From: Hakan Basagaoglu
Sent: Monday, December 03, 2007 8:21 PM
To: Philip Justus
Cc: Chandrika Manepally; Jude Mcmurry; Philippe Dubreuilh; John Sulima; John Bradbury; Jack Guttmann; James Rubenstone; Randall Fedors
Subject: Comments on SDS3-01AIN-2 Phils Comments Draft Nov19-07
Attachments: Comments on Phil's write-up.doc

Phil, (and et al.)

In the attached file, I have interspersed my my comments and thoughts in your write-up. When you get a chance, could you(and the others) please look at it and let me know if you have any concerns with my comments. Based on this very early draft, the main issues that deserve further justification/investigation:

- the effects of (sub)horizontal fractures in DOE's numerical models (possibly by replacing no-flow lateral boundaries with free-drainage boundaries (given that they consider isotropic permeabilities in their model). The no-flow lateral boundaries promote the vertical flow and confine the infiltrated water inside the flow domain until it reaches the lower free drainage boundary. In this case, it is implicitly assumed that 90% of the (experimentally uncaptured) infiltrated water diverted at the Niche and left the flow domain around the Niche. However, it might be possible that 90% of the "uncaptured" infiltrated water never reached the Niche, might have been diverted away by subhorizontal fractures and crossed the flow domain boundaries. I think this issue is important also for seepage calculations)

- the rational for the enhance matrix diffusion coefficient and the active fracture parameter and the connection/relation between these two parameters.
The active fracture parameter was proposed to account for the matrix-fracture interfacial area. Hence, the enhanced fracture matrix interfacial area should be captured by adjusting the AFM parameter. Because the AFM parameter is already a fitting parameter, a more accurate (fractured-informed) prediction of the 'total available' fracture matrix interfacial area may require only tweaking of the AFM parameter to match the tracer data without resorting another fitting parameter (namely fracture-matrix interfacial enhancement factor).

This and other issues have been discussed in the attached file.

Any comments, feedback, and suggestions will be greatly appreciated. Thank you.

Hakan Basagaoglu

Hearing Identifier: HLW_YuccaMountain_Hold_EX
Email Number: 518

Mail Envelope Properties (000001c83613$fdfaf4e0$e6c8a281)

Subject: Comments on SDS3-01AIN-2 Phils Comments Draft Nov19-07
Sent Date: 12/3/2007 8:21:27 PM
Received Date: 12/3/2007 8:21:28 PM
From: Hakan Basagaoglu

Created By: hbasagaoglu@cnwra.swri.edu

Recipients:
"Chandrika Manepally" <cmanepally@cnwra.swri.edu>
Tracking Status: None
"Jude Mcmurry" <jmcmurry@cnwra.swri.edu>
Tracking Status: None
"Philippe Dubreuilh" <pdubreuilh@cnwra.swri.edu>
Tracking Status: None
"John Sulima" <John.Sulima@nrc.gov>
Tracking Status: None
"John Bradbury" <John.Bradbury@nrc.gov>
Tracking Status: None
"Jack Guttmann" <Jack.Guttmann@nrc.gov>
Tracking Status: None
"James Rubenstone" <James.Rubenstone@nrc.gov>
Tracking Status: None
"Randall Fedors" <Randall.Fedors@nrc.gov>
Tracking Status: None
"Philip Justus" <Philip.Justus@nrc.gov>
Tracking Status: None

Post Office: cnwra.swri.edu

Files	**Size**	**Date & Time**
MESSAGE	2016	12/3/2007 8:21:28 PM
Comments on Phil's write-up.doc	166976	

Options
Priority: Standard
Return Notification: No
Reply Requested: No
Sensitivity: Normal
Expiration Date:
Recipients Received:

<u>U.S. NUCLEAR REGULATORY COMMISSION, OFFICE OF NUCLEAR MATERIAL SAFETY AND SAFEGUARDS, REVIEW OF THE U.S. DEPARTMENT OF ENERGY'S AGGREEMENT RESPONSES RELATED TO THE POTENTIAL GEOLOGICAL REPOSITORY AT YUCCA MOUNTAIN, NEVADA: ADDITIONAL INFORMATION NEEDS FOR KEY TECHNICAL ISSUE AGGREEMENTS TO STRUCTURAL DEFORMARTION AND SEISMICITY (SDS) 3.01 AND ADDITIONAL INFORMATION NEED (AIN)-2</u>

<u>1.0 INTRODUCTION</u>

The U.S. Nuclear Regulatory Commission (NRC) issue-resolution goal during the prelicensing period is to identify and resolve, if possible, technical issues prior to receipt of a License Application. NRC staff resolution of an issue during prelicensing does not preclude the raising of related issues during the licensing proceedings. Also, an equally important, NRC resolution of an issue during prelicensing does not prejudge NRC staff's evaluation of the issue during the licensing review. NRC staff considers an issue resolved during prelicencing when it has no further questions not comments about how U.S. Department of Energy (DOE) is addressing an issue. Pertinent new information could raise new questions or comments about a previously resolved issue.

