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APR 13 1995

Lindsay A. Lovejoy, Jr.
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P.O. Drawer 1508
Santa Fe, N.M. 87504-1508

1/20/95

Dear Mr. Lovejoy:

Thank you for your comments on the Non-Salado Position Paper. Your interest and participation in the Systems Prioritization Method (SPM) is greatly appreciated. Enclosed is the Carlsbad Area Office's response to the questions you have expressed regarding this paper.

If you have any questions regarding these responses, please contact George T. Basabilvazo of my staff at (505) 234-7488.

Sincerely,

for Michael H. McFadden
Assistant Manager
Office of Regulatory Compliance

Enclosure

cc w/o enclosure:
J. Mewhinney, CAO
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NON-SALADO FLOW AND TRANSPORT POSITION PAPER
QUESTIONS AND RESPONSES

Question/Comment 1. The paper lists its objectives (draft at 1-2) and states that "[objective 4 will be addressed after the positions regarding conceptual and mechanistic models are finalized." The paper is therefore incomplete, making comment very difficult. Please advise when objective 4 -- identification of conceptual and computational models and appropriate parameter values for use in the second iteration of the SPM -- will be addressed. Please provide a copy of the position paper which addresses those issues, and hold a stakeholder meeting to receive comments.

Response. DOE/CAO believes that we can best answer the stakeholders' concerns by conducting an open and scientifically sound process, incorporating stakeholder comments and questions on work that is in process. We encourage you to review work that is in progress by requesting appropriate data and by utilizing the excellent expertise available to the State of New Mexico. The DOE/CAO will continue to use a combination of meetings and Position Papers to communicate with the stakeholders.

Question/Comment 2. The comment that the features, events and processes to be considered are those described in the scenario development position paper does not illuminate much (draft at 2-1). That paper, which is itself under revision, mentions various FEPs, screens some out, says that some may be screened out later based on arguments not yet developed, and leaves others in. The draft needs to state clearly, naming them, exactly which scenarios are considered to be included in PA. The discussion at pages 2-1 and 2-2 does not do this.

Response. Issues related to selection of FEPs and screening of scenarios are discussed in the Scenario Position Paper; these issues are outside the scope of the Non-Salado Flow and Transport Position Paper. With regard to the PA, it is premature to select final scenarios because PA is an iterative process. New experimental data and computational studies may refine the selection of FEPs, scenarios, and computational models. These changes will be reflected in future versions of the Position Papers and communicated with the stakeholders.

Question/Comment 3. It is said that vugs are an important part of Culebra porosity (at 3-10). How, if at all, are vugs modeled as an element of the flow model? If they are not explicitly modeled, how are they accounted for?

Response. Vugs may be important in Culebra porosity; however, fractures are the most important aspect of Culebra flow and transport for compliance. It is unlikely that vugs are interconnected, so they represent a part of the matrix porosity that will retard flow relative to the more rapid transport in fractures. The presence of vugs is included as a part of the matrix

porosity.

Question/Comment 4. The clay fraction in the Culebra is disputed and affects retardation factors applicable to radionuclide transport. EEG has shown that the data supporting any particular clay percentage are inadequate. (See EEG comments on Compliance Status Report, Nov. 1994, at 6-9). The claims as to clay content must be withdrawn (draft at 3-10, lines 20-28).

Response. The Position Paper does not claim that conceptual models which take credit for clay linings can be supported. The latest version of the Position Paper, dated December 15, 1994, has been modified to clarify this point (see page 3-48, lines 29-35).

Question/Comment 5. The draft refers to a mean thickness of the Culebra of 7.7 meters (at 3-11). Should not a thickness value be derived from data specifically concerning the travel path of released radionuclides? On the thickness issue, Table 2-2 cannot be located.

Response. Table 2-2 has been provided. The estimated travel path is a consequence of modeling studies; the thickness of the Culebra is not.

Question/Comment 6. The draft says that two "conceptual models" of non-Salado groundwater flow exist (at 3-17). In fact, as "conceptual model" is defined in the SPM glossary and in general, the confined aquifer and groundwater basin models are not different conceptual models but are mathematical models designed to represent flow and transport in different areas, but adhering to the same conceptual model, is this not so?

Response. The two models are in fact different conceptual models that may be compatible with each other for certain interpretations and uses.

Question/Comment 7. What is the document cited as "Swift et al., 1994" on page 3-12?

Response. The citation is apparently the one on page 3-17, line 30, of the December 15, 1994 draft. The appropriate reference is: "Incorporating Long-Term Climate Change in Performance Assessment for the Waste Isolation Pilot Plant," Peter N. Swift et al., SAND93-2266, February, 1994.

Question/Comment 8. It is said that in the groundwater basin model differences in elevation of the water table generate the gradients to drive groundwater flow (at 3-18). Are other driving gradients, such as density differences, represented in the model? Which gradients are

represented?

Response. The regional groundwater basin model assumes a single-density flow. Only gradients associated with hydraulic head variations are represented in the model.

Question/Comment 9. There is reference (at 3-18) to pumping tests, geochemical data, and hydrologic modeling studies related to vertical flow through Rustler confining units. Please provide citations to the pertinent materials.

Response. The appropriate references for geochemical studies and pumping tests are, respectively:

Siegel et al., *"Hydrogeochemical Studies of the Rustler Formation and Related Rocks in the Waste Isolation Pilot Plant Area, Southeastern New Mexico,"* SAND88-0196.

Beauheim, R., *"Analysis of Pumping Tests of the Culebra Dolomite Conducted at the H-3 Hydropad at the Waste Isolation Pilot Plant (WIPP) Site,"* SAND86-2311.

