hiss (1975a).

Based on the observed range of chloride concentrations in the Capitan aquifer and the estimated flow rate, the mass transport capacity of the reef has been estimated (Page 4-8 of the report). If dissolution is assumed uniform over the interface of the Castile-Capitan contact throughout the basin (a distance of approximately 110 kilometers), the average dissolution rate is approximately one millimeter per year, or an estimated 4 to 30 meters in 10,000 years, based on the range of aquifer parameters and chloride concentrations. This estimate conservatively assumes that the observed chloride concentration in the reef is due only to dissolution at the Castile-Capitan interface and that the dissolution rate remains constant for the 10,000-year period.

The location of the reef with respect to the WIPP site, more than 16 kilometers away, suggests that the dissolution mechanism as discussed in the report would not impact the facility in 10,000 years. As discussed in the report, the mass transport capacity of the reef is much greater than that of the Bell Canyon aquifer such that convective dissolution may be responsible for some of the observed chloride concentration in the reef. Convective dissolution can be generated by a circulation cell in which lower density (lower concentration) fluid ascends pores or fractures, becomes higher in density (and concentration) due to dissolving of halite, and subsequently descends due to its higher density. A density convection cell resulting in fluid movement can develop in a horizontal direction in addition to a vertical direction. However, development of a horizontal convection cell is hindered as the width of the cell increases (the distance between the discharge point of saturated brine water and the dissolution front). Since the length to width ratio of a fracture extending from the reef toward the WIPP facility would be very large, the horizontal advance of dissolution from the Capitan aquifer into the Castile is not anticipated to be very aggressive.

As a very conservative estimate of the mass flux through a fracture capable of supporting convective dissolution in a horizontal direction, studies by Warner and Arpaci (1968) and Cheesewright (1968) concerning natural convection at a vertical face were reviewed. These investigations did not consider the full circulation effect with reversal of flow. The circulation pattern in a fracture extending from the Capitan into the Castile Formation would involve horizontal flow of unsaturated brine from the reef toward the dissolution front, downward flow during salt dissolution at the front, and flow of saturated brine back toward the reef (flow reversal). Applying the results reported in the above reference is believed to be conservative because the reversal of flow would tend to reduce the horizontal density gradient which drives the circulation cell.

Warner and Arpaci (1968) and Cheesewright (1968) identified the following relationship for the Nusselt number (N_s) for natural convection at a vertical face for Rayleigh numbers (R_s) up to 10^{12}:

$$N_s = 0.1\ R_s^{1/3}$$

This is identical to Equation (3-6) of the report which was utilized in the assessment of potential convective dissolution through a vertical fracture. Based on this relationship and the relationship for the Rayleigh number [Equation (3-5)], an estimate of the total mass flux associated with convective flow in a horizontally propagating fracture can be determined. Substitution of Equation (3-5) for the Rayleigh number into the previous expression for the Nusselt number results in an equation for the convective mass transport through the fracture.

The potential dissolution cavity resulting from convective mass flux through a horizontal fracture was assumed to take the form of either a cylinder with a vertical axis or a rectangular tunnel with equal depth and width. For an assumed one-millimeter fracture, the hypothetical mass flux is 93 kg/yr per meter of fracture height. The hypothetical dissolution cylinder would have a diameter of 24 meters in 10,000 years, or a hypothetical rectangular tunnel would have a depth and width of 21 meters.

Considering a one-centimeter fracture and convective mass transport from the Capitan aquifer, the hypothetical mass flux is 930 kg/yr per meter of fracture height. The dissolution diameter for a cylindrical cavity would be about 100 meters in 10,000 years. The dissolution depth and width of a rectangular tunnel would be 66 meters in 10,000 years.

This analysis is conservative in that there is no consideration of return of saturated brine through the fracture to the Capitan and the associated reduction of the density gradient which drives the circulation cell. Due to the difficulty in sustaining a convection cell over a distance of several kilometers, it is reasonable to assume that the location of a hypothetical cavity would be near the reef where halite units are adjacent the aquifer. While it is clear that the potential convective dissolution rate from a hypothetical fracture could be transported by the Capitan aquifer, it is believed that development of a cavity over the period of study will not impact the WIPP facility, which is more than 16 kilometers from the reef.

General Comment No. VI

"Brine in Castile

The report has disregarded the importance of pressurized brine in the Castile formation vis-a-vis the question of salt removal from Castile and Salado (p. 2-12). Even though these brine occurrences do not seem to be connected to the DMG aquifer, they may not be completely isolated. Also, the brine is found in large volumes. The most recent estimate of the volume of the brine reservoir encountered by WIPP-12 is 30 million barrels (Popielak, NAS-WIPP panel presentation, Aug. 1982) which would occupy 170 million cu. ft of space. The significance of such large volumes of pressurized brine to the question of removal of salt through dissolution in the same formation should be discussed more thoroughly in the report."

Response:

Observations made in the WIPP-12 and ERDA-6 boreholes indicate storage of brine in fractures and flow through fractures of up to five millimeters aperture, with the majority of storage appearing to be in microcracks as discussed further in the brine reservoir report (in preparation). The brine pressures are well above that of the Bell Canyon aquifer, which indicates that they are not hydraulically connected. The brine reservoirs have formed in response to deformation and are isolated from one another by zones of Castile anhydrite of very low permeability. In addition, the brine pockets and wide (up to five millimeters) fractures are typically associated with the uppermost anhydrite unit in the sequence rather than with the lowermost unit overlying the Bell Canyon aquifer. Therefore, although the presence of fractures cannot be ruled out, it is believed that the low Castile Formation permeabilities discussed in Section 2.2 are representative of most of the lower anhydrite unit overlying the DMG.

These observations indicate that the presence of pressurized brine in the Castile Formation does not affect the salt dissolution calculations and discussions given in the report. However, a discussion of this subject will be added to the report as indicated in the response to Comment No. 7.

Comment No. 1

"Page 1-3, 8th and 9th line from top and Page 2-7, 2nd paragraph: The WIPP Safety Analysis Report (Page 2.7-33, Figures 2.7-20 and 2.7-21) indicates that a northwest-southeast trending fault may exist on the interface between the Delaware Mountain Group and the Castile Formation. The fault is located approximately 9 km northeast of ERDA-9 and would be within 1 km of the repository as presently planned. The potential existence of a fault and its consequent hydrologic effects on the repository should be addressed in this report."

Response:

The WIPP Safety Analysis Report (SAR) states that "on the Delaware sandstone, roughly 9,500 feet above the Morrow horizon, a possible fault (interpreted from seismic reflection data,[31]) trends in a northwest direction for about nine miles (Figure 2.7-20), with about 200 feet of indicated displacement." Figures 2.7-20 and 2.7-21 of the WIPP SAR suggest that this potential fault is located within one kilometer of the site and at the Bell Canyon-Castile interface. The existence of this fault was based on connecting a series of "features" seen on the seismic reflection processed sections. A subsequent interpretation of the reflection data indicates that the existence of this fault is highly unlikely (Powers, 1982). Amendments to the SAR which are published subsequent to issuance of the Sandia National Laboratories site deformation report (currently in preparation) will no longer indicate a fault. Available permeability measurements and drill stem test data indicate very low values of permeability and we have incorporated these available measurements into our investigation. Even if localized fracturing of the Bell Canyon aquifer was present, the net ground water flow and salt transport rates would not be affected because aquifer zones without fracturing upgradient and downgradient of the fracture zone would control the flow.

The last sentence of the first paragraph on Page 1-3 will be deleted. The last sentence of the first paragraph on Page 2-7 will be revised to read:

> "Continuous post-Wolfcampian strata indicate that major faulting had ceased before the middle Permian and hence is not believed to affect the Bell Canyon, Castile, or Salado formations."

Comment No. 2

"Page 1-4, 1st paragraph: The statement, "When placed in salt beds which have remained generally stable since deposition in the Permian time (more than 230 million years ago), the waste buried in the WIPP facility may reasonably be expected to remain isolated from the biosphere for thousands of years" ignores everything that has happened to the salt beds since their deposition, viz. uplift, tilting, folding, salt tectonics, intrusion by a dike, collapse along breccia chimneys, dissolution, formation of cavities filled with huge reservoirs of brine, erosion, etc. It is clearly misleading, detracts from a satisfactory resolution of the question of future stability and isolation of the WIPP repository, and should therefore be removed from this report."

Response:

A discussion of the geologic history of the Delaware Basin is presented in Section 2.1.1. The first sentence of Page 1-4 will be revised as follows:

> "It has been proposed to locate the WIPP facility in Permian age salt beds (formed more than 230 million years ago) in order to isolate the radioactive waste from the biosphere for a period of at least several thousand years."

Comment No. 3

"Page 1-4, 1st paragraph: Similarly, the sentence following the above mentioned one states that radioactive decay will reduce the hazard to "negligible levels" in a few thousand years. The Pu-239 inventory will be essentially the same as at closure and the statement is incorrect."

Response:

A time period of a few thousand years is sufficient to allow complete decay of the highest activity fission products, Cs-137 and Sr-90, but not for some of the lower activity species such as Pu-239. However, we agree that the statement might be misleading. The sentence beginning on the fourth line of Page 1-4 will therefore be reworded to read:

> "This period is sufficient to allow virtually complete decay of the short-lived high activity nuclides such as Cs-137 and Sr-90 and thus to substantially reduce the hazard posed by the waste."

Comment No. 4

"Page 1-7, 3rd and 4th bulletted conclusions: The terms "insignificant," "no significance," and "not greatly increase" are qualitative. They should be either replaced by or appear with the respective quantifiable number from Chapter 5 or Appendix B."

Response:

The actual values of potential dissolution and flow rate which are referenced in Section 1.4, "Summary and Conclusions," are presented and discussed in more detail in the appropriate sections of the report. The last two bullets on Page 1-7 will be revised as follows:

- "Based on an analysis of potential changes in the hydrologic characteristics (e.g., hydraulic gradient and associated flow rate) of the Bell Canyon aquifer, an increase in flow rate of even one order of magnitude (from an estimated rate of 0.135 cubic meter per year per meter of width to 1.35 cubic meter per year per meter) would not increase the salt removal from the Castile Formation by more than 17 percent (from a calculated rate of approximately 0.3 centimeter in 10,000 years to less than 0.4 centimeter in 10,000 years). The unlikely occurrence of a change in hydrogeologic characteristics and the associated potential dissolution are not anticipated to have any effect on the facility integrity.

- An analysis of implausible "worst-case" dissolution rates associated with both diffusive and convective dissolution at the Bell Canyon aquifer-Castile Formation interface suggests that the structural integrity of the WIPP facility located more than 400 meters above would not be affected. In this analysis, it was determined that the theoretical maximum cavity radius would be seven meters over a fracture and one meter above a circular porous zone in a period of 10,000 years."

Comment No. 5

"Page 2-7, section on "Fracturing": In which formations are the two sets of joints located? Does the joint set extend into or is it located within the transmitting sandstones of the DMG? These joints may be capable of transmitting water and thus have a high dissolution potential. The dissolution effects of convection along a joint or fracture in the DMG should be addressed in the report."

Response:

The WIPP SAR (Page 2.6-35) indicates that "two sets of joints, striking northwest and northeast" have been identified in the Delaware Basin rocks. The joints, believed to be pre-Cenozoic in age, are exposed near Carlsbad Caverns and filled with early Cretaceous sandstone and conglomerate.

The potentiometric surface for the DMG aquifer in the Carlsbad area implies northerly flow with no evidence of large scale fracturing. For example, there are no indications of the radical changes in flow field which might be expected in an extremely fractured area. Similarly, 40 kilometers to the east of the outcrop of the joint, the potentiometric surface in the DMG dips gently northeastward with no apparent discontinuities or steep gradients (Figure 2-5). These data all strongly suggest that large scale fracture flow is absent from the DMG aquifer. If localized fracturing of the aquifer was present, the net ground water flow and salt transport rates would not be affected because the aquifer zones without fracturing upgradient and downgradient of the fracture zone would control the flow. As a result, the report calculations have been based on the measured permeabilities of the aquifer with the assumption of porous media flow.

The second paragraph of Page 2-7 will be reworded as follows:

> "Although no major faults are known to exist at the WIPP site, data from boreholes drilled at the site indicate that jointing has occurred. Data concerning joint frequency and orientations are extremely sparse. Joint orientations are described as 'two sets of joints striking northwest and northeast' (U.S. Department of Energy, 1980b). The joints are exposed near Carlsbad Caverns, more than 40 kilometers from the WIPP site. These joints have been identified in Delaware Basin rocks and may extend into the water bearing sandstone."

Comment No. 6

"Page 2-8, 19th through 29th line from top: The values of permeability presented here appear to be taken from Table 6 of Hiss's (1975a) report and are average permeabilities on a county by county basis. Figure 21 of Hiss's (1975a) report indicates that permeabilities near the WIPP site range from less than 1 md to 59 md (<0.3 m/year to 18 m/year for pure water at 20°C)."

Response:

The permeabilities of the Bell Canyon aquifer near the WIPP site, as indicated in Figure 21 of Hiss's (1975a) report, are generally in the range of 1 to 25 millidarcys (md) (less than 0.3 to 8 meters per year). The 59 md (18 meters per year) measurement is isolated and does not appear to be representative of the site area permeability. The range of permeabilities used in the salt dissolution sensitivity analysis (Appendix B) was from approximately 1.7 to 15 md (0.5 to 4.5 meters per year), with an average value of 6 md (1.8 meters per year). The average Bell Canyon permeability on a county by county basis ranges from approximately 3 to 10 md (1.1 to 2.9 meters per year). These permeabilities are average values for all areas around the WIPP site and were utilized because the potentiometric surface for the Bell Canyon aquifer is relatively uniform. Specifically, the available data do not indicate a significantly variable flow field which would be observed with large variations in the aquifer permeability. However, the sensitivity analysis presented in Appendix B has been modified to include a permeability of 18 meters per year and this result will be incorporated in the report.

The fourth sentence of the last paragraph on Page 2-8 of the report will be clarified as follows:

> "The hydraulic conductivity of the Bell Canyon aquifer [based on core sample measurements (Hiss, 1975a)] ranges from 1.1 to 2.9 meters per year and averages approximately 1.8 meters per year. One measurement of hydraulic conductivity of 18 meters per year has also been reported; however, it does not appear representative of the basin."

The sentence beginning on the twentieth line of Page B-2 will be reworded as follows:

> "As shown in Figure B-1(A), an increase in Bell Canyon aquifer hydraulic conductivity from 1.8 to 18.0 meters per year would result in a 17 percent increase in dissolution height (H/H_{ref}, dimensionless dissolution rate, increases from 1.0 to 1.17)."

The seventh and eighth lines of Table B-1 will read:

"Hydraulic Conductivity of Bell Canyon Aquifer, K	0.5 to 18.0	Meters per year (m/yr)"

In Table B-2, the seventh, eighth, and ninth lines will read:

"Hydraulic conductivity	0.5	Meters per	-72	0.14	-57
of Bell Canyon aquifer,	1.8*	year (m/yr)	-	0.34*	-
K	18.0		+900	0.40	+17"

In addition, a fourth point will be added to Figure B-1(A) representing $K = 18.0$ meters per year and $H/H_{ref} = 1.17$.

The sensitivity of the average dissolution rate when assumed to be controlled only by the Bell Canyon aquifer flow rate is presented in Figure B-2 of the report. As is evident from the figure, an order of magnitude increase in the flow rate of the Bell Canyon aquifer from 0.135 to 1.35 m^3/yr-m (based on a permeability increase from 1.8 to 18.0 meters per year) would result in an average dissolution of less than 4 centimeters in 10,000 years.

Comment No. 7

"Page 2-11, last paragraph: The report should indicate that the hydraulic conductivity of the Castile anhydrites is not limited to porous flow. At WIPP-12 a fracture in Anhydrite III-IV of the Castile is capable of producing over 300 gallons per minute of brine (D'Appolonia Consulting Engineers, Inc., "Data File Report, ERDA-6 and WIPP-12 Testing," Volume IV A, Activity WIPP-12.2, Feb., 1982). At ERDA-6 a fractured zone in Anhydrite II of the Castile is capable of producing over 20 gallons per minute of brine (D'Appolonia Consulting Engineers, Inc., "Data File Report, ERDA-6 and WIPP-12 Testing, Volume II A, Activity ERDA-6.7, Feb., 1982). Preliminary calculations by EEG staff members indicate hydraulic conductivities of 2000 m/day for the fractured zone at WIPP-12 and 5 m/day for the fractured zone at ERDA-6. These values of hydraulic conductivity are at least six orders of magnitude greater than the values presented here."

Response:

Observations made in the WIPP-12 and ERDA-6 boreholes indicate storage and flow of brine in fractures with apertures of up to 5 millimeters. The majority of storage appears to be in microcracks which have formed in response to deformation and which are isolated from one another by zones of Castile anhydrite of very low permeability. In addition, the brine pockets and wide (up to 5 millimeters) fractures are typically associated with the uppermost anhydrite unit in the sequence rather than with the lowermost unit overlying the Bell Canyon aquifer. In general, Anhydrite I of the Castile Formation appears not to be affected by the deformations and associated fractures identified in the upper anhydrites. Thus, it is not likely that the lower anhydrite would be fractured.

It is therefore concluded that the low permeabilities discussed on Page 2-11 are appropriate for most of the lower anhydrite units overlying the DMG. The report will, however, be reworded to include a description of the occurrence of fractures associated with brine pockets and to state that the occurrence of such fractures in Anhydrite I cannot be ruled out.

The last sentence of the first paragraph on Page 2-12 will be deleted and the following will be added at the end of the paragraph:

> "Observations made in the WIPP-12 and ERDA-6 boreholes indicate storage and flow of brine in fractures of up to 5 millimeters aperture, with the majority of storage appearing to be in microcracks. The brine reservoirs have formed in response to deformation and are isolated from one another by zones of Castile anhydrite of very low permeability. In addition, the brine pockets and wide (up to 5 millimeters) fractures are typically

> associated with the uppermost anhydrite unit in the sequence rather than with the lowermost unit overlying the Bell Canyon aquifer. Although the presence of fractures cannot be ruled out, it is believed that the low permeabilities discussed in the previous paragraph are representative of most of the lower anhydrite unit overlying the DMG."

Comment No. 8

"Page 2-12, 2nd paragraph: Does the source of salt in the Castile brines come from dissolution of halite overlying or underlying the anhydrite layers?"

Response:

D'Appolonia has performed an extensive series of hydrologic and geochemical tests on the brine reservoirs occurring in WIPP-12 and ERDA-6 since the draft DMG hydrology report was prepared. The geochemical evidence strongly suggests that the brines were not produced by the dissolution of evaporite units either by ground or meteoric waters. The very high content of bromide (up to 990 milligrams per liter) and the ratios of Na, K, Cl, SO_4, and Ca to bromide all correspond very well to the expected values for evaporated seawater. This evidence suggests that the most likely origin of the brines is original residue from seawater evaporation. They therefore appear to be original evaporated Permian seawaters that may have dissolved very small amounts of evaporite minerals during transport to the anhydrite fracture system.

Comment No. 9

"Page 2-12, 2nd paragraph: The statement is made that Castile brine "pockets exhibit different (mostly higher) potentiometric surfaces than the Bell Canyon". It appears that all the potentiometric surfaces for the brine pockets are higher than those for the Bell Canyon."

Response:

The second sentence of the first paragraph on Page 2-12 will be reworded to read:

> "These 'reservoirs' have higher potentiometric surfaces than the Bell Canyon aquifer and do not appear to be connected with the DMG."

Comment No. 10

"Page 2-13, 2nd paragraph: Are the contours on Figure 2-7 "averaged" over the various water bearing units of the Rustler Formation or are they the contours for the Culebra only? They look like they represent water levels in the Culebra. A recent draft report (Gonzalez, D. D., "Fracture Flow in the Rustler Formation: Waste Isolation Pilot Plant (WIPP) Southeast New Mexico (Draft Interim Report)," SAND 82-1012, May 1982) has changed the Culebra contours from those presented in previous reference works (Mercer, J. W. and B. W. Orr, 1977; Mercer, J. W. and B. R. Orr, 1979; Mercer, J. W. and D. D. Gonzalez, 1981). Figure 1 through Figure 4 indicate how conceptions of the head in the Rustler Formation and the Culebra Dolomite have changed with time."

Response:

The origin of the potentiometric surface and extent of brine aquifer presented in Figure 2-7 for the Rustler Formation are based on the USGS Open-File Report 77-123, 1977 (Mercer and Orr, 1977), as reproduced in the WIPP SAR. These data do not differentiate between Culebra and Magenta dolomites or other zones. Based on the reference cited in the comment, it appears that the potentiometric surface has been redefined although its level when converted to fresh water with a specific gravity of 1.0 is below the observed potentiometric surface for the Bell Canyon aquifer. The second paragraph of Page 2-13 will be revised as follows:

> "The potentiometric surface of the Rustler Formation is generally lower than the potentiometric surface in the DMG. The brine aquifer and the potentiometric surface identified in the Rustler Formation are shown in Figure 2-7. These representations are based on data compiled from wells during the period 1962 through 1973 (Mercer and Orr, 1977). Recently, some revision to the potentiometric surface has been suggested (Gonzalez, in preparation) but the estimated level is below that of the Bell Canyon aquifer."

Comment No. 11

"Page 3-4, Section 3.2.1: This section deals with possible mechanisms for salt dissolution. This section appears to put forth only the ideas developed by Anderson (1978) and Anderson and Kirkland (1980). If any other ideas exist, they are not presented. No additional ideas for potential deep dissolution mechanisms are put forth. The possibility of dissolution from flow in joints or fractures in the Delaware Mountain Group and Castile anhydrite rocks should be addressed."

Response:

Two basic mechanisms or processes for the removal of salt from the halite regions are discussed in the report. These are molecular diffusion and convection associated with ground water flow induced by a density gradient. The major hypothesis for deep dissolution which involves the Bell Canyon Formation as the source of water and the sink for saturated brine is that put forward by Anderson (1978) and Anderson and Kirkland (1980). This hypothesis invokes the mechanisms of diffusion and convection for salt removal and is specifically addressed in Section 3.2.1.

Other hypotheses which involve the DMG as the sink for saturated brines are downward percolation of meteoric water and Lambert's (in preparation) "strata-bound dissolution" mechanism. These may be regarded as subsets of the hypotheses treated in the report since the total mass of salt that can be removed is controlled by the flow and mass transport rate in the underlying aquifer, regardless of the dissolution mechanism believed to occur in the basin. Removal rates calculated in Chapters 3.0, 4.0, and 5.0 which are limited by the Bell Canyon flow rate apply to all possible mechanisms which use the Bell Canyon as a sink.

To clarify the report with respect to these points, the following changes will be made. The last sentence of the first paragraph on Page 3-5 will be deleted. On Page 3-5 after the first paragraph, the following paragraph will be added:

> "Other hypotheses which include the DMG as the sink for saturated brines are downward percolation of meteoric water and Lambert's (in preparation) hypothesis of "strata-bound dissolution." In the latter, water dissolves salt while migrating approximately horizontally along soluble strata. The Bell Canyon aquifer could potentially provide a sink for saturated brines produced by this mechanism. In the context of this report, both these mechanisms may be regarded as subsets of the Anderson (1978) hypothesis since they require flow through Castile anhydrites in order to reach the Bell Canyon Formation. Thus, the calculated maximum rates of salt removal above a fracture or through a porous zone discussed in Chapters 3.0, 4.0, and 5.0 apply approximately to all hypotheses. Since the physical processes described by Anderson (1978) and Anderson and Kirkland (1980) are readily analyzed, their hypothesis is used in subsequent sections to illustrate the mechanisms for salt removal."

Comment No. 12

"Page 3-5, Section 3.2.2: This section quantifies the amount of salt that can be diffused through the lower anhydrite of the Castile Formation by means of either a fracture or a porous medium. The results indicate that the fracture will propagate upward at a rate of 3×10^{-5} meter per year and that, in the porous medium case, a dissolution front would propagate upward at a rate of 3×10^{-6} meter per year.

The analysis is based on the assumption that steady state is reached. This approach is probably correct for the porous medium approach because the porous medium has been in place for more than 200,000,000 years. On the other hand, fractures can form at any time. In a fracture the initial unsteady state rates of dissolution and diffusion of salt should be very large compared to those of the steady state because of the steep concentration gradient which forms at the top of the fracture. The amount of salt that can be dissolved at unsteady state by a fracture should be quantified here.

Both the fracture and porous medium rate of diffusion calculations should include the range of Delaware Mountain Group NaCl concentrations beause the amount and rate of dissolution are dependent on this. These calculations should show that dissolution of halite will occur faster at the upgradient parts of the Delaware Mountain Group than at the downgradient parts."

Response:

In the steady state calculation of diffusion through a fracture, a constant chloride concentration gradient of 1.2 kg/m^3 per meter was assumed based on a 100 meter fracture height above the Bell Canyon aquifer, existing chloride concentration in the aquifer of 70 kg/m^3 (actual range is generally 100 to 150 kg/m^3), and a concentration of 190 kg/m^3 (saturation) at the top of the fracture. Under such conditions, the fracture propagates upward at a rate of 3×10^{-5} meter per year. These calculations were presented as an approximate illustration of the propagation rate of a fracture which originates at a distance of less than 100 meters above the Bell Canyon aquifer. It was assumed that by the time the fracture reaches the 100 meter level, a gradually varying concentration gradient would have developed from 70 to 190 kg/m^3.

For the case of instantaneous formation of a 100 meter fracture, the initial gradient at the top of fracture is very steep, thereby resulting in a high initial propagation rate. The transient propagation rate and the time to reach steady state were calculated for such a fracture and the results will be documented in the final report.

Section 3.2.2 will document the results of the transient analysis of diffusion through a fracture and the calculated fracture propagation rate as a function of time. The following paragraph will be inserted after the second paragraph on Page 3-6:

"In the above discussion of dissolution by diffusion through a fracture, it was assumed that a uniformly varying steady state concentration gradient had developed. For the case of instantaneous formation of a 100 meter fracture, the initial gradient at the top of fracture is very steep, thereby resulting in a high initial propagation rate. As salt dissolution and fracture propagation continue, the variation in salt concentration becomes more gradual due to diffusion in the fracture and the propagation rate declines. To evaluate the rates at which a fracture moves under these conditions, a transient form of the one-dimensional diffusion equation (Crank, 1975) was solved. The parameter values are identical to the ones used in the steady state calculation except that the initial chloride concentration in the fracture is assumed to be constant at 70 kg/m^3 instead of uniformly varying at a 1.2 kg/m^3 per meter gradient. The analytical solution of the diffusion equation gives the chloride concentrations at different locations within the fracture as a function of time. From this information, the concentration gradient at the top of the fracture, which determines the salt dissolution and propagation rates, is determined.

The calculated propagation rates for different times beyond initial fracture formation are as follows:

TIME (years)	PROPAGATION RATE (meters per year)
1	290×10^{-5}
10	230×10^{-5}
100	89×10^{-5}
1,000	29×10^{-5}
10,000	9×10^{-5}
100,000	3×10^{-5}

This analysis indicates that on the order of 100,000 years is required for the upward fracture movement to decline to the assumed steady state rate of 3×10^{-5} meter per year. The initial rate (e.g., time = 1 year) is almost 100 times greater; but after 1,000 years, the rate is less than 10 times the assumed steady state value. Integration of the above results for the first 10,000 years after fracture formation yields a total propagation distance of approximately 2.0 meters. These transient results indicate that fracture propagation due to diffusion is a relatively slow process, even immediately after fracture formation."

The first sentence of the third paragraph on Page 3-7 will be revised as follows:

> "The above discussion demonstrates that diffusion-controlled dissolution is an extremely slow process. The steady state propagation of a single fracture is approximately 3 x 10^{-5} meter per year and the steady state movement of a dissolution front above a porous zone is on the order of 3 x 10^{-6} meter per year. For instantaneous formation of a fracture, the initial propagation rate is approximately 3 x 10^{-3} meter per year, decreasing to 2.9 x 10^{-4} meter per year in 1,000 years, and reaches steady state conditions (3 x 10^{-5} meter per year) in 100,000 years."

Comment No. 13

"Pages 3-8 to 3-10, "Threshold of Convection in Fractures and Porous Media": This section is used to estimate the width of a fracture required to initiate brine density flow. This is done by approximating the width of a fracture with the radius of a tube. A study performed by Wooding (Wooding, R. A., "Instability of a Viscous Fluid of Variable Density in a Vertical Hele-Shaw Cell," Journal of Fluid Mechanics, Vol. 7, Jan. through Apr. 1960, pp. 501-515) tends to indicate that this is not the correct approach. Using a mathematical model of water and mass transport between two parallel plates, he found that the width required to initiate density flow was dependent on the length of the parallel plates. Wooding (1960) verified his results with a Hele-Shaw analog model. The results of his study indicated that brine density flow could occur in fractures much smaller than the 0.5 and 1.5 millimeters indicated in this report. If a fracture is assumed to have smooth parallel sides, then a fracture with a width of 1 mm has a high hydraulic conductivity (0.7 m/s) and is capable of transmitting significant amounts of salt."

Response:

The conditions necessary for the onset of convection in a fracture must depend on the geometry of the aperture as was recognized on Page 3-9 of the draft report. The rationale for using cylindrical geometry is that it gives an approximate order of magnitude of the fracture width required for the onset of convection without the need to specify a hypothetical aspect ratio. The results were shown on Page 3-10 to confirm the approximate magnitude of critical fracture width deduced by Anderson and Kirkland (1980). Wooding's (1960) results may be used to deduce that for an aspect ratio (length/width) of 20, the critical fracture width for solute-driven convection would be about 0.4 millimeter, whereas an aspect ratio of 100 would yield a critical width of about 0.2 millimeter.

The choice of an appropriate aspect ratio is complicated by the general roughness of geologic fracture surfaces which would hinder convection in long, thin fractures but, even with a high aspect ratio (100), the result is not substantially different from that (0.5 to 1.5 millimeter) given in the draft report.

Given the variability in critical width, the important conclusion of Section 3.2 is that, if convection occurs, the Nusselt number is so high that the fracture rapidly saturates with salt. Removal rates therefore are controlled by the properties of the aquifer rather than by the critical width of potential fractures as discussed in Section 5.2.1.

The report will be modified by adding the following comments after the first paragraph on Page 3-10:

> "The above calculations of critical fracture width are based on the simplifying assumption of cylindrical geometry. If the fracture is to be elongated, then the minimum width for convection is reduced, although the results are of similar order to those given above. Wooding's (1960) study may be used to estimate a minimum fracture width of approximately 0.4 millimeter for an aspect ratio (length/width of the fracture) of 20 and approximately 0.2 millimeter for an aspect ratio of 100. As aspect ratio increases, the irregularity of fracture surfaces and occasional fill materials will tend to interfere progressively with the convective process so that modeling as long, thin, parallel-sided fractures is inappropriate. It is likely, therefore, that the cylindrical model gives a reasonable and conservative order of magnitude calculation of the critical fracture width for solute-driven convection."

Comment No. 14

"Page 3-10, 2nd paragraph: A basis or reference for the statement "It is doubtful whether single fractures of one millimeter or more in aperture could remain open and continuous in Anhydrite I" should be provided. While the drilling in the Castile Formation has not indicated any significant fluid producing fractures in Anhydrite I, they have been observed in the higher anhydrites of the Castile. The most notable example of a fracture occurs at WIPP-12 about 3,010 feet below land surface. This fracture is capable of producing several hundred gallons per minute of flow and it could be classed as open and continuous."

Response:

Fluid pressures in the brine reservoir intercepted by WIPP-12 and ERDA-6 are substantially greater than those in the Bell Canyon aquifer, extending up to about 70 percent of the calculated lithostatic value. As a result, fluid pressure may be expected to keep fractures open within a brine reservoir even if there is a general tendency for plastic deformation to close them. In the case of a fracture connecting the lowermost Castile anhydrite to the DMG, however, the fluid pressure would be less able to keep the fracture open because the aquifer cannot be pressurized by deformation in the same way as a small localized reservoir can. Thus, it is to be expected that fractures connected to the Bell Canyon aquifer will be mechanically much less stable than those within limited pressurized reservoirs.

The first sentence of the second paragraph on Page 3-10 will be modified to read:

> "There is some doubt as to the long-term stability of a one millimeter fracture in a deformable medium such as the Anhydrite I unit of the Castile Formation."