The review addresses information DOE supplied October 31, 2007, letter (Gil, 2007) to the Director, Division of High-Level Waste Repository Safety, NRC. The letter responded to a staff structural deformation and seismicity (SDS) 3.01 and additional information need (AIN)-2. The letter presented information on the analysis of the Alcove 8/Niche 3 test data using performance based and risk-informed approach, discussion of supporting information on Alcove 8/Niche 3 Fault Test (with a particular emphasis on temporal variability in infiltration and seepage, and the justification for the enhanced matrix diffusion), Alcove 8/Niche 3 Large Plot Test (with particular emphasis on temporal variability in seepage rates and lack of significant tracer transport at Niche 3), and seepage observed in the South Ramp of the Exploratory Studies Facility (ESF). The letter also provided brief summary on the relationship between geology and UZ Flow and seepage Modeling; continuum and discrete fracture hydrological models, use of fracture information to parameterize hydrological models. The letter also presented summary responses to the four major issues raised by the NRC.

This report contains a staff review of DOE's responses to structural deformation and seismicity (SDS) 3.01 and additional information need (AIN)-2, based on information provided by Gil (2007).

Comments DOE'S RESPONSE TO THE FOUR SPECIFIC CONCERNS OF THE NRC STAFF

1. **The analysis of the fault test in the documents referred to in Williams (2006) involved little or no direct consideration of the available fracture data or lithostratigraphic data for the Alcove 8-Niche 3 field tests, nor did the conceptualization of the model in the documents incorporate features similar to the 3-dimensional depiction of fractures between Alcove 8 and Niche 3 that DOE originally provided in response to SDS 3.01 to indicate that the Alcove 8-Niche 3 tests would be fracture-informed (Brocoum 2001).**

<u>Summary Statements of DOE's response</u>.

“The direct use of a discrete fracture model or explicit representation of fractures in the fractured rock mass adjacent to the fault is not warranted.” (p.18)
Comment #1 {I would suggest removing this statement from our responses. Because, DOE mentioned in their responses that NRC already approved DOE’s continuum -based models used for UZ flow and transport simulations (p.18). In such models, fractures can not be explicitly represented at the test site scale or at the repository scale.

In continuum-based models, physical and chemical parameters (and processes) are averaged over a REV (representative elementary volume) and the collective effects of such parameters and processes are represented by so-called effective (volume-averaged) parameters. So, once/if we have already approved their continuum modeling approach, then we have already accepted the fact that we would not expect “explicit” representation of fractures in their models.

So, I would remove this sentence not to conflict by ourselves.

“…in the quantification of the site-scale UZ flow and transport the effects of fractures are less direct (than, for example, in the case of drift degradation processes where fracture geometric information plays a more direct and more significant role) and can be incorporated through the use of hydrologic and transport properties.” (p. 18)

Comment #2: {I am highly skeptical about their claim (i.e., the effects of fractures are less direct) in this statement. But, I believe we can use their claim in Comment # 4 below to lay out our disagreement more strongly.

“To capture the effects of fractures on site-scale UZ flow and drift seepage, the fractures are directly represented in the continuum models through the use of measured properties, such as permeability, whose values are affected by the presence and nature of the fractures.” (p. 18)

Comment #3: {Going back to Comment #1: This comment is consistent with the general description of their continuum-based modeling approach. Because their continuum-based modeling approach was already approved by NRC, I suggest removing this statement}

“Hydrologic properties that are affected by the fractures are included in the models by using appropriate fracture hydrologic properties.” (p. 18)

Comment #4: {same as above, this statement consistent with their continuum-modeling approach. So, I suggest removing it}

“For site-scale flow and transport models there is no need to include other information about the fractures.” (p. 18)

Comment #5 {I like to keep this statement in our responses.

I disagree with their statement because of two reasons. They speculated that 90% of the infiltrated water likely diverted at/near Niche 3 due to capillary barrier effects. However, they have no solid evidence for whether 90% of the infiltrated water actually ever reached the Niche 3 (they are currently assuming that most of the 90% of the infiltrated water diverted around the Niche 3). It is possible that 90% of the infiltrated water might have taken away from the fault well above the niche through tilted (subhorizontal) percolation paths that intersect the fault and extend outside the test site, and hence 90% of the infiltrating water might have never reached the Niche (see also comments #23 and 24).

Although the continuum-based model does not explicitly incorporate fracture geometries, the effect of subhorizontal fractures should have been tested with the numerical models (see also comments #20). In DOE's numerical simulations, all infiltrated water was forced to remain in the test site and flow through the fault zone by specifying lateral boundaries of the flow domain as no-flow boundaries without any physical justification.

The effect of subhorizontal fractures intersecting the fault (that may have carried a significant portion of the infiltrated water away from the fault zone) can be simulated by replacing the no flow lateral boundaries with the free-drainage boundary condition (partially or entirely), given that the effects of horizontal fractures on UZ flow was already incorporated using isotropic permeabilities (i.e., non-zero horizontal permeabilities) in DOE's models. The effects of the lateral boundary conditions could be important to build more confidence on the capillary barrier effect of the Niche 3 and seepage calculations under less deep percolated water volumes.