Question/Comment 10. It is said that flow models have not considered vertical flow through areas of nonuniform infiltration, such as sinkholes (at 3-19, lines 33-39). Given the presence of sinkholes in the vicinity as reported, what will be done to model such flow and to determine its importance?

Response. Regional-scale effects that might be associated with non-uniform infiltration have not been observed in field monitoring to date. Evidence for point infiltration would be "mounds" of hydraulic head, which have not been observed. Two-dimensional models have been calibrated to the observed data for the Culebra, which include any effect from potential point infiltration on the regional hydrology; thus, point infiltration is accounted for in an indirect manner in the two-dimensional models. Similarly, point infiltration is accounted for at a very large scale by parameter variation in three-dimensional regional models.

Question/Comment 11. The draft states that three-dimensional simulations show that the current different flow directions in the Magenta and the Culebra can be explained as a product of regional heterogeneity and vertical flow through the Tamarisk (at 3-21, -22). Please demonstrate how this is so or cite the source so demonstrating. An understanding of the site hydrology, including the different flow directions, is important to a compliance demonstration. This theoretical explanation should be set forth so that it can be evaluated.

Response. The three-dimensional simulations are being performed as part of the regional modeling studies. The results from these studies, including the detailed models and

simulations, will be published at the conclusion of the studies.

Question/Comment 12. The draft says that the three-dimensional numerical representation of the groundwater basin model (SECO_3D) is currently being developed (at 3-22). Is the model available for computer exercises by outside groups, such as our office, or EEG?

Response. The SECO-3D is not currently available for release because it is still under development. It will be released when development is complete and quality assurance standards have been met.

Question/Comment 13. Please explain what hydraulic conductivity data are employed in the groundwater basin model, as described in text on page 3-24 (lines 3-17). What plans exist to develop additional field data to support such model? What plans exist to represent localized features disrupting confining layers?

Response. Conductivity values used in regional modeling are estimates based on available field information and literature values of conductivity in similar rock types. Sensitivity analyses are conducted to examine the effect of uncertainty associated with these values.

The regional model is designed to enhance conceptual understanding of the site and the basin, rather than estimate release rates for performance assessment. The use of sparse and/or analogue data is accepted practice for the regional modeling studies.

Question/Comment 14. There is reference to evaluation of changes in groundwater flow due to climate changes, subsidence over potash mines, and shallow boreholes (draft at 3-24) and to a specific simulation of flow fields at two different recharge rates (at 3-25). What are the published reports concerning such studies? If no reports are published, please provide the materials supporting the claims in the draft.

Response. The issues on page 3-24 are discussed in Sections 3.4.1, 3.4.2 and 3.4.4 of the Position Paper. The issue on page 3-25 is discussed in Appendix A of the Position Paper.

Question/Comment 15. What plans exist to study whether a shift in flows to the west may result in shorter travel times (as discussed at 3-25)?

Response. At this time, there are no plans to study the hypothesized shift.

Question/Comment 16. References supporting data should be provided for the statements concerning a transient simulation of the effect of climate change on flow patterns (at 3-25,

lines 17-22) and of the effect of climate change on the Dewey Lake saturation zone (at 3-25, lines 30-39).

Response. The effects of climate change are being evaluated through three-dimensional modeling of the groundwater basin. The results are not available at this time, but will be published at the conclusion of the study.

Question/Comment 17. What is the impact on flow through the Dewey Lake of the increase in saturated thickness described at 3-25, lines 33-39?

Response. The possible impact is that gradients could be steeper and saturated zones could be more extensive laterally. However, due to the sorptive capacity of the Redbeds and the likely instability of colloids in these beds, transport of radionuclides in the Dewey Lake is thought not to occur and the introduction of radionuclides to these strata is assumed to be inconsequential for compliance.

Question/Comment 18. The draft states that results from the three-dimensional model suggests that the apparent inconsistency between hydrogeological and geochemical data as to flow patterns can be reconciled (at 3-29, lines 17-22; 3-30, lines 27-35). Please provide information supporting such an assertion.

Response. The three-dimensional, regional modeling that is currently in progress provides a better basis than previous geochemical studies for interpretation of flow patterns. The results of this modeling are not available for release at this time, but will be published at the conclusion of the study.

Question/Comment 19. Claims are made about the frequency of encounter of Castile brine reservoirs (at 3-31). What support is there for the implication that in oil and gas drilling operations a brine reservoir will be detected if encountered and will be recorded if detected?

Response. A brine reservoir, if encountered, will be detected in oil and gas drilling operations because drillers must maintain appropriate pressures and circulation rates in boreholes. It is normal practice that such an encounter will be recorded.

Question/Comment 20. The draft states that Castile brine volume shall in future calculations be limited to 1×10^7 m^3 maximum (at 3-33). The 1992 PA is said to have used a volume of 2×10^8 m^3. I have looked at the 1992 PA, volume 3, and do not find a fixed volume. Rather, in Table 4.3-1 these reservoir parameters appear (units in meters):

	median	range		distribution type
radius, equiv.	2.32×10^2	3×10^1	8.6×10^3	constructed
thickness	1.2×10^1	7	6.1×10^1	uniform

The reference is given as Reeves et al., 1991. These values would appear to generate a maximum in excess of 1×10^7 m^3. (However, the ranges were not sampled values.) DOE must better justify use of a maximum value (with unstated values for minimum and mean) of 1×10^7 m^3 based on figures in a 1983 report, in preference to figures based on a study in 1991. DOE must discuss in this connection all the available data on Castile brine occurrences, such as the data in EEG-17.