Comment No. 15

"Page 3-10, last line: The validity of the equation $Ns \approx 0.1 R_s^{1/3}$ should be examined. It appears that this relationship was originally derived by Elder (1967) although this report attributes it to Golitsyn (1979). Elder (1967) presented data which indicate that the above equation is valid for $\approx 5 \times 10^8 < R_s < \approx 5 \times 10^{10}$. Elder (1967) has other relationships for $R_s < \approx 5 \times 10^8$, but none for $R_s > \approx 5 \times 10^{10}$. The value of R_s used in the calculation involving the above equation is 1.2 x 10^{21}, which is many orders of magnitude higher than the known range of valid R_s values for that equation ($\approx 5 \times 10^8 < R_s < \approx 5 \times 10^{10}$)."

Response:

From a review of the literature discussed by Golitsyn (1979), Warner and Arpaci (1968), and their references, it appears that the relationship $Ns \approx 0.1 R_s^{1/3}$ is both a theoretically predicted and experimentally observed limit for very high Rayleigh numbers ($R_s \gg 10^6$, Warner and Arpaci, 1968). While experiments only appear to have been performed to Rayleigh numbers on the order of 10^{10} (Golitsyn, 1979), no substantial deviations from this relationship have been observed experimentally. The data of Knapp and Podio (1979) provide a check on the validity of this Nusselt-Rayleigh relationship at a very high Rayleigh number (10^{19}). At this Rayleigh number, the predicted Nusselt number is within an order of magnitude of that obtained from Knapp and Podio's empirical dispersion coefficients. This tends to confirm the general applicability of the above equation in the approximate manner employed by the report.

Comment No. 16

"Page 3-11, 2nd paragraph: The comparison of the dispersion coefficients calculated from the Knapp and Podio (1979) experiments to the diffusion coefficient could be erroneous. This comparison is made on Page 3-11 of the report as support for the contention that convection mass flux is 10^6 times higher than diffusive mass flux. Knapp and Podio (1979) treated the salt transport as a purely dispersive process. Wooding (1959), who studied the same phenomenon, included both a convection term and a diffusive term in his analysis. The large value of the Knapp and Podio (1979) dispersivity estimates tend to indicate that convection is occurring. Essentially, the dispersion coefficient determined by Knapp and Podio (1979) approximates the convection of brine as a dispersive process.

Knapp and Podio (1979) performed four experiments in their study of salt transport in boreholes. Three tests were run in a bore tube with a diameter of four inches. Two of these tests were run with an induced velocity in the borehole; one was run with no induced velocity. The fourth test was run in a two-inch-diameter borehole and had no induced velocity. The calculated dispersivities ranged from 45 cm^2/sec to 48 cm^2/sec for experiments run in the four-inch bore tube and was 12 cm^2/sec in the two-inch bore tube. Knapp and Podio (1979) concluded that the dispersivity depends on the cross-sectional area of the bore tubes. If these dispersivities are corrected for the "radius of a fracture" of 0.001 meter, the dispersivity would be very small, say on the order of 10^{-6} m^2/sec. This would yield a Nusselt number of about 10^3 instead of 10^6 and would dispute the contention that convection mass transport is 10^6 times higher than diffusive mass transport."

Response:

The discussion in the draft report is aimed at deducing the convective mass transport from Knapp and Podio's dispersivity estimate and comparing it with predictions based on the relationship N_s Z 0.1 $R_s^{1/3}$. The fact that Knapp and Podio obtained empirical dispersivities which are six orders of magnitude greater than the diffusion coefficient implies that convective transport in their experiments is six orders of magnitude greater than diffusive transport, i.e., N_s Z 10^6. This observation is in reasonably good agreement with convection theory which predicts an N_s of about 3 x 10^5 (Page 3-11 of the draft report). Thus, the experiments confirm the general validity of the Nusselt-Rayleigh relationship at a very high Rayleigh number (approximately 10^{19}).

Knapp and Podio observed some dependence of dispersivity on tube diameter. They also found a dependence of dispersivity-salinity relationships on tube diameter. Although such effects are not predicted in the simple convection theory used in the report, they may be quantitatively understood by considering the nature of the experiments and the fitting procedures used. First, Knapp and Podio's experiments started with a concentration discontinuity in their simulated well bore which is expected to lead to short-term transient transport effects. The initiation of rapid convection in such a system could depend on tube diameter although in the long-term, as steady state is approached, tube diameter is expected to be relatively unimportant. Secondly, by forcing actual convective transport data to fit a diffusion (or dispersion) equation, Knapp and Podio introduced empirical parameters which cannot readily be justified. Although their data are of considerable interest, their theoretical treatment is very limited and impossible to extrapolate with confidence. Thus, the complex dispersivity composition and dispersivity tube width relationships they obtained cannot be put into a good theoretical framework because they apply to a physical process different from the one actually observed. In such a case, a relationship between dispersivity and tube diameter may appear to be present but simply be an artifact of the fitting procedures used. There is certainly no justification for a linear extrapolation of Knapp and Podio's apparent dispersivity-width relationship in the manner suggested by the comment.

In conclusion, Knapp and Podio's experiments provide a reasonable test of the Nusselt-Rayleigh number relationship at high values of R_s, but their data cannot be extrapolated using dispersion theory. Paragraph 2 of Page 3-11 will be modified to clarify the discussion. The first sentence will be revised to read:

> "Some recent experiments on salt transport in wide bore tubes (Knapp and Podio, 1979) tend to confirm the approximate convective mass transport relations derived and discussed here."

The third sentence will be reworded as follows:

> "They found that the data, although produced by a vigorous convective process, could be modeled by using a semiempirical equation of the same form as the diffusion equation (Fick's second law):"

Comment No. 17

"Page 3-12, 5th line from top: The reason for believing that fractures of one millimeter or more are unlikely to exist should be given. Wooding's (1960) results indicate that convection in a fracture of less than one millimeter width can exist."

Response:

Discussions of the long-term stability of fractures and critical fracture widths for convection are presented in the responses to Comment

Nos. 13 and 14. In response to this comment, the first two sentences of the second paragraph on Page 3-12 will be changed as follows:

> "Although the long-term stability of large fractures in Anhydrite I is open to doubt, it is conceivable that zones of small fractures, 0.01 millimeter or less in width, could remain stable for extended time periods. Although they would be below the critical radius for convection (approximately 0.2 to 1.5 millimeters), convection could occur in multiple fracture zones where inflow takes place in some fractures and descent in others, similar to the method discussed by Anderson and Kirkland (1980)."

Comment No. 18

"Page 3-14, 3rd paragraph: If a fracture were to propagate itself, (i.e., dissolve only the salt directly above it) it would reach the repository in less than 20 years at a rate of 28 cubic meters per square meter per year.

It seems very unlikely for a front to propagate as a square tunnel. Does any literature exist or has any been reviewed to indicate what shape forms when salt dissolves?"

Response:

D'Appolonia has reviewed the available literature on cavity shapes resulting from salt dissolution. Very little information is applicable to the problem addressed by the DMG report. The most comprehensive work that has been found (Jessen, 1973) is for solution mining where the principal dissolution mechanism is injection of large volumes of fresh water. The principal dissolution mechanism for the Castile halite is natural solute-driven convection due to the vertical density gradient in a fracture or porous zone resulting from the variation in salt concentration.

Salt dissolution within a cavity is a very complex mechanism depending on many variables, including concentration (degree of saturation) and chemical composition of the solute fluid, temperature, viscosity, pressure, size and shape of the dissolution cavity, effects of surface irregularities and insoluble inclusions as well as lamination of layers of different mineralogical composition (solubility), and the angle of contact between the salt and fluid. A discussion of this very complex mechanism in full detail is beyond the scope of the report. The shape of a dissolution cavity formed by this process cannot be analytically predicted in terms of the relative rates of vertical and horizontal spreading as the cavity enlarges. This would be a complex function of the flow patterns within the cavity and the material characteristics of the halite formation. However, certain simplifications and assumptions allow an estimate of the size and shape of the dissolution cavity to be made.

D'Appolonia has evaluated data on solutioning presented by Jessen (1973) and has used it to evaluate the potential dissolution of the Castile halite. The shape of the dissolution cavity would be affected mainly by:

- o Impurity (laminae layers) content in the halite.
- o Chemical and physical properties of these impurities, mainly their solubility ratio (impurity versus pure salt).
- o Effect of the angle of contact between the salt and solution fluid. The maximum rate of salt removal occurs when the contact surface is inclined about 70 degrees from vertical and the fluid is below it.

After applying various simplifications and assumptions, it was determined that the dissolution cavities would resemble a rounded trapezoid with the base slightly shorter than the top and a general width to height ratio of approximately 1:1 for pure halite. WIPP-12 analysis results indicate that the 2 to 5 percent range of anhydrite impurities is representative of the Halite I Formation. Considering the inclusion of anhydrite impurities in the Castile and Salado formations, the theoretical cavity ratio (width to height) could be as large as 10:1 (assuming a dissolution ratio of halite to anhydrite of 157:1). These results show that assuming a rectangular tunnel geometry (1:1 width to height ratio), or a cylindrical shape (2:1 width to height ratio) as for the worst case analysis, is conservative in terms of upward movement of a dissolution cavity.

The first sentence of the last paragraph on Page 3-14 will be modified as follows:

> "The potential dissolution front would probably take the form of a cavity whose shape is governed by a very complex mechanism depending on many variables. Using the available information from the literature (Jessen, 1973) and adapting it for the Castile and Salado halite dissolution problem, it was determined that the cross-section of the cavity may resemble a rounded trapezoid with the base slightly shorter than the top. The width to height ratio is estimated to be about 1:1 for pure halite and, considering the influence of anhydrite impurities, the ratio may be as high as 10:1. To simplify further calculations, a conservative rectangular tunnel shape

(width to height ratio of 1:1) was used. If the dissolution front were to propagate as such (Figure 3-7), then for a 1.5 millimeter wide fracture it would have advanced less than 0.2 meter in one year and less than 20 meters in 10,000 years."

Comment No. 19:

"Page 3-15, 1st line to 6th line from top: This calculation assumes that there is no flow or dispersive flux through the DMG. What is the effect of flow and dispersion through the DMG on the time for salinity buildup to saturation? It is possible that a fracture extending into the DMG could transport the salt away toward the reef at a high rate and saturation would never be reached. It is highly probable that, due to the sparse drilling activity in the DMG, vertical fractures were missed during drilling."

Response:

The calculation on Page 3-15 dealing with time to reach saturation for the case of a large fracture connecting Castile halite and Bell Canyon Formation is not significantly affected by dispersion and ground water flow. The latter parameters are only important if the fracture density becomes extremely small such that the dissolution rate approaches that expected for diffusive flux. Since the calculated salt transport rate of 6×10^4 kg/m^2 is many orders of magnitude greater than the rate at which the Bell Canyon aquifer can remove salt from a fracture, it is reasonable to ignore the effects of flow and dispersion for the purposes of the illustration.

The remainder of Comment No. 19 involves a speculation that transport within fractures in the DMG could be more important than porous media flow. If there were any indication in the current data base that fracture permeability is important over a significant portion of the basin, then it could be addressed. Available permeability measurements and drill stems test data indicate very low values of permeability and we have incorporated these available measurements into our modeling study. Further discussion of the effects of fractures in the DMG is given in the response to Comment No. 5.

Comment No. 20

"Page 4-4, "Hydraulic Conductivity" section: The range of hydraulic conductivity should be extended from 1 md to 59 md (0.3 m/year to 18 m/year). See comment regarding Page 2-8."

Response:

Consistent with the response to Comment No. 6, the fifth sentence of the last paragraph on Page 2-8 will be revised to read:

"The hydraulic conductivity of the Bell Canyon aquifer [based on core sample measurements (Hiss, 1975a)] ranges from 1.1 to 2.9 meters per year and averages approximately 1.8 meters per year. One measurement of hydraulic conductivity of 18 meters per year has also been reported; however, it does not appear representative of the basin."

The first sentence of the third paragraph on Page 4-4 will be reworded as follows:

"Based on laboratory measurements of the core samples (Hiss, 1975a) and analysis of drill stem tests in Borehole AEC-7 as discussed in Section 2.2, the representative hydraulic conductivity of the Bell Canyon aquifer ranges from 1.1 to 2.9 meters per year with a weighted average value of 1.8 meters per year.

Comment No. 21

"Page 4-5, "Chloride Concentrations" section: The chloride data in Hiss's (1975a) Figure 26 tend to confirm the existence of the 100 kg/m^3 contour on the upgradient end of the Bell Canyon aquifer."

Response:

The Bell Canyon aquifer flow direction near Whites City is toward the north which is not the direction of flow observed through the section (Figure 4-1) used for the salt dissolution analyses (northeastward). Chloride concentrations in this section generally increase in the direction of flow and no chloride concentration measurements were available at the upgradient end of the section which is approximately 20 kilometers southeast of Whites City. As a result, it is believed that there is little evidence for the existence of the 100 kg/m^3 contour in this particular area.

The potential dissolution associated with the Bell Canyon aquifer mass transport capacity was evaluated through average dissolution calculations and two implausible worst case analyses. These analyses are not sensitive to the existing chloride concentration identified near Whites City. The upgradient chloride concentration of 100 kg/m^3 in the 16,500 meter section (Figure 4-1) used for the average and worst case dissolution analyses is well defined by existing data (Hiss, 1975a).

Comment No. 22

"Page 4-6, Section 4.3.1: This is a good approach to use as a first approximation because the method is insensitive to whether the source of salt is by diffusion from above, convection from above or from some other source. In essence, the amount of calculated salt input by this method can be assumed to be from dissolution of overlying halite. Thus the amount of halite dissolved is probably overestimated. However, the basic assumption in this model as applied to the DMG is porous media flow in the DMG aquifer. In addition, the model presented here does not include longitudinal dispersion, which would tend to increase the amount of salt dissolved. Is dispersion insignificant in this case?"

Response:

The discussion in Section 4.3.1 describes the average rate of salt dissolution from the Castile and Salado formations assuming that the Bell Canyon aquifer controls the rate at which salt can be removed from the dissolution zone. For this approach, the mechanism by which salt dissolves is not important; the aquifer mass transport capacity is the key parameter. The product of aquifer flow rate and salt concentration increase in the direction of flow beneath the dissolution area yields the average dissolution rate. An illustration of this process, including dispersion, is provided in Figure 4-4 which shows the numerical model results.

Consideration of the dissolved salt transport in the Bell Canyon aquifer based on porous media flow theory is justified based on the available data on the DMG. (See response to Comment No. 5 for further discussion of the possibilities and effects of fractures in the DMG.) Section 5.1 of the report presents a discussion of the dissolution rate as computed by this technique with respect to possible parameter variations. Fracture flow would have to be significant and widespread to impact the calculations presented in the report.

As part of the sensitivity analysis described in Appendix B, the effect of longitudinal dispersion in the Bell Canyon aquifer on salt dissolution was investigated. The results of the analysis are presented in Figures B-1(F) and B-1(G) and show that salt dissolution is insensitive to the magnitude of longitudinal, as well as transverse, dispersion. Similarly, the effect of longitudinal dispersion on the hand calculation results in Section 4.3.1 is insignificant.

Comment No. 23

"Page 4-7, 8th line from top: The mass of salt dissolved per year or 10,000 years should be presented here. Also, the mass flux and rate of salt being dissolved from underneath the WIPP site should be presented for comparison purposes. An EEG calculation indicates these values are 4.1×10^{-4} kg/yr/m^2 and 0.31 cm/10,000 years, respectively."

Response:

The variation of salt dissolution along the 16,500 meter section beneath the WIPP facility is shown by the dimensionless mass flux curve in Figure 4-4. The hand calculations presented in Section 4.3.1 are intended only to give approximate ranges of average salt dissolution. More detailed results are given in Section 4.4.2. The mass flux curve shows that the mass flux directly beneath the WIPP facility (Zone II) ranges from 80 to 70 percent of the average mass flux rate (Note 5 in Figure 4-4). Accordingly, the mass dissolution rate of salt varies from 6.6 x 10^{-4} to 5.8 x 10^{-4} kg/yr-m^2. For a salt density of 2,160 kg/m^3, the mass rates correspond to vertical movements of 0.31 and 0.27 centimeter per 10,000 years, respectively.The third sentence on Page 4-7 of the report will be clarified as follows:

> "Based on a flow rate of 0.135 m^3/yr-m and the observed chloride concentration profiles, the average thickness of salt removal in the basin was calculated to range between 0.07 and 0.62 centimeter in 10,000 years. This corresponds to a chloride concentration difference (ΔC) between upgradient and downgradient ends of the aquifer varying from 10 to 100 kg/m^3 across the basin, resulting in a salt removal rate ranging from 2.2 to 22.2 kg/yr-m, respectively."

The following statement will be added after the last sentence of the first paragraph on Page 4-7:

> "The calculated variation of salt dissolution along a 16,500 meter section beneath the WIPP facility is presented in Section 4.4.2 which describes numerical model results."

Comment No. 24

"Page 4-9, 2nd paragraph: It would be interesting to see the amount of salt that can be dissolved by the mechanism described in this paragraph. Would it be large enough to dissolve Salado salt laterally from the reef to the repository? Would it also be large enough to account for the amount of salt being transported by the Capitan Reef aquifer? However, the decreasing concentration of chloride downgradient along the eastern side of the Capitan Reef (see Page 4-5) tends to indicate that convective dissolution of the overlying Salado is not occurring in this part of the aquifer. Active convective dissolution would tend to increase the chloride."

Response:

See discussion on General Comment V regarding the potential dissolution associated with the Capitan Reef.

Comment No. 25

"Page 4-10, 2nd and 3rd paragraphs: The mass balance model described here should have a longitudinal dispersive term included."

Response:

The discussion on Page 4-10 refers to the numerical modeling of salt dissolution and subsequent transport in the Bell Canyon aquifer. In the modeling analysis, both longitudinal and transverse dispersion were included. A detailed presentation of the equations of flow and mass transport, which are solved in the numerical model and include longitudinal and transverse dispersion, is presented in Appendix A. The sensitivity analysis, which includes these terms, is discussed in Section 4.4.3 and Appendix B of the report. It demonstrates that the dispersivity of the Bell Canyon aquifer has negligible effect on the salt dissolution rate.

Comment No. 26

"Page 4-13, 8th line from bottom: The average value of vertical removal of 0.34 cm per 10,000 years obtained from the numerical approach agrees very well with the 0.31 cm per 10,000 years obtained from the analytical approach. What is the range of vertical salt removal over the 16,500 m long line underneath the repository?"

Response:

Figure 4-4 shows that along the 16,500 meter length the vertical salt removal varies from approximately 60 percent greater than to 40 percent less than the average salt removal rate for the entire section. Taking the average rate as 0.31 centimeter per 10,000 years, the range of vertical salt removal is 0.50 to 0.19 centimeter per 10,000 years.

Comment No. 27

"Page 4-14, Section 4.4.3: The sensitivity analysis with respect to hydraulic conductivity should be extended to 18 m/yr. See comment regarding page 2-8."

Response:

See response to Comment No. 6.

Comment No. 28

"Page 4-16, 3rd to 6th line from top: One of the reasons the numerical approach concluded diffusion as the source of salt to the DMG is that the model assumed diffusion as the source to start with. The model was then calibrated to determine the diffusion coefficent, which happened to be in the range of acceptable values. It can only be concluded that diffusion is a possible explanation but by no means the only one."

Response:

The purpose of the numerical analysis was to illustrate, using an accurate representation of the geology and transport processes in the DMG, that salt dissolution by continuous diffusion along the 16,500 meter section beneath the WIPP facility could produce the observed chloride concentrations in the Bell Canyon aquifer. Although localized convective dissolution could be present, it is unlikely that convection occurs over a widespread area because the associated large mass dissolution rate would far exceed the capacity of the aquifer to remove the dissolved salt. The potential dissolution cavity sizes and shapes that could result from localized convective mechanisms are discussed in the worst case analyses of Chapter 5.0. For both an average and localized basis, convection and diffusion are presented only as examples of dissolution mechanisms that could exist in the Castile and Salado formations considering the available hydrogeologic data. The Bell Canyon aquifer salt transport capacity, which is independent of the dissolution mechanisms, is concluded to be the primary parameter controlling dissolution rates.

In the final report, the third, fourth, and fifth sentences of the paragraph beginning at the bottom of Page 4-15 will be restated as follows:

> "Both analytical and numerical calculations indicate that salt dissolution by continuous diffusion along the 16,500 meter section beneath the WIPP facility could produce the observed chloride concentrations in the Bell Canyon aquifer. Although localized convective dissolution may be present, it is unlikely that convection occurs over a widespread area because the associated large mass dissolution rate would far exceed the capacity of the aquifer to remove the dissolved salt."

Comment No. 29

"Page 4-16, last paragraph: Has an estimate of the rate of salt dissolution from the reef toward the repository been obtained? Page 4-9 indicates that the reef transports 20×10^6 to 440×10^6 kg/year of chloride, of which only about 3×10^6 kg/year is accounted for. If the remainder of the chloride transported by the reef comes from the brine density flow indicated here, how large a cavity would form in the Salado? What is the structural integrity of such a cavity? How fast would a cavity advance toward the repository? No sound basis is provided for the argument that the salt removal potential of the reef will not affect the repository."

Response:

See discussion on General Comment V concerning potential dissolution associated with the Capitan Reef.

Comment No. 30

"Page 5-2, 6th line from bottom: "0.34 centimeter" should read "0.31 centimeter." It appears that this paragraph is discussing the rates of dissolution determined from the analytical model. Page 4-7, third paragraph, indicates that the amount of salt dissolved is 0.3 centimeter in 10,000 years."

Response:

In the analytical evaluation of salt dissolution, a chloride concentration increase of 50 kg/m^3 (100 to 150 kg/m^3) based on field data was used to represent the salt dissolution along the Bell Canyon aquifer. This increase corresponds to an average dissolution rate of 0.31 centimeter per 10,000 years. The model predicted an increase of approximately 55 kg/m^3 chloride or 0.34 centimeter salt removal per 10,000 years. Due to the relatively small amount of concentration data available and the assumptions required to perform some of the calculations, the difference between 0.31 and 0.34 centimeter per 10,000 years as the average dissolution rate is insignificant. Solely for comparison purposes, the model result of 0.34 centimeter was adopted in Section 5.1.

Comment No. 31

"Page 5-3, 1st line: "0.7" should be "0.6." The dissolution process described on Page 5-2 has a linear relationship between flow thickness and dissolution height. If the aquifer thickness is doubled, the dissolution height should double."

Response:

As discussed in the response to Comment No. 30, the average salt removal rate of 0.34 centimeter per 10,000 years, as determined from the model results, was used for comparative purposes in Section 5.1. Given the same 55 kg/m^3 chloride concentration increase in the Bell Canyon aquifer, doubling the aquifer thickness to 60 meters would increase the salt removal rate to 0.68 centimeter, or approximately 0.7 centimeter per

10,000 years. The difference between using a 0.31 to 0.62 centimeter increase or a 0.34 to 0.68 centimeter increase is insignificant for the purposes of the illustration.

To clarify the discussion, the second paragraph on Page 5-2 will be reworded. The ninth sentence in the second paragraph will be revised to read:

> "For a Bell Canyon aquifer flow rate of 0.135 m^3/yr-m and a chloride concentration increase of 55 kg/m^3 (numerical model results), the average dissolution cavity height in 10,000 years would be 0.34 centimeter."

The eleventh sentence will read:

> "If the aquifer thickness is assumed to be 60 meters rather than 30 meters, the average salt removal corresponding to a 55-kg/m^3 chloride increase would be 0.68 centimeter per 10,000 years and 17 centimeters in 250,000 years."

Comment No. 32

"Page 5-4, 5th line from top, ref. Figure 5-2: The "dissolution controlled by diffusion" curve on Figure 5-2 does not pass through the point defined by flow rate = 0.135 m^3/yr/m and height = 0.34 cm. The results of the numerical modeling as presented on Page 4-13 indicate that flow rate = 0.135 m^3/yr/m and height = 0.34 cm is the solution to the numerical modeling problem. Is the "dissolution" curve in Figure 5-2 correct?"

Response:

The straight-line curve in Figure 5-2, representing dissolution controlled by the Bell Canyon aquifer, is based on a constant increase in chloride concentration of 50 kg/m^3 (Note 1 in the figure) over an aquifer length of 16,500 meters. The curve defining dissolution controlled by diffusion is derived from the numerical model predictions of dissolution-induced concentration increases for varying aquifer flow rates. The diffusion-controlled dissolution curve shown in Figure 5-2 of the April 1982 draft report was constructed for illustrative purposes using a smaller diffusion coefficient than was determined by the calibration. The sensitivity analysis described in Appendix B shows the effect of the diffusion coefficient on the salt dissolution rate. The dissolution curve using the calibrated diffusion coefficient value of 8.7×10^{-3} m^2/yr (Table B-2) increases from 0.14 centimeter at 0.038 m^3/yr-m, to 0.34 centimeter at 0.135 m^3/yr-m, and remains approximately horizontal at 0.40 centimeter from 0.2 to 0.5 m^3/yr-m. Figure 5-2 of the final report has been redrafted to show the results obtained from the calibrated diffusion model.

Comment No. 33

"Page 5-6: Going back to the analytical model; for an aquifer flow rate of 0.135 m^3/yr/m and a chloride concentration change of 50 kg/m^3 in 16,500 m, underneath the repository, the amount of salt dissolved over the 16,500 m line is 4.1×10^{-4} kg/yr/m^2. If this amount of chloride all dissolved from one fracture, the amount of chloride passing through this fracture is 6.8 kg/yr/m, which is slightly less than the 10 kg/yr/m being used here. Therefore, the results presented in Figure 5-2 may be slightly higher than what can actually occur, subject to any sensitivity analysis and the assumption of porous flow in the DMG. An approach to maximize salt dissolution would be to use a chloride concentration change of 150 kg/m^3, which is the change from one end of the DMG to the other. Using this approach, one gets about 20 kg/yr/m of salt dissolution through a fracture."

Response:

The discussion beginning at the bottom of Page 5-6 and continuing through the first paragraph on Page 5-7 refers to the "worst case" dissolution of salt through a fracture as controlled by the Bell Canyon aquifer. The steady state diffusion case referred to in Comment No. 33 is discussed in Section 4.4. In a fashion similar to the procedure used to estimate average salt dissolution, the rate at which salt can be removed from a fracture (i.e., the dissolution rate) is assumed to equal the product of the aquifer flow rate and the downgradient increase in salt concentration over the concentration already existing directly below the fracture. The chloride concentration beneath the fracture was assumed to be 120 kg/m^3, which is low compared to the observed concentration below the WIPP facility, and the downgradient concentration was set equal to the maximum or saturation level of 190 kg/m^3 chloride. For a flow rate of 0.135 m^3/yr-m, the potential chloride removal rate from the fracture is approximately 10 kg/yr-m (product of 0.135 m^3/yr-m and 70 kg/m^3) which corresponds to salt removal at a 16 kg/yr-m rate. Clarification of this and other items regarding the worst case analysis in the report is presented following Comment No. 35.

Comment No. 34

"Page 5-6, 5th line from bottom: As mentioned earlier, work by Wooding (1960) indicates that convection can occur in fractures smaller than 1.5 millimeter."

Response:

This comment is addressed in the responses to Comment Nos. 13 and 35.

Comment No. 35

"Page 5-6, 2nd paragraph: The calculation of fracture width is not quite clear. If a fracture is capable of transporting 6×10^4 kg/m^2/yr (Page 3-11) of salt, it is capable of transporting 3.64×10^4 kg/m^2/yr of chloride. If fracture width is calculated by QWC/3.64×10^4 kg/m^2/year, then the fracture width curve in Figure 5-4B should be lowered. Again, Wooding (1960) indicates convection can occur in fractures with an aperture smaller than 1.5 mm."

Response:

The salt transport rate of 6×10^4 kg/yr-m^2 through a fracture, referred to on Page 3-11, is the rate that could develop due to convective mass transport in a fracture. However, since 6×10^4 kg/yr-m^2 is four orders of magnitude greater than the Bell Canyon aquifer's capacity to remove the salt from the fracture, this dissolution rate is unrealistic. As discussed in the response to Comment No. 33, a conservative estimate of the Bell Canyon aquifer's capacity to remove chloride from a fracture is 10 kg/yr-m (16 kg/yr-m salt removal). Potential cavity sizes above a fracture were estimated for a range of chloride removal rates (0 to 30 kg/yr-m) as shown in Figure 5-4. For a given removal rate, the fracture width is the minimum width that could transport the indicated amount of chloride from the dissolution zone down to the Bell Canyon aquifer (100 meters) by a combination of convective and diffusive mechanisms. The magnitudes of convective and diffusive transport that could develop were determined by combining Equations (3-1), (3-5), and (3-6) to give an analytical expression for total salt transport through a fracture as a function of the diffusion coefficient, fracture height, fluid viscosity, concentration difference between the top and bottom of the fracture, and the fracture width. By holding the chloride concentration difference constant at 70 kg/m^3 (190 to 120 kg/m^3 variation) and equating salt transported through the fracture with salt removed by the aquifer, the fracture width becomes a linear function of the Bell Canyon salt removal rate. For a particular salt removal rate, a larger fracture than indicated on the curve of Figure 5-4 could produce the same dissolution rate.

To better explain the fracture width calculation, a revision in the text of Section 5.2.1 will be made. The two sentences beginning on the ninth and twelfth lines of Page 5-6 will be rephrased as follows:

> "Similar to the procedure used to estimate average salt dissolution, the rate at which salt can be removed from the fracture (i.e., the dissolution rate) is assumed to equal the product of the aquifer flow rate and the downgradient increase in salt concentration above the concentration existing directly below the fracture. For the worst case analysis, the downgradient chloride concentration is the maximum or saturation value, 190 kg/m^3 (315 kg/m^3 salt concentration)."

The second paragraph on Page 5-6 will be revised to read:

> "Figure 5-4(A) illustrates the rate of development of a cylindrical cavity for a chloride removal rate of 10 kg/yr-m (16 kg/yr-m salt removal rate). The dissolution rate is based on an aquifer flow rate of 0.135 m^3/yr-m and a chloride concentration increase from 120 kg/m^3 beneath the fracture to 190 kg/m^3 downgradient of the fracture. Figure 5-4(B) illustrates the potential cavity sizes in 10,000 years for a range of chloride removal rates. Also shown are the minimum fracture widths required to transport the indicated chloride removal rates. For a given removal rate, the fracture width is the minimum width that could transport the indicated amount of chloride from the dissolution zone down to the Bell Canyon (100 meters) by a combination of convective and diffusive mechanisms. The magnitudes of convective and diffusive transport that could develop were determined by combining Equations (3-1), (3-5), and (3-6) to give an analytical expression for total salt transport through a fracture as a function of the diffusion coefficient, fracture height, fluid viscosity, concentration difference between the top and bottom of the fracture, and the fracture width. By holding the chloride concentration difference constant at 70 kg/m^3 (190 to 120 kg/m^3 variation) and equating salt transported through the fracture with salt removed by the aquifer, the fracture width becomes a linear function of the Bell Canyon aquifer salt removal rate. For a particular salt removal rate, a larger fracture than indicated on the curve of Figure 5-4 would produce the same dissolution rate.

The paragraph beginning at the bottom of Page 5-6 will be reworded as follows:

> "A chloride removal rate of 10 kg/yr-m (equivalent to a halite dissolution rate of about 16 kg/yr-m) requires a minimum fracture width on the order of 0.3 millimeter and may result in a dissolution cavity, based on the geometry shown in Figure 5-3, with a radius of approximately 7 meters in 10,000 years for the implausible worst case. Figure 5-6 illustrates the computed hypothetical cavity relative to the stratigraphy beneath the WIPP facility. The computed width of fracture is a minimum value required to sustain the indicated chloride transport rate. If the fracture width were greater than the minimum value, the rate of salt removal would be

> unchanged. This is because the dissolution rate is limited to 16 kg/yr-m due to the Bell Canyon aquifer salt removal capacity."

Comment No. 36

"Page 5-12, bottom paragraph: The simple statement that 400 m thickness over a 1 m cavity with a 94 m diameter should be enough structural support is weak and not convincing. Some more justification of this idea should be provided."

Response:

The size of the potential cavity (either the seven meter radius or one meter height with 93 meter diameter) in comparison with the overburden strata thickness of more than 400 meters between the WIPP facility floor and the potential cavity is negligible. The very slow propagation of the dissolution front would be even slower than the closure due to salt creep, which means that the potential cavity could never reach the theoretical dimensions used in the discussion. Also, the overall deformation of the overburden would have only limited upward propagation and would most likely occur only within the lower portions of Halite I.

