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

FEB 2 3 1995

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

Dear Mr. Lovejoy:

Enclosed is our response to questions raised regarding the Disposal Room and no (cp) Cuttings Position Paper. I appreciate your interest and continued participation in the Systems Prioritization Method.

If you have any questions regarding this response, please contact Robert A. Bills at (505) 234-7481.

Sincerely,

Michael H. McFadden
Assistant Manager
Office of Regulatory Compliance

Enclosure

cc w/enclosure:
M. McFadden, CAO
P. Brewer, SNL
L. Shephard, SNL
W. Weart, SNL
N. Prindle, SNL
P. Swift, SNL
C & C File

Disposal Room and Cuttings Model
Questions and Responses

1. *There is a discussion of the relation between porosity and permeability as affects 40 CFR 191 undisturbed performance, direct release upon human intrusion, and flow between boreholes in an E1E2 scenario. The draft should explain these sensitivities more fully with quantification. (at 2).*

Response: As you note in your comment, Section 1.1 of the draft white paper contains a qualitative discussion of the impact of porosity and permeability on compliance with 40 CFR 191. This level of detail (qualitative) seems appropriate for Section 1.1, which is in the Introduction. Further technical details on the waste compaction models and the backfill consolidation model are found in Sections 3.2.2 and 3.2.3, respectively. Section 3.3 discusses the waste and backfill flow model. Section 3.5.2 discusses the relationship between porosity and permeability in the waste flow model in more detail.

The SPM-2 calculations constitute a sensitivity calculation by themselves but not one in which it is possible to break out the importance of individual parameters. If you desire these sensitivities, the 1992 PA would be a reasonable, if no longer entirely accurate, starting point.

2. *The draft explains that new gas storage volume may be created if the lateral in situ stress in the interbed is less than the minimum in situ stress (at 6). The draft states that evidence of such a stress has not been acquired. May the postulated situation affect gas migration? Are experiments planned to investigate this concern?*

Response: Undisturbed argillaceous salt beds usually have a hydrostatic (uniform) stress state. Even in the presence of rooms and panels, the mechanism of creep tends to return the mass of in situ salt to a uniform stress state. Near a room (where stress is usually the least uniform), rising gas pressure may open existing or new fractures in the anhydrite interbeds, resulting in additional gas volume and potential migration paths. A more detailed fracture model is under development and will be used to evaluate the response of fractures and their potential effect on compliance.

3. *The draft says that porosity is modeled as constant after an intrusion event (at 6-7). Is there no situation in which porosity may increase after an intrusion? If so, would flow within the repository be enhanced? Is the constant porosity assumption the same one as is referred to in vol. 4 of the 1992 PA at 4-15 and 4-16, in which porosity is held constant until pressure rises above its previous maximum? The 1992 PA points out its approach may underestimate closure and overestimate porosity, (at 4-16), the implications of these assumptions must be quantified.*

Response: Porosity could increase after an intrusion, if the intrusion happened before substantial gas generation occurred. The resulting permeability increase would be of minor significance, however, because the very conservative value of permeability assigned to the waste is already so high that for all practical purposes the waste offers no resistance to flow, as described in Section 3.3.2.1 of the position paper. The constant porosity assumption is the same referred to in Volume 4 of the 1992 PA. Changing porosity following an intrusion has been shown to be small (Appendix J of Volume II of the position paper), and therefore should have no discernible effect on performance assessment results.

4. *The draft says that the assumption of quarter-room symmetry leads to errors whose magnitude is "always suspect" and recommends the half-room model (at 20-21). Will the half-room model be used in future analyses?*

Response: As stated in Section 3.1.6, the half-room model will be used in all future analyses.

5. *There must be substantial concern about the assumption that waste is a homogeneous mixture throughout the repository (at 21). Such an assumption is unrealistic to begin with. What commitment has DOE made to achieve homogeneity in fact? Is it not possible to identify conservative values or to sample among values of different probabilities, with respect to parameters affecting the closure model? This assumption is a major weak link in the draft.*

Response: In response to your and other's concerns regarding this matter, an additional parameter that specifies the active brine flow fraction has been added to the SPM-2 analyses. This parameter will be used to allow gas flow along preferential paths in the waste. Additionally, concentrations of particular waste would represent barriers to flow in the form of low permeability regions.