Response. The DOE has decided to base SPM-2 estimates for the volume of possible brine reservoirs on the WIPP-12 encounter, whose volume was approximately 1×10^7 m^3. The 1992 PA did use a fixed volume, rather than the published ranges. Note that Table 4.3-1, Volume 3, 1992 PA indicates that only those parameters shown in bold were sampled.

Question/Comment 21. Concerning the asserted error in the storativity range used in the 1992 PA (storativity being a sampled value) I have looked at the publications reflecting the asserted error and do not see the support for the asserted value of 13,469 m^3. Please explain specifically how an error was made and corrected.

Response. The D'Appolonia publications contain the correct value, as stated on page 3-33 of the Position Paper.

Question/Comment 22. The limitation of the number of reservoirs to four is unacceptable (draft at 3-33). The May 5, 1994 presentation on brine reservoirs at the DOE-EPA technical exchange pointedly omitted to state that the TDEM data showed four reservoirs. Al Lappin then stated that "[a] surface-based geophysical study, correlated with known stratigraphies and presence/absence of brine at holes WIPP-12, DOE-1 and ERDA-9, indicates that a high-conductivity zone may be present above the top of the Bell Canyon Formation under a portion of the WIPP waste-emplacement panels." This is the limit of the data. The most sensible course of action, and the only one defensible in light of the data, is to assume that each hole penetrating the Castile encounters a brine reservoir, and that each such reservoir is separately pressurized, given the evidence of the isolation of such phenomena.

Response. The assumption that every penetration of the Castile intersects a brine reservoir is directly in conflict with the evidence for the isolation of brine reservoirs. If this assumption were true, brine reservoirs must be interconnected and their great extent would allow pressure equalization with more normally pressured zones. In fact, the occurrences do behave as high

pressure reservoirs of limited extent and any assumption in conflict with this fact is physically unrealistic.

Question/Comment 23. The discussion of multiphase flow raises the question of sources of gas other than the repository and the Castile. Is it possible that gas would be introduced from formations below the Castile, and, if so, what probability should be assigned? What pressures may exist?

Response. Gas encountered at depth that enters the Culebra will have the tendency, based on general principles of two-phase flow, to force brine into the matrix porosity. Additional flow into the matrix will retard radionuclide transport and the project's current position of ignoring two-phase flow in the Culebra is then conservative. In this situation gas-related issues, such as probability of an event and the likely gas pressure, are not being pursued by DOE/CAO.

Question/Comment 24. What transient effects upon a borehole seal might be caused by the escape of pressurized gas? More broadly, even if the presence of gas in the brine flowing through the Culebra might in itself inhibit radionuclide transport, as was said on January 9, is there any respect in which it is not conservative to ignore a possible encounter with gas, other than gas in the repository?

Response. There are no anticipated deleterious effects from gas at depth on borehole seals because gas, like brine, is a fluid and its behavior is not fundamentally different than brine and because gas at depth will be encountered during drilling of the borehole, not while it is sealed and inactive. Ignoring the possibility of a borehole penetrating gas below the Castile is thought to be conservative because gas is not a medium for radionuclide transport and because gas generally tends to drive brine into the rock matrix, retarding radionuclide transport. Please note that the applicable regulation requires the DOE to use reasonably expected values not conservative values.

Question/Comment 25. Transmissivity values have been conditioned only with the use of data up to the year 1989. There are considerable post-1989 data on transient events, which should also be used to calibrate the fields. See the paper by David Snow, Nov. 9, 1994, attached hereto.

Response. The transmissivity fields are time-invariant for the time scales of interest. Conditioning to a set of data of greater duration will diminish uncertainty, but is unlikely to change average values.

Question/Comment 26. "Transmissivity is assumed to be constant over time." (draft at 3-35). This is unacceptable. Climate change or ongoing processes may change the hydrologic

characteristics of the Culebra. See the paper by Roger Y. Anderson, attached hereto. It is neither scientifically realistic nor conservative, and thus not defensible, to insist that the hydrologic characteristics of a rock body will not change during the time span of interest.

Response. There are no known processes of large enough scale or magnitude to significantly affect transmissivity in supra-Salado units over the region of WIPP in the time span of 10,000 years. Processes affecting geological characteristics generally take much longer, thus this assumption is scientifically realistic. Additionally, the use of multiple transmissivity fields introduces significant stochastic variability that spans the likely range of transmissivity change that might occur due to the effects of long-term processes over 10,000 years.

Question/Comment 27. Storativity data are admittedly sparse (draft at 3-35), and storativity is inaccurately represented as spatially invariant (draft at 3-35). Transient situations are inaccurately represented, and it is not known whether such inaccuracy is conservative. Further, DOE has stated that porosity is insufficiently known to characterize its spatial variability (Compliance Status Report, subsection 2.1.2.6.2).

Response. These issues have been considered by the project and the resulting uncertainties are included through the stochastic variation of appropriate parameters for the performance assessment.

Question/Comment 28. Further, the use of a constant storativity value in calibrating transmissivity fields affects the definition of transmissivity and thus the calculation of velocities in constant-flow conditions. There is reference to possible further work to "solve for both the transmissivity and storativity fields" (draft at 3-36, lines 8-9). What does this refer to, and how will it work? Until it is done, why should the transmissivity fields be considered defensible?

Response. Calibration models could be used to determine multiple parameters such as storativity and transmissivity. At present, there are no plans to calibrate to both storativity and transmissivity; however, the use of calibrated transmissivity fields in PA is reasonable.

