Department of Energy
Carlsbad Area Office
P. O. Box 3090
Carlsbad, New Mexico 88221

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

Dear Mr. Lovejoy:

Thank you for your comments on the 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 Robert Bills of my staff at (505) 234-7481.

Sincerely,

Michael H. McFadden
Assistant Manager
Office of Regulatory Compliance

Enclosure

cc w/enclosure:
J. Mewhinney, CAO
P. Sallani, CAO
C. Wayman, CAO
L. Shephard, SNL

SALADO POSITION PAPER
QUESTIONS AND RESPONSES

Question/Comment 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at the SPM meeting among personnel of Sandia, Westinghouse, and EEG concerning data which are not in the draft and may not be published at all (e.g., concerning the asserted continuous nature of various beds, room pressure effects on brine flow, fracture effects of blasting, PA impact of the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.

> *Response. Performance assessment is an iterative process involving detailed discussions among experts in many disciplines. We believe that we can best answer the stakeholders' concerns by conducting a completely open and scientifically sound process, using a combination of meetings and Position Papers to communicate with the stakeholders. We are committed to providing you with the data that you need and encourage you to request these data. We further encourage you to fully utilize the excellent scientific expertise available to the State of New Mexico.*

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

> *Response. Please note, however, we will consider all Stakeholder comments through the final update of the position paper. Additionally, at your request we have increased the review time for Stakeholders.*

Question/Comment 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.

> *Response. Please see the response to Questions/Comments 1 and 2.*

Question/Comment 4. (a) DOE's stated position on several issues is ambiguous, making it hard to comment. (b) 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?

Response. (a) Please see the response to Question/Comment 1. The DOE thinks that full and open communication with the stakeholders will best answer their concerns.

(b) The Larson and Davies memorandum discusses just one of several alternative models for fracture flow in the anhydrite. There is a body of literature on fracture flow modeling (e.g., Brace, 1977; Walsh and Brace, 1984; Snow, 1965; Witherspoon, 1980) addressing aperture and porosity models. For example, Poiseuilles equation (Brace, Brace and Walsh) couples the response of both porosity and fracture aperture and has been used to correlate electrical resistivity and hydrologic permeability for WIPP anhydrite interbeds and salt.

References:
Boussinseq, J., 1968. Memoire sur l'Influence des Frottements dans les Movements Reguiers des Fluis, J. Math Pure Appl., 13, 377-424 (cited by Witherspoon and others, 1980).
Brace, W.F., 1977. Permeability from Resistivity and Pore Shape, Journal of Geophysical Research, 82, 3343-3349.
Snow, D.T. 1965. A parallel plate model of Fractured Permeable Media, Ph.D. Thesis, University of California, Berkeley, 331p.
Walsh, J.B. and W.F. Brace, 1984. The effect of Pressure on Porosity and the Transport Properties of Rock, Journal of Geophysical Research, 89, 9425-9431.
Witherspoon, P.A., Wang, J.S.Y., Kwai, K. and J.E. Gale, 1980. Validity of the Cubic Law for Fluid Flow in a Deformable Rock Fracture, Water Resources Research, 16, 1016-1024.

The fracture models adapted by the PA and the alternative presented by Larson and Davies are unverified. Larson and Davies only propose an empirical adjustment that is not bounded by theory or physical understanding. However, the development in Larson and Davies has influenced the method whereby permeability parameters are chosen within the PA model, effectively increasing the rapidity of permeability increase. Because neither model is verified and because there is no analog or WIPP-specific data, the PA model has been retained for use in the SPM-2 Baseline Model at this time.

Question/Comment 5. (a) In case of conflicting conceptual models, how will DOE choose one for the technical baseline? What is the criterion? (b) 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.

Response. (a) The conceptual model for fluid flow in the Salado formation has three *different mechanisms: a far-field flow mechanism, a redistribution mechanism for the DRZ and a clay consolidation mechanism. The conceptual model retains these three mechanisms because definitive data do not exist to rule out any of the mechanisms. In addition, the mechanisms are not necessarily in conflict, in the sense that several may be active in repository response, albeit at different spatial locations and at different times after excavation.*

The numerical model for performance assessment calculations will be based on far-field flow. Within the stochastic framework of the performance assessments, this model is capable of predicting a range of brine inflow and outflow quantities that span the expected response for all three hydrologic mechanisms. For example, some vectors within the stochastic framework will result in little brine inflow, which is representative of the expected performance of the clay consolidation flow mechanism (alone). The rationale for choosing the far field flow mechanism as the numerical model for PA is explained in more detail in the Draft Compliance Certification Application, Section 6.3.4, Salado Hydrology, to be published by April, 1995.