"For analyses of the Alcove 8/Niche 3 seepage and transport test data, the dual continuum approach is borne out because there is no correlation between seepage and fractures in Niche3 and only a weak correlation between infiltration and fractures in Alcove 8." (p. 18)

Comment #6 {This is an experimental observation. They analyzed the results using an approved continuum-based modeling approach. I suggest removing this}

"The analyses that support the TSPA are directly fracture-informed by incorporating the effect of fractures through measurements of gas-phase permeability, gas-phase- tracer movements, fracture frequency, and fracture trace length, as well as matrix water saturation and water potential." (p. 18)

Comment #7 {These are experimental measurements, and they will likely –implicitly- reflect the fracture properties. In that sense, they are probably implicitly fracture-informed. Therefore, I suggest removing this statement}

"For simulating the site-scale UZ flow, the model also explicitly incorporates selected faults in the numeric mesh used, and assigns the faults properties separate from the fractured rock mass through the use of site-scale pneumatic measurements and results from borehole UZ 7a." (p. 18-19)

Comment #8 {I think this seem to be a valid approach in continuum-based modeling approach}

"For seepage analyses, the fracture properties as they affect seepage are incorporated into the seepage models through measurements of properties that control seepage." (p. 19)

{Comment #9 {again, from the continuum modeling perspective, I think this is reasonable}

[[list these ??]]

Comment #10 {I believe their statement under Comment #5 is the most open-guarded statement -we should use that to express our concern. They may easily refute the rest by simply saying that "NRC already approved our continuum-based modeling approach"

"Both UZ flow and seepage models have been calibrated and validated against test measurements at Yucca Mountain." (p.19)
Comment #11 {I do not agree with their statement. I do not think that the justification for the enhanced matrix diffusion coefficient was clearly provided in this work. Please see Comment #33}

"Also, the effects of fractures and faults on site-scale unsaturated flow and seepage are directly incorporated through measurement of properties that affect flow, with such properties themselves being dependent on the nature of the fractures and faults." (p. 19)

Comment # 12 {Again, the use of 'effective properties' is central to the continuum-based approach, and this statement seems to be consistent with their approach.}

[[list properties]]

NRC's Reply.

1 – The concern of the staff is that the results of the fault test are unable to be widely used because the initial site conditions of the test volume are poorly described. DOE used hydrologic data derived from lab and borehole tests on lithophysal rocks (few from tsw33), averaged, and applied them to the relatively un-sampled lithophysal unit (tsw33). It used hydrologic data averaged from non-lithophysal unit tests (actually, many from non-faulted tsw34), but applied them to tsw34 where it is juxtaposed with a fault. The fundamental attributes of the fractures, fault, matrix rock and lithophysae in tsw33, the fractures, fault, matrix rock in tsw 34, and the stratigraphic contact between tsw 33 and tsw 34, through which the test-waters were 'forced' were not considered by DOE in its analyses of the test-water.
Comment #13 {In their response letter, they mentioned that the hydrogeological properties of tsw33 and tsw34 are not significantly different (they assumed a higher porosity for the lithophyscal unit to account for the enhanced storage effects of the lithophysal unit); therefore, they seem to believe that the stratigraphic contact between these two units is not highly crucial for the flow. Based on their explanation, if there is a permeability contrast or abrupt changes in the capillary barrier effects across the lithostratigraphic contact, the lithostratigraphic contact would be important in simulating flow. Is there any field or experimental evidence suggesting drastic changes in the capillary barrier effects and permeabilities across the lithostratigraphic contact between tsw33 and tsw34?

{Another note, I heard the use of analogue sites/samples for a number of analyses relevant to the YM project. So, if this is not an uncommon practice, then why shall we question the use of other lithophysal rocks in their analyses (and including a few samples from lithophysal unit), for estimating "average" or ''effective" properties, which is more relevant to the continuum-based modeling approach?

Such local attributes are factors that could explain why 90% of the test-water was not accounted for.
Comment #14 {This is what I am after too. Please see Comment #5}

In retrospect, “skin effects” of the construction methods used to excavate the alcove and niche and prepare the floor of the alcove and roof and sides of the niche to conduct the test (to initiate infiltration and to observe and collect seepage from fault and fractures) were not considered until after the experiments began.
Comment #14 { I agree. But, they already acknowledged this issue in the second and third phases of the large plot test and fault test when they attributed the temporal variability of the infiltration and seepage rates to infill materials}.

On relevant test-volume attributes: ( ) the fault was not characterized sufficiently to know whether water that infiltrated it took paths in open-aperture spaces, in gouge or brecciated rock fragments in a fault zone, or along fault planes; the fault surface, at least in Alcove 8, was slickensided and patchily coated with sepiolite-like clay, which is influences flow;

Comment # 15: In continuum-based model, they can only include the (effective) advective average in the fault zone, which is an average value defined over the entire fault zone. Because the representative elementary volume should be chosen coarse enough to validate the continuum assumption, even if they are capable of measuring flow velocities at different regions in the fault zone, they may not incorporate them into their continuum-based numerical simulation model (See comment #17).

( ) the contact between the units was assumed to be a conductive zone, but without knowledge of whether it is like a bedding plane with aperture, or a gradational contact with permeability contrast with the matrix above and below;
Comment # 16: This is a good point. We should combine it Comment #25. But see also Comment #13.