Question/Comment 29. It is said that variable density flow considerations will not affect groundwater flow directions (Beauheim presentation, Jan. 9, 1995). We do not have the references cited and will study them, but it seems unlikely that they examine the importance of density gradients using a model that incorporates the real-world factors of fluctuating recharge, return to primitive (pre-intrusion) conditions, regional dip, varying bed thickness, and possible climate change. Has the impact of density flow been effectively isolated for study?.

Response. The effect of density variations on flow can be ignored in the vicinity of the WIPP

site, as explained in the response to Question/Comment 30. DOE considers that this issue has been adequately addressed.

Question/Comment 30. The text says that Davies (1989) concludes that ignoring density-driven flow is not acceptable in the southern portion of the WIPP domain (draft at 3-37). How, then, can it be defensible to do so?

Response. The modeling domain for Davies' 1989 work is a region much larger than the WIPP site. Davies' conclusion that ignoring density variation causes poor estimates of calculated flow directions or fluxes is applicable only to a region of the domain that is far removed and to the south of the WIPP site. The corollary to Davies' statement is that density effects on flow can be neglected at the WIPP site and immediately surrounding area. This corollary is consistent with current models for performance assessment.

Question/Comment 31. Previously, PA's have ignored the effect of introducing fluid from an intrusion borehole into the Culebra. The draft says that an increase in the head in the borehole area can be implemented (at 3-37). Will this be done, and, if not, why not?

Response. The study by Reeves et al. (1991) referenced on page 3-37 investigated this scenario. Please note that processes that will be included in future PAs will be determined by analysis of previous PAs, screening and modeling studies, experimental data and expert judgment. A side-bar calculation will be conducted to determine if this scenario will be incorporated in future performance assessments.

Question/Comment 32. The discussion of double-porosity flow omits to mention the existence of a large area in the southern and southwestern part of the site where there are inadequate data to characterize flow and transport. Further, it appears that a seven-well tracer test will be conducted at some location on the site, although we do not have the current plan. Such testing may provide the needed data. Since the tests will be done, apparently, regardless of the outcome of the SPM-2 study, because they are deemed needed, the draft should discuss them. At the May 3-5, 1994 technical exchange, Rick Beauheim proposed that field tracer tests be conducted in accordance with the following plan:

1. Design and execute tracer tests that focus on specific key assumptions in existing and alternative transport models:

    - Matrix Diffusion (double-porosity and alternative models)
    - Anisotropy (double-porosity model)
    - Flow/transport channeling (alternative model)
    - Vertical heterogeneity in Culebra (all models)

- Scaling effects (all models)

2. Examine field-scale chemical retardation (using reactive tracers) in the same sequence of tests to provide the technical basis for extrapolation of lab-scale chemical retardation tests to field scale.

3. Run test sequence in phased approach with pretest predictions for 2nd and 3rd phases based on previous results to maximize information gained from model analysis and for rigorous model testing/validation.

Is it the project's position that such tests are needed to come up with a defensible model of Culebra flow and transport?

Response. The DOE will use the results of SPM-2 and other data to determine what tests are required to demonstrate compliance. As stated in the stakeholder meetings, the DOE will continue to fund the seven hole test until the decisions are made.

Question/Comment 33. At the same meeting Rick Beauheim stated the following objectives of proposed new tracer tests:

1. prove importance of matrix diffusion

2. address vertical heterogeneity (through hydraulic testing)

3. provide sufficient data to verify existence of hydraulic conductivity tensor and anisotropic approach to test interpretation

4. provide data to distinguish among alternative conceptual models--heterogeneous and anisotropic double-porosity models, channeling models, and discrete-fracture models

5. provide defendable ranges of values for important parameters

6. provide information on transport over an order-of-magnitude range in scales

7. address concerns about tracer-injection technique

8. provide data on chemical-retardation processes and properties within the Culebra

9. obtain core samples from field tracer-test site to allow comparison of laboratory and field transport studies

Does the project agree that these objectives all relate to concerns that must be resolved to develop a defensible model of Culebra flow and transport? Please explain any disagreement

and state how these concerns may be resolved by other means.

Response. No, all objectives do not necessarily need to be resolved because specific items may or may not be important to compliance. The data from this testing program will generally allow the development and testing of more detailed hydrological models that may be used in future PA calculations, if more detailed models are in fact needed.

Question/Comment 34. As the draft states, the model parameter of matrix block size is not well characterized (at 3-38). Also, the model is used to develop figures for fracture porosity and other parameters within the SWIFT-II model of continuous, isotropic, radial flow in cubically fractured medium; thus, other parameters in the model are not correctly characterized.

Response. The SWIFT-II model estimates the flow that results from a system described by numerical equations with various parameters; SWIFT-II does not estimate the values of these input parameters. Matrix block size, which is not well-characterized, is only one input parameter in the SWIFT-II model. Other input parameters used in SWIFT-II are determined or estimated from independent lines of evidence and are uncertain to varying degrees; however, the uncertainties in these other parameters are independent of the uncertainty in matrix block size.

Question/Comment 35. Is it accurate to model the Culebra as an orthagonally fractured medium, when fractures do not appear to take that form, in fact, and in reality fractures of different orientation are characterized by different length and different mineral deposits (draft at 3-39, lines 21-25).

Response. The methods used to estimate fracture porosity and matrix-block length from flow tests are in essence calibration methods for applying a particular numerical model to the more complex natural system. It is reasonable to use the calibrated model as a description of the more complex natural system to the extent that the calibration results in a reliable modeling capability based on comparison to test results.