(b) For increased assuredness of public safety, CAO and SNL have often used exceptionally conservative values for many parameters; however, this is not required by 40 CFR 191, which has the requirement to simulate the "reasonably expected" response. CAO/SNL feel that the stochastic framework of the performance assessments is the most appropriate and realistic way to investigate the range of releases for regulatory compliance with 40 CFR 191.

Question/Comment 6. (a) 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). (b) Given these qualifications, what far-field flow model is proposed as an element of the technical baseline, and is it entirely conservative?

Response. (a) Please see the first paragraph of the response to Question/Comment 5, part (a).

(b) Please see the response to Question/Comment 5, part (a), regarding the model for the technical baseline, and part (b), regarding the conservatism of the baseline.

As noted in the response to Question/Comment 5(a), the various mechanisms are not necessarily incompatible. For example, both models are based on potentiometric flow, although the redistribution model limits the total flow from the DRZ. In addition, interpretation of the experimental data from Room Q suggest that both far-field flow and DRZ flow can be active simultaneously. Geophysical surveys in Room Q (Jensen et al., 1993b) show that portions of the DRZ desaturate within a year of excavation. In the early stages of desaturation, the roof appears to desaturate with the brine flowing towards the ribs and floor. These observations are consistent with the DRZ redistribution mechanism. At about two years after excavation, the geophysical surveys show that the DRZ begins to resaturate, consistent with far-field flow.

Question/Comment 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., 1991b, xx at 5-9). Is it still the view of the proponents of the near-field flow model that the far-field model has not been disproved?

Response. The far-field flow model has not been disproved, although one near-field proponent (Deal) still considers it (far-field flow) an unlikely or minor factor. From a theoretical perspective, it is possible that all three mechanisms are active.

Question/Comment 8. There is discussion of GSEEP in the 1990 BSEP report (Deal et al., 1991b 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 model should provide their interpretations of the data.

Response. There is a summary of all existing BSEP GSEEP data in the BSEP 1992-1993 report which is presently in draft and undergoing WID/DOE review. The anticipated publication date is April, 1995.

Question/Comment 9. Several down-drilled holes show steady inflow of brine after six years (Deal et al., 1991b 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?

Response. The seepage into these holes seems to fit the model of brine seepage from clay compaction, which is included in the BSEP 1992-1993 Report, to be published April, 1995.

Question/Comment 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?

Response. Hydraulic testing was performed under the E-0 Drift and shows that holes beneath the center of the drift do not seem to be hydraulically connected. A few holes are locally interconnected at drift intersections. A report on this testing is included in the BSEP 1992-1993 report which will be available soon.

Question/Comment 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?

Response. The holes have not been monitored for a long enough period of time to resolve the question of far-field versus near-field models. If far-field flow is occurring, the holes will ultimately reach a steady-state seepage rate. If no far-field flow occurs, the holes should ultimately dry up. The inflow of brine had not yet reached either condition at the time that volumetric measurements were discontinued (one year ago), so the appropriate model is still uncertain.

Question/Comment 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., 1991b, at 4-35)? Are these trends continuing?

Response. These issues for near-field modeling are discussed in considerable detail in Section 4.7, pp. 4-27 to 4-35, of the BSEP 1990 Report (Deal et al., 1991b). As noted in this report, the model assumed radial flow which is almost certainly not the case. In addition, the near-field model did not consider the effects of clay consolidation. The modeling of seepage into the holes from clay consolidation is included in the BSEP 1992-1993 Report, to be published in April, 1995. In terms of the trend, seepage continues to decrease.

Question/Comment 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., 1991b, 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?