As an aside, it is not clear that inhomogeneities among waste drums will have a significant impact on overall repository performance. In other words, variations in content and release rate among the individual drums are expected to be averaged by the large geologic mass and long transit paths that separate the drums from the accessible environment.

6. *Backfill is assumed to be crushed salt (at 23) - What is the effect of assuming a salt-bentonite 70-30 mixture? Would it not be more realistic to do so?*

Response: Several backfill materials are being evaluated for the WIPP repository, including pure crushed salt, a 70/30 salt/bentonite mixture and a engineered clay material. Until a specific backfill is selected, one material is as realistic as any other. For the SPM-2 calculations, the baseline case assumes no backfill so that the benefits of various backfill materials can be evaluated.

7. *Backfill between the drums is neglected in consolidation estimates (at 23). Why is it not possible to model this backfill?*

Response: The backfill between drums is not modeled because it has a negligible impact on performance. The backfill between drums represents a small part (<18%) of the total backfill volume and is expected to provide little resistance to consolidation of the waste. The backfill between the drums also constitutes a level of detail in closure calculations that can only be addressed through complex, three-dimensional analyses. These analyses will be very expensive and time-consuming without any appreciable benefit to performance analyses.

8. *The draft explains the development of the M-D constitutive law for creep and says that it will be used in "most" future calculations. (at 25). In what circumstances will the Reference Creep Law or another constitutive law be use?*

Response: It is our desire and intention to use the M-D model whenever possible. In a few cases, it may be necessary, because of computing limitations, to use a simpler model. Unfortunately, even Cray computers have their limitations. However, we believe that this will not be necessary and that the M-D model can be used for all future calculations. The statement was inserted more in the interests of absolute openness with the stakeholders rather than what we believe will be the likely outcome.

9. *The waste compaction model calls for an assumption about lateral stresses, factors which strongly affect waste compaction (at 26). The draft justifies an assumption about the lateral stress in relation to vertical stress, based on "indirect guidance" from experiments in which lateral stress was not even measured (at 30). This does not seem justified.*

Response: As noted in the text, drums with metallic and combustible waste compacted in a one-dimensional mode, without a significant change in diameter, during mechanical testing (Butcher et al., 1991b, p. 52). This response is consistent with a uniaxial stress field (no lateral stress), which is the preferred conceptual model for SPM-2. This assumption is well-justified based on the test data.

In addition, past PA analyses have consistently shown that compliance is insensitive to the exact details of the waste compaction model. The Carlsbad Area Office believes that resources should be used to reduce other performance uncertainties.

10. *The discussion of the hypothesized sequence of waste degradation and compaction is at least unclear (at 30-31). It appears that the current model assumes that waste degrades before compaction, an assumption which is defended on the assumption of high waste permeability and high gas storage volume outside the repository. How much is "high" gas storage volume? Please demonstrate that it always obtains.*

Response: The current model does not address the effects of degradation on compaction of waste. For the meaning of "high" gas volume, look in the first Appendix of Volume 2 of the position paper.

11. *Can it be assumed that waste compaction followed by degradation would reduce spalling and cuttings releases? (at 30). Such must be shown.*

Response: Waste compaction followed by degradation should reduce spall and cuttings releases, because this state of waste would have higher strength and resistance to erosion. The present spall model is based on the assumption that the waste has no strength. An overly conservative assumption which is clearly not realistic. The present effort to develop a model of spalling which considers the strength which is possessed by the waste is ongoing and should answer your concern.

12. *Concerning the waste and backfill flow model, the draft observes the difficulty arising from a homogeneous assumption of the room configuration and says that a more detailed model is being initiated (at 35). What is being done? The effect of a model accounting for heterogeneity is said to be "unknown." (at 38).*

Response: See response to question 5.