Question/Comment 36. At the January 9 meeting it was said that for baseline modeling fracture porosity would be fixed at 0.001, matrix porosity would be fixed at a "mean" value of 0.15 based on "core measurements," tortuosity would be given a "medium low measured value" of 0.08, and free-water diffusion coefficients were derived from "laboratory measurements." These values are not reflected in the draft. Please explain how they were derived, and provide the data underlying them. How can values for these parameters be determined from laboratory tests, if the parameters in facts have a distinct meaning as defined in the SWIFT-II model?

Response. The values discussed at the January 9 meeting are related to the SPM-2 studies and are intended for decision making not for compliance.

Question/Comment 37. It has been previously noted that the data available do not exclude channeling behavior; this model should form the baseline. The draft does not assert otherwise (at 3-41).

Response. Channeling behavior is incorporated in the baseline by the physically unrealistic, conservative assumption that the Culebra has only one laterally continuous fracture. This is clearly a worst case assumption and is not reasonably expected.

Question/Comment 38. At the May 3-5, 1994 technical exchange Rick Beauheim reported the feedback by INTRAVAL on tracer test results as follows:

1. Agree that matrix diffusion in a double-porosity framework may be a significant process at WIPP.

2. Feel that conceptual model of uniform matrix-block size (fracture spacing) along individual travel paths but with different average block sizes along different travel paths is not a physically realistic model.

    - An alternative conceptual framework for this model may be different effective surface areas for matrix diffusion along different travel paths. However, this conceptualization may be strongly scale dependent.

3. Agree that conceptual model based on anisotropic transmissivity is a viable model. However, consideration should be given to testing this model with an additional multi-well test configuration and alternate pumping wells, thereby providing better geometrical constraints.

4. Feel that alternative conceptual models that incorporate transport channeling should be used to examine existing tracer test data and that additional field tracer-tests should be considered to differentiate alternative physical transport models.

Does the project position differ from these statements? If so, please state how the project believes that these concerns have been resolved?

Response. The feedback from INTRAVAL on these issues has helped to motivate the new tracer test presently underway at H-19.

Question/Comment 39. At the May 3-5, 1994 meeting it was stated that a channeling model

has the following potential impacts:

1. Tracer transport along multiple fracture channels with varying transport efficiency may be capable of producing breakthrough curves similar to those observed in WIPP tests:

   - significantly different breakthrough behavior along different travel paths
   - breakthrough curves with sharp peaks and tailing on the falling limb

2. Present tracer data is insufficient to determine whether or not channeling flow/transport occurs in Culebra Dolomite.

3. If transport in Culebra occurs as fracture channeling with no matrix diffusion, this would eliminate a significant amount of the potential physical and chemical retardation.

4. If transport in Culebra occurs as fracture channeling with matrix diffusion, there is significant uncertainty in how much surface area will be available for matrix infusion.

Does the project position agree with these statements? If not, please explain why not.

Response. The project does not necessarily agree with these statements but these kinds of reasoning motivated the new tracer test presently underway at H-19.

Question/Comment 40. At the January 9 meeting it was said that the effects of channeling are subsumed in the low value of specific surface used in the baseline model. Please explain in detail how this is so. The draft states that fracture channeling may result in up to a factor of 10 increase in radionuclide release relative to uniform flow over entire fracture planes (at 3-41, lines 13-14). Does the proposed baseline have that effect?

Response. The SPM-2 baseline assumption of one laterally continuous fracture is physically unrealistic and extremely conservative. The effects of channeling in portions of the natural fracture network that have relatively large apertures is incorporated in the physically unrealistic assumption for the baseline.

Question/Comment 41. At the meeting on January 9 we were told that dispersion has been found to be relatively unimportant. Please provide the data supporting such assertion. It will be important to know what model was used in such calculations.

Response. The assertion is supported in the report on brine reservoir breaches by Reeves et al., "Regional Double-Porosity Solute Transport in the Culebra Dolomite Under Brine-Reservoir-Breach Release Conditions: An Analysis of Parameter Sensitivity and Importance," SAND89-7069.

Question/Comment 42. What data underlie the choice of 100m as the baseline parameter for dispersion, as stated at the January 9 meeting?

Response. The choice of 100m as the baseline value for dispersion is an elicited value used in the SPM calculations.

Question/Comment 43. The draft states that no experimental data for actinides exist on physical retardation due to matrix diffusion (draft at 3-47, lines 8-9). Does the draft mean to imply that experimental data exist on chemical, as distinguished from physical retardation, and if so what data?

Response. "Chemical retardation" is not retardation in the physical sense of slowing motion, but is brought about by chemical or physical adsorption onto a surface, as well as by other chemical reactions. A chromatographic column illustrates chemical adsorption very well and retards different compounds to different degrees. The open peer-reviewed chemical literature includes considerable published experimental data on chemisorption and physical adsorption (van der Waals adsorption) of actinide compounds. Some examples are:

Choppin, G. R. 1988. "Humics and radionuclide migration." *Radiochimica Acta.* vol. 44/45: 23 28.

Felmy, A. R., D. Rai, and M. J. Mason. 1991. "The Solubility of Hydrous Thorium(IV) Oxide in Chloride Media: Development of an Aqueous Ion-Interaction Model." *Radiochimica Acta* vol. 55: 177 185.

Francis, A. J. (1985) Low-level radioactive wastes in subsurface soils, in Soil Reclamation Processes: Microbiological Analyses and Applications (R. L. Tate III and D. A. Klein, eds.), 279 331, Marcel Dekker, Inc., New York, New York.

