Attorney General of New Mexico

PO Drawer 1508
Santa Fe, New Mexico 87504-1508

505/827-6000
Fax 505/827 5826

TOM UDALL
Attorney General

MANUEL D. FIERRO
MARIAN MATTHEWS
Deputy Attorneys General

October 21, 1994

Every other page...

Post-it® Fax Note 7671	Date 10/21	# of pages ▶ 7
To Kathy Sanchez	From Lucas Lowry	
Co./Dept. ED	Co. AIM - AGO	
Phone # 7-2850	Phone # 827-6695	
Fax # 7-2836	Fax # 827-4440	

Mr. Robert Bills
U.S. Department of Energy
Carlsbad Area Office
P.O. Box 3090
Carlsbad, New Mexico 88221

Dear Bob:

This letter is written to provide preliminary comments on the PA treatment of Salado fluid flow and transport for purposes of Systems Prioritization.

Initially, it is clear that a subject of this complexity cannot be treated adequately without the assistance of experts in fields such as hydrology and modeling. Our office has attempted to obtain funds to retain such experts, but funding has not been made available.

Second, at the SPM meeting on September 28-29 it was stated that a new version of the Salado white paper would be available by November 15. That draft should be sent to all SPM participants. It should take account of intervening comments and should address the points on which the September 14, 1994 draft (the "draft") is incomplete (see draft at 1). After sufficient time for stakeholders to review the new document, another stakeholder meeting should be held.

The nature of the SPM process, and thus the nature of the comments appropriate to that process, are only slowly becoming apparent. It seems, from the breadth of the draft and the presentation, that all issues involving the available data, conceptual models, and PA modeling of the Salado are being placed on the table for comment, including undisturbed geology and hydrology, room closure and expansion, fluid inflow and escape, and contaminant transport. Stakeholders are invited to give input to the process in the form of their "concerns," which DOE has said it will address, but DOE reserves the right to address stakeholder concerns in a manner of DOE's choosing. Further, DOE is assembling a project technical baseline comprised of data which is either

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defensible or, by some test, the most conservative approach for use in demonstrating compliance both with 40 CFR 191 and with RCRA no-migration rules, as the basis for evaluating proposed activities for compliance demonstration. In this context, the following overall problems arise:

1. There is doubt whether the draft contains sufficient information for the stakeholders to express their concerns. There was much discussion at the SPM meeting among personnel of Sandia, Westinghouse, and EEG concerning data which are not in the draft and may not be published at all (e.g., concerning the asserted continuous nature of various beds, room pressure effects on brine flow, fracture effects of blasting, PA impact of the selection between competing Salado brine flow models, alternative preferential flow models and their impact, brine flow thermal tests, GSEEP brine flow, brine in a Marker Bed 140 hole, brine observations in Room G generally, brine observation in the Air Intake Shaft, the anhydrite fracture model). Even if I had the requisite expertise, without the same data that DOE has it is not possible to comment effectively.

2. Preparation and discussion time was inadequate for the range of subjects presented.

3. The setting of an informal exchange is a difficult one in which to express concerns about complex technical issues; a written exchange is better but only if complete information is furnished and responses to inquiries are provided.

4. DOE's stated position on several issues is ambiguous, making it hard to comment. For example, does the proposed anhydrite fracture model accept the arguments contained in the Larson and Davies memorandum attached as Appendix D? If not, will the technical baseline do so?

5. In case of conflicting conceptual models, how will DOE choose one for the technical baseline? What is the criterion? Given the number of coupled nonlinear processes involved, it may not be easy to identify the model which gives rise to the greatest release. Further, what is conservative for purposes of 40 CFR 191 may not be conservative for purposes of RCRA no-migration rules.

6. There are questions about what are styled as competing models of brine flow in the Salado. Beauheim et al., 1991 and 1993a are cited in support of the far-field Darcy flow model (draft at 25). However, there are uncertainties expressed in these materials, leading to the question whether the uncertainties have been treated in a conservative manner. Beauheim et al. (1991)

says that the number of tests discussed in that report is too small to allow firm conclusions on a repository or regional scale (at 121), finds insufficient data to describe the relationship between an excavation's age and size and the properties of the DRZ (at 123), and concludes that "the interpreted results of the Salado permeability tests conducted to date are inconclusive with respect to the question of whether or not continuous interconnected porosity exists within the Salado." (at 128; see also 130, 131). After further tests, Beauheim et al. (1993a) concludes that the "applicability of Darcy's Law to flow under the low gradients naturally existing in the Salado Formation remains uncertain" and "the presence or absence of hydraulic anisotropy in halite is uncertain" (at 141). The 1993 report also says that an assumption of Darcy flow provides a reasonable approach to understanding flow through evaporites, at least under high gradient conditions (at 141). Given these qualifications, what far-field flow model is proposed as an element of the technical baseline, and is it entirely conservative?