> *Response. The tests were not performed. At this time, it is not considered necessary or possibly even feasible to differentiate between the conceptual models in a realistic time frame.*

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

> *Response. It is possible that three different flow mechanisms are operational in the repository. If the sub-horizontal holes dry up completely, it suggests that there is no far-field flow. If the holes reach a steady-state it implies that some far field flow does occur, but it does not disprove short-term flows from brine redistribution in the DRZ nor from clay consolidation. From a regulatory and compliance viewpoint, this distinction in conceptual models may be unimportant for the numerical simulations of repository response, as discussed in the response to Question/Comment 5(a).*

Question/Comment 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., 1991b, at 4-36)?

> *Response. Deal thinks that it is reasonable for the initial PA modeling to use a far-field Darcian flow model. If PA modeling, assuming far-field Darcy flow with reasonable parameters, shows compliance, then there is no reason to expend the time and effort to further refine the numerical flow model.*

Question/Comment 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 clay layers, would it? (Deal et al., 1993, at 4-2).

> *Response. The far-field model is not inconsistent with the data and process as described.*

Question/Comment 17. a) 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). b) Is the hypothesis involving flow through Marker Bed 139 deemed essential to the far-field model (id. 5-4).

Response. Neither hypothesis is essential to the far-field model.

Question/Comment 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?

Response. Please note that volumetric measurements of brine from all BSEP holes had been suspended one year ago. Hole-by-hole explanations are included throughout the series of BSEP reports, explaining the individual cases. Most were removed from the monitoring program because they were destroyed, became inaccessible for a variety of reasons (mostly safety), or were shown to be contaminated by construction brine and not usable to understand natural brine seepage and chemistry. The latest published summary exists as Table A-1, Appendix A, BSEP 1991 report (Deal et al., 1993). If you note the individual hole of interest and the date it was abandoned, the explanation will be found in a BSEP report published subsequent to that date.

Question/Comment 19. The early reports of the BSEP emphasize 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 there been a change in the nature of the observations? What was the cause of the earlier variability?

Response. The observations of variable brine inflows are repeated and accurate. The causes may vary some from location to location, and have been discussed repeatedly in the BSEP reports. A good summary (but not necessarily the last word) is contained in the BSEP 1990 Report, Section 2.7.5, p. 2-30 to 2-31 (Deal et al., 1991b). A more specific discussion concerning the L-1S holes is presented in Section 2.1.2.6 of the BSEP 1988 Report (Deal et al., 1989).

Question/Comment 20. The draft, describing the characterization of the DRZ says that certain of the 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?

Response. Although some DRZ characterization programs do not provide PA model parameters, many existing or completed programs do provide PA model parameters for porosity, permeability and initial saturation. These parameters are used to define the initial conditions for the SPM-2 calculations; the range of these parameters will be refined as dictated by ongoing studies.

A variety of laboratory and in situ testing programs have helped to characterize the DRZ for PA analyses. For example, porosity and permeability data, as determined for the anhydrite interbeds at different confining pressures and pore pressures during the Salado Two-Phase Flow Laboratory Program, can be used to define the model for the DRZ. In situ hydrologic testing within portions of the DRZ provides additional data on the DRZ parameters (Beauheim et al., 1993b). Geophysical tests (seismic and electric) in the WIPP underground have mapped the variations in porosity, permeability and saturation within the DRZ (Borns and Stormont, 1989; Jung et al., 1991; Jensen et al., 1993b) Finally, Geologic Site Characterization (Powers et al., 1978) and Site Preliminary Design Validation (Bechtel National, 1986) programs have characterized the water content of the host salts. This information has been integrated into the description of the technical baseline model for SPM-2.

Question/Comment 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. a) 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? b) What are the implications of McTigue's model for total brine inflow? c) Are the omissions listed on page 14 of the 1990 McTigue memorandum conservative ones?

Response. (a) The McTigue model is the same model as the DRZ Redistribution Model. The DRZ Redistribution Model states that some of the observed brine inflow in the WIPP underground is brine that was originally stored in the porosity of the intact salt (the protolith) that subsequently became the DRZ. As the DRZ developed, porosity and permeability increase, allowing the brine to flow into the excavation. The Redistribution Model does not contradict the Far-Field Flow Model and may actually be a specialized subset or subdomain of the Far-Field Flow Model.

(b) McTigue's model, as a stand-alone model, limits potential brine inflow to the volume initially stored in the DRZ. When the stress field re-establishes equilibrium after repository closure, all brine flow would cease according to this model.

(c) The omissions were made for numerical and conceptual simplicity in a preliminary model. They are not necessarily conservative.