Gregory, J. 1978. Flocculation by inorganic salts. In *The Scientific Basis of Flocculation* (K. J. Ives, ed.), 89 99, Sijthoff and Noordhoff.

Harvie, C. E., N. Møller, and J. H. Weare. 1984. "The Prediction of Mineral Solubilities in Natural Waters: The Na-K-Mg-Ca-H-Cl-SO_4-OH-HCO_3-CO_3-CO_2-H_2O System to High Ionic Strength at 25°C." *Geochimica et Cosmochimica Acta* vol. 48: 723 751.

Hiemenz, P. C. 1986. *Principles of Colloid and Surface Chemistry*. Marcel Dekker, Inc., New York, New York.

Honeyman, B. D. 1991. "Surface chemistry, colloids and trace-element scavenging." In

Marine Particles: Analysis and Characterization; D. C. Hurd and D. W. Spencer, eds.; Geophysics Monograph 63: 437 451, American Geophysical Union, Washington, D.C.

Lieser, K. H., A. Ament, R. Hill, R. N. Singh, U. Stingl, and B. Thybusch. 1990. "Colloids in groundwater and their influence on migration of trace elements and radionuclides." *Radiochimica Acta* 49, 83 100.

Lieser, K. H., B. Gleitsmann, S. Peschke, and T. Steinkopff. 1986a. "Colloid Formation and Sorption of Radionuclides in Natural Systems." *Radiochimica Acta* vol. 40: 39 47.

Lieser, K. H., B. Gleitsmann, and T. Steinkopff. 1986a. "Sorption of trace elements or radionuclides in natural systems containing groundwater and sediments." *Radiochimica Acta* vol. 40: 33 37.

Lieser, K. H., B. Gleitsmann, S. Peschke, and T. Steinkopff. 1986b. "Colloid formation and sorption of radionuclides in natural systems." *Radiochimica Acta* vol. 40: 39 47.

Lloyd, M. H., and R. G. Haire. 1978. "The chemistry of plutonium in sol-gel processes." *Radiochimica Acta.* vol. 25: 139 148.

Lyklema, J. 1978. "Surface chemistry of colloids in connection with stability." In *The Scientific Basis of Flocculation*, 3 36; K. J. Ives, ed.; Sijthoff and Noordhoff, Dordrecht, The Netherlands.

Stumm, W. (1992) *Chemistry of the Solid-Water Interface*. John Wiley & Sons, Inc.

Question/Comment 44. There is a description of the fracture characteristics of the Culebra (subhorizontal fractures with "adjacent" clay concentrations; high-angle fractures without clay) (draft at 3-48, lines 29-40; 3-49, lines 1-10). What is the source of these statements (other than Beauheim & Holt (1990))? Please provide the data relied upon, if unpublished.

Response. The references are described on page 3-48, lines 8-9, and in Beauheim & Holt (1990).

Question/Comment 45. What is the data referred to as "observations" of flow data (draft at 3-39, lines 12-14)? Please provide the data, if unpublished.

Response. There are no data referred to as "observations" at the page and line numbers indicated. We are unable to locate the intended reference.

Question/Comment 46. The baseline assumption is that advective flow occurs through the high-angle fracture set only (draft at 3-50). It is more accurate to say that clay associated with fractures is discounted?

Response. Confining flow in the conceptual model to the high-angle fractures conservatively discounts the effects of clay on radionuclide transport.

Question/Comment 47. The draft says that a weighted distribution coefficient is used, calculated from expert panel estimates of K_ds. (draft at 3-50). This is not defensible. The expert panel estimates are not supported by data.

Response. The purpose of citing the source of an estimate is to allow the reader to make his or her own judgment about the applicability of that estimate. The expert panel did use data, however, the report has not been published.

Question/Comment 48. The draft also refers to the "baseline conceptual model, with $K_d = 0$" (at 3-51). Is the baseline model that $K_d = 0$ or not? What is the role in the baseline of the stated position that the project will "take no credit for adsorption" (draft at 3-52)?

Response. Modeling with $K_d = 0$ is equivalent to taking "no credit for adsorption" and is a bounding position, as stated on page 3-52. However, other conceptual models do use a non-zero K_d, such as conceptual model 2 in Table 3.3-1.

Question/Comment 49. A linear isotherm model of adsorption is not conservative or defensible for any actinide, is it? See draft at 3-52. Also, the assumption of chemical equilibrium between solutes and solid phases is not found (draft at 3-53).

Response. Many compounds exhibit linear adsorption isotherms; surface adsorption is more often represented by a linear isotherm than by any other single functional relationship. A linear adsorption isotherm may be more "conservative" than, for example, an s-shaped isotherm for some values of amounts of solid and concentrations in solution, but not for others.

The citation (page 3-53) points out that insufficient data are available to distinguish between any isotherm. Assumption of chemical equilibrium between dissolved and undissolved states of a compound is a conservative assumption as far as dissolution is concerned; if the dissolved concentration is not an equilibrium concentration, it will be less than the equilibrium concentration because equilibrium will not have been attained.

Question/Comment 50. It is claimed that K_ds are independent of scale and that spatial variations will be due to heterogeneity, length of equilibration time, and different processes. (draft at 3-53, 3-54). What tests are planned to account for these influences? We would not agree with a randomly based distribution function.