7. There are also questions about the alternative near-field flow model (or models). The 1990 Brine Sampling and Evaluation Program (BSEP) report states that far-field flow "seems unlikely or hypothetically impossible but remains an important modeling concept that has not been disproved." (Deal et al., 1991 at xx and 5-9). Is it still the view of the proponents of the near-field flow model that the far-field flow model has not been disproved?

8. There is discussion of GSEEP in the 1990 BSEP report (Deal et al., 1991 at 2-11). At the September 29 meeting there was reference to a new data set relating GSEEP brine to the chemistry of Marker Bed 140 and a possible path from Marker Bed 140 to GSEEP. The new data should be provided to stakeholders. The proponents of different models should provide their interpretation of the data.

9. Several down-drilled holes show steady inflow of brine after six years (Deal et al., 1991 at 2-15). See also the 1991 report (Deal et al. 1993, at 2-28 and 2-30). What is the explanation for this continuing flow, as proposed by the proponents of the near-field model? When will this flow stop under their theory?

10. The observations in the 1990 report about brine-filled fractures under intersections in the northern part of the repository (at 2-19) appear to conflict in theory with the observations in the 1991 report (Deal et al. 1993, at 5-4). Please comment. Has there been testing of conductivity below the E0 drift?

11. The subhorizontal 46 meter holes were still producing brine in 1990. (Deal et al. 1991, at 2-34). They continued to flow in 1991. (Deal et al, 1993, at 2-34). What does that fact imply with respect to the far-field versus near-field models?

12. How do proponents of the near-field model explain the facts that in the subhorizontal holes inflow is not dropping off as rapidly as predicted by the near-field modeling and that total volume of brine is greater than expected for near-field flow only (Deal et al. 1991, 15 4-35)? Are these trends continuing?

13. The 1990 BSEP report compares models of flow into a 7.6 cm hole and points out the utility of smaller scale drill hole experiments to address the far-field versus near-field flow question. (Deal et al, 1991, at 4-17). A test plan is attached (Appx. E). Have these proposals been carried out, and if not, why not? If so, what are the results?

14. What is the estimated time necessary to distinguish between far-field and near-field flow by experimental means?

15. Do the proponents of the near-field model now assert that a far-field model with a lower permeability may be appropriate (Deal et al. 1991, at 4-36)?

16. Is there no far-field flow model consistent with present data which would allow brine to be introduced from halite (including impure halite) into clay layers for conduction to rooms and drifts? Such a model would not limit brine inflow to the volume of the clay layers, would it? (Deal et al., 1993, at 4-2).

17. Is the hypothesis involving flow through vertical fractures in the ribs (walls) of the rooms deemed essential to the far-field model? (Deal et al., 1993, at 5-2). Is the hypothesis involving flow through Marker Bed 139 deemed essential to the far-field model? (id. 5-4).

18. In the draft (at 28) it is said that of 119 drillholes initially involved in the BSEP program only 14 are still monitored. How did this come about, and is the remaining sample skewed in any way?

19. The early reports of the BSEP emphasizes the variability of brine observations in different, sometimes nearby, locations (Deal and Case, 1987, at ES-2; Deal et al., 1987, at 17). The discussion at the September 29 meeting emphasized the geologic continuity of the strata and did not remark about the local

variation in brine observations. Has these been a change in the nature of the observations? What was the cause of the earlier variability?

20. The draft, describing the characterization of the DRZ, says that certain tests of the DRZ do not provide PA with modeling parameters such as porosity, permeability, and initial saturation (at 26). Is there any plan to develop such data? Could such data be employed to better characterize the initial conditions of the repository for PA analysis?