The third and fourth sentences of the last paragraph on Page 5-12 will be reworded as follows:

> "As is evident in Figure 5-6, more than 400 meters of overburden exist between the implausible worst case potential cavities and the floor of the WIPP underground facility. As a result, the propagation of the dissolution front would cause a creep deformation of the overburden salt prompting closure of the cavities. Considering the extremely small volume of salt removed in comparison with the total strata thickness, the vertical propagation of this deformation would probably be limited to the lower section of the Halite I Formation."

Comment 37

"Table 4-1: What is the basis for the dispersivity of 3.048 meters shown in Table 4-1?"

Response:

Values of longitudinal dispersivity ranging between 0.01 and 100 meters and transverse dispersivity values of between 0.001 and 50 meters have been used in mass transport evaluations (Freeze and Cherry, 1979). The dispersivity is a function of grain size distribution, anisotropic characteristics of the porous medium, and the tortuosity of the medium. In the simulation of salt dissolution in the DMG, a value of 3.048 meters (10 feet) was used for both longitudinal and transverse dispersivity. This value was selected based on literature data for similar materials (Freeze and Cherry, 1979 and Bear, 1975); but, due to the inherent variability of this parameter, a range of dispersivities (0.3048 to 30.48 meters) was investigated in the sensitivity analysis. Figure B-1 shows that the salt dissolution rate is very insensitive to the dispersivity of the Bell Canyon aquifer, indicating that the use of 3.048 meters in the salt dissolution simulation has little impact on the conclusions of the study.

REFERENCES

The following references are used in the responses to Comment Nos. 1 through 37 and General Comment Nos. I through VI. Each reference appears in the format that will be used in the final report:

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FRACTURE FLOW IN THE RUSTLER FORMATION

SAND 82-1012

"Equal Opportunity Employer"

STATE OF NEW MEXICO

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HEALTH & ENVIRONMENT department

September 8, 1982

Mr. Joseph M. McGough
Project Manager of WIPP
WIPP Project Office
U.S. Department of Energy
Albuquerque Operations Office
P. O. Box 5400
Albuquerque, New Mexico 87115

Dear Mr. McGough:

Enclosed are our review comments regarding the draft report "Fracture Flow in the Rustler Formation: Waste Isolation Pilot Plant (WIPP), Southeast New Mexico (Draft Interim Report)", SAND82-1012, May, 1982, Sandia National Laboratories.

We are looking forward to hearing your comments regarding our review.

Sincerely,

Robert H. Neill
Director

RHN:eg

2-058AG2-18-2-1-1

Enclosure

cc: TSC, IEA
W. Weart, Sandia

REVIEW COMMENTS

CONCERNING

FRACTURE FLOW IN THE RUSTLER FORMATION:

WASTE ISOLATION PILOT PLANT (WIPP)

SOUTHEAST NEW MEXICO

(DRAFT INTERIM REPORT)

Comments by

Environmental Evaluation Group

Environmental Improvement Division

N. M. Health and Environment Department

P. O. Box 968

Santa Fe, NM 87503

September 8, 1982

REVIEW OF "FRACTURE FLOW IN THE RUSTLER FORMATION: WASTE ISOLATION PILOT PLANT (WIPP), SOUTHEAST NEW MEXICO (DRAFT INTERIM REPORT)"

INTRODUCTION

A detailed review of the draft interim report entitled "Fracture Flow in the Rustler Formation: Waste Isolation Pilot Plant (WIPP), Southeast New Mexico" has been made by staff members of the Environmental Evaluation Group. The draft report presents some new information and ideas regarding the hydrology of the Rustler aquifer, most notably the direction of flow and the transport capabilities of the aquifers. The following comments are intended to get more clarification of some of the data interpretations presented in the report and to present some additional concerns the Environmental Evaluation Group has regarding flow in the Rustler Formation.

The "Costs and Merits Evaluation for Stipulated Agreement Activities" (pages 37 and 38) attached to the August 31, 1981 letter from Schueler to Goldstein does not clearly state the items that are to be addressed in the interim report. However, the comments represent the concerns of the Environmental Evaluation Group at this time and should be addressed in the final report regarding fracture flow in the Rustler, which is due during February, 1983.

DISCUSSION

The following items were referred to in the "Cost and Merits Evaluation for the Stipulated Agreement Activities," and subsequent correspondence between Schueler and Goldstein. We believe they were not adequately addressed in the Interim Report.

Proposed Work

- The best model to represent the flow path and aquifer characteristics in the Rustler will be developed.

Expected Results

- If the results are such that additional consequence analyses should be run, they will be incorporated into the study.

Schueler Letter

Item 10 in the October 30, 1981 letter from Schueler to Goldstein reads: "If results of tracer studies warrant, a model for multiple fracture flow will be developed. Discrete fracture flow (a one fracture flow path) is not considered to be a credible mechanism; this will be indicated and discussed in the report(s) on the Rustler aquifers."

It is recognized that some of these omissions would logically be presented in the final, rather than the interim report. However, it appears the fracture flow study is being conducted on two premises that have not yet been justified:

1. That the Culebra aquifer is critical and thus it is not necessary to study the Magenta aquifer. The conclusions that initial flow in the Culebra is to the southeast away from Nash Draw raises the question of whether flow in the Magenta may reach the more permeable Nash Draw area in shorter time. (see our comments regarding page 43). It is noted that the Cost and Merit Evaluation refers to Rustler aquifers and makes no mention of restricting investigations and evaluations to the Culebra aquifer.

2. That discrete fracture flow (a one fracture flow path) is not credible. A justification of this conclusion was promised in the October 30, 1981 letter and is necessary before one can dismiss the need to model discrete fracture flow.

GENERAL COMMENTS

Portions of the report summarize the transmissivity, storage coefficient, porosity and dispersivity of the various field test data. It appears that the details of these tests are presented in Bentley et. al. (1981, in preparation), Ward and Gonzalez (1981, in preparation) and Gonzalez et. al. (in preparation), which are not yet available. It would have been very useful if these reports were available for the Environmental Evaluation Group to consult while reviewing the draft fracture flow report. In addition, Walter (1981, in preparation) would have been useful to review in order to evaluate the three-well technique for anisotropy. These reports should be made available to the Environmental Evaluation Group prior to issuance of the final report on fracture flow.

The report summary could address several additional items. At present it concludes the following:

- Culebra flow through the WIPP site is to the southeast
- the direction of the major component of the transmissivity tensor is from northwest to southeast
- depth averaged hydraulic conductivities decrease eastward from Livingston Ridge.

It also summarizes the transmissivities storage coefficients and porosities determined from the various tests. An additional item that needs to be addressed in the summary section concerns the nature of the fracture hydraulic conductivity. The report mentions double porosity flow (page 23) and discrete flow (page 35 and 44) but uses methods of analyses developed by Papadopulos (1965), Grove and Beetem (1971) and Sauty (1980), which are based on porous media flow. The report appears to conclude (page 35 and 44) that the dominant mode of transport in the Culebra is through discrete fractures. If this is true, the data may have to be reevaluated with models of flow through discrete fractures. A conclusion concerning the nature of fracture flow should be made before the regional transport model is developed.

The physical meaning of the values of the transmissivity storage coefficient, anisotropy, porosity and dispersivity in terms of fracture flow should be addressed.

For instance, is the anisotropy due to alternating vertical bands of highly transmissive rock and low transmissive rock that trend norhtwest to southeast or due to karst channels recharging the aquifers.

The porosity of 18% determined from the H-2 nest of wells appears high for a fractured rock. At the H-6 nest of wells the porosity appears to exhibit some directional characteristics, when it should not.

In addition, the data presented in the report indicate that Culebra flow in the repository area is to the southeast. Previous studies have indicated that flow is to the south and southwest. This data may indicate other discharge areas for the Culebra. An increased flow path that might result from the gradient presented in this report indicates that the Magenta may provide faster releases of radionuclides to the biosphere than the Culebra.

SPECIFIC COMMENTS

"ABSTRACT", 13th to 18th lines

The term "principal to minor transmissivity tensor" should be changed to "major to minor components of the transmissivity tensor." The term "principal transmissivity component" should be changed to "the principal direction of the major component of the transmissivity tensor" or "The orientation of the principal axes of the transmissivity tensor is northwest by southeast for the major component and northeast by southwest for the minor component." A statement about the variability of the transmissivity with respect to distance from the outcrop should also be included in the "abstract."

page 4, 2nd paragraph

Mercer and Gonzalez (1981) indicate a strong westward gradient from WIPP to Nash Draw in the Magenta dolomite. Have any calculations of travel time from WIPP to Nash Draw been made for the Magenta and compared to travel times for the Culebra?

The Culebra's southeast gradient through the repository, as indicated on Figure 16, shows that contamination from a repository breach may either never reach Nash Draw or may take longer than the 40,000 years previously estimated. If the Culebra travel times are significantly increased, the Magenta may provide quicker radioactive releases to the biosphere than the Culebra. Estimates of travel time from the repository to Nash Draw through the Magenta should be provided in this paragraph.

What are the data and assumptions that went into the calculation of the 40,000 year travel time from ERDA-9 to Malaga Bend? Is this number taken from some other work?

page 7, 1st and 2nd lines

The sentence should be changed to read something like "the major and minor components and principal directions of the transmissivity tensor."

page 8, bottom paragraph

Figure 16 indicates that the hydraulic gradient is to the southeast through the facility. Why does this paragraph say south and then southwest toward Malaga Bend? The statement appears to be referring to the previously assumed flow path shown in Figure 3, but it certainly is not clear.

page 9

Table 1 should be checked for errors. The "Fresh Water Altitude" for P-18 on the table and the altitude used for construction of Figure 16 appear to differ by 100 feet. Other "Fresh Water Altitudes" that appear to need checking belong to wells H-5, H-8, H-9, H-10, W-28 and W-30. In addition, the surface altitude of H-9 is 100' higher than that presented in Seward ("Abridged Borehole Histories for the Waste Isolation Pilot Plant (WIPP) Studies," SAND 82-0080). This may lower the fresh water altitude to 2976 at H-9. It is probable that the fractured nature of the dolomite may be causing the anomalous water levels. The water level at H-5 appears to be associated with a structural anomaly of the Culebra Dolomite (see attached Figures 1 and 2 and comment regarding page 44 and 45).

page 11, 1st paragraph

References for transmissivity values should be provided.

page 14, 10th line

Line should read "minor components and principal directions of the transmissivity tensor."

page 14, 2 bottom lines

It would be nice if the reference were already published to check the theory and to see the report contents.

page 15, 1st paragraph

The description of test procedures indicate that the "a" well at each pad was pumped. However, the anisotropy results of Table 2, Table 3 and Table 4 provide no results from pumping the "a" well. Why are the results from pumping the "a" well not presented? Was the "a" well pumped at all?

page 15, last paragraph

If the tracer curves are insensitive to dispersion (dispersivity), how can it be estimated?

page 18, lines 6, 15 and 17

Change "principal" to "major."

page 19, Table 2

According to the theory of anisotropic aquifers developed by Papadopulos (1965), the response of well H-4C from pumping H-4B should produce the same T and S values as the response of well H-4B from pumping well H-4C. The T estimates for these wells differ by a factor of two for tests one and two. The range of T (not including pumping wells) for all three tests is 0.8 feet squared per day to 1.7 feet squared per day. What is the cause of this discrepancy?

page 23, 1st paragraph

Were any methods such as images, tried to locate any of the barrier boundaries? What could these boundaries be attributed to? For instance, could the boundaries be due to vertical fractures filled with an impermeable material or to a less fractured nearby region of dolomite?

The shape of the curve on Figure 7 is interesting. The early part of the curve (prior to the formation of the straight line) may be a result of no storage of water in the fractured part of the aquifer or possibly a horizontal fracture overlain by a porous block (see "Well Hydraulics in Heterogeneous Aquifer Formations" by T. D. Streltsova-Adams in Advances in Hydroscience, Vol. 11, Academic Press, 1978). In addition,it appears questionable that the flat part of the curve is attributable to flow from the blocks to the fractures. According to Streltsova-Adams (see Proceedings, Second

Invitational Well-Testing Symposium held by Earth Sciences Division, Lawrence Berkeley Laboratory, on October 25-27, 1978) the flat part of the data should not show if the ratio $(S_f + S_m)/S_p$ (S_f and S_m are storage coefficients of the fractures and the porous matrix, respectively) is less than about 5 to 11. In other words, if the straight lines on Figure 7 are less than about 0.7 to 1.0 log cycles apart, which they are on Figure 7, then the flat part of the curve should not show. This brings several questions to mind:

1. Is it possible that the straight line shows up between 200 minutes and 2000 minutes on Figure 7? If this is the case, then the data after 2000 minutes including the flat part may be attributable to the transition period between flow in fractures and "induced response."
2. If the straight line is correct and there is no double porosity system, is the flat part of the data and the "induced response" caused by a highly permeable fracture or karst channel near the well test?
3. Is the anistotropy observed in these tests due to recharge into the rocks from a highly permeable fracture or karst channel? Such a response would cause the lines of equal drawdown to have oval shapes rather than the elliptical ones caused by anisotropy. Unless there are data from more than three observation wells it may be very difficult to tell the difference between an anisotropic aquifer and an aquifer with a recharge boundary.

Figure 7 of the report shows the response of the H-4a and H-4b wells due to pumping the H-4c well. Was the response of H-4a and H-4c due to pumping H-4b similar to the data shown on Figure 7 such that a double porosity system was indicated?

page 23, last paragraph

It appears questionable that chloride is the ideal tracer to use to determine if leakage is occurring between Rustler aquifers at the H-4 site. Table 4 of Mercer et. al. indicates that the chloride concentration of both the Magenta and Culebra is 7500 mg/ℓ (Mercer J.W., Paul Davis, Kevin F. Dennehy, and Carole L. Goetz, "Results of Hydrologic Tests and Chemistry Analyses, Wells H-4A, H-4B, and H-4C at the Proposed Waste Isolation Pilot Plant Site, Southeastern New Mexico", Water-Resources Investigations 81-36, U.S. Geological Survey, May, 1981).

page 25, 2nd paragraph

The transmissivities as presented here are extremely small for a fractured rock and would tend to indicate that fracture flow is not that significant, at least in the areas that were tested. Any radionuclide transport in the Culebra would tend to be very slow because of the low transmissivities. Do fractures or karst channels capable of transmitting water exist near the WIPP site?

Table 9.2 of Walton (Walton, William C., Groundwater Resource Evaluation, McGraw-Hill, 1970) indicates the values of specific storage presented here are those for "sound rock." This would tend to indicate that any fractures in the Culebra, at least in the areas tested, are well cemented or that clean, open fractures are far apart. This suggests that fracture flow is not extensive over much of the Culebra but may be confined to long channels or fractures outside the area of influence of the pump tests. Does flow in open fractures exist in areas not tested by the pump or tracer tests?

page 27, 3rd paragraph

Was the aquifer pumped clear prior to tracer injection for the second test? If not, did the non-completion of the first tracer test affect the results of the second test? Is it possible that Segments I and II of Fig. 12 are due to the first test and Segments III and IV to the second test? If so, some type of deconvolution would be necessary to interpret the results.

page 28, 3rd paragraph

The Grove and Beetem (1971) model needs to be corrected for anisotropy. Was this done?

What were the ranges of porosities and dispersivities used in the Grove and Beetem (1971) analyses and how did they compare with the data?

page 32, 3rd paragraph

The significance of the porosities of 0.17 and 0.18 should be discussed here. These values are extremely high for a fractured rock. A fractured rock typically has a fracture porosity of 0.01 - 0.02 and less (Streltsova, 1976). The porosity values presented here are typical for a porous media. If it is assumed that a fractured system operates at the H-2 well sites, then tracer diffusion from the fracture into the porous matrix could account for the high porosity. The effect of this diffusive process has been shown to increase travel times from one point to another when compared to a process without diffusion into the matrix. (See Grisak and Pickens, "Solute Transport Through Fractured Media I: The Effect of Matrix Diffusion, Water Resources Research, vol. 16, no. 4, Aug., 1980, pp. 719-730 and Grisak et al., "Solute Transport through Fractured Media 2: Column Study of Fractured Till," Water Resources Research vol. 16, no. 4, Aug., 1980, pp 731-739). The net effect of increasing the travel time would be a high porosity. Grisak and Pickens also indicated that the diffusion of solute into the matrix would be more significant for low velocities of fluid flow in the fracture than for high velocities. With the hydraulic conductivity of the Culebra at about 0.032 feet per day, fluid velocities are probably small.

The causes of the various segments on Figure 12 should be explained. For instance, were they caused by diffusion into the matrix at one time and out of the matrix at another time? Are they caused by a convolution of the two tracer tests?

page 35, 2nd paragraph

Sauty's (1980) method should be modified for the anisotropy determined from the pump tests at the H-6 site. Since the principal axes of the transmissivity tensor are known for this site, the modification is:

$$n = \frac{Qt}{\pi b \left(\sqrt{\frac{Tyy}{Txx}}\, x^2 + \sqrt{\frac{Txx}{Tyy}}\, y^2 \right)}$$

where Q = pumping rate

b = aquifer thickness

t = time of match point

x,y= coordinates of well slugged with tracer

Txx= major transmissivity component

Tyy= minor transmissivity component

n = porosity.

In the above equation, the pumping well is at the origin and the major component of the transmissivity tensor runs between wells H-6b and H-6c as indicated from the well test data. The corrected porosities are 9.1% for the H-6b to H-6c test and 0.97% for the H-6a to H-6c test.

The 0.97% porosity appears reasonable for a fractured rock and occurs along the major axis of the transmissivity tensor. The 9.1% appears reasonable for a porous media. There is, however, almost an order of magnitude difference between the two. Since porosity is not anisotropic the disparity is probably caused by a heterogeneity in the Culebra. The suggestion of a discrete zone of flow i.e. a long highly permeable fracture, a set of parallel fractures or a karst channel, appears reasonable. Whether or not this discrete flow can be modelled adequately appears questionable. The problem lies in determining the number of discrete fractures and their locations.

<u>page 38, 1st paragraph</u>

The Safety Analysis Report (page 2.6-35) indicates that two sets of joint exist in the Delaware Basin. One of these sets strikes NW to SE, in the same direction as the major component of the transmissivity tensor. Does this joint set have some relationship to the principal axes of the transmissivity tensor? What is the possibility that vertical or near vertical fractures formed by the joint set could be missed by the drilling activities and subsequently untested by the pump testing program?

<u>page 38, 2nd paragraph to page 39, 1st paragraph</u>

The physical significance of the range of porosities to the following parameters should be discussed in more detail:

- travel times
- fracture flow or porous media flow
- the directional characteristics of the porosity.

The hydraulic conductivities should also be discussed in terms of fracture flow and porous media flow. The hydraulic conductivity values quoted in this report are averaged over the thickness of the Culebra i.e. the hydraulic conductivity is assumed to be uniform throughout the thickness of the Culebra. In a fractured media, the hydraulic conductivity is not uniform. It is peaked in a fracture and near zero outside a fracture. How would the hydraulic conductivity vary throughout the thickness of the Culebra? What effect would this variation have on travel times?

page 39, 1st paragraph

Figure 16 indicates that flow through the H-6 site is initially SSE. However the flow path appears to curve to the southeast away from Nash Draw. What is the basis for concluding that flow through H-6 reaches Nash Draw? In addition, a flow to the south-southeast would eventually have to turn toward the southwest in order to reach Nash Draw.

It further appears that Figure 16 may need some refinement. The figure does not appear to have contours based on the fresh water altitudes at H-8, H-9 and H-10. In addition, the fresh water elevation at P-18 is extremely low compared to the elevations at the other wells. The validity of the fresh water altitude is questionable because of the low hydraulic conductivity at that well. How does the fresh water altitude map change if fresh water altitudes at H-8, H-9 and H-10 are included in the map construction and P-18 is eliminated?

page 41, bottom paragraph

What is the basis for assuming 10 miles to the southeast? The data are all within 5 miles of the WIPP site. Most of the "path which would exceed 10 miles" is located in an area of very low hydraulic conductivity and in an area of unknown hydraulic gradient. In view of the southeast gradient over the study area, (Figure 16 of the draft report) how can the radionuclides discharge at Malaga Bend? The direction of the hydraulic gradient would have to turn southwest in order for a radionuclide to discharge at Malaga Bend. At present there are no data to support this. Are there other discharge areas for the Culebra?

page 43, 1st paragraph

If travel time for a non-absorbing radionuclide in the Culebra becomes greater than 40,000 years, is it possible that the Magenta's westward flow from the WIPP to Nash Draw would provide a quicker radioactive release to the biosphere than flow in the Culebra? If so perhaps future studies should concentrate on flow in the Magenta rather than on the Culebra.

page 44 ad 45, Continuing Investigations

The following are suggestions that should be useful in the continuing study of fracture flow in the Rustler:

1. A review of aerial photographs through the area defined by WIPP-29, WIPP-25, WIPP-33, H-6 and H-5 should be made to see if any geomorphic features associated with either karst hydrology or fracture hydrology exist there. Thermal infrared photographs may be useful in locating springs or shallow ground water flowing in subsurface channels. This suggestion is made for several reasons:
   a. Larry Barrows, in a presentation to EEG, indicated that an elevated gravity anomaly existed in this area. He attributes it to a possible karst channel in the Culebra.
   b. A structure contour map (see Figure 1) on top of the Culebra was generated from Table 1 of the report. It indicates a depression extending through the WIPP-25, H-6, H-5 area. This depression may be associated with a karst channel.
   c. A fresh water altitude map (see Figure 2) for the Culebra was constructed from the data presented in Table 1 of the report. The figure indicates that a ground water high is associated with the depression. The ground water high indicates a potential for some flow to the west.

   If possible the review of the aerial photographs should extend from the northern part of Nash Draw to Malaga Bend.

2. The application of inverse techniques to flow in the Rustler should be interesting. For the most part inverse techniques are in their infancy and are designed for porous media flow, not discrete flow. If it is decided

that fracture flow can be modeled as a porous media, then the inverse technique developed by Neuman and Yakowitz ("A Statistical Approach to the Inverse Problem of Aquifer Hydrology, 1: Theory," Water Resources Research, vol. 15, no. 4, pp 845-860, 1979), Neuman et al. ("A Statistical Approach to the Inverse Problem of Aquifer Hydrology, 2: Case Study," Water Resources Research, vol. 16, no. 1, pp. 33-58, 1980) and Neuman ("A Statistical Approach to the Inverse Problem of Aquifer Hydrology, 3: Improved Solution Method and Added Perspective, "Water Resources Research, vol. 16, no. 2, pp 331-346, 1980) should be tried. It appears that, at present, this is the only technique that has been published with an application to a real problem. Before the inverse techniques are applied to the Rustler, it should be decided whether flow in the Culebra is discrete or porous.

3. It appears that any contamination from a repository breach in Zone II would flow to the southeast. This is based on the flow paths as determined from Figure 15 and Figure 16 of the report. The area southeast of the WIPP should be studied further in terms of piezometric head, flow direction and discharge areas.

4. The tracer test at H-7 should be run similarly to the one at H-6. This should provide some more insight into the directional characteristics of the porosity and the areal extent of this phenomenum. Because the hydraulic conductivities at H-6 and H-7 are almost the same, the time required to run a test at H-7 should be about the same as at H-6.

   If possible, two two-well tracer tests should be run at H-4 in order to determine the porosity along the major and minor components of the transmissivity tensor.

5. If it has not been done, the Grove and Beetem (1971) model, the Sauty (1980) model and the SWIFT model, if it is used, should be modified to account for the anisotropy of the Rustler Formation.

TYPOGRAPHIC ERRORS

page 11, 2nd line

"definied" should be "defined"

page 25, bottom 3 lines

Either "yields" or "obtains" should be eliminated.

page 26

"(C)" should be "(°C)."

"(mhos)" should be "(μmhos)."

page 28, 2nd line from bottom

"Increases in displace" should read "Increases in ∝ displace."

page 36

"H-6b-c" should be "H-6a-c."

page 37

H-6a-c" should be "H-6b-c."

page 43, 3rd line

"members" should be "numbers."

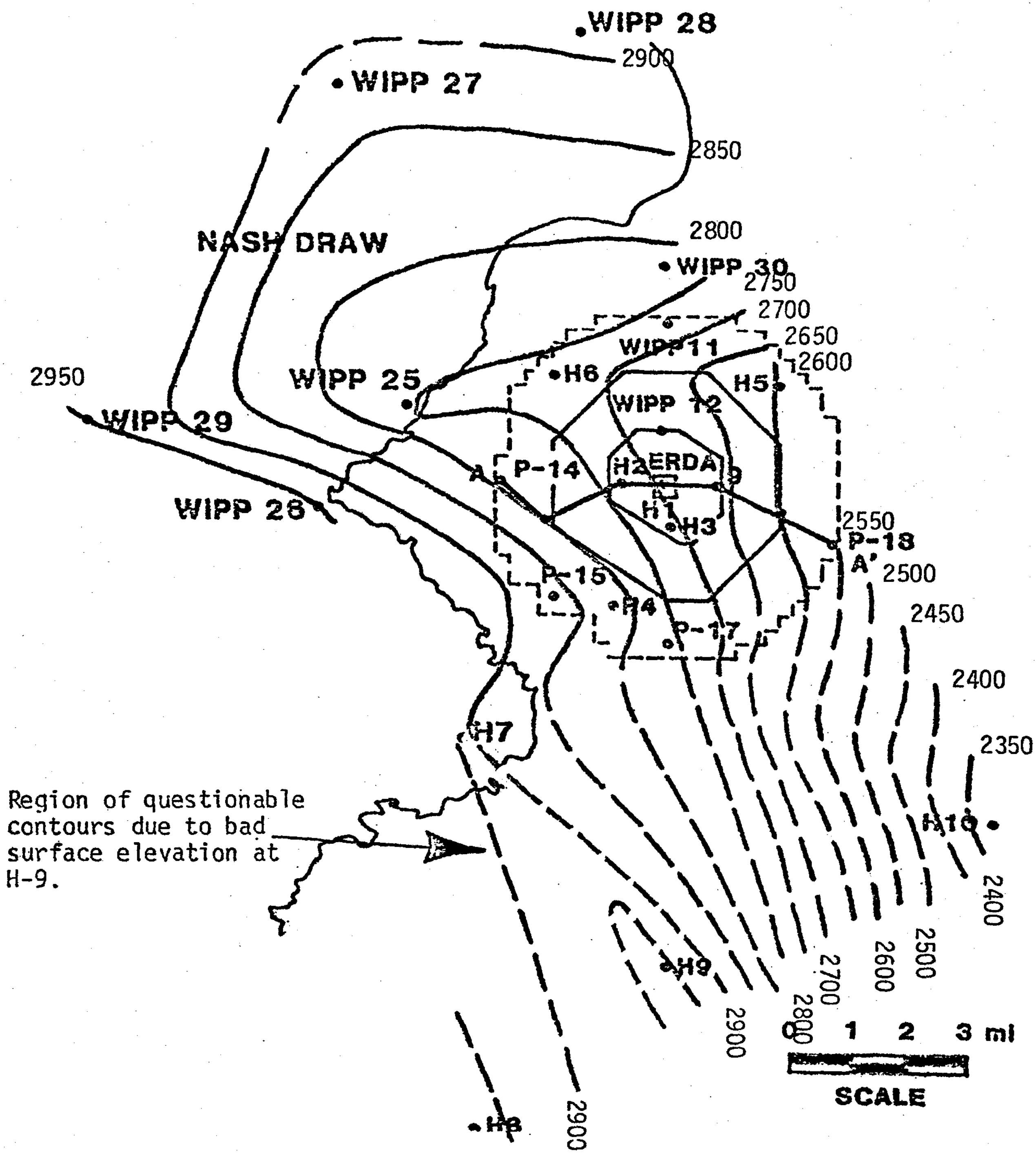

Figure 1. Structure Contours on Top of Culebra

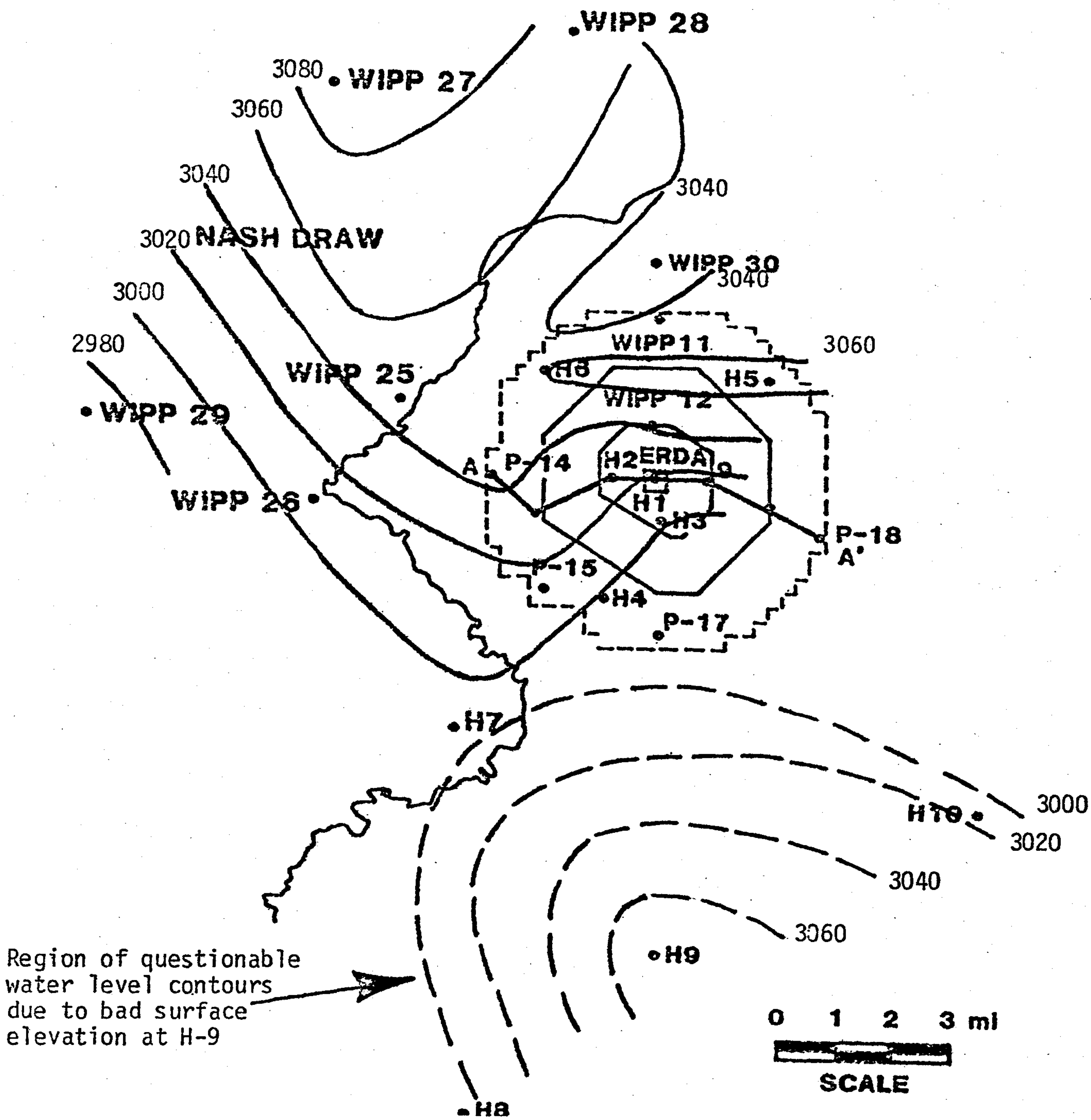

Figure 2. Fresh Water Altitude Contours for Culebra

RESPONSE TO EEG COMMENTS ON
"DRAFT INTERIM REPORT ON FRACTURE FLOW IN THE RUSTLER FORMATION"

EEG Comment: (Proposed Work - p. 1)

In the "Cost and Merits Evaluation for the Stipulated Agreement Activities," the following proposed work was identified: "The best model to represent the flow path and aquifer characteristics in the Rustler will be developed." This item was not adequately addressed in the interim report.

DOE Response:

The interim report does not address specifically the best model for flow path and aquifer characteristics of the Rustler. This "best model" will be formulated when the tracer/pump tests are completed.

EEG Comment: (Expected Results - p. 2)

In the "Cost and Merits Evaluation for the Stipulated Agreement Activities" the following expected results were identified: "If the results are such that additional consequence analyses should be run, they will be incorporated into the study." This item was not adequately addressed in the interim report.

DOE Response:

The consequence analyses, if warranted, will be performed by TSC for DOE. Though they are part of the study, the EEG should not expect consequence analyses as part of the interim or final reports on fracture flow in the Rustler.

EEG Comment: (Schueler letter, premise 1, p. 2)

The premise that the Culebra aquifer is critical and thus, it is not necessary to study the Magenta aquifer has not yet been justified.