Response. The distribution coefficient (K_d) is a function of the physico-chemical reaction between an adsorbent (the solid surface in this case) and an adsorbate (the compound being adsorbed). It is an intensive property of the surface and depends on temperature, pressure and other intensive thermodynamic functions. Like any intensive property, it can be determined for a small sample of a system and will still apply to a larger portion of the same system. Moreover, the distribution function is a function of an interface and is not affected unless the interface is affected. Different systems and different interfaces may exhibit different distribution functions, so that flow over a changing surface may result in different distribution functions over different parts of the surface.

Question/Comment 51. Is any lab work contemplated to confirm the stated conclusion that "hard sphere" carrier colloids will agglomerate and settle (draft at 3-55)?

Response. An extensive literature exists that examines the properties of hydrophobic ("hard sphere") colloids. Hydrophobic colloids have discrete well-defined particle-fluid boundaries. They are stabilized in very low ionic strength solutions by electrostatic effects caused by the electric double layer surrounding the particles (Hiemenz, 1986). At high ionic strengths, however, the electric double layer surrounding the "hard sphere" colloids collapses and virtually eliminates electrostatic repulsion between particles (Derjaguin and Landau, 1941; Verwey and Overbeek, 1948; Matijeviloc, 1973). Van der Waals forces of attraction between the particles then cause agglomeration, forming particles large enough to settle under gravity. This phenomenon has been known empirically for well over a century (see, e.g., Hardy, 1900) and was theoretically explained in the middle of this century (Derjaguin and Landau, 1941; Verwey and Overbeek, 1948). This principle is used today in municipal water purification systems. The WIPP project does not need to perform additional work to demonstrate this principle.

Derjaguin, B. V., and L. Landau. 1941. "A theory of the stability of strongly charged lyophobic sols and the coalescence of strongly charged particles in electrolytic solutions." *Acta Phys.-Chim. USSR*. vol. 14: 633.

Hardy, W. B. 1900. "A preliminary investigation of the conditions which determine the stability of irreversible hydrosols." *Proceedings of the Royal Society of London*. vol. 66: 110 125.

Hiemenz, P. C. 1986. *Principles of Colloid and Surface Chemistry*. Marcel Dekker, Inc., New York, New York.

Honeyman, B. D. 1991. "Surface chemistry, colloids and trace-element scavenging." In *Marine Particles: Analysis and Characterization*; D. C. Hurd and D. W. Spencer, eds.; Geophysics Monograph 63: 437 451, American Geophysical Union, Washington, D.C.

Honeyman, B. D., and P. H. Santschi. 1992. "The role of particles and colloids in the transport of radionuclides and trace metals in the oceans." In *Environmental Particles*, vol. 1: 379 423; J. Buffle and H. P. van Leeuwen, eds.; Lewis Publishers, Inc., Chelsea, Michigan.

Matijeviloć, E. 1973. "Colloid Stability and Complex Chemistry." *Journal of Colloid and Interface Science*, vol. 43: 217 245.

Verwey, E. J. W., and J. T. G. Overbeek. 1948. *Theory of the Stability of Lyophobic Colloids. The Interaction of Sol Particles Having an Electric Double Layer.* Elsevier Publishing Company, Inc., New York, New York.

Question/Comment 52. What is the schedule for completion of the "ongoing colloidal experimental program" (draft at 3-56, line 12)?

Response. The colloid experimental program is scheduled for completion prior to submittal of the CCA.

Question/Comment 53. What is the schedule for completion of the experiments involving actinide intrinsic colloids, referred to at draft 3-57, line 31?

Response. The colloid experimental program is scheduled for completion prior to submittal of the CCA.

Question/Comment 54. The discussion of colloid transport is tied to the question of mean pore throat diameter of the Culebra matrix (at 3-58). Given the heterogeneity of that rock body, how can the pore throat diameter be deemed established for the entire area of interest?

Response. The Culebra formation is composed of microcrystalline dolomite and, despite general heterogeneity in macroscopic fabric, the microscopic rock fabric is remarkably uniform. This situation is consistent with observations of microcrystalline carbonates in general. Kelley and Saulnier, SAND90-7021, analyzed pore throat diameter for samples collected on a regional scale in the Los Medanos region and found that the measured maximum and minimum pore throat diameter varied over about one order of magnitude for all samples, which is remarkably uniform for the geologic environment.

Question/Comment 55. At the January 9 meeting we were given new information about a "source-term baseline for mobile actinides" in Hans Papenguth's presentation. These data depart from the inventory limits model which Craig Novak said a month previous were the only defensible data. Use of such expert panel solubility values is indefensible.

Response. The WIPP project is developing the Chemical Model for mobile actinide concentrations. This model will be based on a realistic description of the important chemical phenomena that could mobilize actinides in the WIPP disposal rooms. This model follows an established formalism for both dissolved and colloidal species. It will provide a realistic estimate for the actinide source term once it is fully developed. As an aside, this issue is fully discussed in the Actinide Source Term Position Paper; it is outside the scope of the Non-Salado Flow and Transport Position Paper.

Question/Comment 56. Without any quantification of the scope of dissolution from brine mixing, it cannot be deemed accounted for. The discussion at 3-59, 3-60 is inadequate.

Response. Doe considers that we are using a reasonable and conservative approach because chemical reactions, which may increase transmissivity by dissolution, will also decrease radionuclide content by coprecipitation; because the radionuclide decrease is disregarded, the PA will overestimate the radionuclide transport to the accessible environment.

Question/Comment 57. The discussion of climate change (draft at 3-62 et seq.) is limited to the single effect of a rise in the water table. The paper by Roger Y. Anderson enclosed with this letter discusses several other effects of climate change which have not been accounted for in the 2-D or 3-D modeling reported by DOE. These processes must be modeled to analyze climate change adequately.