( ) the local fracture density is likely to vary with distance from the fault, but was assumed to take the average fracture density value from an aggregate of widely spaced sources;
Comment # 17: I believe this is reasonable in the continuum-based modeling approach. The fault is relatively small. They need to either divide the fault zone into a number of subzones or used effective parameters for the entire fault zone, different from the fractured surrounding rock. If they divide the fault zone into a number of subzones, they may represent spatial variability in fracture densities better, but that will introduce more rock properties (higher uncertainty) to predict/fit using the infiltration and seepage data. Therefore, their approach seems to be acceptable to me.

( ) the subhorizontal fracture sets present in the test volume have physical attributes that affect local flow; such potential local flow paths are not captured in the hydrologic effects data that was used as a surrogate for the characteristics of flowing fractures;
Comment # 18: I think this is a good point. Please See Comment #20, 23, and 24.

( ) the porosity of lithophysal rocks is variable; some lithophysal-rich rocks have all the lithophysae completely filled with minerals, some with little mineralization. It would not be appropriate to assume that hydrologic data used to represent lithophysal rock tsw33 that were derived by averaging data from a variety lithophysal rocks with variable porosities actually represented tsw33, unless the lithophysal characteristics of tsw33 were described as about average. This was not done for tsw33 in the Alcove 8/Niche 3 test volume.

Comment # 19: I think the use of averaged (effective) parameters is central to the continuum-based modeling approach. I agree with the concern that the porosity is a spatially variable property, but such spatial variability over a representative elementary volume(REV) can not be captured in continuum-based modeling approach. Moreover, the definition of the representative elementary volume is not clearly defined in highly fractured systems. But the underlying assumption of the continuum-based modeling approach simply states that the fractures are ubiquitous in the flow domain (as in porous domains) such that their average behavior can be defined over a particular volume. It is never clear, and will never be clear, what is this particular/correct REV in continuum-based modeling approach. But this assumption needs to be invoked to be able to implement continuum-based numerical simulations. In this case, we have to compromise from spatial variability of some properties, but need to rely on average/effective parameters (and processes). Hence, their approach seems reasonable from the numerical/continuum-based simulations standpoint.

This is why the staff does not consider the Alcove 8/Niche 3 tests to be "fracture-informed," Or, for that matter, also not "lithostratigraphically-informed."

****

2. The interpretation of the fault test results did not sufficiently consider structural features such as the intersection of the subvertical fault with subhorizontal fractures throughout the tuff, and the potential role that these subhorizontal fractures had in diverting water from the fault and bypassing Niche 3.

Summary statements of DOE's response.

"In the Alcove 8/Niche 3 tests, because of the large water input, all fractures that could flow, including horizontal fractures, did flow. However, that does not mean that horizontal fractures need to be explicitly represented in a discrete fracture model, nor does it mean that horizontal fractures need to be explicitly treated as discrete fractures within a dual continuum model." (p. 19)

Comment # 20. I agree with their disagreement on "explicit" or "direct"representation of horizontal fractures in their models (not possible in continuum-based models). However, the 'effects' of horizontal fractures should have been captured by defining horizontal and vertical permeabilities **and** flexing the lateral boundary conditions (changing from no-flow to free-flow boundary condition). The latter would not necessarily force to flow vertically along the fault zone, but would allow losses of some fractions of the infiltrated water through horizontal fractures. I like to refer to their statement above in our responses.

"The effect of the horizontal fractures on UZ flow is incorporated through the use of isotropic permeabilities, and test results match sufficiently well with the dual continuum models used." (p. 19)

Comment # 20. I partially agree with their statement. The test results matched the data based on the assumption that the entire infiltrated water remains in the flow domain and can not escape across the lateral boundaries (in other words, they assume that the 90% of the infiltrated water diverted only at Niche 3. Is this true? I do not know. Do they know for sure?

No, they do not know it either). No flow lateral boundaries probably promote the vertical flow more. But, the use of free drainage/flow boundaries at lateral boundaries would promote the effects of horizontal fractures. In this case, DOE may still fit the data (with probably different sets of rock properties data), but they can capture the 'effects' of horizontal fractures better without representing them 'explicitly' in their model. I like to refer to their statement above in our responses.

"Analyses of the observations there (South Ramp during the winter of 2004-2005) indicate that UZ flow was controlled by faults and lithostratigraphic contacts instead of horizontal fractures. Additionally, similar observations were made during the excavation of the ESF…" (p. 19)

Comment # 21. This sounds reasonable. Because, when the infiltration rate is high, the gravitational force of the infiltrating water will likely be much stronger than the capillary forces exerted by fractures, and flow will occur through the more permeable zones (across the fault) without substantial horizontal diversions.

Hence, it is not surprising to see that most of the seepage occurred along the fault zone above the Niche under the high infiltration case. However, if the infiltration rate was low, the relative strength of the gravitational force of the infiltrating water would be smaller as compared to capillary forces. Hence, the infiltrating water will likely divert more horizontally. Therefore, it may not be quite right to rule out the effects of horizontal fractures by looking at a special case involving high infiltration rates. I like to refer to their statement above in our responses.

"…adjacent to such preferential flow paths as faults or lithostratigraphic contacts, fractures of all orientations become wetter because of more water being present in the preferential flow paths." (p. 19)

Comment # 22. There will be higher saturations in fractures in response to higher infiltration rates, as I should be expected. At high infiltration rates, more seepage should be expected from high permeability zones, as the strength of the high gravitational force of the infiltrating water would likely be much higher than the strength of the capillary forces exerted by initially dry or partially-dry fractures. I think this is a trivial argument, and does not contribute much to the discussion.