DOE Response:

The premise that the Culebra is the critical aquifer has been justified for years on the basis of fluid volume, transport times, and discharge point. If the additional data and analysis indicate this premise is no longer justified, additional consequence analysis for the Magenta may be appropriate.

The evaluation of fracture flow in the Rustler Formation has been restricted to the Culebra aquifer on the basis of available hydrologic evaluation of the three fluid-bearing zones of the Rustler and their relationship to release scenarios developed in the EIS. In short, the Culebra Dolomite exhibits the most potential to contain fluids and to be capable of solute transport over a large distance. The Rustler-Salado contact is practically devoid of transmissive property (10^{-4} ft^2/day) and the Magenta Dolomite varies from 10^{-1} to 10^{-4} under the site. Many holes show the Magenta devoid of fluids especially along the east flank of Nash Draw where it appears the Magenta is draining downward across fractured anhydrite and the ensuing gypsum (W-26, 28 and H-7a) and into the underlying Culebra aquifer.

EEG Comment: (Schueler letter, premise 2, p. 2)

The premise that discrete fracture flow is not credible has not yet been justified.

DOE Response:

Discrete fracture flow (one fracture flow path) is certainly inappropriate in view of the discussions on p. 23, for example. On pp. 35 and 38, discrete flow is described as appropriate for H-6, but this is not as one fracture flow path which might describe the system. Discrete fracture flow is not a credible mechanism for solute transport across the WIPP to a discharge area near Malaga Bend. As mentioned in the interim report, we have definite plans to model for multiple fracture block flow (double media). Furthermore, it is geologically unreasonable to conceive that one fracture exists across the WIPP towards the discharge area. The revision of the interim report will add statements summarizing the justification for this idea.

EEG Comments: (General comments, para. 1, p. 3)

The reports currently in preparation which contain details of the various field tests would be useful to the EEG to consult while reviewing the report.

DOE Response:

Yes, the reports in preparation which were cited would be useful to the EEG. The reports are being readied as Contractor Reports (SAND documents) with the intent of publishing and distributing each at or before the time of publication of the revised interim report on fracture flow.

EEG Comment: (General comments, para. 3, p. 3)

A conclusion concerning the nature of fracture flow should be made before the regional transport model is developed.

DOE Response:

The nature of the fracture flow is the point of the work being conducted. Porous media (continuous) methods were used; however, as a first approximation in analyzing the results of the hydraulic and tracer tests. This use is justified because they are not subject to the conceptual uncertainties that cloud the use of discrete fracture models and which make the results based on discrete fracture models subject to controversy. Certainly, a conclusion regarding fracture flow should be made before the regional transport model is completed.

EEG Comment: (General comments, para. 4, p. 3)

The physical meaning of the values of the transmissivity storage coefficient, anisotropy, porosity and dispersivity in terms of fracture flow should be addressed.

DOE Response:

The final report shall include a glossary of terms commonly used in the hydro-world, i.e, fracture conductivity, double porosity, porous media flow, discrete fractures, matrix permeability, transmissivity, anisotropy, porosity, dispersivity and storage. Again, the point of continuing the investigations is to determine the contribution (physical meaning?) of fracture flow. The site specific meaning of various hydraulic properties is the focus of the program.

EEG Comment: (General comment, para. 5, p. 3)

Is the anisotropy due to alternating vertical bands of highly transmissive rock and low transmissive rock that trend northwest to southeast or due to karst channels recharging the aquifers?

DOE Response:

The comment about anisotropy seems related to paragraph 4 in some way. At present, there is no hydraulic test which, unsupported by independent information, can show the cause of anisotropy. The fact that anisotropy tests at three sites were in relative agreement with each other have allowed some inferences to be drawn regarding the regional nature of the flow system. Anisotropy is explained in the report as due to fracturing, which is evidently caused by dissolution and subsidence. There is a possibility that the principal tensor orientation reflects a fracture trend due to tectonic forces. Additional testing for anisotropy will show if the direction for the principal tensor remains the same as in a broader tectonic process. A note regarding the uncertainty here will be added to the text.

There is no fundamental geohydrological process that seems appropriate to this site for developing "alternating vertical bands of highly transmissive rock and low transmissive rock." The higher transmissivities correlate with areas subjected to more apparent Rustler/top Salado salt dissolution. Here, that is interpreted as resulting in fracturing which complements the natural porosity of fluid-bearing zones in the Rustler. These holes do not indicate any direct penetration of cavernous karst features.

We strongly disagree with the use of the word karst to describe the flow system in the Rustler Formation. At the present time data are insufficient to draw such a conclusion. Furthermore, the word karst can and has been used to describe the most disparate observations, from submicroscopic solution enlargements along a fracture, to man-size caverns and house-swallowing sinkholes, such as those that occur in Florida. Unfortunately, it is often times the more lurid definition that sticks in people's minds when the word is brought up. We do not attribute the results of the anisotropy and tracer tests to karst; neither do we preclude its possible existence at the WIPP. We merely state that the use of the word is premature and therefore, inappropriate. It causes emotional responses due to its several definitions, and it puts the investigator in the almost indefensible position of proving that every break in slope of a drawdown curve is not due to a karst feature.

EEG Comment: (General comment, para. 6 & 7, p. 4)

The porosity of 18% determined from the H-2 nest of wells appears high for a fractured rock. At the H-6 nest of wells the porosity appears to exhibit some directional characteristics, when it should not. In addition, the data presented in the report indicate that Culebra flow in the repository area is to the southeast. Previous studies have indicated that flow is to the south and southwest. This data may indicate other discharge areas for the Culebra. An increased flow path that might result from the gradient presented in this report indicates that the Magenta may provide faster releases of radionuclides to the biosphere than the Culebra.

DOE Response:

Thin-section porosity at H-2 is as high as 10 percent. A matrix porosity of 18 percent for the Culebra at H-2 does not seem unreasonable. because there have not been any tests for anisotropy at the H-2 site (the reviewer may believe there have been), we do not draw any specific conclusions about the nature of flow at that site. At H-6 porosities were determined along the major (1%) and minor (11%) components of flow. Keeping in mind the concept of double-porosity medium (fracture-block concept), it is reasonable to find directional characteristics in this type of matrix. Although we presently interpret flow southeast across the site, we have no supporting data to alter our beliefs that the ground water discharges near Malaga Bend. The increased flow path does not discredit the

Culebra as the major vehicle for transport because of the previous discussion on the Magenta.

EEG Comment: (Abstract, 13th to 18th line, p. 4)

The term "principal" to minor transmissivity tensor" should be changed to "major to minor components of the transmissivity tensor." The term "principal transmissivity component" should be changed to "the principal direction of the major component of the transmissivity tensor" or "The orientation of the principal axes of the transmissivity tensor is northwest by southeast for the major component and northeast by southwest for the minor component." A statement about the variability of the transmissivity with respect to distance from the outcrop should also be included in the "abstract."

DOE Responses:

The terminology regarding transmissivity tensors is in need of change. Tensors shall be described in terms of major and minor components and to a principal direction of either a major or minor component. The reference made to distance from the outcrop is confusing. Does the reviewer mean Nash Draw? We do refer to the variation in transmissivity relative to the east flank of Nash Draw. The comment may imply inferences about recharge areas being the Nash Draw "outcrop" areas which were not ready to draw yet.

EEG Comment: (2nd para., p. 4)

Mercer and Gonzalez (1981) indicate a strong westward gradient from WIPP to Nash Draw in the Magenta dolomite. Have any calculations of travel time from WIPP to Nash Draw been made for the Magenta and compared to travel times for the Culebra?

The Culebra's southeast gradient through the repository, as indicated on Figure 16, shows that contamination from a repository breach may either never reach Nash Draw or may take longer than the 40,000 years previously estimated. If the Culebra travel times are significantly increased, the Magenta may provide quicker radioactive releases to the biosphere than the Culebra. Estimates of travel time from the repository to Nash Draw through the Magenta should be provided in this paragraph.

What are the data and assumptions that went into the calculation of the 40,000 year travel time from ERDA-9 to Malaga Bend? Is this number taken from some other work?

DOE Response:

The comment about Magenta travel times seems to be leading to an inference that Nash Draw is where the Magenta discharges. Instead, the travel time to the probable common discharge point is appropriate. Travel time is tempered by flux. The additional consequence analysis, if necessary, is the appropriate comparison.

The calculation of the 40,000 year travel time resulted from a request by D. D. Gonzalez to Intera Groundwater Consultants. The date of the request was January 1979 and was based on a very limited set of hydrologic parameters, including transmissivity, storativity and hydraulic head at four locations and only estimates in areas near Laguna Grande de La Sal and towards Malaga Bend. The assumed thickness and porosities for the Culebra Dolomite were 30 feet and 10% respectively. The particle tracking model (SWIFT) determining the streamline and travel time for a non-absorbing tracer particle released at the WIPP site calculated a streamline proceeding due south from the center of the WIPP for about five miles, then west south-westward towards Laguna Grande de La Sal, then south towards Malaga Bend. Over 80% of this travel time is attributed to flow in the five-mile long reach south of the site where our understanding of the hydraulic characteristics have not changed appreciably since 1979, except for porosities being calculated at 18% at H-2. Further discussion will be included in the final report as well as a reference to the Intera work, dated 5-22-79.

EEG Comment: (Page 8, bottom para., p. 5)

Figure 16 indicates that the hydraulic gradient is to the southeast through the facility. Why does this paragraph say south and then southwest toward Malaga Bend? The statement appears to be referring to the previously assumed flow path shown in figure 3, but it certainly is not clear.

DOE Response:

The southeast flow across much of the site still appears correct. The flow is expected to swing to the southwest based on H-8, 9, and 10. The contours of Figure 16 will be revised for the interim version, and we expect to perform additional testing in the southeast portion of the site to verify contours and hydraulic properties. The interim report will be clarified.

EEG Comment: (Page 9, p. 5)

Table 1 should be checked for errors. The "Fresh Water Altitude" for P-18 on the table and the altitude used for construction of Figure 16 appear to differ by 100 feet. Other "Fresh Water Altitudes" that appear to need checking belong to wells H-5, H-8, H-9, H-10, W-28 and W-30. In addition, the surface altitude of H-9 is 100' higher than that presented in Seward ("Abridged Borehole Histories for the Waste Isolation Pilot Plant (WIPP) Studies," SAND82-0080). This may lower the fresh water altitude to 2976 at H-9. It is probable that the fractured nature of the dolomite may be causing the anomalous water levels. The water level at H-5 appears to be associated with a structural anomaly of the Culebra Dolomite (see attached Figures 1 and 2 and comment regarding page 44 and 45).

DOE Response:

P-18 and W-30 seem anomalously low, and both wells have very low transmissivity. They will be monitored to see if they have truly regained static equilibrium. If they have not, then the use of these nonrecovered water levels makes the water level at H-5 seem anomalously high and may lead to erroneous conclusions about the formation around H-5. in addition, we are in the process of double checking those elevations at all H- and W- sites where hydro-data have been collected.

EEG Comment: (Page 11, 1st para., p. 5)

References for transmissivity values should be provided.

DOE Response:

References will be given as appropriate.

EEG Comment: (Page 14, 2 bottom lines, p. 5)

It would be nice if the reference were already published to check the theory and to see the report contents.

DOE Response:

Agreed. See previous comment.

EEG Comment: (Page 15, 1st para., p. 6)

The description of test procedures indicates that the "a" well at each pad was pumped. However, the anisotropy results of Table 2, Table 3 and Table 4 provide no results from pumping the "a" well. Why are the results from pumping the "a" well not presented? Was the "a" well pumped at all?

DOE Response:

The "a" wells could not be pumped, though the report implied they were. The tests for anisotropy require only two wells be pumped within a three-well array; however, at each pad the "a" wells developed downhole or pump complications which prohibited their pumping. Clarification will be made in the interim report.

EEG Comment: (Page 15, last para., p. 6)

If the tracer curves as insensitive to dispersion (dispersivity), how can it be estimated?

DOE Response:

The fact that the Grove and Beetem breakthrough curves are relatively insensitive to dispersivity means only that they do not give precise values of dispersivity. Single well "pump-back" and two-well convergent flow tests are the best method to determine dispersivity and these tests are being pursued.

EEG Comment: (Page 19, Table 2, p. 6)

According to the theory of anisotropic aquifers developed by Papadopulos (1965), the response of well H-4C from pumping H-4B should produce the same T and S values as the response of well H-4B from pumping well H-4C. The T estimates for these wells differ by a factor of two for tests one and two. The range of T (not including pumping wells) for all three tests is 0.8 feet squared per day to 1.7 feet squared per day. What is the cause of this discrepancy?

DOE Response

In theory, the same effective transmissivity should be observed in the observation wells in an anisotropic aquifer, but not the storage coefficient. Obviously if the observation well data yield the same T and S and if the wells are the same distance from the pumped well, then the aquifer is isotropic. Also, only in an ideal aquifer will the same values be obtained from observation wells. However, the sensitivity of the anisotropy results to errors in the drawdown interpretation should be investigated, and sensitivity tests are included in our final product.

EEG Comment: (Page 23, 1st para., pp. 6-7)

Were any methods, such as images, tried in order to locate any of the barrier boundaries? What could these boundaries be attributed to? For instance, could the boundaries be due to vertical fractures filled with an impermeable material or to a less fractured nearby region of dolomite?

The shape of the curve on Figure 7 is interesting. The early part of the curve (prior to the formation of the straight line) may be a result of no storage of water in the fractured part of the aquifer or possibly a horizontal fracture overlain by a porous block (see "Well Hydraulics in Heterogeneous Aquifer Formations" by T. D. Streltsova-Adams in Advances in Hydroscience, Vol. 11, Academic Press, 1978). In addition, it appears questionable that the flat part of the curve is attributable to flow from the blocks to the fractures. According to Streltsova-Adams (see Proceedings, Second Invitational Well-Testing Symposium held by Earth Sciences Division, Lawrence Berkeley Laboratory, on October 25-27, 1978) the flat part of the data should not show if the ratio $(S_f + S_m)/Sp$ (S_f and S_m are storage coefficients of the fractures and the porous matrix, respectively) is less than about 5 to 11. In other words, if the straight lines on Figure 7 are less than about 0.7 to 1.0 log cycles apart, which they are on Figure 7, then the flat part of the curve should not show. This brings several questions to mind:

1. It is possible that the straight line shows up between 200 minutes and 2000 minutes on Figure 7? If this is the case, then the data after 2000 minutes including the flat part may be attributable to the transition period between flow in fractures and "induced response."

2. If the straight line is correct and there is no double porosity system, is the flat part of the data and the "induced response" caused by a highly permeable fracture or karst channel near the well test?

3. Is the anisotropy observed in these tests due to recharge into the rocks from a highly permeable fracture or karst channel? Such a response would cause the lines of equal drawdown to have oval shapes rather than the elliptical ones caused by anisotropy. Unless there are data from more than three observation wells, it may be very difficult to tell the difference between an anisotropic aquifer and an aquifer with a recharge boundary.

Figure 7 of the report shows the response of the H-4a and H-4b wells due to pumping the H-4c well. Was the response of H-4a an H-4c due to pumping H-4b similar to the data shown on Figure 7 such that a double porosity system was indicated?

DOE Response:

Image-well theory was applied to drawdown data in an effort to locate groundwater "barriers," which could be attributed to skin effects, wellbore storage, pumping variations, elastic deformation, and formation barriers. A formation barrier may consist of abrupt changes in aquifer properties, such as porosity, conductivity, fracture density or orientation, recharge and discharge zones, transient or steady-state flow, vertical/horizontal permeability. Barriers may be the result of one or a combination of geologic or hydrologic parameters. Vertical communication with known overlying and underlying aquifers is practically negligible throughout the WIPP facility on the basis of observed differences in hydraulic potential and conductivities and general chemistry. We believe that the reviewer means that the slope of the transitional curve should not be zero, not that it should not show. Also, Streltsova-Adams assumes in her report that the matrix has zero permeability. If, in our case, the matrix has some permeability, then the shape of the drawdown curve may be different from her examples.

In regards to the three questions raised by the reviewer:

1. We think that the curvature of the early-time data on Figure 7 is pronounced. On log-log paper this portion of the curve is straight with nearly unit slope indicating full fracture flow or well-bore storage (probably the latter). It is entirely possible that the data after 2000 minutes is in a transitional period, but we think it is more consistent to treat the data between 2000 and 5000 minutes as a good straight-line (Jacob approx.) solution, and between 5000 and 8000 minutes as transitional (or induced response). Past 8000 minutes the line becomes approximately parallel to the earlier data. (The "INDUCED RESPONSE" arrow on Figure 7 points to the wrong part of the curve and will be corrected in the interim report.)

2. If the first break in the drawdown curve is attributed to hitting a recharge boundary, then it follows that the second break must be due to a barrier boundary. Furthermore, the shapes and permeabilities of both boundaries must be such that the effect of the second boundary must completely negate the first so drawdown may continue as if neither existed. We agree that several interpretations are possible, given that little is known about the system, but we do not believe that the drawdown data alone support the existence of a recharge boundary. In regards to "karst channel near the well," please refer to earlier discussion about so-called karst.

3. According to our dictionary, the definitions of "oval" and "elliptical" are the same. The drawdown data we used for anisotropy determinations was early-time, hopefully taken before any breaks, boundaries, or possible induced response affected the results. The anisotropy results should be free from these effects.

Some of this discussion will be included in the revision of the draft.

Well H-4a could not be pumped and the H-4b test was not run for a sufficient length of time to see the second break in the drawdown curve.

EEG Comment: (Page 23, last para., p. 7)

It appears questionable that chloride is the ideal tracer to use to determine if leakage is occurring between Rustler aquifers at the H-4 site. Table 4 of Mercer et. al. indicates that the chloride concentration of both the Magenta and Culebra is 7500 mg/l (Mercer, J. W., Paul Davis, Kevin F. Dennehy, and Carole L. Goetz, "Results of Hydrologic Tests and Chemistry Analyses, Wells H-4A, H-4B, and H-4C at the Proposed Waste Isolation Pilot Plant Site, Southeastern New Mexico," Water-Resources Investigations 81-36, U.S. Geological Survey, May, 1981).

DOE Response:

The objective was to determine if leakage did occur during the tests; we had few other tools at out disposal (such as piezometers in confining zones or adjacent water-bearing units) to assess leakage, so water chemistry (temperature, ph, conductivity, chloride) was looked at during these tests as an alternative means. The results are not conclusive, but indicate that no leakage occurred. The text will be revised to indicate the uncertainty at H-4.

EEG Comment: (Page 25, 2nd para., p. 8) --

The transmissivities as presented here are extremely small for a fractured rock and would tend to indicate that fracture flow is not that significant, at least in the areas that were tested. Any radionuclide transport in the Culebra would tend to be very slow because of the low transmissivities. Do fractures or karst channels capable of transmitting water exist near the WIPP site?

Table 9.2 of Walton (Walton, William C., Groundwater Resource Evaluation, McGraw-Hill, 1970) indicates the values of specific storage presented here are those for "sound rock." This would tend to indicate that any fractures in the Culebra, at least in the areas tested, are well cemented or that clean, open fractures are far apart. This suggests that fracture flow is not extensive over much of the Culebra but may be confined to long channels or fractures outside the area of influence of the pump tests. Does flow in open fractures exist in areas not tested by the pump or tracer tests?

DOE Response:

The hydraulic conductivities observed at the WIPP are actually large compared to fractured crystalline rock. For example, the lowest transmissivity measured in our tests was at the H-5 site (0.04 feet squared per day), and corresponds to a hydraulic conductivity of 2×10^{-3} feet per day, which is an average for fractured crystalline rocks (Stripa Mine Project Report, 1980). It cannot be concluded that fluid movement will be slow because transmissivities are small. Solute transport may be quite rapid in a fracture flow situation. Determining solute transport capability is, of course, a major reason to perform tracer tests, which will yield indications of fluid velocities in the natural flow system.

The values of specific storage measured at the WIPP are of the order of 10^{-6}/ft, which, using Walter (Table 9.2) is reasonable for fissured and jointed rock. Lohman (Ground-Water Hydraulics, USGS Prof. Paper 708) uses a value of 10^{-6}/ft as a way to estimate storage coefficients for confined aquifers in general. A rock, such as a fairly rigid dolomite, could have a very low specific storage and still have measurable matrix and fracture porosity. Low specific storage does not mean that fracture flow is not extensive; it may only mean that there is a lack of significant compressibility in the system, both from fractures and matrix. For the reviewer to carry his suggestion one step further and imply that low specific storage within the area of pumping influence is evidence that open fracture or channel (karst?) flow exists outside the area of pumping influence, is, of course, unanswerable.

EEG Comment: (Page 27, 3rd para., p. 8)

Was the aquifer pumped clear prior to tracer injection for the second test? If not, did the non-completion of the first tracer test affect the results of the second test? Is it possible that Segments I and II of Figure 12 are due to the first test and Segments III and IV to the second test? If so, some type of deconvolution would be necessary to interpret the results.

DOE Response:

Different tracers were used in each test; thus, no interference existed.

EEG Comments (Page 28, 3rd paragraph, p. 8)

The Grove and Beetem (1971) model needs to be corrected for anisotropy. Was this done? What were the ranges of porosities and dispersivities used in the Grove and Beetem (1971) analyses and how did they compare with the data?

(Page 32, 3rd paragraph, pp. 8-9)

The significance of the porosities of 0.17 and 0.18 should be discussed here. These values are extremely high for a fractured rock. A fractured rock typically has a fracture porosity of 0.01 - 0.02 and less (Streltsova, 1976). The porosity values presented here are typical for a porous media. If it is assumed that a fractured system operates at the H-2 well sites, then tracer diffusion from the fracture into the porous matrix could account for the high porosity. The effect of this diffusive process has been shown to increase travel times from one point to another when compared to a process without diffusion into the matrix. (See Grisak and Pickens, "Solute Transport Through Fractured Media I: The Effect of Matrix Diffusion, Water Resources Research, vol. 16, no. 4, Aug., 1980, pp. 719-730 and Grisak et al., "Solute Transport through Fractured Media 2: Column Study of Fractured Till," Water Resources Research vol. 16, no. 4, Aug., 1980, pp. 731-739). The net effect of increasing the travel time would be a high porosity. Grisak and Pickens also indicated that the diffusion of solute into the matrix would be more significant for low velocities of fluid flow in the fracture than for high velocities. With the hydraulic conductivity of the Culebra at about 0.032 feet per day, fluid velocities are probably small.

The causes of the various segments on Figure 12 should be explained. For instance, were they caused by diffusion into the matrix at one time and out of the matrix at another time? Are they caused by a convolution of the two tracer tests?

(Page 35, 2nd paragraph, pp. 9-10)

Sauty's (1980) method should be modified for the anisotropy determined from the pump tests at the H-6 site. Since the principal axes of the transmissivity tensor are known for this site, the modification is:

$$n = \frac{Qt}{b \quad \frac{\overline{Tyy}}{Txx} \, x^2 + \frac{\overline{Txx}}{Tyy} \, y^2}$$

where Q = pumping rate
b = aquifer thickness
t = time of match point
x,y = coordinates of well slugged with tracer
Txx = major transmissivity component
Tyy = minor transmissivity component
n = porosity.

In the above equation, the pumping well is at the origin and the major component of the transmissivity tensor runs between wells H-6b and H-6c as indicated from the well test data. The corrected porosities are 9.1% for the H-6b to H-6c test and 0.97% for the H-6a to H-6c test.

The 0.97% porosity appears reasonable for a fractured rock and occurs along the major axis of the transmissivity tensor. The 9.1% appears reasonable for a porous media. There is, however, almost an order of magnitude difference between the two. Since porosity is not anisotropic, the disparity is probably caused by a heterogeneity in the Culebra. The suggestion of a discrete zone of flow, i.e., a long highly permeable fracture, a set of parallel fractures or a karst channel, appears reasonable. Whether or not this discrete flow can be modelled adequately appears questionable. The problem lies in determining the number of discrete fractures and their locations.

DOE Responses:

Several good points are brought out here. We do not yet believe that we can define the flow system at the H-2 site. A double porosity system is appealing, but anisotropy needs to be determined and further tracer tests along different flow paths need to be conducted at the site to define the flow system. Again, we do not believe that the hydraulic conductivity at the H-2 site precludes the possibility or rapid fluid movement. The final report will include the results of additional tracer and anisotropy tests and respective modification to the code developed by Grove and Beetum.

Much discussion can be related to what value of porosity is typical of fractured rock vs. porous media. A minimum of effort has been spent on acquiring field data through extended tracer and anisotropy tests to evaluate and determine what these values are and what they mean. We hope to solidify our thoughts on double porosity media after the conclusion of our tests at H-6 and 7. At this point, the number of discrete fractures and their locations are not the problem -- neither is the notion of karst channel domination. A macroscopic point of view is the solution.

The H-6 tracer results certainly imply the existence of both fracture and matrix flow, at least under the flow regime set up by the test itself. There is not necessarily a disparity in the porosity determinations, nor need the difference be caused by local heterogenities.

EEG Comment: (Page 38, 1st para., p. 10)

The Safety Analysis Report (page 2.6-35) indicates that two sets of joint exist in the Delaware Basin. One of these sets strikes NW to SE, in the same direction as the major component of the transmissivity tensor. Does this joint set have some relationship to the principal axes of the transmissivity tensor? What is the possibility that vertical or near vertical fractures formed by the joint set could be missed by the drilling activities and subsequently untested by the pump testing program?

DOE Response:

See previous discussion of principal tensor and fracture trends. The testing program would not test a set of non-intersecting or non-interconnected fractures by definition. The fact that dipping fractures are intersected requires interconnection of even vertical fractures with the borehole though the zone of influence has limits.

EEG Comments:

(Page 38, 2nd para. to Page 39, 1st para., pp. 10-11)

The physical significance of the range of porosities to the following parameters should be discussed in more detail:

- o travel times
- o fracture flow or porous media flow
- o the directional characteristics of the porosity.

The hydraulic conductivities should also be discussed in terms of fracture flow and porous media flow. The hydraulic conductivity values quoted in this report are averaged over the thickness of the Culebra, i.e., the hydraulic conductivity is assumed to be uniform throughout the thickness of the Culebra. In a fractured media, the hydraulic conductivity is not uniform. It is peaked in a fracture and near zero ouside a fracture. How would the hydraulic conductivity vary throughout the thickness of the Culebra? What effect would this variation have on travel times?

(Page 39, 1st para., p. 10)

Figure 16 indicates that flow through the H-6 site is initially SSE. However, the flow path appears to curve to the southeast away from Nash Draw. What is the basis for concluding that flow through H-6 reaches Nash Draw? In addition, a flow to the south-southeast would eventually have to turn toward the southwest in order to reach Nash Draw.

It further appears that Figure 16 may need some refinement. The figure does not appear to have contours based on the fresh water altitudes at H-8, H-9 and H-10. In addition, the fresh water elevation at P-18 is extremely low compared to the elevations at the other wells. The validity of the fresh water altitude is questionable because of the low hydraulic conductivity at that well. How does the fresh water altitude map change if fresh water altitudes at H-8, H-9 and H-10 are included in the map construction and P-18 is eliminated?

DOE Response:

How hydraulic conductivity varies within the Culebra, both vertically and horizontally, will be a very difficult study. Cores taken from and measurements taken within the new ventilation shaft will help us. In our final analysis, a variation of parameters shall be input to the final regional model to simulate a variation of travel times under differing conditions.

The potentiometric surface as shown on Figure 16 typifies a very transmissive system approaching the WIPP from the north-east but encountering, in effect, a leaky boundary defined by the decrease in hydraulic conductivity from west to east and probably influenced by the presence of "salt" within the Rustler and lower transmissivities in the Culebra Dolomite. As the flux of groundwater encounters a less permeable portion of the aquifer, it resists flow and takes the more plausible avenue - down Nash Draw where we find transmissivities much greater in a number of wells. Figure 16 exemplifies the site specific information collected on and within the boundaries of the facility. The final report will include a refinement of the data, which will include tracer and anisotropy tests at locations south-east and south of the site. The validity of the use of fresh water altitudes based on fluid density influenced by low conductivity is also our concern. A final suite of W.L. measurements and density determinations will be taken and evaluated for inclusion in the final report. There are no better estimates for discharge areas other than near Malaga Bend or south of Laguna Grande de la Sal.

EEG Comment: (Page 41, bottom para., pp. 11-12)

What is the basis for assuming 10 miles to the southeast? The data are all within 5 miles of the WIPP site. Most of the "path which would exceed 10 miles" is located in an area of very low hydraulic conductivity and in an area of unknown hydraulic gradient. In view of the southeast gradient over the study area (Figure 16 of the draft report), how can the radionuclides discharge at Malaga Bend? The direction of the hydraulic gradient would have to turn southwest in order for a radionuclide to discharge at Malaga Bend. At present there are no data to support this. Are there other discharge areas for the Culebra?

DOE Response:

See previous comments.

EEG Comment: (Page 43, 1st para., p. 12)

If travel time for a non-absorbing radionuclide in the Culebra becomes greater than 40,000 years, it is possible that the Magenta's westward flow from the WIPP to Nash Draw would provide a quicker radioactive release to the biosphere than flow in the Culebra? If so, perhaps future studies should concentrate on flow in the Magenta rather than on the Culebra.

DOE Response:

See previous comments on Magenta.

EEG Comment: (Pages 44 and 45, Continuing Investigations, pp. 12-13)

The following are suggestions that should be useful in the continuing study of fracture flow in the Rustler:

1. A review of aerial photographs through the area defined by WIPP-29, WIPP-25, WIPP-33, H-6 and H-5 should be made to see if any geomorphic features associated with either karst hydrology or fracture hydrology exist there. Thermal infrared photographs may be useful in locating springs or shallow ground water flowing in subsurface channels. This suggestion is made for several reasons:

   a. Larry Barrows, in a presentation to EEG, indicated that an elevated gravity anomaly existed in this area. He attributes it to a possible karst channel in the Culebra.

   b. A structure contour map (see Figure 1) on top of the Culebra was generated from Table 1 of the report. It indicates a depression extending through the WIPP-25, H-6, H-5 area. This depression may be associated with a karst channel.

   c. A fresh water altitude map (see Figure 2) for the Culebra was constructed from the data presented in Table 1 of the report. The figure indicates that a ground water high is associated with the depression. The ground water high indicates a potential for some flow to the west.

   If possible, the reviw of the aerial photographs shoud extend from the northern part of Nash Draw to Malaga Bend.

2. The application of inverse techniques to flow in the Rustler should be interesting. For the most part, inverse techniques are in their infancy and are designed for porous media flow, not discrete flow. If it is decided that fracture flow can be modeled as a porous media, then the inverse technique developed by Neuman and Yakowitz ("A Statistical Approach to the Inverse Problem of Aquifer Hydrology, 1: Theory," Water Resources Research, vol. 15, no. 4, pp. 845-860, 1979), Neuman et al. ("A Statistical Approach to the Inverse Problem of Aquifer Hydrology, 2: Case Study," Water Resources Research, vol. 16, no. 1, pp. 33-58, 1980) and Neuman ("A Statistical Approach to the Inverse Problem of Aquifer Hydrology, 3: Improved Solution Method and Added Perspective, "Water Resources Research, vol. 16, no. 2, pp. 331-346, 1980) should be tried. It appears that, at present, this is the only technique that has been published with an application to a real problem. Before the inverse techniques are applied to the Rustler, it should be decided whether flow in the Culebra is discrete or porous.

3. It appears that any contamination from a repository breach in Zone II would flow to the southeast. This is based on the flow paths as determined from Figure 15 and Figure 16 of the report. The area southeast of the WIPP should be studied further in terms of piezometric head, flow direction and discharge areas.

4. The tracer test at H-7 should be run similarly to the one at H-6. This should provide some more insight into the directional characteristics of the porosity and the areal extent of this phenomenon. Because the hydraulic conductivities at H-6 and H-7 are almost the same time, the time required to run a test at H-7 should be about the same as at H-6.

   If possible, two two-well tracer tests should be run at H-4 in order to determine the porosity along the major and minor components of the transmissivity tensor.

5. If it has not been done, the Grove and Beetem (1971) model, the Sauty (1980) model and the SWIFT model, if it is used, should be modified to account for the anisotropy of the Rustler Formation.

DOE Response:

Item 1.a. Bachman (1980, 1981) examined karst features extensively through Nash Draw, along the Pecos, and in the site area. He attributed the fill and depression at WIPP 33 to a karst-type process by which Nash Draw expands. In his field work and review of aerial photos, he does not attribute geomorphic features at the site to karst processes. Barrows found anomalously low gravity indicating which he infers as due to removal of mass by dissolution (= karst). Barrows does not restrict karst to the Culebra - it is more likely in the gypsum units by his log correlations.