Question/Comment 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).

*Response. For the DRZ Redistribution Model, the potential volume of brine available around a 100 m long room in the initial pore structure, with 0.01 initial porosity, that becomes part of the DRZ ranges from approximately 28 m^3 (1 m thick DRZ) to 151 m^3 (4 m thick DRZ). With a 0.02 initial porosity, the range is between 57 m^3 (1 m thick DRZ) to 302 m^3 (4 m thick DRZ). These volumes provide the **UPPER LIMIT** of brine available for redistribution. Some brine remains in the pore structure due to residual saturation and being in dead-end or non-connected pores or pore networks closed by secondary chemical reactions. Much of the redistributed brine is lost to unsaturated mine circulation air (Borns and Stormont, 1988, 1989). The brine lost to mine circulation will not be available to interact with the room contents at later times such as after closure.*

The model prediction for far-field brine inflow ranges from 25 to 110 m^3 (Beraun, 1988; Nowak, 1988; McTigue, 1988; Webb and Larson, SAND94-0932; Christian-Frear and Webb, SAND94-3173; Freeze et al., SAND93-1986). BSEP has presented numerous modeling efforts considering both far-field, near-field, and clay consolidation models in numerous reports: Section 5, BSEP 1988 (Deal et al., 1989); Section 4, BSEP 1990 (Deal et al., 1991b); Section 4, BSEP 1991 Report (Deal et al., 1993) and the BSEP 1992-1993 Report (to be published in April, 1995).

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

Response. (a) The cited reference will be provided.
(b) and (c) Complete characterization of fracture processes is not practical and may not be necessary for performance assessment. The stochastic assumptions for performance assessment will capture the range of uncertainty presently associated with the potential fracture mechanisms. The issues related to fractures will be further evaluated once the results of the SPM-2 analyses are available.

Question/Comment 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.

Response. The cited references will be provided.

Question/Comment 25. Please also provide the data generated by Larry Brush concerning rates of gas generation by corrosion (Appx. B, p. 8, ?F).

Response. These data will be provided.

Question/Comment 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 Brook-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 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?

Response. Laboratory experiments to measure relative permeability, and capillary and threshold pressure of Salado anhydrite interbed material are planned as part of the Salado Two-Phase Flow Laboratory Program. The tests are described in Howarth (1994).

Webb has evaluated the Brooks and Corey and the van Genuchten/Parker models with non-WIPP data (Review of Two-Phase Characteristic Curves for Porous Media and Application to the WIPP, Sand93-3912, in revision prior to DOE review and publication). The Brooks and Corey model fits the data better than the van Genuchten/Parker approach. Therefore, the Brooks and Corey model seems to be the better model and is chosen more often in the sampling scheme.

Rather than specifying arbitrary, extreme and unrealistic sets of curves, both the Brooks and Corey and the van Genuchten/Parker approaches, which relate the relative permeabilities to the capillary pressure curve, have been used with parameter ranges from extensive data-model comparisons. This strategy provides a solid foundation for the technical baseline, using a wide range of realistic two-phase characteristic curves based on existing analog data.

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

Response. Yes, capillary pressure data are being obtained as part of the Salado Two-Phase Flow Laboratory Program. Twelve capillary pressure tests were performed on Marker Bed 139 samples in 1994 and the data are reported in Beauheim et al., 1994.

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

Response. The materials will be provided.

Question/Comment 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, simulation 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?

Response. Complete characterization of fracture processes is not practical or necessary for performance assessment. The stochastic assumptions for performance assessment capture the range of uncertainty presently associated with the potential fracture mechanisms. The issues related to fractures will be further evaluated once the results of the SPM-2 analyses are available.

Question/Comment 30. (a) 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? (b) 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? (c) Please make available the paper by Fewell referred to on page 1 of Appendix D.

Response. (a) The porosity model and the aperture model are unverified. As noted in the response to Question/Comment 29, complete characterization of fracture mechanisms is neither practical nor necessary. However, development of the aperture model has impacted the method whereby porosity and permeability parameters are chosen for the PA model, effectively increasing the range of uncertainty.
(b) J is now allowed to exceed 3 and a maximum permeability is now sampled.
(c) There was an error with respect to this paper. The correct reference is Key (SAND94-0381, in management review) which describes the fracture model.