"Although it is possible to speculate that horizontal fractures were responsible for the bulk of the infiltrating water in Alcove 8 that did not become seepage in Niche 3, there is no evidence that this process occurred." (p. 19-20)

Comment # 23. I disagree with their statement. One can argue that probably the bulk portion of the infiltrating water never reached to Niche. The bulk portion might have been removed from the flow domain through subhorizontal fractures way above the Niche. Without knowing, the effects of the horizontal fractures, such effects should not be ruled out. The model should be flexible enough to address potential effects of subhorizontal fractures on the seepage rates and patterns. I like to refer to their statement above in our responses.

"In other Project tests, a large fraction of the infiltrating water does not seep simply because of wetting of the rock mass and capillary effects." (p. 20)

Comment # 24. I disagree with their comments (see also Comment 23). It might be possible that a large fraction of the infiltrating water has never reached the Niche, and the remaining volume that reached the Niche was not sufficient to breach the capillary barrier at the Niche ceiling. It is possible that the large fraction of infiltrating water might have been crossed the lateral boundaries along subhorizontal percolation paths above the Niche. Without any solid evidence, there seems to be a great uncertainty in DOE's statement above.

[[is this relevant ??]]

"…the validated UZ flow and seepage models do reasonably match various test results, without explicit use of discrete horizontal fractures. Also, there is a lack of observations of preferential horizontal flow, although there are observations of preferential flow in faults and at lithostratigraphic contacts. Therefore, in lieu of convincing field evidence, direct incorporation of discrete horizontal fractures into these models in not warranted at this time." (p. 20)

Comment # 25. Please see my response to Comment #20 above.

NRC's response:

DOE's explanation of the unexpectedly low test-water recovery was not comprehensive. It mentioned the possibility, based on observational evidence elsewhere, of lateral diversion within the fault, lateral diversion in subvertical fractures that intersect the fault, and along the lithostratigraphic contact. Observational evidence from the test volume, such as from the many boreholes within the test volume, or from the fault itself and surrounding rocks that are exposed in the ESF would have been convincing. Also, the three-dimensional models of the subvertical and subhorizontal intersecting fracture-sets and the intersecting fault in the test volume might have been useful in estimating lateral escape routes of test-water. This concern was designed to elicit such explanations.

Comment #26: In light of preceding comments, I do agree with Phil's concern above. The effects of horizontal fractures should not be ruled out without any compelling field and experimental evidence.

****

3. **Further, the interpretation of the fault test results did not directly evaluate connection of the fault to subhorizontal fractures, connection of subhorizontal fractures to vertical fractures, connection of lithophysae to both sets of fractures and, consequently, the potential role that these fracture-connected lithophysae had in trapping infiltrated water.**

Summary statements of DOE's response:

"…even though the lithophysae are not represented explicitly as entities in the numerical model mesh, the effects of the lithophysae on flow properties have been represented." (p. 20)

"A casual glance at the more lithophysae-rich portions of rock clearly shows that the lithophysae must have an effect on porosity. For this reason, the lithophysal unit is modeled with larger porosity for the Alcove 8/Niche 3 tests." … "the effects of lithophysae on fracture porosity were incorporated through calibration to the water-arrival-time measurements." (p. 20)

"Any effects on permeability are automatically included through gas-phase permeability testing." (p. 20)

"Lithophysae behave like all large openings in that they present a capillary barrier to water entry. Under accelerated flow conditions in Alcove 8/Niche 3, a portion of the lithophysae did appear to participate in fracture flow. However, this participation is expected to be much less under lower flow rates representative of ambient or repository conditions." "…the effects of fracture porosity on site-scale UZ transport have been shown to be small. Furthermore, lithophysal porosity has no effect on drift seepage under steady flow conditions characteristic of the UZ below the PTn. Therefore, the incorporation of lithophysal porosity into the site-scale UZ flow and drift seepage models is not warranted." (p. 20)

"…discrete, individual lithophysae have not been incorporated into the UZ flow and drift seepage models, because it is not feasible to incorporate the numerous discrete lithophysae into these models." (p. 20)

"The current approach based on the use of dual permeability continuum models is reasonable, as demonstrated by the fact that the validated models for UZ flow and drift seepage do reasonably match test results, without explicit use of discrete, individual lithophysae." (p. 20)

<u>NRC response</u>:

DOE dwelt on the need to consider lithophysal porosity to establish UZ flow at site-scale and for seepage estimates, which is an important attribute to model, but one that did not warrant measurements from the lithophysal rock being modeled. NRC's concern is that the behavior of water in lithophysal zones reflects local site conditions and is not readily teased from models that use data averaged from a variety of lithophysal rocks. The use of dual permeability continuum models may be reasonable in theory, but the staff contends that the interpretation of results from such models to a site-specific test ought to consider, and possibly constrain, the potential, and possibly special, effects of site-specific conditions that deviate from the assumptions and simplifications necessarily incorporated into the model.