Item 1.b. Whether the gravity anomaly and structure contour maps show karst channels or not is still speculation. Perhaps comparing these maps with similar ones in regions of known karst will help somewhat. If the structure contour map does delineate a west-east karst channel, it cuts through some of the highest transmissivities tested at the WIPP (WIPP 25 and H-6), as well as the lowest (WIPP 30 and H-5). It also trends parallel to the minor component direction of the transmissivity tensor determined at H-6 and H-5; that is, the transmissivity is least in the direction of the channel.

Item 1.c. Figure 16 will be revised as previously stated. WIPP 30 is still being monitored, and all potentiometric data will be revised to be current for the interim report.

Item 2. Those suggestions are well taken and shall be considered. Inverse techniques are subject to criticism; however, significant strides are being made towards utilizing these techniques and determining whether fracture media can be treated as porous media (Neuman, U of Arizona); C. Wilson, and J. B. Long, LBL). We do have the insight to perform these evaluations in determining whether we are dealing with fracture or porous - the objective of our site specific studies.

Item 3. Locations for testing in the southeastern part of the site have been of some interest. DOE 1 was considered, but the operations may have been unsuitable for the conversion of the hole to hydro testing. However, that pad and borehole continue to be candidates for further testing. Anisotropy tests at H-9 and H-10 are also being considered.

Item 4. See report, p. 7, last line; also p. 45.

Item 5. See p. 44.

HEALTH & ENVIRONMENT department

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February 18, 1983

Mr. Joseph M. McGough
Project Manager
WIPP Project Office
U.S. Department of Energy
Albuquerque Operations Office
P. O. Box 5400
Albuquerque, New Mexico 87115

Dear Mr. McGough:

Subject: DOE response to EEG comments on "Draft Interim Report on Fracture Flow in the Rustler Formation"

We have evaluated the DOE response to EEG comments on the above cited document. For the most part, the responses were adequate. In some cases, additional text descriptions are suggested in our evaluation. One point, however, will require additional investigation and discussion.

We question the premise that only the Culebra aquifer is critical for contaminant transport scenarios. The Magenta should also be considered a migration pathway. Although the transmissivity of the Magenta is nearly a factor of 10 smaller than that of the Culebra, the flux through each aquifer is also dependent on the hydraulic gradient. Based on the limited data available to EEG at this time, the hydraulic gradient to the northwest in the Magenta (Mercer and Gonzalez, 1981) is steeper than the gradient to the south or southeast in the Culebra. Therefore, the flux through each aquifer may not be as different as the transmissivity difference would suggest. If the Magenta discharges into the Culebra near Nash Draw, as is suggested by the DOE response to the EEG comment regarding the Schueler letter dated Oct. 30, 1981, premise 1, (see page 2 of the enclosure), then a repository breach could send contaminants south--southeast thrugh the Culebra as DOE has suggested and northwest through the Magenta and eventually into the Culebra at Nash Draw. The contaminant in the Culebra at Nash Draw would move toward Malaga Bend as has been suggested for a contaminant in the Culebra moving southeast from the WIPP. Although the discharge point may be the same, the travel times may differ greatly. However, present data are inadequate to precisely define the discharge points. Therefore until such time as the flow direction in the Culebra is accurately known, two migration paths should be considered.

Enclosed are our detailed evaluations of DOE responses to EEG comments.

Sincerely,

Robert H. Neill
Director

RHN:KR:eg

cc: TSC, IEA

EEG EVALUATION OF

THE DOE RESPONSE TO EEG COMMENTS ON

"DRAFT INTERIM REPORT ON FRACTURE FLOW IN THE RUSTLER FORMATION"

EEG Comment: (Proposed Work - p. 1)

In the "Cost and Merits Evaluation for the Stipulated Agreement Activities", the following proposed work was identified: "The best model to represent the flow path and aquifer characteristics in the Rustler will be developed." This item was not adequately addressed in the interim report.

DOE Response:

The interim report does not address specifically the best model for flow path and aquifer characteristics of the Rustler. This "best model" will be formulated when the tracer/pump tests are completed.

EEG Evaluation:

The response is adequate at this time. It was hoped that the draft preliminary report would contain some initial ideas regarding the best model to use.

EEG Comment: (Expected Results - p. 2)

In the "Cost and Merits Evaluation for the Stipulated Agreement Activities" the following expected results were identified: "If the results are such that additional consequence analyses should be run, they will be incorporated into the study." This item was not adequately addressed in the interim report.

DOE Response:

The consequence analyses, if warranted, will be performed by TSC for DOE. Though they are part of the study, the EEG should not expect consequence analyses as part of the interim or final reports on fracture flow in the Rustler.

EEG Evaluation:

The response is adequate at this time. However, it is EEG's understanding that an additional report regarding a consequence analysis of fracture flow in the Rustler Formation may be prepared by the TSC. It should be pointed out that this additional report is part of the stipulated agreement and should be prepared prior to the site validation declaration.

EEG Comment: (Schueler letter, premise 1, p. 2)

The premise that the Culebra aquifer is critical and thus, it is not necessary to study the Magenta aquifer has not yet been justified.

DOE Response:

The premise that the Culebra is the critical aquifer has been justified for years on the basis of fluid volume, transport times, and discharge point. If the additional data and analysis indicate this premise is no longer justified, additional consequence analysis for the Magenta may be appropriate.

The evaluation of fracture flow in the Rustler Formation has been restricted to the Culebra aquifer on the basis of available hydrologic evaluation of the three fluid-bearing zones of the Rustler and their relationship to release scenarios developed in the EIS. In short, the Culebra Dolomite exhibits the most potential to contain fluids and to be capable of solute transport over a large distance. The Rustler-Salado contact is practically devoid of transmissive property (10^{-4} ft^2/day) and the Magenta Dolomite varies from 10^{-1} to 10^{-4} under the site. Many holes show the Magenta devoid of fluids especially along the east flank of Nash Draw where it appears the Magenta is draining downward across fractured anhydrite and the ensuing gypsum (W-26, 28 and H-7a) and into the underlying Culebra aquifer.

EEG Evaluation:

The premise that the Culebra is the critical aquifer seems to be based on old data. Piezometric head maps for the Culebra Dolomite as presented in the interim report and Mercer and Gonzalez (1981) are different from maps published earlier (see "Final Environmental Impact Statement, Waste

Isolation Pilot Plant." DOE/EIS-0026, Oct., 1980; or "Review and Analysis of Hydrogeologic Conditions near the site of a Potential Nuclear-Waste Repository, Eddy and Lea Counties, New Mexico," USGS, OFR 77-123, February, 1977). The earlier maps treated the Culebra Dolomite and Magenta Dolomite as one hydrologic unit. The Culebra Dolomite and Magenta Dolomite have been treated as two distinct hydrologic units only since 1979. Since then, data have shown a westward dipping hydraulic gradient in the Magenta Dolomite. The data in the interim report even changes some of the conceptions regarding the distribution of hydraulic conductivity than that presented in the "Final Environmental Impact Statement."

The EEG concern regarding this matter is the potential contamination of the Culebra Dolomite in Nash Draw caused by a repository breach into the Magenta Dolomite. See addition evaluation of EEG comment regarding 2nd paragraph, page 4 (pages 9 and 10 of this evaluation).

EEG Comment: (Schueler letter, premis 2, p. 2)

The premise that discrete fracture flow is not credible has not yet been justified.

DOE Response:

Discrete fracture flow (one fracture flow path) is certainly inappropriate in view of the discussions on p. 23, for example. On pp. 35 and 38, discrete flow is described as appropriate for H-6, but this is not as one fracture flow path which might describe the system. Discrete fracture flow is not a credible mechanism for solute transport across the WIPP to a discharge area near Malaga Bend. As mentioned in the interim report, we have definite plans to model for multiple fracture block flow (double media). Furthermore, it is geologically unreasonable to conceive that one fracture exists across the WIPP towards the discharge area. The revision of the interim report will add statements summarizing the justification for this idea.

EEG Evaluation:

The response is adequate.

EEG Comments: (General comments, para. 1, p. 3)

The reports currently in preparation which contain details of the various field tests would be useful to the EEG to consult while reviewing the report.

DOE Response:

Yes, the reports in preparation which were cited would be useful to the EEG. The reports are being readied as Contractor Reports (SAND documents) with the intent of publishing and distributing each at or before the time of publication of the revised interim report on fracture flow.

EEG Evaluation:

The response is adequate.

EEG Comment: (General comments, para. 3, p. 3)

A conclusion concerning the nature of fracture flow should be made before the regional transport model is developed.

DOE Response:

The nature of the fracture flow is the point of the work being conducted. Porous media (continuous) methods were used; however, as a first approximation in analyzing the results of the hydraulic and tracer tests. This use is justified because they are not subject to the conceptual uncertainties that cloud the use of discrete fracture models and which make the results based on discrete fracture models subject to controversy. Certainly, a conclusion regarding fracture flow should be made before the regional transport model is completed.

EEG Evaluation:

The response is adequate. The intent of the comment was to make sure that the flow system was well understood before any computer modeling was started. It sometimes happens that modeling is started before the flow system is understood.

EEG Comment: (General comments, para. 4, p. 3)

The physical meaning of the values of the transmissivity, storage coefficient, anisotropy, porosity and dispersivity in terms of fracture flow should be addressed.

DOE Response:

The final report shall include a glossary of terms commonly used in the hydro-world, i.e., fracture conductivity, double porosity, porous media flow, discrete fractures, matrix permeability, transmissivity, anisotropy, porosity, dispersivity and storage. Again, the point of continuing the investigations is to determine the contribution (physical meaning?) of fracture flow. The site specific meaning of various hydraulic properties is the focus of the program.

EEG Evaluation:

The intent of the comment was to get a feel if the transmissivities, storage coefficients, porosities, etc., were typical, high, or low for a fractured rock. Perhaps they could be related to data from other sites or rock types. After talking with other hydrologists regarding some of the above terms, the EEG feels that a glossary is a good idea.

EEG Comment: (General comment, para.. 5, p.3)

Is the anisotropy due to alternating vertical bands of highly transmissive rock and low transmissive rock that trend northwest to southeast or due to karst channels recharging the aquifers?

DOE Response:

The comment about anisotropy seems related to paragraph 4 in some way. At present, there is no hydraulic test which, unsupported by independent information, can show the cause of anisotropy. The fact that anisotropy tests at three sites were in relative agreement with each other have allowed some inferences to be drawn regarding the regional nature of the flow system. Anisotropy is explained in the report as due to fracturing, which is evidently caused by dissolution and subsidence. There is a possibility that the principal tensor orientation reflects a fracture trend due to tectonic forces. Additional testing for anisotropy will show if the direction for the principal tensor remains the same as in a broader tectonic process. A note regarding the uncertainty here will be added to the text.

There is no fundamental geohydrological process that seems appropriate to this site for developing "alternating vertical bands of highly transmissive rock and low transmissive rock." The higher transmissivities correlate with areas subjected to more apparent Rustler/top Salado salt dissolution. Here, that is interpreted as resulting in fracturing which complements the natural porosity of fluid-bearing zones in the Rustler. These holes do not indicate any direct penetration of cavernous karst fractures.

We strongly disagree with the use of the word karst to describe the flow system in the Rustler Formation. At the present time, data are insufficient to draw such a conclusion. Furthermore, the word karst can and has been used to describe the most disparate observations, from submicroscopic solution enlargements along a fracture, to man-size caverns and house-swallowing sinkholes, such as those that occur in Florida. Unfortunately, it is often times the more lurid definition that sticks in people's minds when the word is brought up. We do not attribute the results of the anisotropy and tracer tests to karst; neither do we preclude its possible existence at the WIPP. We merely state that the use of the word is premature and therefore, inappropriate. It causes emotional responses due to its several definitions, and it puts the investigator in the almost indefensible position of proving that every break in slope of a drawdown curve is not due to a karst feature.

EEG Evaluation:

The EEG comment responded to here was related to the previous comment and nothing specific was intended by it. A note or paragraph relating the anisotropy to tectonic forces or other geologic factors is a good idea.

We understand your concern regarding karst hydrology. Our consultant in karst hydrology believes that dissolution prongs along fractures are advancing eastward from Nash Draw. The rate of advance is very slow and the prongs should not affect the WIPP site for many tens of thousands of years. However, enlargement of fractures is a major concern and we will continue to pursue it.

EEG Comment: (General comment, para. 6 & 7, P. 4)

The porosity of 18% determined from the H-2 nest of wells appears high for a fractured rock. At the H-6 nest of wells the porosity appears to exhibit some directional characteristics, when it should not. In addition, the data presented in the report indicate that Culebra flow in the repository area is to the southeast. Previous studies have indicated that flow is to the south and southwest. This data may indicate other discharge areas for the Culebra. An increased flow path that might result from the gradient presented in this report indicates that the Magenta may provide faster releases of radionuclides to the biosphere than the Culebra.

DOE Response:

Thin-section porosity at H-2 is as high as 10 percent. A matrix porosity of 18 percent for the Culebra at H-2 does not seem unreasonable. Because there have not been any tests for anisotropy at the H-2 site (the reviewer may believe there have been), we do not draw any specific conclusions about the nature of flow at that site. At H-6 porosities were determined along the major (1%) and minor (11%) components of flow. Keeping in mind the concept of double-porosity medium (fracture-block concept), it is reasonable to find directional characteristics in this type of matrix. Although we presently interpret flow southeast across the site, we have no supporting data to alter our beliefs that the ground water discharges near Malaga Bend. The increased flow path does not discredit the Culebra as the major vehicle for transport because of the previous discussion on the Magenta.

EEG Evaluation;

The response to the high porosity at H-2 is adequate.

The response to the direction characteristics is not quite clear. Are you implying that flow between H-6b and H-6c is through discrete fractures and flow between H-6a and H-6b is through pores? The exact implication should be brought out better in the report.

The response regarding the discharge area of the Culebra Dolomite is adequate. It is hoped that the revision of Figure 16 will indicate better discharge areas of the Culebra Dolomite.

Responses regarding the Magenta Dolomite are presented elsewhere in this evaluation.

EEG Comment: (Abstract, 13th to 18th line, p. 4)

The term "principal to minor transmissivity tensor" should be changed to "major to minor components of the transmissivity tensor." The term "principal transmissivity component" should be changed to "the principal direction of the major component of the transmissivity tensor" or "The orientation of the principal axes of the transmissivity tensor is northwest by southeast for the major component and northeast by southwest for the minor component." A statement about the variability of the transmissivity with respect to distance from the outcrop should also be included in the "abstract."

DOE Responses:

The terminology regarding transmissivity tensors is in need of change. Tensors shall be described in terms of major and minor components and to a principal direction of either a major or minor component. The reference made to distance from the outcrop is confusing. Does the reviewer mean Nash Draw? We do refer to the variation in transmissivity relative to the east flank of Nash Draw. The comment may imply inferences about recharge areas being the Nash Draw "outcrop" areas which were not ready to draw yet.

EEG Evaluation:

The response regarding the transmissivity tensor terminology is adequate. The "Review Comments" also indicated that terminology regarding the transmissivity tensor on page 7, 1st and 2nd lines; page 14, 10th line; and page 18, 6th, 15th, and 17th lines also need correcting and should be changed accordingly.

It appears that the decrease of the transmissivity with respect to increasing distance from the east flank of Nash Draw is an important result of the present work. We believe the result should be included in the abstract. It was not included in the abstract of the reviewed report.

The comments regarding the abstract were intended to improve the abstract. No other inferences should have been drawn from the comments.

EEG Comment: (2nd para., p.4)

Mercer and Gonzalez (1981) indicate a strong westward gradient from WIPP to Nash Draw in the Magenta dolomite. Have any calculations of travel time from WIPP to Nash Draw been made for the Magenta and compared to travel times for the Culebra?

The Culebra's southeast gradient through the repository, as indicated on Figure 16, shows that contamination from a repository breach may either never reach Nash Draw or may take longer than the 40,000 years previously estimated. If the Culebra travel times are significantly increased, the Magenta may provide quicker radioactive releases to the biosphere than the Culebra. Estimates of travel time from the repository to Nash Draw through the Magenta should be provided in this paragraph.

What are the data and assumptions that went into the calculation of the 40,000 year travel time from ERDA-9 to Malaga Bend? Is this number taken from some other work?

DOE Response:

The comment about Magenta travel times seems to be leading to an inference that Nash Draw is where the Magenta discharges. Instead, the travel time to the probable common discharge point is appropriate. Travel time is tempered by flux. The additional consequence analysis, if necessary, is the appropriate comparison.

The calculation of the 40,000 year travel time resulted from a request by D.D. Gonzalez to Intera Groundwater Consultants. The date of the request was January 1979 and was based on a very limited set of hydrologic parameters, including transmissivity, storativity and hydraulic head at four locations and only estimates in areas near Laguna Grande de La Sal and towards Malaga Bend. The assumed thickness and porosities for the Culebra Dolomite were 30 feet and 10% respectively. The particle tracking model (SWIFT) determining the streamline and travel time for a non-absorbing tracer particle released at the WIPP site calculated a streamline proceeding due south from the center of the WIPP for about five miles, then west south-westward towards Laguna Grande de La Sal, then south towards Malaga Bend. Over 80% of this travel time is attributed to flow in the five-mile long reach south of the site where our understanding of the hydraulic characteristics have not changed appreciably since 1979, except for porosities being calculated at 18% at H-2. Further discussion will be included in the final report as well as a reference to the Intera work, dated 5-22-79.

EEG Evaluation:

The comment regarding travel time in the Magenta Dolomite is leading to an inference that the Magenta discharges there. The DOE response to the EEG comment regarding the Schueler letter, premise 1, seems to indicate that this is the case.

We disagree with the statement that "travel time to the probable common discharge point is appropriate." It is probable that the Culebra Dolomite is a usable aquifer in Nash Draw. It is productive and contains relatively good quality water. The Culebra Dolomite water quality could be deteriorated by a repository breach into the Magenta Dolomite and subsequent transport into the Culebra Dolomite. Such contamination may make the Culebra Dolomite an unusable aquifer in Nash Draw. This is the EEG concern regarding transport in the Magenta Dolomite.

The Culebra Dolomite south of the WIPP site has a low transmissivity and poor quality water. It is unlikely that it would be a usable aquifer. Therefore, for a breach into the Culebra, a travel time through the Culebra from the WIPP to Malaga Bend is appropriate.

The response regarding the calculation of the 40,000 year travel time is adequate.

EEG Comment: (Page 8, bottom para., p.5.)

Figure 16 indicates that the hydraulic gradient is to the southeast through the facility. Why does this paragraph say south and then southwest toward Malaga Bend? The statement appears to be referring to the previously assumed flow path shown in figure 3, but it certainly is not clear.

DOE Response:

The southeast flow across much of the site still appears correct. The flow is expected to swing to the southwest based on H-8, 9, and 10. The contours of Figure 16 will be revised for the interim version, and we expect to perform additional testing in the southeast portion of the site to verify contours and hydraulic properties. The interim report will be clarified.

EEG Evaluation:

In addition, it should be clearly indicated which wells were used and not used to construct Figure 16 of the draft interim report. The figure, as presented in the report, leads to confusion with regard to travel paths.

EEG Comment: (Page 9, p.5)

Table 1 should be checked for errors. The "Fresh Water Altitude" for P-18 on the table and the altitude used for construction of Figure 16 appear to differ by 100 feet. Other "Fresh Water Altitudes" that appear to need checking belong to wells H-5, H-8, H-9, H-10, W-28 and W-30. In addition, the surface altitude of H-9 is 100' higher than that presented in Seward ("Abridged Borehole Histories for the Waste Isolation Pilot Plant (WIPP) Studies," SAND82-0080). This may lower the fresh water altitude to 2976 at H-9. It is probable that the fractured nature of the dolomite may be causing the anomalous water levels. The water level at H-5 appears to be associated with a structural anomaly of the Culebra Dolomite (see attached Figures 1 and 2 and comment regarding page 44 and 45).

DOE Response:

P-18 and W-30 seem anomalously low, and both wells have very low transmissivity. They will be monitored to see if they have truly regained static equilibrium. If they have not, then the use of these nonrecovered water levels makes the water leval at H-5 seem anomalously high and may lead to erroneous conclusions about the formation around H-5. In addition, we are in the process of double checking those elevations at all H- and W- sites where hydro-data have been collected.

EEG Evaluation:

The response is adequate.

EEG Comment: (Page 11, 1st para., p.5)

References for transmissivity value should be provided.

DOE Response:

References will be given as appropriate.

EEG Evaluation:

The response is adequate.

EEG Comment: (Page 14, 2 bottom lines, p. 5)

It would be nice if the reference were already published to check the theory and to see the report contents.

DOE Response:

Agreed. See previous comment.

EEG Evaluation:

The response is adequate.

EEG Comment: (Page 15, 1st para., p. 6)

The description of test procedures indicates that the "a" well at each pad was pumped. However, the anisotropy results of Table 2, Table 3 and Table 4 provide no results from pumping the "a" well. Why are the results from pumping the "a" well not presented? Was the "a" well pumped at all?

DOE Response:

The "a" wells could not be pumped, though the report implied they were. The tests for anisotropy require only two wells be pumped within a three-well array; however, at each pad the "a" wells developed downhole or pump complications which prohibited their pumping. Clarification will be made in the interim report.

EEG Evaluation:

The response is adequate.

EEG Comment: (Page 15, last para., p. 6)

If the tracer curves are insensitive to dispersion (dispersivity), how can it be estimated?

DOE Response:

The fact that the Grove and Beetum breakthrough curves are relatively insensitive to dispersivity means only that they do not give precise values of dispersivity. Single well "pump-back" and two-well convergent flow tests are the best method to determine dispersivity and these tests are being pursued.

EEG Evaluation:

The response is adequate. Clarification regarding the statement should be made in the text.

EEG Comment: (Page 19, Table 2, p. 6)

According to the theory of anisotropic aquifers developed by Papadopulos (1965), the response of well H-4C from pumping H-4B should produce the same T and S values as the response of well H-4B from pumping well H-4C. The T estimates for these wells differ by a factor of two for tests one and two. The range of T (not including pumping wells) for all three tests is 0.8 feet squared per day to 1.7 feet squared per day. What is the cause of this discrepancy?

DOE Response:

In theory, the same effective transmissivity should be observed in the observation wells in an anisotropic aquifer, but not the storage coefficient. Obviously if the observation well data yield the same T and S and if the wells are the same distance from the pumped well then the aquifer is isotropic. Also, only in an ideal aquifer will the same values be obtained from observation wells. However, the sensitivity of the anisotropy results to errors in the drawdown interpretation should be investigated, and sensitivity tests are included in our final product.

EEG Evaluation:

It appears that the transmissivity presented in Table 2 (and Table 3 and Table 4) is the effective transmissivity as used in the draft report and is the determinate of the transmissivity tensor. It further appears that the storage coefficient presented in Table 2 is really the quantity:

$$S(\text{table}) = \frac{S}{r^2} \; \frac{Txx\; y^2 - 2Txy\; xy + Tyy\; x^2}{Txx\; Tyy - Txy^2}$$

where S is the aquifer storage coefficient; Txx, Txy, and Tyy are the various components of the transmissivity tensor; x and y are the coordinates of the observation well if the pumping well is at the origin of the axes; and r is the distance between the pumping well and the observation well ($r^2 = x^2 + y^2$).

If the aquifer is homogeneous but not isotropic, then the drawdown response at H-4C caused by pumping H-4B should be the same as the drawdown response at H-4B caused by pumping H-4C. The observation well drawdown in a homogeneous anisotropic aquifer is

$$\$ = \frac{Q}{4\pi(Txx\; Tyy - Txy^2)} \; W\;(Uxy)$$

where

$$Uxy = \frac{S}{4t} \; \frac{Txy\; x^2 - 2T_{xy}\; xy + T_{xx}y^2}{(Txx\; Tyy - Txy^2)}$$

and \$ is the observation well drawdown, Q is the pumping rate, S is the aquifer storage coefficient, W (Uxy) is the well function with the argument Uxy and x and y are the coordinates of the observation well if the pumping well is at the origin of the axes. If the H-4B well is the pumping well as in Test 1, then the observation well H-4C has the coordinates x and y. If the role of each well is reversed as in Test 2 the coordinates of the H-4C well, now the observation well, are now -x and -y, which, when entered into the above Uxy equation, produce the same Uxy values as positive x and y. Therefore, the drawdown response at the H-4B well caused by pumping the H-4C well is the same as the drawdown at the H-4C well caused by pumping the H-4B well. This is the only well pair where this is true. The respective effective transmissivity and storage coefficient (table) for the two wells should be equal. However, the effective transmissivity differs by a factor of 2 and the storage coefficient (table) differs by a factor of 1.5. These are large differences and should be explained.

The sensitivity analyses proposed are a good idea.

<u>EEG Comment</u>: (Page 23, 1st para., pp. 6-7)

Were any methods, such as images, tried in order to locate any of the barrier boundaries? What could these boundaries be attributed to? For instance, could the boundaries be due to vertical fractures filled with an impermeable material or to a less fractured nearby region of dolomite?

The shape of the curve on Figure 7 is interesting. The early part of the curve (prior to the formation of the straight line) may be a result of no storage of water in the fractured part of the aquifer or possibly a horizontal fracture overlain by a porous block (see "Well Hydraulics in Heterogeneous Aquifer Formations" by T.D. Streltsova-Adams in <u>Advances in Hydroscience</u>, Vol. 11, Academic Press, 1978. In addition, it appears questionable that the flat part of the curve is attributable to flow from the blocks to the fractures. According to Streltsova-Adams (see <u>Proceedings, Second Invitational Well-Testing Symposium</u> held by Earth Sciences Division, Lawrence Berkeley Laboratory, on October 25-27, 1978) the flat part of the data should not show if the ratio $(S_f + S_m)/Sp$ (S_f and S_m are storage coefficients of the fractures and the porous matrix, respectively) is less than about 5 to 11. In other words, if the straight lines on Figure 7 are less than about 0.7 to 1.0 log cycles apart, which they are on Figure 7, then the flat part of the curve should not show. This brings several questions to mind:

1. Is it possible that the straight line shows up between 200 minutes and 2000 minutes on Figure 7? If this is the case, then the data after 2000 minutes including the flat part may be attributable to the transition period between flow in fractures and "induced response."

2. If the straight line is correct and there is no double porosity system, is the flat part of the data and the "induced response" caused by a highly permeable fracture or karst channel near the well test?

3. Is the anisotropy observed in these tests due to recharge into the rocks from a highly permeable fracture or karst channel? Such a response would cause the lines of equal drawdown to have oval shapes rather than the elliptical ones caused by anisotropy. Unless there are data from more than three observation wells, it may be very difficult to tell the difference between an anisotropic aquifer and an aquifer with a recharge boundary.

Figure 7 of the report shows the response of the H-4a and H-4b wells due to pumping the H-4c well. Was the response of H-4a and H-4c due to pumping H-4b similar to the data shown o Figure 7 such that a double porosity system was indicated?

DOE Response:

Image-well theory was applied to drawdown data in an effort to locate groundwater "barriers," which could be attributed to skin effects, wellbore storage, pumping variations, elastic deformation, and formation barriers. A formation barrier may consist of abrupt changes in aquifer properties, such as porosity, conductivity, fracture density or orientation, recharge and discharge zones, transient or steady-state flow, vertical/horizontal permeability. Barriers may be the result of one or a combination of geologic or hydrologic parameters. Vertical communication with known overlying and underlying aquifers is practically negligible throughout the WIPP facility on the basis of observed differences in hydraulic potential and conductivities and general chemistry.

We believe that the reviewer means that the slope of the transitional curve should not be zero, not that it should not show. Also, Streltsova-Adams assumes in her report that the matrix has zero permeability. If, in our case, the matrix has some permeability, then the shape of the drawdown curve may be different from her examples.

In regards to the three questions raised by the reviewer:

1. We think that the curvature of the early-time data on Figure 7 is pronounced. On log-log paper, this portion of the curve is straight with nearly unit slope indicating full fracture flow or well-bore storage (probably the latter). It is entirely possible that the data after 2000 minutes is in a transitional period, but we think it is more consistent to treat the data between 2000 and 5000 minutes as a good straight-line (Jacob approx.) solution, and between 5000 and 8000 minutes as transitional (or induced response). Past 8000 minutes the line becomes approximately parallel to the earlier data. (The "INDUCED RESPONSE" arrow on Figure 7 points to the wrong part of the curve and will be corrected in the interim report.)

2. If the first break in the drawdown curve is attributed to hitting a recharge boundary, then it follows that the second break must be due to a barrier boundary. Furthermore, the shapes and permeabilities of both boundaries must be such that the effect of the second boundary must completely negate the first so drawdown may continue as if neither existed. We agree that several interpretations are possible, given that little is known about the system, but we do not believe that the drawdown data alone support the existence of a recharge boundary. In regards to "karst channel near the well," please refer to earlier discussion about so-called karst.

3. According to our dictionary, the definitions of "oval" and "elliptical" are the same. The drawdown data we used for anisotropy determinations was early-time, hopefully taken before any breaks,

boundaries, or possible induced response affected the results. The anisotropy results should be free from these effects.

Some of this discussion will be included in the revision of the draft.

Well H-4a could not be pumped and the H-4b test was not run for a sufficient length of time to see the second break in the drawdown curve.

EEG Evaluation:

If possible, distances and directions to barriers, estimated from the method of images should be provided. A description of possible barriers mentioned here should also be included in the text.

Streltsova-Adams does not assume that the matrix has zero hydraulic conductivity; she assumes that the transmissivity of the porous blocks is so small compared to the transmissivity of the fractures that the transmissivity of the blocks can be neglected. If the blocks had zero hydraulic conductivity, then a mechanism to transport fluid from the blocks to the fractures would not exist and there would be no transition curve.

The response to question 1 is adequate for the most part. The log-log plot that shows the unit slope should be included and described in the report. In addition, a USGS publication (Reed, J.E., "Type Curves for Selected Problems of Flow to Wells in Confined Aquifers," Techniques of Water-Resources Investigations of the United States Geological Survey, Book 3, Chapter B3, 1980) indicates that the drawdown response at an observation well far from a pumping well of finite diameter is much steeper than unit slope. Therefore, the unit slope is not likely to be caused by wellbore storage effects, but more likely the full fracture flow mentioned in the DOE response.

The response to question 2 is adequate.

The response to question 3 is adequate. However, the EEG dictionary defines oval as an egg shape. Perhaps "egg shaped" instead of "oval" would have been a better term.

The response to the comment concerning the H-4 pumping is adequate.

EEG Comment: (Page 23, last para., p. 7)

It appears questionable that chloride is the ideal tracer to use to determine if leakage is occurring between Rustler aquifers at the H-4 site. Table 4 of Mercer, et. al., indicates that the chloride concentration of both the Magenta and Culebra is 7500 mg/l (Mercer, J.W., Paul Davis, Kevin F. Dennehy, and Carole L. Goetz, "Results of Hydrologic Tests and Chemistry Analyses, Wells, H-4A, H-4B, and H-4C at the Proposed Waste Isolation Pilot Plant Site, Southeastern New Mexico," Water-Resources Investigations 81-36, U.S. Geological Survey, May, 1981.

DOE Response:

The objective was to determine if leakage did occur during the tests; we had few other tools at our disposal (such as piezometers in confining zones or adjacent water-bearing units) to assess leakage, so water chemistry (temperature, ph, conductivity, chloride) was looked at during these tests as an alternative means. The results are not conclusive, but indicate that no leakage occurred. The test will be revised to indicate the uncertainty at H-4.

EEG Evaluation:

The response is adequate.

EEG Comment: (Page 25, 2nd para., p. 8)

The transmissivities as presented here are extremely small for a fractured rock and would tend to indicate that fracture flow is not that significant, at least in the areas that were tested. Any radionuclide transport in the Culebra would tend to be very slow because of the low transmissivities. Do fractures or karst channels capable of transmitting water exist near the WIPP site?

Table 9.2 of Walton (Walton, William C., Groundwater Resource Evaluation, McGraw-Hill, 1970) indicates the values of specific storage presented here are those for "sound rock." This would tend to indicate that any fractures in the Culebra, at least in the areas tested, are well cemented or that clean, open fractures are far apart. This suggests that fracture flow is not extensive over much of the Culebra but may be confined to long channels or fractures outside the area of influence of the pump tests. Does flow in open fractures exist in areas not tested by the pump or tracer tests?