21. At the September 29 meeting no one spoke in support of the model proposed by McTigue in the 1990 memorandum in Appendix E of the draft. In the view of the far-field proponents, are there data (e.g., as to capacitance) that can only be explained through a model such as McTigue's? What are the implications of McTigue's model for total brine inflow? Are the omissions listed on page 14 of the 1990 McTigue memorandum conservative ones?

22. It would assist comparison of models to have estimates of the rate of brine flow and the cumulative brine flow projected by the far-field model, the near-field model proposed in the BSEP reports, and the Darcy-flow model discussed in McTigue (1993).

23. Certain further questions are raised by the February 21, 1994, memorandum, Appendix B. I request that the memorandum by Sam Key of RE/SPEC dated September 3, 1993 describing the fracture model be made available. The Fracture Expert Group termed the model a reasonable "first effort" (Appx. B at 1). What further efforts are planned? Will the experimental data deemed necessary be the Fracture Expert Group be pursued (Appx. C at 5)?

24. Please make available the Westinghouse analysis and the Barry Butcher study referred to on page 2 of Appendix B (paragraph E) which relate to the initial brine saturation values.

25. Please also provide the data generated by Larry Brush concerning rates of gas generation by corrosion (Appx. B, p. 2, ¶F).

26. The memorandum (Appx. B) emphasizes the importance of the relative permeability submodel used to determine two-phase flow (at 3-4). What experiments are planned to determine whether the Van Genuchten-Parker or the Brooks-Corey submodel more accurately describes relative permeability? Are there any data justifying selection of the Van Genuchten-Parker model for 16 of 50 iterations and the Brooks-Corey model for the remainder? Further,

the recent SPM-1 report states that the Brooks-Corey and Parker-Van Genuchten two phase flow relationships may not capture the extremes in flow behavior in the Salado (at B-3). What models express the extremes? Should they not be incorporated in the project technical baseline?

27. There is also reference to the lack of capillary pressure data (Appx. B at 4). Will such data be obtained?

28. Please make available the materials used in presentations to the Fracture Expert Group on March 23-25, 1993.

29. The Fracture Expert Group recommended (a) a literature study on flow in jointed rock masses, (b) introduction of residual saturation as a sampled variable, (c) a study of time step and grid block size to determine the adequacy of resolution, and (d) 3-D hydrological simulations with independent software. The Group also recommended improvements in the BRAGFLO modeling of flow in the anhydrites as modified by fracturing. (Appx. C at 5-7). The recommendations included (1) in situ fluid and gas-driven slow fracturing tests; determination of the horizontal component of in-situ stress in Marker Beds 130 and 139; examination of MB 138 and 139 for structures and fractures important to porosity and permeability; a hydrological repository analogue experiment to seek BRAGFLO validation; "bracketing" of parameters used in the first-order model; development of field-scale averages for locally measured parameters used in BRAGFLO; (2) in the laboratory, bracketing the variability in hydrologic parameters of the anhydrites and cross-correlating them; measurement of anhydrite mechanical properties; measurement of multiphase flow; study of flow characteristics in altered anhydrite to test Darcy flow hypothesis; (3) as to fracturing itself, examination of crack path stability and site heterogeneity to test the hypothesis of axial symmetry of the crack front; investigation of channeling and fingering; investigation of in situ stress as to whether fracturing will be vertical or horizontal; investigation of basically whether the equilibrium state of anhydrite fracturing can be predicted from first principles; (4) and coupled mechanical and hydrological simulations involving the development of expressions for continuum porosity and permeability with reference to observed damage; stress-strain models for anhydrite and halite; simulations of room closure and inflation; relation of continuum porosity and permeability to crack extensions; coupled mechanical-hydrological simulations. To what extent will DOE pursue these recommendations?

30. The October 11, 1993 memorandum by Larson and Davies (Appx. D) raises a basic question as to the appropriateness of the porosity model versus an aperture model of fracturing. Will the

concerns raised in this paper be explored? Will the recommendations on page 6 of the memorandum -- a distribution allowing J to reach 40 or 50 and, later, a new correlation between porosity change and element permeability -- be adopted? Please made available the paper by Fewell referred to on page 1 of Appendix D.

Thank you for considering these comments.

Very truly yours,

Lindsay

LINDSAY A. LOVEJOY, JR.
Assistant Attorney General

LAL:mh

cc: Robert H. Neill, EEG
Kathleen Sisneros, NMED
Larry Weinstock, EPA
Christopher Wentz, EMNR