Approximately half of the upper portion of the test volume (lower part of tsw33) consists of mostly lithophysal tuff. The lower test unit (upper part of tsw34) consists of mostly non-lithophysal tuff. The lithophysae in lithophysal tuff are variable in size, shape, distribution (actually absent in some subhorizontal zones) and in the amount of in-filling (from completely filled with air, to completely filled with calcite and other minerals). This is also the case for lithophysal layers that occur in the 'non-lithophysal' tsw34 model unit. Furthermore, the fracture network that pervades the lithophysal tuff has different characteristics, such as density, length, orientation (direction by the compass, and proportion that are subvertical vs sub horizontal), mineral in-filling. Therefore, the degree of connectivity of fractures with lithophysae and with other fractures is a site-specific condition that, with variability of lithophysae (enumerated above), influences the local UZ flow system.

When interpreting test results from computer flow models that import parameters that were averaged from various other lithophysal rock units that represent hydrologic properties of, in this particular concern, tsw33 lithophysal tuff, consideration of the local conditions in the test volume may lend opportunity for and credibility to reasonable alternative explanations of test results. This was not done for the Alcove 8/Niche 3 fault test results.

Comment #27: I believe the assumption of an order of higher porosity to represent lithophysal porosity for tsw33 is reasonable in continuum-based models. I would not expect that their model would be able to consider the spatial variability in lithophysal units. Because their continuum-based model operates with averaged/effective parameters, the averaged porosities for tsw33 and tsw34 from samples from these rocks and/or analogue sites/samples seem to be reasonable.

****

4. **Instead of considering unrepresented features and processes such as those mentioned above, DOE increased the modeled effective fracture-matrix interface area in the model to account for those unrepresented features and processes. This approach increased the effectiveness of matrix diffusion, which allowed the model to reproduce the observed fault test results. Application of this approach in predictive modeling at other locations is uncertain.**

Summary statements of DOE's response:

"…although the "unrepresented" features are not represented explicitly in a numeric model mesh, the effects of these features on flow are incorporated." "…to whatever extent these features affect flow, those effects have been measured and accounted for in the flow properties used in the site-scale UZ flow and drift seepage models." (p. 21)

Comment #28: This is consistent with the general theme of the continuum-based models. So, I suggest removing this statement from our responses.

"Even with a representation of fractures through their effects on flow properties, there was a mismatch between measurements and predictions for transport. This result shows that some other aspect of the model must be corrected to explain the transport response…the effective diffusion coefficient was increased. This increase did produce a better correlation between measured and predicted transport results, indicating that the adjustment may be reasonable." (p. 21)

Comment #29: This seems reasonable. But, I wanted to see the connection between the active fracture model parameter and the enhanced matrix diffusion coefficient (for the portability of the AFM parameter and the concept of enhanced matrix diffusion), because both of them are proposed to account for the interfacial area between matrix and fractures at a 'particular' scale. Please see Comment #33.

DOE went on to use the larger diffusion coefficient in interpreting "lab measurements of matrix diffusion used in combination with the estimated wetted fracture-matrix interface area" and concluded that the parameter, 'wetted fracture-matrix interface area,' "may very well be underrepresented in terms of the effective area available for matrix diffusion." (p. 21)

See Comment #29.

“DOE recognizes that there is some uncertainty in increasing matrix diffusion in the site-scale UZ transport model…” “Therefore, this adjustment in the effective matrix diffusion coefficient has not been implemented in the TSPA compliance case, and will only be used in a TSPA sensitivity analysis.”

See Comment #29.

“The validated models for UZ flow and transport are suitable for their intended use, because the results do not under-represent the rate of UZ flow or solute transport in field tests and therefore does not under-represent risk.” (p. 21)

See Comment #29.

“The explicit use of discrete, individual fractures, small faults, and lithophysae is not feasible for the UZ modeling domain and therefore, would not result in an improvement in the modeling capability.” “For this reason, the direct incorporation of discrete, individual fractures, small faults and lithophysae in the UZ flow and transport models is not necessary, and the scaling of the effective matrix diffusion coefficient is a reasonable approach to investigate the sensitivity of these processes to UZ transport.” (p. 21)

Comment #30: This is consistent with the general theme of the continuum-based models. So, I suggest removing this statement from our responses.

NRC’s response:

The staff was concerned that DOE might apply its UZ site-scale model (recalibrated with a higher effective matrix diffusion coefficient) for projecting the test results to new or untested areas or otherwise for predicting flow properties in new or untested areas without any additional technical basis.
Comment #31: I suggest removing this statement from our responses. The next sentence outlines the major concern very well (and I have some additional concerns and thoughts in that)

The staff is concerned that tweaking a model parameter, in this case – increasing effective matrix diffusion coefficient – to render the model’s prediction compatible with field observation, is insufficient, in and of itself, to justify the validity of the new parameter value or its potentially elevated importance. Moreover, the fact of such a calibration does not justify the use of the new parameter as a basis for conducting sensitivity studies. Additional technical basis is needed.

Comment #32: I think this is a very good point.

The staff did not mean to imply that its concern was that DOE models did not “explicitly include representations of observable geologic features” (p. 20) in its UZ flow and drift seepage models. Our concern was, and remains, that specific conditions and features of the test volume that might influence flow and, therefore, might affect model results were not getting due consideration in the interpretation of the test results (given that DOE was not explicitly including such features in its models).