DOE Response:

The hydraulic conductivities observed at the WIPP are actually large compared to fractured crystalline rock. For example, the lowest transmissivity measured in our tests was at the H-5 site (0.04 feet squared per day), and corresponds to a hydraulic conductivity of $2x10^{-3}$ feet per day), which is an average for fractured crystalline rocks (Stripa Mine Project Report, 1980). It cannot be concluded that fluid movement will be slow because transmissivities are small. Solute transport may be quite rapid in a fracture flow situation. Determining solute transport capability is, of course, a major reason to perform tracer tests, which will yield indications of fluid velocities in the natural flow system.

The values of specific storage measured at the WIPP are of the order of 10^{-6}/ft, which, using Walton (Table 9.2) is reasonable for fissured and jointed rock. Lohman (Ground-Water Hydraulics, USGS Prof. Paper 708) uses a value of 10^{-6}/ft as a way to estimate storage coefficients for confined aquifers in general. A rock, such as a fairly rigid dolomite, could have a very low specific storage and still have measurable matrix and fracture porosity. Low specific storage does not mean that fracture flow is not extensive; it may only mean that there is a lack of significant compressibility in the system, both from fractures and matrix. For the reviewer to carry his suggestion one step further and imply that low specific storage within the area of pumping influence is evidence that open fracture or channel (karst?) flow exists outside the area of pumping influence is, of course, unanswerable.

EEG Evaluation:

Comparisons of the transmissivity and/or hydraulic conductivities with other fractured rocks should be made in the report. The comparison should be made to other dolomitic rocks not just fractured crystalline rocks.

The values of specific storage presented in the interim report range from 3.5 x 10^{-7}/ft to 8.3 x 10^{-7}. The higher values seem to occur at H-5 and H-6 and the low value at H-4. Perhaps fissures and joints could exist at H-5 and H-6. However, the specific storages calculated at these wells are at the upper limit of the "sound rock" values and slightly below the lower limit for "fissured and jointed rock." The specific storage calculated at H-4 is in the "sound rock" range.

EEG Comment: (Page 27, 3rd para., p.8)

Was the aquifer pumped clear prior to tracer injection for the second test? If not, did the non-completion of the first tracer test affect the results of the second test? Is it possible that Segments I and II of Figure 12 are due to the first test and Segments III and IV to the second test? If so, some type of deconvolution would be necessary to interpret the restults.

DOE Response:

Different tracers were used in each test; thus, no interference existed.

EEG Evaluation:

The response is adequate. Perhaps this should be brought out in the report.

EEG Comments: (Page 28, 3rd paragraph, p. 8)

The Grove and Beetem (1971) model needs to be corrected for anisotropy. Was this done? What were the ranges of porosities and dispersivities used in the Grove and Beetem (1971) analysis and how did they compare with the data?

(Page 32, 3rd paragraph, pp. 8-9):
The significance of the porosities of 0.17 and 0.18 should be discussed here. These values are extremely high for a fractured rock. A fractured rock typically has a fracture porosity of 0.01 - 0.02 and less (Streltsova, 1976). The porosity values presented here are typical for a porous media. If it is assumed that a fractured system operates at the H-2 well sites, then tracer diffusion from the fracture into the porous matrix could account for the high porosity. The effect of this diffusive process has been shown to increase travel times from one point to another when compared to a process without diffusion into the matrix. (See Grisak and Pickens, "Solute Transport Through Fractured Media I: The Effect of Matrix Diffusion, Water Resources Research, vol. 6, no. 4, Aug., 1980, pp. 719-730 and Grisak, et al. "Solute Transport through Fractured Media 2: Column Study of Fractured Till,"Water Resources Research, vol. 16, no. 4, Aug., 1980,, pp. 731-739). The net effect of increasing the travel time would be a high porosity. Grisak and Pickens also indicated that the diffusion of solute into the matrix would be more significant for low velocities of fluid flow in the fracture than for high velocities. With the hydraulic conductivity of the Culebra at about 0.032 feet per day, fluid velocities are probably small.

The causes of the various segments on Figure 12 should be explained. For instance, were they caused by diffusion into the matrix at one time and out of the matrix at another time? Are they caused by a convolution of the two tracer tests?

Page 35, 2nd paragraph, pp. 9-10:
Sauty's (1980) method should be modified for the anisotropy determined from the pump tests at the H-6 site. Since the principal axes of the transmissivity tensor are known for this site, the modification is:

$$n = \frac{Qt}{b\left(\frac{T_{yy}}{T_{xx}} x^2 + \frac{T_{xx}}{T_{yy}} y^2\right)}$$

where Q = pumping rate

b = aquifer thickness

t = time of match point

x,y = coordinates of well slugged with tracer

Txx = major transmissivity component

Tyy = minor transmissivity component

n = porosity

In the above equation, the pumping well is at the origin and the major component of the transmissivity tensor runs between wells H-6b and H-6c as indicated from the well test data. The corrected porosities are 9.1% for the H-6b to H-6c test and 0.97% for the H-6a to H-6c test.

The 0.97% porosity appears reasonable for a fractured rock and occurs along the major axis of the transmissivity tensor. The 9.1% appears reasonable for a porous media. There is, however, almost an order of magnitude difference between the two. Since porosity is not anistropic, the disparity is probably caused by a heterogeneity in the Culebra. The suggestion of a discrete zone of flow, i.e., a long highly permeable fracture, a set of parallel fractures or a karst channel appears reasonable. Whether or not this discrete flow can be modelled adequately appears questionable. The problem lies in determining the number of discrete fractures and their locations.

DOE Responses:

Several good points are brought out here. We do not yet believe that we can define the flow system at the H-2 site. A double porosity system is appealing, but anisotropy needs to be determined and further tracer tests along different flow paths need to be conducted at the site to define the flow system. Again, we do not believe that the hydraulic conductivity at the H-2 site precludes the possibility or rapid fluid movement. The final report will include the results of additional tracer and anisotropy tests and respective modification to the code developed by Grove and Beetem.

Much discussion can be related to what value of porosity is typical of fractured rock vs. porous media. A minimum of effort has been spent on acquiring field data through extended tracer and anisotropy tests to evaluate and determine what these values are and what they mean. We hope to solidify our thoughts on double porosity media after the conclusion of our tests at H-6 and H-7. At this point, the number of discrete fractures and their locations are not the problem - neither is the notion of karst channel domination. A macroscopic point of view is the solution.

The H-6 tracer results certainly imply the existence of both fracture and matrix flow, at least under the flow regime set up by the test itself. There is not necessarly a disparity in the porosity determinations, nor need the difference be caused by local heterogenities.

EEG Evaluation:

The response to the anisotropy correction to the Grove and Beetem model is adequate. The comment regarding the ranges of porosity and dispersivity was not responded to. We were concerned about the apparent emphasis on the late time data for the curve match and the humps in the data.

The response regarding the tracer test at the H-2 pad is adequate. Anisotropy and tracer tests should be run at the H-2 pad. The EEG will be waiting for results from these tests.

The response to the H-6 comment is adequate. However, it should be pointed out that zones of discrete flow in a porous media is a heterogenity.

EEG Comment: (Page 38, 1st para., p. 10)

The Safety Analysis Report (page 2.6-35) indicated that two sets of joints exist in the Delaware Basin. One of these sets strikes NW to SE, in the same direction as the major component of the transmissivity tensor. Does this joint set have some relationship to the principal axes of the transmissivity tensor? What is the possibility that vertical or near vertical fractures formed by the joint set could be missed by the drilling activities and subsequently untested by the pump testing program?

DOE Response:

See previous discussion of principal tensor and fracture trends. The testing program would not test a set of non-intersecting or non-interconnected fractures by definition. The fact that dipping fractures are intersected requires interconnection of even vertical fractures with the borehole though the zone of influence has limits.

EEG Evaluation:

The fact that the boreholes intersect dipping fractures should be brought out better in the report.

EEG Comments:

(Page 38, 2nd para. to Page 39, 1st para., pp. 10-11)
The physical significance of the range of porosities to the following parameters should be discussed in more detail:

- o travel times
- o fracture flow or porous media flow
- o the directional characteristics of the porosity

The hydraulic conductivities should also be discussed in terms of fracture flow and porous media flow. The hydraulic conductivity values quoted in this report are averaged over the thickness of the Culebra, i.e., the hydraulic conductivity is assumed to be uniform throughout the thickness of the Culebra. In a fractured media, the hydraulic conductivity is not uniform. It is peaked in a fracture and near zero outside a fracture. How would the hydraulic conductivity vary throughout the thickness of the Culebra? What effect would this variation have on travel times?

(Page 39, 1st para., p. 10)
Figure 16 indicates that flow through the H-6 site is initially SSE. However, the flow path appears to curve to the southeast away from Nash Draw. What is the basis for concluding that flow through H-6 reaches Nash Draw? In addition, a flow to the south-southeast would eventually have to turn toward the southwest in order to reach Nash Draw.

It further appears that Figure 16 may need some refinement. The figure does not appear to have contours based on the fresh water altitudes at H-8, H-9 and H-10. In addition, the fresh water elevation at P-18 is extremely low compared to the elevations at the other wells. The validity of the fresh water altitude is questionable because of the low hydraulic conductivity at that well. How does the fresh water altitude map change if fresh water altitudes at H-8, H-9 and H-10 are included in the map construction at P-18 is eliminated?

DOE Response:

How hydraulic conductivity varies within the Culebra, both vertically and horizontally, will be a very difficult study. Cores taken from and measurements taken within the new ventilation shaft will help us. In our final analysis, a variation of parameters shall be input to the final regional model to simulate a variation of travel times under differing conditions.

The potentiometric surface as shown on Figure 16 typifies a very transmissive system approaching the WIPP from the north-east but encountering, in effect, a leaky boundary defined by the decrease in hydraulic conductivity from west to east and probably influenced by the presence of "salt" within the Rustler and lower transmissivities in the Culebra Dolomite. As the flux of groundwater encounters a less permeable portion of the aquifer, it resists flow and takes the more plausible avenue - down Nash Draw where we find transmissivities much greater in a number of wells. Figure 16 exemplifies the site specific information collected on and within the boundaries of the facility. The final report will include a refinement of the data, which will include tracer and anisotropy tests at locations south-east and south of the site. The validity of the use of fresh water altitudes based on fluid density influenced by low conductivity is also our concern. A final suite of

W.L. measurements and density determinations will be taken and evaluated for inclusion in the final report. There are no better estimates for discharge areas other than near Malaga Bend or south of Lagune Grande de la Sal.

EEG Evaluation:

Changing the values of parameters in the final regional transport model is good. However, the purpose of the comment was to get some feel if the reported values of hydraulic conductivity and porosity are typical or atypical of fractured rock. This does not appear to have been addressed in the response.

Until the vertical variation in hydraulic conductivity can be answered, the EEG suggests that references to hydraulic conductivity in the report be deleted. The hydraulic conductivities in the report are calculated by dividing the transmissivity by the aquifer thickness. This implies that the transmissivity is uniform over the aquifer thickness, when, in fact, the transmissivity may be concentrated in one or two fractures. In addition, it may be better to use the prosity-thickness product, which is the number really estimated from the tracer tests, rather than the calculated porosity.

The responses to the Figure 16 and the H-6 flow path comments are adequate.

EEG Comment: (Page 41, bottom para., pp. 11-12)

What is the basis for assuming 10 miles to the southeast? The data are all within 5 miles of the WIPP site. Most of the "path which would exceed 10 miles" is located in an area of very low hydraulic conductivity and in an area of unknown hydraulic gradient. In view of the southeast gradient over the study area (Figure 16 of the draft report), how can the radionuclides discharge at Malaga Bend? The direction of the hydraulic gradient would have to turn southwest in order for a radionuclide to discharge at Malaga Bend. At present, there are no data to support this. Are there other discharge areas for the Culebra?

DOE Response:

See previous comments.

EEG Evaluation:

It is hoped that the refinement of the data used to construct Figure 16 will support the previous responses.

EEG Comment: (Page 43, 1st para., p.12)

If travel time for a non-absorbing radionuclide in the Culebra becomes greater than 40,000 years, it is possible that the Magenta's westward flow from the WIPP to Nash Draw would provide a quicker radioactive release to the biosphere than flow in the Culebra? If so, perhaps future studies should concentrate on flow in the Magenta rather than on the Culebra.

DOE Response:

See previous comments on Magenta.

EEG Evaluation:

See our evaluation of your previous response to Magenta comments.

EEG Comment: (Pages 44 and 45, Continuing Investigations, pp. 12-13)

The following are suggestions that should be useful in the continuing study of fracture flow in the Rustler:

1. A review of aerial photographs through the area defined by WIPP-29, WIPP-25, WIPP-33, H-6 and H-5 should be made to see if any geomorphic features associated with either karst hydrology or fracture hydrology exist there. Thermal infrared photographs may be useful in locating springs or shallow ground water flowing in subsurface channels. This suggestion is made for several reasons:
    a. Larry Barrows, in a presentation to EEG, indicated that an elevated gravity anomaly existed in this area. He attributes it to a possible karst channel in the Culebra.
    b. A structure contour map (see Figure 1) on top of the Culebra was generated from Table 1 of the report. It indicates a depression

extending through the WIPP-25, H-6, H-5 area. This depression may be associated with a karst channel.

c. A fresh water altitude map (see Figure 2) for the Culebra was constructed from the data presented in Table 1 of the report. The figure indicates that a ground water high is associated with the depression. The ground water high indicates a potential for some flow to the west.

If possible, the review of the aerial photographs should extend from the northern part of Nash Draw to Malaga Bend.

2. The application of inverse techniques to flow in the Rustler should be interesting. For the most part, inverse techniques are in their infancy and are designed for porous media flow, not discrete flow. If it is decided that fracture flow can be modeled as a porous media, then the inverse technique developed by Neuman and Yakowitz ("A Statistical Approach to the Inverse Problem of Aquifer Hydrology, 1: Theory, "Water resources research, vol. 15, no. 4, pp. 845-850, 1979), Neuman et al. ("A Statistical Approach to the Inverse Problem of Aquifer Hydrology, 2: Cast Study, "Water Resources Research, vol. 16, no. 1, pp. 33-58, 1980) and Neuman ("A Statistical Approach to the Inverse Problem of Aquifer Hydrology, 3: Improved Solution Method and Added Perspective, "Water Resources Research, vol. 16, no. 2, pp. 331-346, 1980) should be tried. It appears that, at present, this is the only technique that has been published with an application to a real problem. Before the inverse techniques are applied to the Rustler, it should be decided whether flow in the Culebra is discrete or porous.

3. It appears that any contamination from a repository breach in Zone II would flow to the southeast. This is based on the flow paths as determined from Figure 15 and Figure 16 of the report. The area southeast of the WIPP should be studied further in terms of piezometric head, flow direction and discharge areas.

4. The tracer test at H-7 should be run similarly to the one at H-6. This should provide some more insight into the directional characteristics of the porosity and the areal extent of this

phenomenon. Because the hydraulic conductivities at H-6 and H-7 are almost the same, the time required to run a tst at H-7 should be about the same as at H-6.

If possible, two two-well tracer tests should be run at H-4 in order to determine the porosity along the major and minor components of the transmissivity tensor.

5. If it has not been done, the Groe and Beetem (1971) model, the Sauty (1980) model and the SWIFT model, if it is used, should be modified to account for the anisotropy of the Rustler Formation.

DOE Response:

Item 1.a. Bachman (1980, 1981) examined karst features extensively through Nash draw, along the pecos, and in the site area. He attributed the fill and depression at WIPP 33 to a karst-type process by which Nash Draw expands. In his field work and review of aerial photos, he does not attribute geomorphic features at the site to karst processes. Barrows found anomalously low gravity which he infers as due to removal of mass by dissolution (=karst). Barrows does not restrict karst to the Culebra - it is more likely in the gypsum units by his log correlations.

Item 1.b. Whether the gravity anomaly and structure contour maps show karst channels or not is still speculation. Perhaps comparing these maps with similar ones in regions of known karst will help somewhat. If the structure contour map does delineate a west-east karst channel, it cuts through some of the highest transmissivities tested at the WIPP (WIPP 25 and H-6), as well as the lowest (WIPP 30 and H-5). It also trends parallel to the minor component direction of the transmissivity tensor determined at H-6 and H-5; that is, the transmissivity is least in the direction of the channel.

Item 1.c. Figure 16 will be revised as previously stated. WIPP 30 is still being monitored, and all potentiometric data will be revised to be curent for the interim report.

Item 2. Those suggestions are well taken and shall be considered. Inverse techniques are subject to criticism; however, significant strides are being made towards utilizing these techniques and determining whether fracture media can be treated as porous media (Neuman, U of Arizona); C. Wilson, and J.B. Long, LBL). We do have the insight to perform these evaluations in determining whether we are dealing with fracture or porous - the objective of our site specific studies.

Item 3. Locations for testing in the southeastern part of the site have been of some interest. DOE 1 was considered, but the operations may have been unsuitable for the conversion of the hole to hydro testing. However, that pad and borehole continue to be candidates for further testing. Anisotropy tests at H-9 and H-10 are also being considered.

Item 4. See report, p. 7, last line; also p. 45.

Item 5. See p. 44.

EEG Evaluation:

The responses to Items 1b, 1c, 2, 3 and 5 are adequate. The response to 1a indicates that perhaps the Rustler Formation gypsum units should be studied in more detail to determine if karst channels exist in those units. Channels in the gypsum could be a pathway for radionuclide transport. The response to Item 4 indicates only that tests will be run, but gives no information on how they will be run.

BRECCIA PIPES

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STATE OF NEW MEXICO

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September 21, 1982

Joseph M. McGough
WIPP Project Manager
U. S. Department of Energy
Albuquerque Operations Office
P. O. Box 5400
Albuquerque, NM 87115

Dear Mr. McGough:

Enclosed is a summary of our comments on the Draft "Evaluation of Breccia Pipes in Southeastern New Mexico and Their Relation to the WIPP Site," by R. P. Snyder and L. M. Gard, Jr., USGS.

We would appreciate your consideration of these comments in the preparation of the final report. As with the other formal reports required by the Stipulated Agreement, we would like to have our staff and yours meet to discuss the final version of the document before its publication.

Sincerely,

Robert H. Neill

Robert H. Neill
Director

RHN:LC:eg
2-062-AG2-17-1-1
Enclosure

cc with attachment:
- George S. Goldstein, Ph.D., Secretary, Health & Environment Department
- Joe Hewett, Secretary, Highway Department
- Charles Turpen, Secretary, Energy and Minerals
- Jeff Bingaman, Attorney General
- Russell F. Rhoades, Director, Environmental Improvement Division
- Joe Canepa, Attorney at Law
- James K. Otts, Chairman, Rad-Waste Consultation Committee
- D. T. Schueler, Assistant Manager for Project of Energy Programs
- Wendell Weart, Sandia Laboratories
- TSC, IEA

EEG Comments on the USGS Draft Open File Report: Evaluation of Breccia Pipes in Southeastern New Mexico and Their Relation to the WIPP Site. by R. P. Snyder and L. M. Gard, Jr., USGS

INTRODUCTION

These comments are based upon a critical reading of the report and many of the references cited in it. The report has achieved in most respects, the purpose of presenting "all available pertinent up-to-date data and analyses concerning the existence of breccia pipes in the basin and the reef, potential for future breccia pipe development, and their significance to WIPP," as required by the Stipulated Agreement between the State of N. M. and DOE. In fact, the report has exceeded the requirements and expectations in many respects. A few recommendations to improve the quality of the report are given below.

GENERAL COMMENTS

Mechanics and Age of Formation of Breccia Pipes Over the Reef:

The authors of this report have done an excellent job in studying and reporting the characteristics of the known breccia pipes viz. at Hills A and C. Their conclusions on the mechanics and age of formation of breccia pipes (pp. 93-104) are primarily based on these detailed studies.

The authors have hypothesized that the initial collapse took place in a cavity in the Capitan limestone with the Tansill and Yates formations dropping into the cavity until the thick beam of the Fletcher Anhydrite was reached. In the second stage, the Fletcher beam failed, resulting in a catastrophic collapse of all the overlying formations in this cavity. Subsequent removal of salt from the Rustler and Salado formations around the margins of the pipe resulted in the outward-dipping beds at the surface.

Basically, this appears to be a sound hypothesis. However, a few difficulties with it are listed below.

a. Neither of the two exploratory boreholes (WIPP-31 and WIPP-16) drilled in the breccia pipe Hills A and C was drilled deep enough to encounter the Capitan Reef limestone. In fact, WIPP-16 was drilled only to the middle of Salado and the anhydrite cored at the bottom (1903 to 1981 feet) of WIPP-31, "is tentatively assigned to the Fletcher Anhydrite" (p. 42).

b. The probability of right conditions existing for the proposed mechanism appear to be very high all over the reef. Why then do the four known breccia pipes (Hills A, B, C and Wills-Weaver) lie within a five mile radius? Such localization indicates additional restrictive conditions which are not included in this hypothesis.

c. The outward dip of the Mescalero Caliche beds from the known pipes (e.g. at Hill A) has been interpreted to indicate "removal of halite from around the pipe" (p. 100). This removal of halite has been ascribed to a dissolution front which is very briefly described on p. 31. In the pipes too, halite is missing from the Rustler formation. At WIPP-31 drilled at Hill A, the cores show a chaotic collapse involving the Rustler formation, whereas WIPP-16 (at Hill C) encountered nearly intact Rustler formation in the pipe. And yet, the halite is missing from the Forty-niner and Tamarisk members of Rustler as found inside the pipe in WIPP-16. Did the collapse at Hill C occurred <u>after</u> the regional removal of salt from Rustler formation at this point? If so, how does one explain the outward dipping of the Triassic and younger strata at Hill C?

d. The mechanism of removal of halite and other soluble minerals from the fragments of Salado and Rustler formations found in the breccia pipe at Hill A (WIPP-31) is hypothesized on p. 98. It states that, following the collapse of Fletcher Anhydrite "beam", the unsaturated water filling the cavity would be forced upwards. Later, "much of the halite would be dissolved by this water and eventually the now saturated water would move downward and out through the existing paths in the reef." It is not clear how this would have occurred since the standing water implies either an impermeable base or a high hydraulic head and what would cause either of the two conditions to change for dissolution of salt and removal of the resulting brine?

Possibility of Breccia Pipes in the Basin

The report has concluded that, "known locations where deep dissolution occurs and forms structures called breccia pipes are limited to areas over the buried Capitan Reef." In support of this conclusion, the report has presented the results of investigation of suspected breccia pipes in the Delaware Basin and has shown why those features are not breccia pipes. However, the description of "Karst domes" (p. 14) needs to be improved to clearly show how "the formation of these domes is related to dissolution of the soluble portions of the units." Similarly, the idea of "blanket dissolution" (p. 84) to explain the features such as at WIPP-32 needs to be described more fully.

The report has not considered the possibility of a breccia pipe forming at depth in the basin. A description of Anderson's "brine density flow" (e.g. Anderson, 1980) and a discussion of why this mechanism is not expected to be forming a breccia pipe at depth through upward stopping, at the WIPP site, should be included in this report. The question of occurrence of breccia pipes in the Delaware Basin should be addressed from a genetic point of view, using the information from other evaporite basins and specific stratigraphic and hydrologic information from the Delaware Basin. Not having found one is not an argument against the potential presence of one.

SPECIFIC COMMENTS

Page 1, last sentence: This sentence, obviously added as an afterthought, does not belong in an otherwise excellent and scholarly report, and should therefore be deleted.

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Page 54, last sentence: The sentence states that the pipes at Hills A and C occurred at widely spaced times. On page 103, line 11, the statement is made that the two pipes formed at nearly the same time. Some clarification is desirable.

Page 92, last sentence: Why is it assumed that the pipes go down only to Zechstein formation and not even lower? Is there other evidence of preferential, deep-seated dissolution of salt from the Zechstein formation?

Page 93, line 15: The title of this section includes the age of the breccia pipes. The section, however, is a summary of earlier work and avoids assigning an age to the pipes. The section on page 105, which deals with possible effect on the WIPP site, mentions an age of 400,000 - 500,000 years ago.

Page 105: The possibility of occurrence of a breccia pipe in the Basin should be discussed here.

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EEG Comments on Draft USGS report by Snyder & Gard entitled
"Evaluation of Breccia Pipes in Southeastern New Mexico
and their Relation to the WIPP Site"

Comments Reflecting Alternate Interpretations

General comment c.--"The outward dip of the Mescalero Caliche beds from the known pipes (e.g. at Hill A) has been interpreted to indicate "removal of halite from around the pipe" (p. 100). This removal of halite has been ascribed to a dissolution front which is very briefly described on p. 31. In the pipes too, halite is missing from the Rustler formation. At WIPP-31 drilled at Hill A, the cores show a chaotic collapse involving the Rustler formation, whereas WIPP-16 (at Hill C) encountered nearly intact Rustler formation in the pipe. And yet, the halite is missing from the Forty-niner and Tamarisk members of Rustler as found inside the pipe in WIPP-16. Did the collapse at Hill C occur after the regional removal of salt from Rustler formation at this point? If so, how does one explain the outward dipping of the Triassic and younger strata at Hill C?"

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RESPONSE.--The USGS has considered a wide range of interpretations, perhaps including the above, within their peer review and approval system. The interpretations presented are the ones they feel best explain the mechanisms involved. They suggest that alternates may be presented in technical journals with wide scientific forums, if sufficient reasons exist.

Comments which deal with previous work

"Possibility of Breccia Pipes in the Basin

The report has concluded that, "known locations where deep dissolution occurs and forms structures called breccia pipes are limited to areas over the buried Capitan Reef." In support of this conclusion, the report has presented the results of investigation of suspected breccia pipes in the Delaware Basin and has shown why those features are not breccia pipes. However, the description of "Karst domes" (p. 14) needs to be improved to clearly show how "the formation of these domes is related to dissolution of the soluble portions of the units." Similarly, the idea of "blanket dissolution" (p. 84) to explain the features such as at WIPP-32 needs to be described more fully."

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RESPONSE.--The comments are directed toward questions on previous work which is accepted and credited for this report. This is not the proper forum for their discussion.

Comments which would require additional studies

General comment b.--"The probability of right conditions existing for the proposed mechanism appear to be very high all over the reef. Why then do the four known breccia pipes (Hills A, B, C and Wills-Weaver) lie within a five mile radius? Such localization indicates additional restrictive conditions which are not included in this hypothesis."

"Page 42, last sentence: Reasons for not drilling through at least the Fletcher should be outlined."

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RESPONSE.--The above comments are not central to the question of facility integrity, nor to the intent of the report. Perhaps other agencies or entities would be interested in funding their pursuit.

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Comments to be considered in later USGS Publications

Specific Comments

"Page 3, first paragraph: Questions 3, 4, 5 and 7 need more detailed treatment in the report."

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"Page 54, last sentence: The sentence states that the pipes at Hills A and C occurred at widely spaced times. On page 103, line 11, the statement is made that the two pipes formed at nearly the same time. Some clarification is desirable."

RESPONSE.--All of these comments deal with a request for more information, or, additional thoughts to consider which are not central to the purpose of determining the importance of breccia pipes to the integrity of the facility. These more properly belong in the realm of scientific inquiry and we understand the USGS is preparing a report in their Circular series which will treat these questions.

Comments which are primarily editorial in nature

General comment a.--"Neither of the two exploratory boreholes (WIPP-31 and WIPP-16) drilled in the breccia pipe Hills A and C was drilled deep enough to encounter the Capitan limestone. In fact, WIPP-16 was drilled only to the middle of Salado and the anydrite cored at the bottom (1903 to 1981 feet) of WIPP-31, "is tentatively assigned to the Fletcher Anhydrite" (p. 42)."

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"Page 80, line 6: "does increase" should be changed to "increases"."

RESPONSE.--We believe you will find that most of these were cleared up in the final, approved version of the report. Those which were not are principally a matter of editorial style of the USGS. Incidentally WIPP-16 was bottomed in the Rustler, not the Salado.

BRINE RESERVOIR REPORT

TME-3153

HEALTH & ENVIRONMENT department

"Equal Opportunity Employer"

STATE OF NEW MEXICO

ENVIRONMENTAL EVALUATION GROUP
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February 24, 1983

Mr. Joseph M. McGough
Project Manager
WIPP Project Office
U.S. Department of Energy
P.O. Box 5400
Albuquerque, New Mexico 87115

Dear Mr. McGough:

Enclosed are our review comments on the draft of "Brine Reservoirs in the Castile Formation, Southeastern New Mexico" (TME-3153). Appropriate personnel from EEG would be happy to meet again with the authors of this report, if further clarifications of our comments are desired. We will look forward to hearing your response to these comments.

Sincerely,

Robert H. Neill
Director

RHN:LC:eg

2-112AG2-21-10-1

cc: TSC, IEA

REVIEW COMMENTS
ON
THE DRAFT OF "BRINE RESERVOIRS IN THE CASTILE FORMATION, SOUTHEASTERN NEW MEXICO"

(TME-3153, December 1982)

by

Environmental Evaluation Group
Environmental Improvement Division
N. M. Health and Environment Department
P. O. Box 968
Santa Fe, NM 87503

February, 1983

EEG Review Comments on the draft of "Brine Reservoirs in the Castile Formation, Southeastern New Mexico" (TME-3153)

INTRODUCTION

These review comments are based upon a critical reading of the report and many of the references cited in the report. The report was evaluated to see whether it has achieved its stated purpose of determining "the characteristics and origin of these reservoirs and evaluate their potential impact on the integrity and stability of the WIPP site" (Executive Summary, p. 2, TME-3153), and whether the conclusions are supported by observed facts, experiments and analyses.

The comments are divided under the categories of Geology, Hydrology and Chemistry, following the organization of the subject report.

GENERAL COMMENTS

1. GEOLOGY

Location of the Brine

It should be realized at the outset that each borehole which encountered brine in the Castile formation did not necessarily encounter a separate "reservoir," unless it can be so proven on the basis of geology, geohydrology or geochemistry.

It is accepted that there are thirteen reported encounters of pressurized brine in the Castile formation in the northern Delaware Basin (12 shown on Fig. G-11 plus "H and W Danford Well No. 1" in Sec. 9, T 22S, R 29E- see EEG-7, p. 66). In addition, there are "numerous reports of small brine occurrences with sub-artesian heads in other parts of the basin" which suggests " a fairly uniform distribution of fluid throughout the Castile" (subject Report, p. G-41).

Previous DOE documents (e.g. TME-3080) stated that brine is restricted in a 6 mile wide "deformation front" which borders the buried Capitan Reef to the south. The subject document has extended this zone to "six to twelve miles" (p. G-37), presumably to include the Belco-Hudson and WIPP-12 encounters. There doesn't appear to be any scientific reason for drawing these boundaries, because if a future well encounters brine, say, 15 miles away from the reef, the boundary would have to be extended further. In any case, the twelve mile zone covers the entire WIPP site and therefore it is difficult to see any significance in this arbitrary exclusion.

Concerning the brine encounters that did not flow to the surface, the report states that, "none are located in the area covered by Figure G-11" (p. G-37). It is impossible to make a statement like this, since most rotary drilling operators for deep hydrocarbon wells would not report brine encounter in Castile unless it created a problem for them.

Stratigraphic and Structural Control of Brine Encounters

Pressurized brine has been found in Anhydrite-III unit of Castile in every reported case. There is some question about ERDA-6 where R. Y. Anderson and C. L. Jones interpreted the upper Anhydrite layer where brine was found as A-II on the basis of visual charcteristics. It is however, very likely, that this anhydrite was A-III also (Powers, personal communication).

While it is probably true that most pressurized brine encounters are related to at least some structural disturbance, Figure G-11 does not show it. Brine occurrence numbers 1, 2, 3, 4, 5, 6 and 9 (Fig. G-11) do not seem to be related to any structure at all. According to Snyder (personal communication) the structure contour map of Fig. G-11 was prepared by C. L. Jones as a very preliminary map. EEG recommends that the final version of this report should have a map prepared by using the up-to-date borehole information as well as the seismic data such as shown in Barrows' map (Fig. G-12).

The statement, "Examination of structures in Fig. G-6 reveals that deformation is mainly confined to the Castile formation...the underlying Delaware Mountain Group does not appear to be widely involved in the structures under discussion" (p. G-39) is based on a misleading drawing of WIPP-12, ERDA-6 and ERDA-9 boreholes in Fig. G-6. How can one use a borehole to correlate horizons below its total depth? Only WIPP-12, ERDA-6 and AEC-7 show structures in Castile. WIPP-12 and ERDA-6 stopped short of the Bell Canyon and AEC-7 did not penetrate deep enough to show the structure in Bell Canyon.