Response. Alternative hypotheses for climate change effects will be evaluated during scenario screening and incorporated into future PAs if their effects cannot be screened out. Issues related to screening of scenarios are discussed in the Scenario Position Paper; these issues are outside the scope of the Non-Salado Flow and Transport Position Paper.

Question/Comment 58. The draft says that local gradients can exceed the slope of the surface (at 3-62 lines 24-26). How may this come about with relation to climate change, and how is it modeled?

Response. The slope of the water table is generally a subdued expression of the land-surface topography. "Subdued" means that all of the variation in land-surface elevation will not be expressed in the water table elevations; thus, it is easy to conceive of the situation in which a flat spot in topography is not a flat spot in the water table; i.e., the water table slope exceeds

the local topographic slope. This is a natural condition of subsurface fluid flow.

Climate changes expressed as changing rates of infiltration can affect the local groundwater gradients but will not introduce any new component or process that is not already present. Regional modeling uses boundary conditions that allow for changing infiltration rates. Climate change is then modeled through the input data for boundary conditions; no changes to the conceptual model are required.

Question/Comment 59. There are statements as to the possible changes in flow direction and gradient associated with climate change and a higher water table (at 3-62, lines 27-37). Please cite the source of these statements, and, if unpublished, provide the data supporting the statements..

Response. These statements are based on general principles of groundwater flow and preliminary results of three-dimensional modeling to evaluate the potential effects of climate change on the groundwater basin. The results are not available for release at this time, but will be published at the conclusion of the study.

Question/Comment 60. It is said that a wetter climate could increase the thickness, lateral extent, and flow directions in the saturated zone of the Dewey Lake and Dockum Group (at 3-63). What would be the effect in an intrusion scenario? Has this been modeled, and is there any plan to do so?

Response. There is little or no effect from an intrusion scenario because the Dewey Lake has (1) a large sorptive capacity and (2) chemistry that indicates that colloids will be unstable. Radionuclide transport in the Culebra is unlikely in this situation and releases in the Dewey Lake are assumed to be of no consequence in the PA.

Question/Comment 61. The draft clearly does not answer all questions concerning shallow drill holes (at 3-63, 3-64). The groundwater basin model has yet to be used to analyze that situation in any published materials. What, in any case, is the supposed effect if Culebra flow "pivots" to the west-southwest? The issue must be addressed.

Response. Qualitatively, gradients increase by a factor of approximately 2, which tends to increase groundwater velocities; however, conductivities in the west-southwest direction are lower than along the high-T zone.

Question/Comment 62. The discussion of mining and subsidence is likewise incomplete. It seems to limit itself to increases in conductivity of Rustler strata. Even as to that issue the

paper reaches no conclusion and cites no research. Moreover, mining can do more than increase conductivity in the Rustler. It can create a highly conductive channel -- the mine workings - in the Salado, a question that has not been faced.

Response. Mine workings would need to be directly connected to the repository for the high conductivity channels to be important; however, the likely strata for a potash mine are above the Salado, leaving a large undisturbed region between the mine and the repository.

Changes in topography of a couple of meters on the local scale of mine subsidence is insignificant in regional flow. The impact of fracturing is suggested to be similar to that of shallow boreholes, which is discussed from general principles in section 3.4.2 of the Position Paper

Question/Comment 63. In Tom Corbet's presentation on January 9 we were told that for the SPM-2 baseline flow would be partitioned to the Dewey Lake, Magenta and Culebra according to their respective hydraulic heads. What are the hydraulic head data to be used for that purpose for the Dewey Lake and the Magenta? We were also told that 25% of the flow to the Dewey Lake would be deemed released. What is the source of that figure, and why should it be deemed conservative in all instances?

Response. The parameters used for hydraulic heads and partitioning were determined from SPM-2 elicitations and are described in the SPM-2 final report. The 25% released figure was also determined during elicitations.

At the time of elicitations, the effects of sorption and colloid instability in the Dewey Lake were not considered. The SPM-2 baseline, considering such effects, assumes no radionuclide releases can occur via the Dewey Lake.

Question/Comment 64. I refrain from detailed comment on Chapter 4, the data narrative, because it does not deal with the use of specific data in modeling repository performance.

Response. Comment noted.

Question/Comment 65. The draft acknowledges the difficulty of identifying the undisturbed potentiometric surface of the Culebra in light of numerous intrusions (shafts, wells) in the area (at 4-12). The enclosed paper by David Snow emphasizes the nature of the problem. What effort will be made to deal with this serious issue, which is especially troublesome in light of the significant water level increases recently noted in numerous wells about the site? DOE has not yet faced this issue.

Response. Many of the concerns raised by Dr. Snow had been addressed prior to Dr. Snow's comments in La Venue et al., "Ground-Water Flow Modeling of the Culebra Dolomite, Volume 1: Model Calibration," SAND89-7068/1.

Question/Comment 66. The Salado Flow and Transport position paper states (at p. 12) that the non-Salado paper will disucss the scenario of a borehole penetrating a fractured anhydrite. Where is that discussion?.

Response. The Salado paper states that the Non-Salado paper discussed the properties of the borehole and its interactions. This discussion is included by reference to Reeves et al. (1991) on page 3-31 of Revision 1.

Question/Comment 67. When may we expect that results of 3-D modeling of the groundwater basin will be published?

Response. The current schedule indicates this will not be available prior to January, 1996. A specific release date has not been determined.