DOE should improve its technical basis for apply----------by considering whether its understanding of conditions and features of the test volume (e.g., the fault, fracture network properties, lithophysal properties, lithostratigraphic contact properties) have a local significance (because it is unique or rare)------

Comment #33: I would suggest removing the last two paragraphs above.

I also would like to stress that the connection between the active model parameter and the enhanced matrix diffusion coefficient is missing. If DOE resorta to more fracture informed analyses, they may not need to use a fudging factor of 45 to match the tracer data. This can be done either or combinations of: (a) a more accurate estimate for the 'total available' fracture matrix interfacial area; (b) tweaking the active fracture model parameter to reflect the large-scale matrix diffusion process, which has been already considered a fitting parameter. Here is the rationale:

The active fracture model parameter, γ, was originally proposed to estimate the reduced interfacial area between fractures and matrix in unsaturated flow domains (Eq. 13 in Liu et al., WRR 1998). The AFP was set to 0.41 for fracture flows in repository horizons in their numerical simulations for the fault test (Liu et al, JCH, 2004; p. 49, Table 1). As we all know that γ is not a measurable quantity, but a fitting parameter.

Based on effective saturation, S_e, in fractures (that would vary with the infiltration rates), γ can be used estimate the interfacial area between fractures and matrix from:

$$\frac{A'_{fm}}{A_{fm}} \cong S_e^{1-\gamma} \qquad (1)$$

In the above equation, A'_{fm} is the reduced fracture-matrix interfacial area, and A_{fm} is the total available fracture matrix area.

Hence, from Eq. (1),

$$A'_{fm} \cong A_{fm} \cdot S_e^{1-\gamma} \qquad (2)$$

The effective matrix-diffusion coefficient can be written as:

$$D_{eff} \cong A'_{fm} \cdot k \qquad (3)$$

where D_{eff} is the effective matrix-diffusion coefficient [m^2/s], A'_{fm} is the reduced fracture-matrix interfacial area [m], and k is the lab-scale mass-transfer rate coefficient [m/s]. Liu et al. (2007, JCH, 90, 41) suggested that the 'scale-dependency' of D_{eff} is possibly due to larger fracture-matrix interfacial areas at larger scales (please recall that this is the same justification for the enhancement factor of 45). This is consistent with Eq. (2) as well, indicating that the mass-transfer rate at a lab scale can be upscaled, if we know the reduced fracture-matrix interfacial area at a particular test scale.

Let's take another step and combine Eqs. (2) and (3), which results in

$$D_{eff} \cong A_{fm} \cdot S_e^{1-\gamma} \cdot k \qquad (4)$$

I have not seen this expression in their AMR's or journal articles. But, this equation suggests that:

D_{eff} can be adjusted either at any scales (can be upscaled, if you will)

(a) by adjusting the active fracture parameter, γ, and/or

(b) by having a better estimate for the total available fracture-matrix interfacial area, A_{fm} at the test scale

In this case, there is no need to seek/use a magic fudging factor of '45' to match the tracer breakthrough curves. γ is already treated as a fitting parameter. If they know the average (presumably) steady effective saturations (for a given infiltration rates), they may calibrate D_{eff} by adjusting γ only (in Eq. 4) for a given (assumed) A_{fm}.

Eq. (4) simply indicates that a better estimate for A_{fm} (as much as possible) should be used for more reliable estimates for D_{eff} (without resorting to a fudging/enhancement factor of 45). I believe a better estimate for A_{fm} may be obtained by using all fracture information, as Phil has highlighted in the agreement letters .

If they can use all available fracture information for better estimate of A_{fm}, then they only need to calibrate only their γ to match the breakthrough curves. This is OK, because γ is already a fitting parameter in their numerical models.

****

DOE's OVERALL CONCLUSIONS AND NRC'S FEEDBACK [all on p. 22]

1 - "The information of the fracture network as observed at Alcove 8 and Niche 3 was incorporated into the Alcove 8/Niche 3 test bed model using a dual continuum approach. The continuum modeling approach used for analyses of e Alcove 8/Nich 3 test data, the site-scale UZ flow model, and the seepage abstraction model is justified based on the fracture densities and the required length scale of resolution of each of these models."

NRC's feedback:

[hydrol response…]

Comment #34: As I highlighted above, I do not have any problems with the volume-averaged parameters and processes in their continuum-based numerical models. I agree with their comments that they can not incorporate discrete fractures and lithophysal properties explicitly in their models; however, their model should be flexible to be able to incorporate the "effects" of horizontal fractures by considering both vertical and permeability fields and (partially or entirely) free-drainage/flow boundaries along the lateral boundaries. I believe negligence the effects of of horizontal fractures in flow simulations has not been justified. This concern has been highlighted in many comments above.

2 - "…a discrete fracture modeling approach is not needed for the flow and transport analyses of the Alcove 8/Niche 3 test data."

NRC's feedback:

The modeling of fractures by use of a discrete fracture modeling approach is not a requirement for analyses of the Alcove 8/Niche 3 test data. However, an understanding of the alternative conceptual models of the fracture-matrix-lithostratigraphic interrelationships is needed to quantitatively interpret the results of the analyses, where quantification is necessary to understand and use the results. For example…. [hydrol responses] etc

Comment #35: See Comment 34.