The Age of Deformation

"Between late Permian and Pleistocene" (p. G-2) is indeed the widest possible "bracket" for the age of deformation. Anderson and Powers (1978) concluded that "the salt structure (in ERDA-6) is inferred as post-microfolding." Kirkland and Anderson (1970) showed that microfolding in the basin follows Cenozoic structural trends, which resulted from the uplift and tilting of the basin in early to mid-Cenozoic time (King, 1948), or in late Cenozoic (Pliocene) according to TME-3153 (p. G-9).

The subject report concludes, "The most likely mechanism is formation during or immediately after deposition as a result of slumping and flow, although tectonic stresses cannot be ruled out." (p. G-23). The only reference cited in support of this conclusion is a paper by Riley and Byrne (1961)-p. G-22. Enclosed is a copy of two figures from this paper showing photographs of structures created by piling three layers of different density materials (Fig. 1). Of course some flowage and some deformation is seen, resulting from density contrasts. Ramberg (1963, 1967, 1968) has developed an elaborate theory of gravity--controlled tectonics based on such experiments. However, the Castile microstructures have resulted from tectonic stresses during late Cenozoic, because they are basin-wide, show close relationship with megafolds and the microfold axes generally parallel the trend of Cenozoic tectonism in the area (Kirkland and Anderson, 1970). The report almost correctly summarizes the discussion on the age of deformation, "by concluding that the deformation is probably Cenozoic, and could have occurred between 25 to 1 million years ago" (p. G-43). this implies that the postulation, "the salt structures developed in response to the latest stage of basinal tilting in late Pliocene to early Pleistocene time" (p. G-43) is accepted. Since the Pliocene began 7 million years ago, the "bracket" should be 7 to 1 million years.

In this connection, Kirkland and Anderson (1970) is wrongly cited on p. G-48--this paper rejects "syndepositional movement" as the cause of microfolds.

Brine Reservoir Formation

A mechanism for the formation of brine reservoirs is discussed on page G-44 to G-47 of the subject report. Conceptually, the postulated model of producing fractures in anhydrite by the upward movement of the underlying salt, appears acceptable. However, the discussion should be expanded to explain the following points.

1. How this would happen mechanically by using average reported values for the tensile strength of anhydrite and the expected values of tensile stresses generated by underlying salt flow.
2. How were the "typical average elongations for the structures around WIPP-12 and ERDA-6" calculated (p. G-44).
3. The postulated mechanism would open the fractures in an anhydrite at the upper surface. Almost all the brine filled fractures have been encountered in the lower part of anhydrite-III layer. The explanation that these holes are probably on the flanks of structures (p. G-46) does not satisfactorily explain this contradiction since both WIPP-12 (Fig. G-12) and ERDA-6 (Fig. G-11) appear to be in the crest area of the domal structures.

Pressurization of Brine Reservoirs

The theory of dilatancy (p. G-47) does not appear to provide a reasonable explanation for the differences in reservoir pressures in ERDA-6 and WIPP-12. Using the tidal efficiency (T.E.) from groundwater hydrology (see Walton, 1970), the theoretical reservoir pressures due to a thickness of overburden can be calculated. The tidal efficiency, in general terms, is a measure of the ability of the reservoir rock to transfer a pressure change outside the reservoir to the reservoir fluid. The tidal efficiency can be defined by

$$T.\ E. = \frac{\alpha}{\alpha + \phi\beta}$$

where α = compressibility of the reservoir rock

β = compressibility of water

ϕ = porosity

β is 4.4×10^{-10} pa^{-1} and the range of α given in TME 3153 is

2.9×10^{-9} pa^{-1} $\leq \alpha \leq 1.45 \times 10^{-7}$ pa^{-1}.

For a range of porosity of 0.1 to 10 percent, the tidal efficiency varies from 1.000 to 0.985. The calculated reservoir pressures in a particular reservoir differ by less than 50 psi for the range of porosity above. This indicates that increased porosity due to fracturing will have little effect on the reservoir pressure. W. Weart, at the February 15, 1983 Quarterly Meeting between DOE and EEG mentioned some preliminary model studies at Sandia National Labs that indicate rock movement and fracture closure as a possible repressurization mechanism.

The dilatancy theory works well in the fractured metamorphic rocks cited in McNaughton (1953). If the rock compressibility is 1×10^{-10} pa^{-1} (within the range of sound rock and jointed rock as given in Freeze and Cherry, 1979) the calculated pressure difference at WIPP-12, for example, is 850 psi for a porosity variation of 0.1 to 10 percent.

The anhydrite compressibility is not in the range necessary to invoke the dilatancy theory. A much more likely explanation for the pressure differences is brine migration. The above comments are presented in greater detail in an upcoming EEG report.

Geologic Evidence of Brine Origin

The discussion under this heading in the subject report (pp. G-48, 49) can be summarized as follows.

1. Small quantities of fluids which may have been present in the rock matrix may be trapped interstitially or within grains. (No quantitative values of assumed original amount of fluids and loss during compaction and diagenesis have been presented to reach this conclusion).
2. Gypsum dehydration would explain the origin of water in the brine, but "the evidence for primary gypsum is not compelling, although dehydration waters cannot be ruled out as a minor source of brine reservoir fluid" (p. G-49).
3. "Groundwater or meteoric water does not appear to be a plausible fluid source at WIPP-12, based on the lack of evidence of dissolution features and the tight contacts observed" (p. G-49). This line of reasoning does not take into account the possibility of brine migration to its present location after its formation elsewhere.

The "evidence" presented under this section is mostly negative. What is the geologic evidence of brine origin?

2. HYDROLOGY

Reservoir Pressure

The subject report uses the "fact" of the reservoir pressures being different from each other as a strong argument favoring the lack of interconnection between different encounters of brine in the Castile formation (pp. H-1, H-36, H-55, H-56). Table H-1 lists 12 pressurized brine encounters with measured or estimated "formation pressure." Only 4 of the 12 pressures (ERDA-6, WIPP-12, Belco and Gulf Covington) were measured; the rest were estimated by using the pressure of the drilling mud necessary to stop the brine flow in the well. There are so many possibilities of errors in the estimated pressures that these should be completely ignored. Among the 4 measured pressures, the differences are not significant--ERDA-6 and Belco had 2060 psig and 2075 psig respectively, Gulf Covington reported 1972 psig with conflicting data for the initial flow rate and WIPP-12 measured 1828 psig pressure. Using this data, it is difficult to see how one can reach to conclusions like, "The persistence of high and different hydraulic heads in the Castile brine reservoirs over millions of years is the principal evidence for their isolation." (p. H-1).

The maximum pressures for wells ERDA-6 and WIPP-12 are given in terms of wellhead pressure and reservoir pressure. Using the two pressures for each well and the depth to the reservoir, (p. H-44, H-51) the fluid pressure gradient is calculated to be 0.537 psi/ft. for each well (p. H-39). The average fluid pressure gradient from the D'Appolonia Data File Report is 0.533 psi/ft for ERDA-6 and 0.538 psi/ft for WIPP-12. Why are the pressure gradients used in the analyses different from the measured data?

In ERDA-6, the different hydraulic pressure gradients affect an 11 psi difference in the maximum wellhead pressure from 615 psig at 0.533 psi/ft to 604 psig at 0.537 psi/ft.

The total reservoir volume estimates are dependent on the total pressure depletion and as such are highly suspect. For example, the wellhead pressure in ERDA-6 as of 1/5/83 was 552.5 psig and is still rising. A Horner plot extrapolation (Fig. 3 attached) indicates the pressure could go as high as 615 psig. If the fluid pressure gradient were 0.533 psi/ft, then the maximum wellhead pressure would be 615 psig. The total pressure depletion would then be zero and the total reservoir volume infinite!

The use of the maximum reservoir pressure of 604 psig versus 615 psig could make a large difference in the total reservoir volume calculation especially as the pressure in ERDA-6 continues to increase.

What is the basis for the statement that the Castile brine reservoirs have maintained their hydraulic heads over millions of years (p. H-1, H-38, H-39)? The elevation of the reservoir relative to sea level has changed over "millions of years" and so has the hydrology of the area.

Volume and Interconnection

The volume of brine reservoir encountered by WIPP-12 has been calculated to be 20 million barrels (p. H-52). Even if the reservoir is assumed to extend through one-half of the 317 feet thickness of Anhydrite-III, the brine would be found underlying an area of 140 million square feet or more than 5 square mile. The actual extent of the brine is probably several times this area, since it is unrealistic to assume that the brine fills the entire lower-half of the anhydrite layer at a uniform porosity of 0.5%.

The volume of ERDA-6 brine has been calculated to be at least 440,000 bbl. (p. H-46). Since this calculation was based on a pressure depletion of 75 psi and the pressure recovery through January 5, 1983 reached 553 psig (which is equal to 51 psi pressure depletion), the volume of the brine reservoir intercepted by ERDA-6 is most likely much larger.

The attempts to correlate the volume calculated from flow tests with the "antiforms" presume the localization of brine within such structures. This attempt is meaningless since the structures are not well defined. In the case of ERDA-6, the structure based on a very sparse borehole data (Fig. G-11) is used (p. H-47). In the case of WIPP-12 the structure interpreted from a seismic time structure map is used (Fig. G-12, p. H-53). The ERDA-6 structure is too large for the calculated brine volume (p. H-47) and the WIPP-12 structure is too small (p. H-53).

The results of interference testing (p. H-37) are inconclusive because the time of testing and observation was not sufficient for the distance between wells. ERDA-6 is about 23,000 feet away from WIPP-12 and the pressure response in the former due to pumping from the latter would take much longer than three weeks (Fig. H-13). Both ERDA-6 and WIPP-12 showed sustained recovery after one year of shut-in, and this recovery is probably generated from within a radius of influence of less than 5000 ft.

Similarly, AEC-7 is 10,000 ft. from ERDA-6 and the pressure response in that well was monitored for less than two months. Even if the tests and the monitoring had continued for a long time, the response due to interference would be extremely small considering the large distances.

Analysis of Flow Tests

All the known methods of analysis of flow test data are listed on pp. H-18 and H-19, yet only the Horner straight line method is actually used. The explanation of why the other methods were unsuitable has been provided in 10 pages (H-19 to H-28). The general explanation is that one or more of the theoretical assumptions were violated. The assumptions are no less violated in Horner's method than in others. All the results should be presented anyway--even in an appendix.

If all the data is not used, there is a possibility of biasing the results. The expression "highest quality data" is used on pages H-41, H-42 and H-43. It seems more apropriate to use all the data and present the results. If the data is poor, the results could be labeled as questionable. A range of values is more useful than a single, carefully chosen, value.

Migration of Brine

The conclusion (p. H-59) that present reservoir pressures are less than lithostatic because of brine flow into fractures, appears to be incorrect. Calculations of the lithostatic pressure by EEG indicate a minimum depressurization of the ERDA-6 brine of 500 psi and of the WIPP-12 brine of 1000 psi. These estimates are considered minimum because the present overburden thickness was used and not a pre-erosional value. In addition, the overburden was assumed to be pure halite. Within the range of formation compressibilities measured for the anhydrite, the variation of porosity of 0.1 to 10 percent caused only a 50 psi difference in the calculated lithostatic pressure.

Therefore it appears unlikely that the reduction of ancient reservoir pressure to the present level is due to increased porosity from fracturing. A more reasonable explanation is that some fluid has migrated to an area of lower potential. As the calculated lithostatic pressure in WIPP-12 is greater than ERDA-6, the WIPP-12 may be more mobile. This would appear to be supported by the observation that WIPP-12 encountered brine in clean, smooth fractures while a recrystallized, brecciated zone held the brine in ERDA-6 (p. G-35). Other brine occurrences of much lower pressure (p. G-37) may represent areas where the brine migration is at a more advanced stage.

3. CHEMISTRY

Samples (Page C-8, Sec. 3.1)

D'Appolonia describes the rationale and techniques for brine sampling very adequately and thoroughly. However, with the excetion of some data reporting tables and figures (Table C-2, C-5, C-6, C-7 and ancillary Figures) which indicate averages, and variation, there is a distinct absence of more rigorous statistical evaluations. Analysis of variance (F-test) and student's t-tests would provide a clearer indication of the reliance which may be placed on certain sample population sets (e.g., Flow Test-3 versus Flow Test-2, etc.) and their respective subsets (downhole versus flowed sample). Such additional tests would appear to be easily performed, given the detail with which D'Appolonia has assembled their basic data.

Evaluation of Mineral Saturation (§Page C-12/13; Sec. 3.3.2)

The use of saturation indices as a means of predicting the degree to which a solution is at (or near) thermodynamic equilibrium with its host rock solid phase is a useful tool. Tables C.3 and C.4 provide a summary of log Ksp and log IAP values for five mineral phases. The discussion on page C-12 and C-13 point out that if the IAP is equal or greater than the Ksp, then the solution is saturated with the phase. The data in Table C.3 and C.4 suggest that calcite as well as halite may not be saturated in ERDA-6 and WIPP-12. The more detailed data in Table C.2 were analyzed by an EEG consultant (Langmuir, 1983) and he concluded that there is undersaturation with respect to sylvite and polyhalite as well. We do agree, however, that the brines appear to be at or very near equilibrium with their surroundings.

Janecke Diagram (Page C-13; Section 3.3.2; Figure C-3)

Further details are needed concerning the Janecke diagram beginning in the last paragraph on p. C-13. This figure appears to be identical to the figure on page 993 of the paper by Harvie and Weare (1980), and shows zones which are saturated with respect to halite. It is not clear how this figure provides information relevant to ERDA-6 and WIPP-12.

Isotope Chemistry (Page C-20, C-39 and C-40, Sections 3.3.4 and 5.1.3)

The linear regression lines shown in Figures C-20 and C-21 using δD versus TDS and $\delta^{18}O$ versus TDS are interesting, but with only two points, the conclusions are not convincing. It is recommended that data on Union also be plotted with that of ERDA-6 and WIPP-12.

Sulfates (Page C-21; Section 3.3.4 , last paragraph)

This paragraph seems to suggest that the sulfate (^{34}S) of the brines is not in equilibrium with the ^{34}S of the rock. If so, this is inconsistent with the statement on p. C-22, 4th paragraph, which states that the $\delta^{34}S$ in sulfate is equal to values characteristsic of sulfates in permian evaporites. Then on p C-41, the last sentence of the first paragraph concludes that the $\delta^{34}S$ values for sulfate in brine are consistently less than the $\delta^{34}S$ values of the rock, which therefore precludes dissolution. Based on Tables C.5 and C.6, this latter statement seems to be correct.

Summary of Findings (Page C-31, Section 4.4)

Since the H_2S, methane and heavier hydrocarbons in WIPP-12 brine appear to have a thermogenic origin, and since the ^{34}S of the brine and anhydrite suggest that the WIPP-12 brines at their present location have not been exposed to high temperatures, one might conclude that the WIPP-12 brine may have originated at depths much greater than their present location. This would also point toward a closer association with the DMG. The data also suggest an origin for ERDA-6 brine which is different from that of WIPP-12. Other possible origins of the thermogenic H_2S and hydrocarbons are discussed in Section 5.1..4, page C-48.

Seawater Evaporation Model (Page C-36, Section 5.1.2)

The 2nd paragraph on p. C-36 states that WIPP-12 brine is "probably saturated" with glauberite, whereas ERDA-6 brine is not. The results in Table C-3 suggest that ERDA-6 brine is more clearly saturated with glauberite than is WIPP-12. This would tend to refute the discussion in this paragraph, and the "Summary" on page C-38.

Seawater Evaporation Model (Pages C-44-45, Section 5.1.3)

Although the data in general seem to support a seawater evaporation model, some of the arguments used are not technically consistent with the literature. For example, suggesting on p. C-45 that reaction with marine clays will increase the δD and $\delta^{18}O$ is contrary to the literature (Savin 1980; Faure, 1977). As indicated in the Table below, the water would be depleted in ^{18}O and enriched in deuterium.

Table 1 Isotope Fractionation Factors for Clay-Water Systems at Earth-Surface Temperatures

MINERAL	OXYGEN	HYDROGEN	REFERENCE
Montomorillonite	1.027	0.94	1
Kaolinite	1.027	0.97	1
Glauconite	1.026	0.93	1
Gibbsite	1.018	0.984	2
Illite	1.0234	-	3

1. Savin and Epstein (1970)
2. Lawrence and Taylor (1972).
3. James and Baker (1976).

SPECIFIC COMMENTS

P. G-9, 2nd para.: The age of the dike has been measured as 30 ± 1.5 m.y. (Urry, 1936) and 34.8 ± 0.8 m.y. (GCR, p. 3-80). Therefore, the first episode of tilting did not occur in "very early Tertiary time" but in "mid-Tertiary time".

P. G-10, 2nd para.: The apparent dip observed in the drifts at the site is approximately 2° to the south (Geotechnical Field Data Report no. 5, p. 3-3).

P. G-17, 1st para.: It is possible that the top Castile anhydrite encountered in ERDA-6 may be A-III rather than A-II. In DOE-1 core, A-III showed thin bedding laminations similar to A-II.

P. G-29, First sentence: What is the correspondence between the fracture orientations (Fig G-10) and the structure contour map of Halite II (Fig. G-11) and the seismic isochron map (Fig. G-12)?

P. G-32, 3rd para.: The equation for compressibility is given as $C_p = \frac{1}{\phi K}$ where ϕ is the effective porosity and K is the bulk modulus of the rock mass.

In this equation, if $\phi \longrightarrow 0$, $Cp \longrightarrow \infty$
and if $\phi \longrightarrow 1$, $Cp \longrightarrow Cp$ of rock mass, instead of brine.
Clearly, this equation is incorrect or correct over a limited porosity interval only.

P. G-37, 2nd para: Fig. G-11 should include the borehole "H and W Danford well no. 1" in Section 9, township 22 South, Range 29 East which encountered pressurized brine in Castile (see EEG-7, p. 66)

P. G-43, 4th para.: The conclusion about the deformation having occurred between 25 to 1 million years ago does not match with the conclusion about the most likely mechanism for deformation being "slumping and flow immediately after deposition" (p. G-23, first sentence), since the deposition occurred 225 million years ago.

P. H-1, 2nd para.: Only two brine occurrences were tested, yet here and at other places in the report reference is made to "other Castile brine reservoirs" and conclusions are drawn concerning all the brine occurrences. How are brine reservoirs identified? Does each well that encounters brine represent a separate reservoir?

P. H-1, 3rd para.: Throughout the report, ages are suggested (million years, millions of years) without any supporting evidence. Where do the ages come from?

P. H-4 section 2.2.2 Why doesn't the drainable volume have a direct bearing on the integrity of the WIPP site repository?

What is meant by drainable volume? Does it include only the large fractures or is the microfracture volume also included?

P. H-10, H-11, section 3.2.1, 2nd para.: How sensitive were the measured flow rates to these affects?

P. H-11, 1st para.: Do the gas/liquid separators restrict flow?

P. H-22, 1st para.: The correction to Horners method for fractures is empirical and based on drainage area. The drainage area is very poorly known. So how can this be justified over another method?

P. H-35, 3rd para.: As the heads in the different brine occurrences differ, lateral flow between brine occurrences is also possible.

P. H-36, 2nd para., last sentence: Substantial volumes of brine will be produced only if larger fractures are intersected. Brine in microfractures or in intact anhydrite would not enter a borehole in large enough volumes to cause a problem in normal drilling. Unless the brine caused a problem (flowing at the surface) the oil and gas drillers would not be interested in it.

Granted that no evidence exists to suggest a regional, homogeneous aquifer in the Castile Formation. However, a regional heterogeneous aquifer may exist. This aquifer would be characterized by zones of fracturing and high permeability (known brine occurrences) and zones of intact anhydrite or microfractures and low permeability. Upon fracturing, a high permeability brine reservoir could form as brine from the low permeability zone entered the fractures.

P. H-37, line 17: "fracture-enhanced" should be "large fracture enhanced." From Fig. H-8, H-9 there is no evidence that any kind of a boundary for the micro-fractures was encountered.

P. H-37, last para.: Granted that the fractures are not uniformly distributed. However if one assumes microfracture or porous media flow between the large-fracture zones of WIPP-12 and ERDA-6, a radial flow, or even a 1-D linear flow model could be used.

All the Horner tests, in fact, assume radial flow and all your results are based on Horner's method. Once you leave the large fractures, the radial flow may not be a bad assumption.

P. H-38, 1st para.: Calculations by EEG indicate that the transducer would need to raise the water level in AEC-7 by almost four feet to cause the 2 psi increase in pressure. Is that reasonable?

Page H-38, last para.: The existing hydraulic gradient may not be indicative of the geologic past. Where do you get "millions of years"?

Page H-39, line 10: It is stated that the disposal facility will be open for a few tens of years, after which it will be sealed and the pressure will return to its present state.

There is no reliable calculation on how fast the creep of salt will encapsulate and compact the repository. TME-3153 suggests 300 years.

Page H-41, bottom of page: The quantity F = correction fator when calculating permeability for a vertically fractured well is presented. An accurate estimate of this correction factor requires a knowledge of x_f/x_c, the fracture penetration. See Figure 11.12, p. 153 of Advances in Well Test Analysis, R.C. Earlougher, SPE, 1977.

Page H-43, last paragraph: Why wasn't a log-log analysis attempted for this early time data?

Page H-43, paragraph 3 and H-44, paragaph 1: Why do you expect the permeability of anhydrite (core test) to be similar to that of halite (DST-2472-1/SBU).

Pages H-45 and H-46: Three volume estimates are presented.

In using the equation $V = \Delta V/(\Delta p c_t)$, it must be remembered that $\Delta p = pi - \overline{p}$ where pi = reservoir pressure prior to any brine flow and $\overline{p}$ is the average reservoir pressure after a known amount of brine flow.

The downhole pressure prior to any testing is a measure of pi. The downhole pressure following flow test 1 is not a good measure of $\overline{p}$. The surface shut-in pressure as of 10/18/82 is a measure of $\overline{p}$ if it can be demonstrated that there is no gas bubble at the head of the well. At best, only the second calculation on page H-46 is trustworthy.

Page H-46, bottom: If the largest volume figure, 2.2×10^6 bbl, is used, the circle radius is 2,646 ft. Although 2.2×10^6 bbl was an upper bound using a reservoir pressure of 1985 psig on October 18, 1982, it would not be an upper bound when ERDA-6 finally reaches equilibrium (which, because k is so small, may take years).

Page H-52: Two volume estimates for WIPP-12 are presented.
The downhole pressure following flow test 2 is not a good measure of $\overline{p}$. The surface shut-in pressure of 168 psig on 10/18/82 is a good measure of $\overline{p}$ if it can be shown that the well does not have a gas cap.

H-54, lines 17-19: Already 80,000 barrels of brine have been produced from WIPP-12 with less than a 40 psi reduction in pressure. 100,000 barrels may be a reasonable volume for a single event, but multiple events could produce much, much more.

Page H-55, bullet one: Measurement in 4 wells out of 12 does not constitute most.

Page 55, Observations 1) and 2): If this means that each encounter of brine represents a different reservoir, then the observation is clearly incorrect.

Page H-56, line 18: Is $K<5 \times 10^{-5}$ md obtained from a Salado sample at the WIPP site? Sandia (SAND 81-7073) obtained an in situ permeability of the Salado salt of about 1.0×10^{-3} md.

Page H-56, 3rd and 4th para.: The data is inconclusive about the interconnection between ERDA-6 and WIPP-12. Extending the conclusion derived from such data to other brine encounters is unscientific.

Page H-57: The permeabilities estimated are dependent on the fractures encountered. The high k at ERDA-6 is less than the low k at WIPP-12. Does that indicate two distinct zones of hydraulic characteristics or simply chance fracture encounters?

Page H-58, 2nd para.: Storage capacity and permeability are hydraulically unrelated parameters. The relationship in ERDA-6 and WIPP-12 is coincidental.

Figures 2 (Exec. Summ.), G-6, H-14: Show the boreholes only down to the total depths. Do not correlate below T.D. Also, the key map scale (in the upper right hand corner) is incorrect.

Figure H-12: Gonzales (in SAND 82-1012) gave the fresh water head of well H-1 as 3012.5 ft.

EDITORIAL COMMENTS

P. G-43, line 14: The word "probably" is misspelled.

P. G-44, line 10: "Figure G-12" should be "Figure G-13".

P. G-45, line 7: "Figure G-11" should be "Figure G-13".

P. C-8, Section 3.0, third line: The word "preceded" is misspelled.

P. C-22, Section 4.0, third line: The word "preceded" is misspelled.

P. C-26s, Section 4.3.2, first line: The word "preceding" is misspelled.

P. C-34, Section 5.1.2, 15th line: "Figure C-29" should be "Figure C-28".

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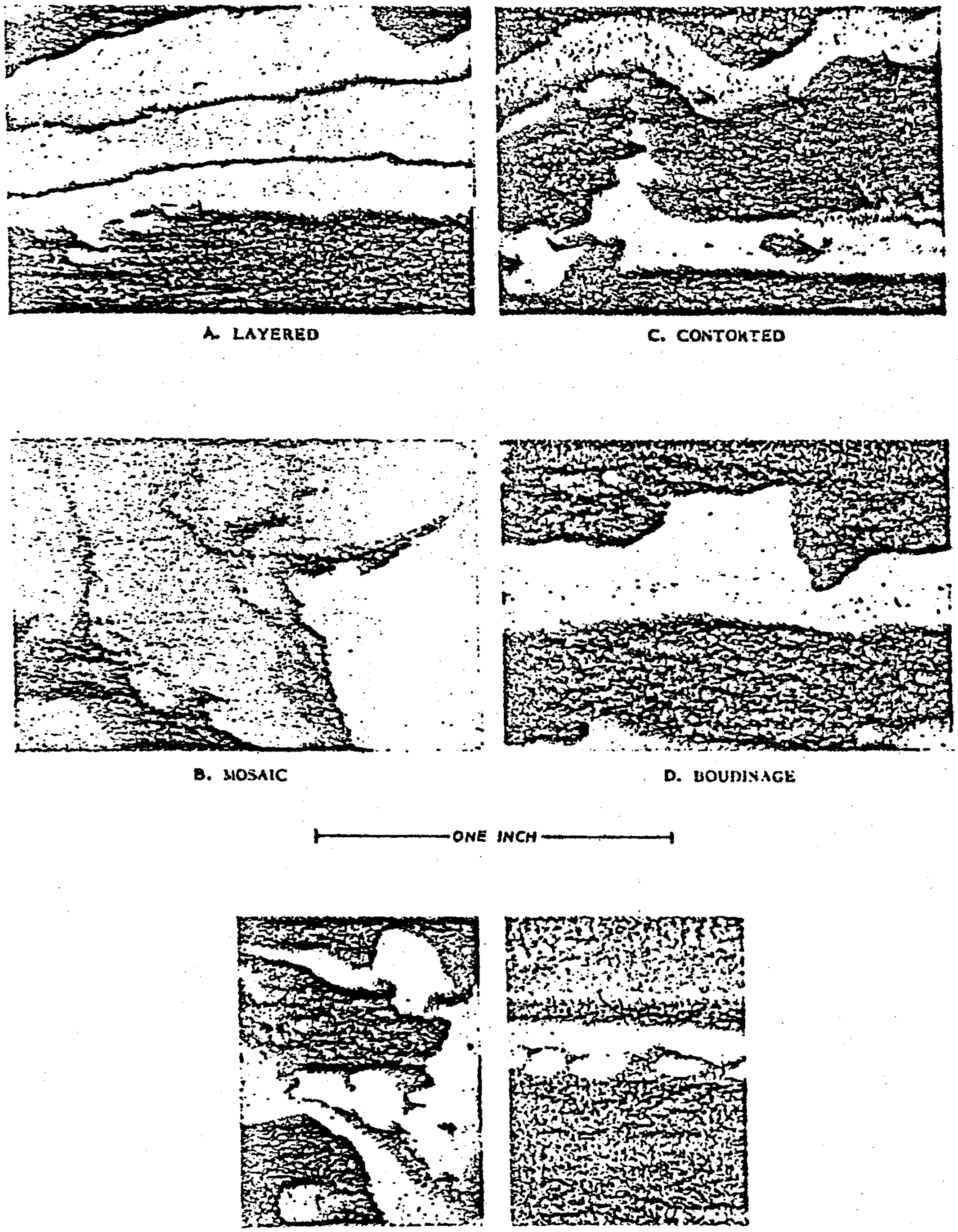

FIG. 4.—Anhydrite structures formed in model experiments.

From Riley and Byrne (1961)

FIGURE 1

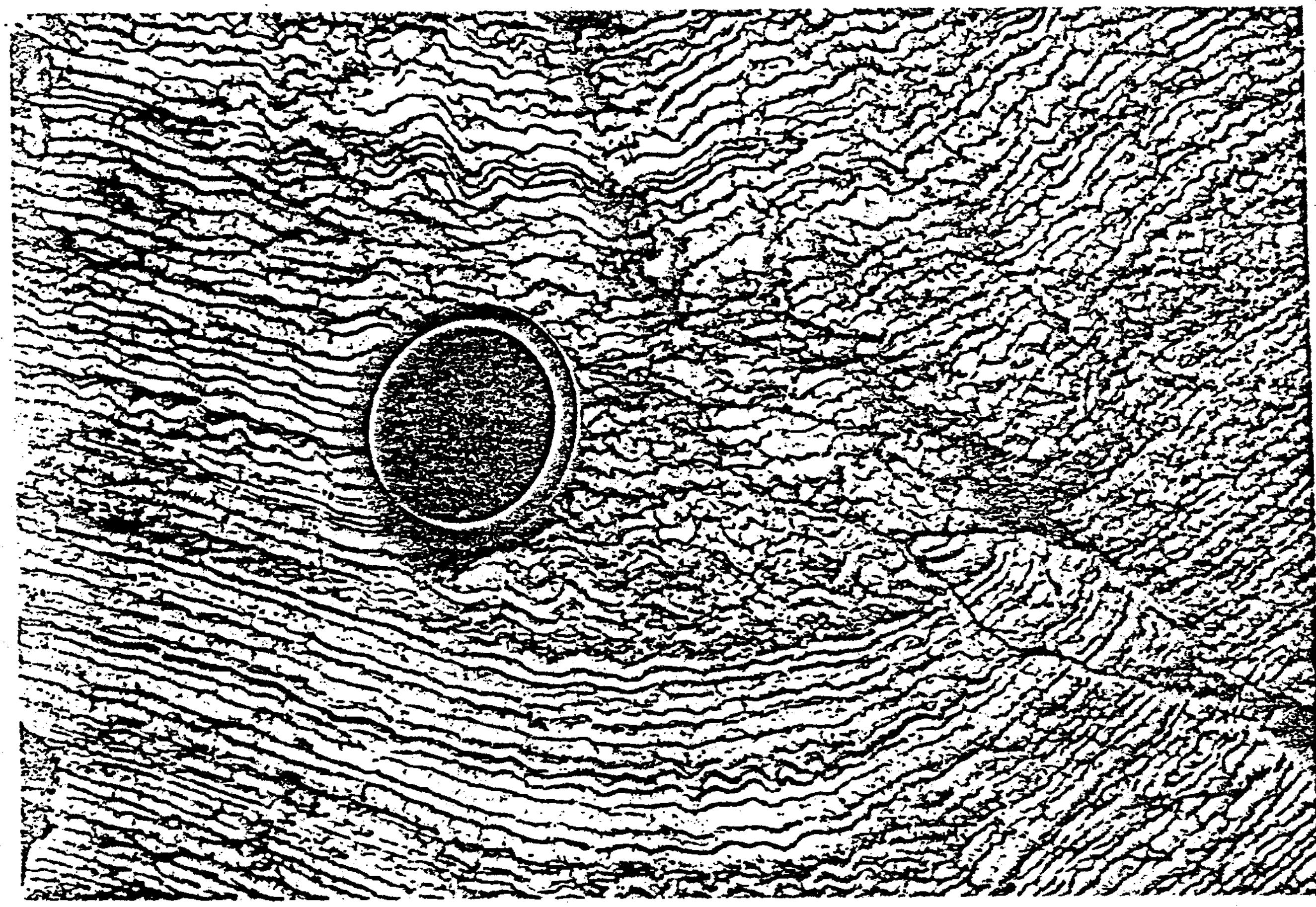

Fig. 12 Microfolding in Castile at the Stateline outcrop, Stop 3-5. Camera lens-cap is for scale.