3 - "…fractures and faults are captured in the Alcove 8/Niche 3 test bed model, as in the site-scale UZ flow model and the seepage abstraction model, through capturing their effects on flow and transport properties."

NRC's feedback:

We do not disagree that the fractures and faults are captured in the test bed model as in the site-scale UZ flow and transport model and the seepage abstraction model. However, the assumption that the average or range of effects on flow and transport of the fractures and fault in the test bed model volume is similar to or otherwise represents the average or range of such effects elsewhere or everywhere just above and below the contact of the UZ flow model layers tsw33 and tsw34 was provided no basis… etc

Comment #36: I think they did. But still there are holes in their modeling efforts. The enhancement factor of 45 and overlooking at the effects of horizontal fractures are the main shortcomings, as discussed in Comment 33.

4 - "The test bed models are all fracture informed in that fracture information (e.g. fracture frequency and air permeability) required to characterize these hydrologic models at the relevant scales has been used.

NRC's feedback:

No basis was provided to support the assumption that the fracture information used was representative of the fracture frequency and other attributes of the upper lithophysal unit near the contact with the middle non-lithophysal unit and the different values of fracture frequency and other attributes of the middle non-lithophysal unit just below the contact with the upper lithophysal unit. There are few such measurements from the lower part of the upper lithophysal unit. For both units, it is not clear how fracture attributes such as fracture frequency varies with distance from a fault such as the one encountered in the test volume. It was also assumed, without discussion, that the effects on flow and transport properties of fractures in the tsw33 and the tsw 34 in the zone of influence of a "small" fault and similar or insignificant from the effects beyond the influence such features… etc

Comment #37: Considering the use of effective parameters and processes, I think their approach sounds reasonable.

5 – "Additional fracture information (e.g., a three-dimensional fracture network description) is either not necessary or is not applicable to continuum hydrologic models."

NRC's feedback:

In this case, where the results were either not predicted or were not able to be completely explained, it is not clear what information that was not considered is significant to explain the results, or is able to offer alternative conceptual models that are plausible… etc
Comment #38: I agree with the DOE's statement. By definition and underlying assumptions, a three-dimensional fracture network description is not applicable in continuum-based modeling.

6 - "Information characterizing the fault and fractures in the Alcove 8/Niche 3 fault test were used in the test bed model, and the presence of subhorizontal fracture connections with the fault was included through permeability properties."

NRC's feedback:

It is not clear that all available information relevant to understanding the environment of the zone that influenced infiltration (tsw33) in the test bed model in Alcove 8, all available information to understanding the environment of the zone of seepage (tsw34; the fractures that seeped), and information from the zone of transfer in proximity to the lithostratigraphic contact that might have been derived from boreholes was used. For example, observations of seepage in Niche 3 indicated that subvertical and subhorizontal fractures intersecting the roof and walls flowed, but at different places, times and rates. Such information was not specifically incorporated into discussions of rock response to the test conditions [if this feedback is retained, verification of my recollection will be needed…by me]… etc

Comment #39: Yes, I have serious concerns on negligence of the effects of subhorizontal fractures in their numerical models, as elaborated in many comments above.

7- "The presence of lithophysae was included through the model calibrations of fracture porosity."

NRC's feedback:

That may be. However, the nature of the lithophysae (size, shape, degree of infilling, distribution of the sizes/shapes/types, and their relationship to intersecting fractures) in the lower part of the upper lithophysal unit, and, in the upper part of the middle non-lithophysal unit, if any, within the test volume, was not recorded. Such site-specific information is needed to support the assumption that the effects of lithophysae on fracture porosity derived from elsewhere are appropriate for the test volume, or otherwise, accounted for… etc

Comment #40: I do agree with the DOE's statement above..

8 – "The analyses of data from the Alcove 8/Niche 3 fault test and the development of the test bed model are performance-based and risk-informed, in keeping with the requirements of the regulation (10 CFR 63)."

NRC's feedback:
[hydrol…etc]

9- "Data from these tests provide additional field evidence for confirming the conceptual model of matrix diffusion, which serves to help validate the site-scale UZ transport models."

NRC's feedback:

[hydrol… etc]

Comment #41: See comment #33.

10 - "Considering uncertainties in the Large Plot tracer transport test, however, the test data, including the enhancement factor for matrix diffusion, are not planned to be incorporated into the TSPA compliance case."

NRC's feedback:

Not a conclusion….[hydrol… etc]

Comment #42: But the AFM parameter is used in TSPA. By definition, this parameter describes the matrix-fracture interfacial area (Eq. 13 in Liu et al.). Hence, the enhanced matrix fracture interfacial area should have been captured with this parameter. This should strengthen for the portability of AFM parameter and for more efficient use of Alcove 8/Niche 3 test results for TSPA. See comment #33.

11 - "Instead, the enhancement factor to the fracture-matrix interface is planned to be used to test the sensitivity of these processes to UZ transport."

NRC's feedback:

Not a conclusion… [combine with #11 and ignore?]

12 - "This use of the Alcove 8/Niche 3 fault test data is reasonable based on the consistency between flow observations and the test bed flow model, the observed tracer transport behavior, and similar observations of enhanced matrix diffusion at other sites."

NRC's feedback:

[this is a hydrol bucket…]

Comment #43: See comments # 42 and 33.