(photo: Lokesh Chaturvedi)

From EEG-7

FIGURE 2

PLANS FOR SIMULATED WASTE EXPERIMENTS

SAND 82-0547

HEALTH & ENVIRONMENT department

"Equal Opportunity Employer"

STATE OF NEW MEXICO

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May 27, 1982

Joseph M. McGough
WIPP Project Manager
U. S. Department of Energy
Albuquerque Operations Office
P. O. Box 5400
Albuquerque, NM 87115

Dear Mr. McGough:

There is attached a summary of our comments and recommendations concerning the Draft of "Simulated Waste Experiments Planned for the Waste Isolation Pilot Plant (WIPP), printed March, 1982.

We would appreciate knowing your response to these views.

Sincerely,

Robert H. Neill
Director

RHN:eg

2-031-AG2-20-2
cc: TSC, IEA
Enclosure

REVIEW COMMENTS

CONCERNING

SIMULATED-WASTE EXPERIMENTS PLANNED FOR THE WASTE ISOLATION PILOT PLANT

DRAFT FOR REVIEW

Printed March, 1982

Comments by

Environmental Evaluation Group

Environmental Improvement Division

Health and Environment Department

P. O. box 968

Santa Fe, New Mexico 87504-0968

May, 1982

General Comment

The description of the experiments often is too brief to permit a satisfactory technical review. We would appreciate receiving a copy of the Test Plan for each experiment when that becomes available in draft so that we may have an opportunity to comment prior to its final preparation.

Specific Comments

1. Section 2.2.2 (a), page 8. EEG agrees that waste form stability and leachability can be evaluated by in situ tests; however, there appears to be very little attention given in the SWE plan to the in situ evaluation of the TRU waste form stability and leachability. There is a need for information at an early stage to validate the TRU waste acceptance criteria, to assure safe waste handling procedures, and to plan for retrieval if deemed necessary. Therefore the SWE should include experiments designed to provide in situ investigations of TRU waste form interactions. Of particular concern is the radiolytic production of explosive gas mixtures in 210ℓ drums within three months to five years (Reference 4). It is recognized that such in situ studies would require drums containing actual TRU wastes, however, because the results are essential to an assessment of retrievability, the experiments should be considered under the SWE program schedule.

2. Section 3.3, pages 20-24 - The "Overtest for Simulated DHLW" experiment has two phases with the second phase being addressed to the elucidation of the mechanics of reestablishing a stable, impermeable backfill in DHLW repository rooms. It is recommended that a similar experiment be planned for the TRU waste repository room. The experiment should be addressed to the consolidation of backfill and TRU-containing drums into a stable, impermeable layer. It would provide data on the time span following closure over which a liquid breach scenario has validity.

3. Section 4.2, page 32-36 - This section does not indicate plans for verification of permeability studies for gases produced in untreated wastes. Earlier laboratory and borehole studies by Sandia (Reference 3) have shown that gas pressures on the order of lithostatic could be produced over a several hundred year period. There is question concerning the validity of these data under repository conditions, since the earlier tests were conducted either under controlled laboratory conditions or at only two depths in one borehole (Reference 4). The studies proposed by BNL on page 190 of Reference 4 should be considered.

4. Section 4.1.2(d), page 32 - Although there is considerable data available on the thermal near-field effects on rock salt, consideration should be given to the thermal effect on plastic flow under actual repository conditions.

5. Section 4.2, pages 32-36
   a) Tests should be included to demonstrate ability to handle explosions or fires involving wastes. Such tests should be scheduled prior to the handling of radioactive wastes.
   b) Experiments designed to verify the radionuclide leach and sorption assumptions used in the long-term consequence modeling of Reference 5 should be initiated at the earliest possible time, since meaningful data would not be available for several years.

c) This section does not indicate whether investigations of the durability of "TRU waste-drums" will include studies of the TRU waste "boxes," such as the DOT-7A FRP coated plywood box, and the RH steel drums. It is suggested that studies of the durability of these additional waste containers be included.

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4. Brookhaven National Laboratory, "Nuclear Waste Management Technical Support in the Development of Nuclear Waste Form Criteria for the NRC," Task 2, Alternative TRU Technologies, NUREG/CR-2333, February 1982.

5. U. S. Department of Energy, "Final Environmental Impact Statement, Waste Isolation Pilot Plant," vol. 1 and 2, October 1980.

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RECEIVED
AUG 30 1982
ENVIRONMENTAL
EVALUATION GROUP

AUG 24 1982

Mr. Robert H. Neill
Director
State of New Mexico
Environmental Evaluation Group
P. O. Box 968
Santa Fe, NM 87503

Dear Mr. Neill:

EEG Comments on Simulated Waste Experiments

Enclosed for your use are responses to the comments and recommendations in your letter dated May 27, 1982, concerning the Draft of "Simulated Waste Experiments Planned for the Waste Isolation Pilot Plant (WIPP)."

Sincerely,

J. M. McGough
Project Manager
WIPP Project Office

WIPP:JMM 82-0539/5985A

Enclosure

cc w/enclosure:
C. C. Little, TSC
C&C File, IEA, TSC
T. Hunter, Org. 9732, SNLA

General Comment

Our response to the EEG review comments corresponds to the EEG specific comments number. The general comment by EEG suggested that the description of experiments was too brief for satisfactory technical review. The descriptions of the Simulated Waste Experiments (SWEs) were intentionally summarized in the document. More emphasis was given to objectives and technical issues. The descriptions provided, however, are more than adequate to determine the purpose, scope, and physical configuration of the experiments. Further details will be developed prior to initiation of each experiment and the test plan for each experiment will be continually revised as the experiment progresses.

Specific Comments

1. Section 2.2.2 (a), page 8: The EEG comment concerned the requirement for in situ evaluation of the TRU waste form stability and leachability.

   Based on the considerations which follow we consider in situ tests to evaluate degradation further as unjustified and not necessary.

   Previous laboratory and field studies conducted by SNL on the TRU waste form defined the limits of waste degradation, by-products and potential consequences on environmental safety of the WIPP (SAND79-1305). These studies were conducted under accelerated conditions (higher levels of actinides, higher temperatures, etc.) to observe by-products of the degradation process. The generation of gas mixtures was well characterized and its impact on long-term storage was assessed.

   Drums of CH TRU wastes that meet the WIPP waste acceptance criteria on gas generation will not generate explosive gas mixtures from radiolytic degradation. Their rate of hydrogen production is too low (reference: SAND79-1245, Chapter 2). Actual drums of such waste have been in semi-enclosed temporary storage for years without significant operational problems associated with gas generation. TRU wastes are, however, in existence which could potentially generate explosible concentrations of gases (reference: SAND79-1245, p. 16-17); these wastes, containing large concentrations of ^{238}PU, would not meet WIPP waste acceptance criteria.

   Storage rooms in the WIPP will be ventilated and monitored, thereby preventing accumulation of hazardous concentrations of gases. Gas monitoring is generally a routine procedure and not an experiment. As such, it was not described in the SWE document, particularly since actual (TRU-contaminated) wastes are not in place during this stage of the WIPP.

2. Section 3.3, pages 20-24: The EEG comment suggested additional testing of backfills for TRU wastes. Response of candidate backfill will be examined not only in the DHLW Overtest Experiment but also in the Plugging and Sealing Tests documented in SAND81-2628. The current R&D program is structured to address the sealing potential of backfills. It includes not only the tests outlined in SAND81-2628, but also corresponding modeling and laboratory tests. Backfill for the TRU waste demonstration rooms is planned primarily for fire protection purposes and not for sealing. Sealing for the TRU waste rooms will be accomplished in access drifts using techniques evaluated in the other planned tests described in Section 4.2, Plugging and Sealing, SAND81-2628.

3. Section 4.2, pages 32-36: EEG comment concerned gas pressure buildup over a period of several hundred years. We believe that for the expected gas generation rate and range of salt permeabilities that gas pressurization will be well within acceptable limits. A study of this subject ("Potential for Salt Fracturing Due to Gas Generation" (DRAFT), 1979, D'Appolonia), in addition to the references cited by EEG, leads to this belief. The references show that significant fractures could only occur at very low permeabilities, lower than anticipated at WIPP, and at gas generation rates higher than expected for the WIPP storage horizon.

   Gas permeability in situ tests, as addressed in Section 4.2.2.1 of SAND81-2628 where a plan for permeability measurements is described for the salt formation at the WIPP facility horizon, will be performed for verification. The results of these tests will be utilized, if needed, to support previous studies on this subject.

4. Section 4.1.2 (d), page 32: EEG comment pertained to the thermal effect on salt creep rate under actual conditions. The effect of temperature on salt creep rate will be evaluted in situ as planned in the heater tests described in Sections 3.2 (12-W/m^2 mockup) and 3.3 (Overtest for Simulated DHLW). Thermal "very" near-field effects on rock salt will be examined by measuring the mock DHLW canister borehole deformation throughout the duration of the test. Detailed plans for obtaining near-field (room) and "very" near-field (around-the-canister) response data will be considered during preparation of the Test Plans.

5. Section 4.2, pages 32-36:

   a) EEG comment related to demonstrating the ability to handle explosions or fires involving storage of waste. The WIPP design has attempted to minimize fire and explosion hazards and planned operating procedures are expected to minimize these hazards both before and after placement of radioactive waste. Code of Federal Regulation (30 CFR Part 57.4-58) prohibits building of fires underground. Therefore, no underground tests to demonstrate ability to handle explosions or fires involving waste are being planned as part of the SWE or any other experimental program.

b) EEG comment suggests initiating early tests designed to verify the radionuclide leach and sorption assumption used in long-term consequence assessments for WIPP are best addressed by laboratory experiments and hydrologic tests of the Rustler aquifer. The additional benefits of local backfills and the limited release rate and solubility of radionuclides will be addressed by experiments with radioactivity described in SAND81-2628. These will address primarily defense high level waste as the effects on TRU wastes are clearly bounded by the assumptions in the SAR.

c) EEG comment suggests studies of the durability of TRU waste "boxes" such as the DOT-7A FRP coated plywood box and the RH canisters be included in the SWE TRU drum durability tests. The planned SWE TRU drum durability tests are essentially demonstrations of adequacy as established from previous studies (SAND79-1305, Chapter 7). This reference also provides adequate justification for the durability of the RH waste canisters. The TRU waste box containers will be overpacked in all cases, hence tests of bare boxes are unwarranted.

TRIP/CONFERENCE REPORT

TECHNICAL SUPPORT CONTRACTOR
WIPP PROJECT
ALBUQUERQUE, NEW MEXICO

REPORT NO. EA:82:0390

DATE OF REPORT August 30, 1982

TRIP DESTINATION or LOCATION of CONFERENCE and DATES

- **Organization visited** EEG visited TSC offices
- **Dates of Trip/Conference** August 26, 1982

SUBJECT of TRIP/CONFERENCE

Discuss Final EEG Comments On Two Stipulated Agreement Reports

ATTENDEES (Name and Organization)

R. H. Neill, NM EEG
M. Little, NM EEG
C. C. Little, TSC-W
J. S. Treadwell, DOE*
T. Hunter, SNLA*
R. Matalucci, SNLA*
K. R. Porter, TSC-D'Appolonia*

* = Part Time

DISTRIBUTION (Name and Organization)

R. K. Brown, TSC
G. L. Hohmann, TSC
W. Baer, TSC
V. F. Likar, TSC
D. K. Shukla, TSC
W. D. Weart, SNLA
T. Hunter, SNLA
J. M. McGough, DOE
J. S. Treadwell, DOE
R. L. Dintaman, DOE

- **SUMMARY of TRIP/CONFERENCE**

DOE, EEG, TSC and SNLA met to discuss the reports entitled Simulated Waste Experiments Planned for the WIPP (SAND82-0547) and Natural Resources Study, WIPP (TME-3156). Both reports have gone to the printer with only minor change to the resource study resulting from the State's final review.

- **DISCUSSION OF TRIP/CONFERENCE TRANSACTIONS** (Use Trip/Conference Continuation Sheet)

Prior to the morning meeting, the State was presented with copies of both reports-Simulated Waste Experiments (SWE) and Natural Resources. Resolution of the State's comments and incorporation of changes felt to be necessary by the reports authors had occurred earlier and the State was provided with the documents as they were to be printed. Although EEG reviewed the Natural Resources Report, they declined the opportunity to review the SWE report.

The final meeting concerned the SWE report and the DOE response to EEG's comments. After some discussion between all parties, it became obvious that there were three areas of concern by the State which were not addressed by SNLA in the SWE report but which are or will be addressed inother WIPP documents. These three areas include:

1. items relating to the WAC and waste certification,

Report Prepared by C. C. Little C.C. [signature] **Report Approved by** [signature] 9/2/82

Organization Institutional and Environmental Assessments

2. items requiring details of specific tests which will not be available until specific plans are developed for each test, and

3. items not related to simulating waste but which will be covered in the overall R&D In-Situ Test Program presently undergoing final review by DOE.

EEG concerns relating to item 1 will be addressed as part of the State's review of other project documents. As such, no revision to the SWE is required. EEG concerns relating to item 2 will be addressed in the detailed SNLA test plans which will be provided to DOE prior to each test. DOE (Treadwell) observed that these plans may be provided to the State if the DOE Project Manager agrees with the State's need to review each plan -- and further observed that there was no apparent reason not to release the plans to the State. TSC (Little) pointed out that the ERDA-6, WIPP-12 and DOE-1 plans had not been released by DOE although the State had participated in a detailed review (through numerous meetings) and that most of their comments had been incorporated in the final plan for each test. EEG (Neill) reiterated the State's request to review each detailed plan. This request has not been resolved pending further consideration by the Project Office. No change will be made in the final SWE report for the second item. Relative to the third item, EEG was informed that SAND81-2628 should be issued in September or October and that no changes in this area were planned for the SWE report. Except as noted relative to EEG review of the detailed test plans, EEG concurred with the DOE/SNLA plan to not modify the SWE report.

During the detailed SWE discussion, EEG requested two documents which TSC agreed to check for availability for release. These are:

"Potential for Salt Fracturing Due to Gas Generation"
(Draft D'Appolonia - 1979)

and

"Long-Range Master Plan for Defense Transuranic Waste Management"
(DOE-TRU 8201 Draft 1982)

Finally, during the SWE discussion, EEG pointed out a potential error in the SAR Table 3.1-1 in that the overpack described does not fit over the FRP box it appears to be intended to protect from fires. TSC (Little) agreed to review the Table for a possible error. Subsequent review indicates that there is not an error, but that the Table depicts containers and overpacks which currently exist in the waste management industry. Prior to shipment of FRP boxes to WIPP, new overpacks will be constructed and placed over the FRP box.

The afternoon meeting consisted of a very brief discussion between EEG and TSC relative to the Natural Resources Study. Two editorial problems were discussed and resolved. The only remaining EEG comments will be discussed further when the revised "Interim" DOE Policy Statement is ready for issuance. Only minor changes have been made to the final Natural Resources Study, and it has been forwarded to the printer for final printing.

Both reports should be issued in final form during September, 1982.

DESIGN VALIDATION PLAN

WIPP-DOE-160

HEALTH ENVIRONMENT department

"Equal Opportunity Employer"

STATE OF NEW MEXICO

ENVIRONMENTAL EVALUATION GROUP
320 Marcy Street
P.O. Box 968
Santa Fe, NM 87504-0968
(505) 827-5481

August 24, 1982

Mr. Joseph M. McGough
Project Manager of WIPP
WIPP Project Office
U.S. Department of Energy
Albuquerque Operations Office
P. O. Box 5400
Albuquerque, New Mexico 87115

Subject: Letter from J. M. McGough to R. H. Neill, Draft Design Validation Plan, JMM 82-0496, 8/3/82

Dear Mr. McGough:

Enclosed are EEG's comments on the report "Preliminary Design Validation Plan Waste Isolation Pilot Plant (WIPP)", which was submitted by you as item 8 of Appendix B of the Stipulated Agreement between the State of New Mexico and the Department of Energy. Should you have any questions regarding these comments, please contact Dr. P. Spiegler.

Sincerely,

Robert H. Neill

Robert H. Neill
Director
AG-046AG2-14-3

RHN:eg

cc: TSC, IEA

Comments on draft

"Preliminary Design Validation Plan Waste Isolation Pilot Plant (WIPP)

by

Environmental Evaluation Group
Environmental Improvement Division
N. M. Health and Environment Department
P. O. Box 968
Santa Fe, NM 87503

August 24, 1982

General Comments

1. The report is considered to be much too general to comply with requirements of the Stipulated Agreement Appendix B, between the State and DOE, and the statement of work described by DOE in the letter from D.T. Schueler to G. S. Goldstein, "Costs and Merits Evaluation for Stipulatd Agreement Activities," August 31, 1981. It is recommended that the report be extensively revised to include a detailed plan with appropriate technical rationale, to construct the test panel and to carry out the tests, and to indicate how the results will validate the key assumptions for design regarding rock mass behavior. It should include (a) establishment of priorities and schedule for various components of the test; (b) preparation of the steps in design, analysis, monitoring and review of the test program; (c) and review of the consistency of the proposed plan with the objectives an rationale for the test.

2. The report relies on references that are partially out of date. See further discussion of references below under "Specific Comments."

Specific Comments

Section 1.1

This section should include a reference to the Stipulated Agreement and a restatement of the work proposed by DOE in the letter from D. T. Schueler to G. S. Goldstein, "Cost and Merit Evaluation for Stipulated Agreement Activities," 8/31/1981.

Section 1.4

The SPDV underground facilities also include an exploratory drift into the area of the repository. This drift was recommended by DOE in lieu of the horizontal coring activities.

Section 1.6

The layout and configuration of the underground openings are not based on experience gained in existing potash mines in the Carlsbad area. In fact, the first underground design was by Serata Geomechanics. The design was rejected when the room closure rates could not be verified. The present design has a slow room closure rate that permits retrieval of waste 5 to 10 years following emplacement. Retrieval of waste and modular design of the repository are important criteria in the present underground design. The criteria require that substantial barriers of undisturbed salt be left between modules to ensure isolation of each module from other modules.

Section 2.1

The objectives of the preliminary design validation plan are to enhance the level of confidence and credibility of the current design of haulageways and storage rooms. To do this, it is necessary to have some preliminary data that will show that the haulageways and storage rooms will remain stable during the waste emplacement and retrieval period. The objectives of the preliminary design validation plan have been succinctly stated in report SAND 81-25/628* as follows:

- To validate the design for the WIPP access shafts and TRU waste disposal demonstration rooms.
- To evaluate the amount and rate of shaft convergence and room creep deformation and to correlate these data with model predictions.
- To perform a preliminary evaluation of creep in salt and of the steady-state creep model.
- To evaluate instrumentation systems for accuracy and the reliability of measurements made with them in rock salt and to document the suitability of the system for future measurements.
- To evaluate the response of in situ formations such as clay seams and other material layers in addition to the salt.
- To collect large numbers of samples of rock salt and other materials and to conduct laboratory and bench-scale tests to determine the mechanical properties of these samples.

----

*R. V. Matalucci, C. L. Christensen, T. O. Hunter, M. A. Molecke, D. E. Munson, Waste Isolation Pilot Plant (WIPP) Research and Development Program: In Situ Testing Plan. Draft report SAND 81-2628.

3.0 plan

This section does not describe a plan, rather, it describes a preliminary proposal for a plan. An excellent description of the plan can be found in report SAND 81-2628,* section 4.1.2.1, preliminary Design Validation, contains much more specific information than is included in this draft and should be incorporated, at least in summary, if it is still current.

4.0 Documentation

Section 4.2
The section should mention that as part of the Stipulated Agreement, a document entitled "Results of SPDV Site Validation Experiment" will be issued in March 1983 (latest revised date from DOE).

5.0 WIPP Design Development

Section 5.1
A statement should be made that indicates that Ref. 5 is outdated due to numerous recent design changes. This reference contains the early underground design by Serata Geomechanics. It was never revised because it was decided to include all the updating in the Title II design. The layout and configuration is not based largely on the experience gained from potash mining in the Carlsbad area (see comments for Section 1.6).

The last sentence which states that the extraction ratio for WIPP is more conservative than the extraction ratio in commercial salt and potash mines is misleading since the two cases have opposite goals. In commercial potash mining the aim is to extract as much ore as possible and to have the rooms close as fast as possible thereafter. (No need for empty drifts in which workers could get lost.) In WIPP the aim is to have haulageways and storage rooms that will remain stable during the waste emplacement and retrieval periods.

----

*R. V. Matalucci, C. L. Christensen, T. O. Hunter, M. A. Molecke, D. E. Munson, Waste Isolation Pilot Plant (WIPP) Research and Development Program: In Situ Testing Plan. Draft report SAND 81-2628.

Section 5.5

The last sentence, "the modeling techniques mentioned in para. 5.3 will attempt to define long term creep behavior" is incorrect. The techniques have only been used to evaluate creep for the first ten years following room construction. Long term creep requires terms that describe the inertia of the backfilling and the waste cans to the creep. The Thermal/Structural Interactions experiments of the WIPP Research and Development Program will provide data on long term creep.

Department of Energy
Albuquerque Operations Office
P.O. Box 5400
Albuquerque, New Mexico 87115

RECEIVED

NOV 29 1982

ENVIRONMENTAL
EVALUATION GROUP

NOV 24 1982

Mr. R. H. Neill
Director
State of New Mexico
Environmental Evaluation Group
P. O. Box 968
Santa Fe, NM 87504

Dear Mr. Neill:

Design Validation Plan

Enclosed for your review is the final draft of the Design Validation Plan required by Item 8 of Appendix B of the Stipulated Agreement. This revision incorporates the results of our discussions of September 21, 1982 on this document. Because of the extensive changes to the document resulting from your comments, we are reissuing the report for your final review. We would appreciate your comments before December 10 since we would like to go to the printer on December 17. This schedule will allow us to issue the final printed report before the end of 1982.

Sincerely,

J M McGough

J. M. McGough
Project Manager
WIPP Project Office

WIPP:JMM 82-0823

Enclosure

cc w/o enclosure:
J. Treadwell, WIPP/PO, AL
J. Smrha, B, AL
C. C. Little, TSC, AL

cc w/enclosure:
C&C File, IEA, TSC

HEALTH
ENVIRONMENT department

"Equal Opportunity Employer"

STATE OF NEW MEXICO

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December 15, 1982

Mr. Joseph M. McGough
Project Manager on WIPP
WIPP Project Office
U. S. Department of Energy
P. O. Box 5400
Albuquerque, NM 87115

Dear Mr. McGough:

Subject: Design Validation Plan, Rev. 4, 11/15/82. Letter WIPP: JMM 82-0823, dated 11/24/82.

We have reviewed the above cited document and are pleased to note that it has been revised to address EEG's comments in my letter of August 24, 1982 as well as those items discussed at our meeting on September 21, 1982. The document provides a good description of the tests to be performed to demonstrate the soundness of the design of WIPP. Aside from a few detailed comments, we have no further comments concerning this document. However, we have one suggestion for the preliminary design validation report.

The design validation plan provides a description of the geological observations that will be made and of the data that will be accumulated during the SPDV phase of the WIPP. It does not convey the impression that data analysis will play an important role in the final design of the WIPP repository. For example, unlike the description of the tests in the document SAND 81-2628, there is no discussion of expected results. Also, there is no mention of possible design alterations. The section, "Drawings, Figures, Logs" of Appendix B, does not indicate any preliminary calculations. We believe that data analysis is important to the validity of the design validation report and the preliminary design validation report should reflect this by having two subsections on expected results; one in section 8 "Geological Behaviour--Shafts", and the other in section 9 "Geological Behaviour--Horizontal Openings."

Detailed comments on the design validation plan are enclosed.

Sincerely,

Robert H. Neill

Robert H. Neill
Director

RHN:PS:eg

2-93-AG2-14-3-1

Enclosure

Detailed Comments on Design Validation Plan

1. Page 6, last line.

   The paragraph should point out that the exploratory drift is to be excavated to the south.

2. Page 7, Figure 1-2.

   The figure should have an arrow indicating the north-south direction.

3. Page 15, 2nd paragraph or line 12.

   The term "through SPDV" appears to be meaningless. SPDV will go on until September '83, which is past March '83.

4. Page 16, item 2a.

   Is there a typing error in the date 4/31?

5. Page 17, 1st paragraph.

   There is no commitment in the paragraph to furnish the data as available after the final design validation report to the EEG.

6. Page 20, item f).

   A statement indicating the locations of drillholes or other plans to monitor Marker Bed 139 and height of clay seams above roof should be added.

7. Page 20, bottom of Page.

   The following item should be added:
   k) Geophysical measurements to determine possible anomalies above or below the drift.

8. Page 22, section 4.1.

   Most of the documents to be furnished under the Stipulated Agreement can be viewed as part of the site characterization program. They should be referenced in this section.

9. Page 23, Figure 4.1.

   This figure should be replaced by the latest design which shows the repository in the southern region of the site.

10. Page 25, Figure 4.2.

    The direction of this cross-section should be indicated.

11. Page 31, 3rd paragraph or line 25.

The sentence "After a retrieval period of five years, the rooms with waste emplaced will have been completely backfilled with salt." is unclear.

12. Page 36, last line.

The sentence is incomplete. It should be as follow: A schedule of activities included in the performance of the Design Validation Plan is presented on p. 16.

13. Page 47, item 13.

The item should point out that the correlation goes from WIPP-12 to DOE-1.

Department of Energy
Albuquerque Operations Office
P.O. Box 5400
Albuquerque, New Mexico 87115

FEB 11 1983

FEB 15 1983
EVALUATION GROUP

Mr. Robert H. Neill
Director
State of New Mexico
Environmental Evaluation Group
P.O. Box 968
Santa Fe, NM 87503

Dear Mr. Neill:

Conference Report for DOE/EEG Meeting, January 26, 1983

Enclosed for your information, is a conference report for the subject meeting on the validation process and forthcoming reports. I believe, based on my observations at the meeting, that we are in agreement on the planned activities and reports leading to DOE's pending decision relative to our proceeding will full facility construction.

At the present time, we are making arrangements for an EEG tour of the underground facility including the complete south drift on February 16, 1983. This will be the earliest opportunity under our present schedule during which all mine conditions will be suitable for such a tour. We also plan to conduct the seventh quarterly review in our Albuquerque office on February 15, 1983. We have arranged for a Ross flight to Carlsbad the morning of February 16. The flight will return late that afternoon.

If there are any questions on our plans, please contact me at your convenience.

Sincerely,

J. M. McGough
J. M. McGough
Project Manager
WIPP Project Office

WIPP:JMM 83-0111

Enclosure

cc:
See Page 2

TECHNICAL SUPPORT CONTRACTOR
WIPP PROJECT
ALBUQUERQUE, NEW MEXICO

REPORT NO. EA:83:0037

DATE OF REPORT January 28, 1983

TRIP DESTINATION or LOCATION of CONFERENCE and DATES

- Organization visited EEG/DOE/TSC/B/SNLA Meeting

- Dates of Trip/Conference January 26, 1983

SUBJECT of TRIP/CONFERENCE

See Attachment I

ATTENDEES (Name and Organization)

DISTRIBUTION (Name and Organization)

R. K. Brown, TSC
G. L. Hohmann, TSC
D. K. Shukla, TSC
H. Taylor, B
J. Smrha, B
W. Weart, SNLA
T. Hunter, SNLA
J. M. McGough, DOE
J. S. Treadwell, DOE
T. Shea, DOE
R. Dintaman, DOE

- SUMMARY of TRIP/CONFERENCE

The parties met to discuss DOE's response to EEG comments on the Design Validation Plan and to discuss documentation being prepared to support the April 1, 1983 DOE decision relative to the initiation of full facility construction. EEG agreed with the resolution of comments and DOE's planned documentation to support their decisions.

- DISCUSSION OF TRIP/CONFERENCE TRANSACTIONS (Use Trip/Conference Continuation Sheet)

The subject meeting was conducted in accordance with the attached agenda (Attachment II). After a brief opening by DOE during which it was explained that the Project Office had reassigned certain functional responsibilities, TSC presented (Attachment III) a summary of the hierarchy of reports which have been and are being prepared to support the suitability of the site and preliminary design. The relationship of documentation to the Stipulated Agreement was discussed to ensure a mutual understanding of required submittals. The greater significance of documentation supporting site suitability was also discussed, and all parties agreed that preliminary design validation will be less important in the decision to proceed because the design is continually evolving over the life of the facility. It was pointed out that all supporting documents will be provided to EEG and the State reading rooms, but that DOE will print a larger number of the documents responding to item 2 of the Stipulated Agreement to respond to public requests for information. At the conclusion of these discussions, all parties were in agreement concerning the planned documentation and schedules for review. EEG did feel that,

Report Prepared by C. C. Little *C.C. Little*

Organization TSC - WIPP Project

Report Approved by *G. L. Hohmann*

although there was no requirement to do so at this time, DOE should address the draft EPA standard on site suitability -- no commitment was made to do this until the regulations are promulgated and DOE has reviewed the final rules.

The next agenda item, discussion of EEG comments on the Design Validation Program Plan, was led by Bechtel. EEG's comment had been provided earlier in a letter to the Project Office and the discussion concerned revisions to the document by Bechtel. EEG was pleased with the revisions to the Design Validation Program Plan. Bechtel indicated they would finalize the report for issuance as a DOE document. Bechtel also pointed out that the Preliminary Design Validation Report presently scheduled for March 31, 1983 would not include a comparison of predictive calculations versus actual results in the mine since earlier predictions were not directly applicable to the final horizon. During the discussions, EEG questioned whether or not the project was continuing to use radar to preview conditions ahead of the continuous minor. DOE indicated that, as had been observed in potash mines, the use of radar was less efficient than periodic probe hole drilling and that the use of radar had been discontinued. EEG also requested further details relative to the northernmost location of TRU waste relative to WIPP-12 under the present design. It was pointed out that this was related to the final decommissioning plan which had not been developed, but that if no TRU waste was permanently disposed of in the experimental area that the northernmost waste would be more than 6000 feet horizontally from WIPP-12. If during decommissioning TRU waste was placed in the experimental area, this waste would be slightly less than one mile horizontally from WIPP-12.

The next agenda item, site suitability report contents, was directed by SNLA. During this discussion, it was pointed out that the summary would include a discussion of the 21 site qualification criteria delineated in WIPP-DOE-116. The summary report would not address the issues in detail, but would instead rely on the other documentation developed in support of this final decision on site suitability.

The final agenda item, contents of the report on results of SPDV site validation experiments, was directed by TSC. A draft table of contents (Attachment IV) was provided and discussed. EEG felt that the report should contain information on the petrography of the inner bedded materials and characterization of the individual aquifers in the shaft mapping report. TSC pointed out that neither of these items were required by WIPP-DOE-116 and that the last item would be very difficult (maybe impossible) to obtain at this time. TSC/DOE agreed to evaluate the possibility of providing the requested petrographic information.

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STATE OF NEW MEXICO

ENVIRONMENTAL EVALUATION GROUP
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HEALTH & ENVIRONMENT department

April 5, 1983

Mr. Joseph McGough
Project Manager on WIPP
WIPP Project Office
U.S. Department of Energy
P.O. Box 5400
Albuquerque, New Mexico 87115

Subject: Design Validation Plan

Dear Mr. McGough:

The document Design Validation Plan (WIPP-DOE-160) Reference 9 attached to your letter of 3/24/83 is sufficiently responsive to issues on questions raised in both correspondence and meetings on the attached reference list. The document fulfills item 8 of the Stipulated Agreement.

Sincerely,

Robert H. Neill

Robert H. Neill
Director

RHN:ps

Attachment

2-123AG-2-14-3-1

cc: TSC, IEA

Providing an independent analysis for the New Mexico Health and Environment Department of the proposed Waste Isolation Pilot Plant (WIPP), a federal nuclear waste repository.

References

1. WIPP:JMM 82-0496, Transmittal of Draft Design Validation Plan, J.M. McGough to R.H. Neill, 8/9/82.

2. AG-046 AG2-14-2, Comments on Draft Preliminary Design Validation Plan Waste Isolation Pilot Plant (WIPP), R.H. Neill to J.M. McGough, 8/24/82.

3. EEG Conference Report 49, 9/28/82.

4. WIPP:JMM 82-0823, Design Validation Plan, J. McGough to R.H. Neill, 11/29/82.

5. 2-93-AG2-14-3-1, Comments on Design Validation Plan, Rev. 4, R.H. Neill to J. McGough, 12/15/82.

6. EEG Conference Report 58, 1/27/83.

7. WIPP:JMM 83-0111, Conference Report for DOE/EEG Meeting, January 28, 1983, J.M. McGough to R.H. Neill, 2/15/83.

8. WIPP:JMM 83-0118, Advance Copy of Design Validation Plan, J.M. McGough to R.H. Neill, 2/9/83.

9. WIPP:JMM 83-0211/6552A, Transmittal of WIPP-DOE-160, Design Validation Plan, January 1983, J.M. McGough to R.H. Neill, 3/24/83.