13. *The values employed for waste permeability appear to assume that "average" drums are uniformly distributed. (at 39). The assumption is unrealistic and nonconservative. Nor is selection of the "lowest value considered likely" necessarily a conservative approach.*

Response: You are correct that the SPM-2 model assumes an average drum that is uniformly distributed in each room of the repository. Please see the answer to question 5, which discusses the impact of heterogeneities in drums on repository performance.

The model for SPM-2 has recently been changed to incorporate a relationship between waste porosity and waste permeability. This change eliminates the constant waste permeability from the new calculations and obsoletes the discussion of the "lowest value considered likely". Appropriate changes will be made in the next draft of this document.

Please note that while the CAO and SNL have often used conservative values for many parameters, this is not required by 40 CFR 191. There the requirement is "reasonably expected". Additionally, the CAO would appreciate seeing the data which were used to make the observation that the assumption is unrealistic and nonconservative.

14. *The draft candidly states that no measurements are now available for the two-phase flow properties of WIPP waste (at 39). The project should address this deficiency, especially with regard to flow toward an intrusion borehole (at 40).*

Response: The computational approach for the SPM-2 analyses is conservative and unrealistic, in the sense that the WIPP waste provides little resistance to fluid flow. Under this circumstance, the two-phase flow properties of the WIPP waste is not important for repository response. More specifically, the permeability of the WIPP waste is assumed to be so much higher (on the order of six orders of magnitude) than the

permeabilities of the surrounding backfill and salt that it is essentially an open conduit for fluid flow and radionuclide transport toward an intrusion borehole.

15. *Data concerning backfill permeability is likewise lacking (at 40). Further, what is to be done about the scenario in which increased pressure enhances backfill permeability (at 40)?*

Response: Experimental data are available for the permeability of consolidating crushed salt backfill (see Figure 4-1 in Butcher et al., 1991a and more recently a figure presented at the EPA/DOE Technical Exchange Salado Formation Process, October 24-26, 1994, presentation on Repository Seals Program). Calculations based on these data show that crushed salt backfill rapidly consolidates into a relatively impermeable mass, with a permeability on the order of 10^{-19} m^2. A salt/bentonite backfill is predicted to consolidate in a similar fashion (Butcher et al., 1991a). Note that these data provide the relationship between porosity and permeability for the backfill, incorporating the effect of increasing pressure on permeability.

Please note that the baseline for SPM-2 is no backfill and that backfill is considered in the activity sets.

16. *Transfer of water from sorbents has not been addressed (at 45). What processes may affect such transfer, among the numerous processes ongoing in a repository undergoing closure? What will be the effect of transfer on other processes, such as gas generation?*

Response: Dry Portland cement or other sorbents are intentionally added to take up moisture from wet sludge, assuring that no free water exists in compliance with waste shipping and acceptance criteria. Water contained within a sorbent is chemically bound to the sorbent and will be difficult to release under repository conditions. Any water contained within a sorbent, such as Portland cement, is therefore not considered available for gas generation.

17. *Wicking is not represented, nor is other capillary flow. Given the wide differences in gas generation rates between humid and inundated conditions, should this not be examined?*

Response: Based on our present understanding, wicking and capillarity should not be important phenomena for gas generation, as discussed in Section 3.3.4 of the draft white paper. The potential for wicking is identified primarily with cellulosics, the decomposition of which is conservatively assumed not to require water. Therefore, whether wicking occurs or not should have virtually no effect on gas generation. Additionally, wicking with cellulosics is unlikely to be important because cellulosics are a small fraction of the waste. In spite of these facts, wicking and capillarity will be evaluated through a set of supplementary calculations during the SPM-2 analyses to demonstrate that they have no significant impact on repository performance.

18. *The draft states that porosity results from BRAGFLO were compared with porosity-time results SANTOS and showed convergence. (at 43). These data have not been published or otherwise made available. Until that is done, the use of the uncoupled porosity surface approach remains a concern.*

Response: This data will be made available to you.

19. *In the 1992 PA certain assumptions were made in modeling room deformation with SANCHO (see 1992 PA vol. 4, at 4-15). There is concern whether the following are accurate or conservative assumptions:*

    a. *Porosity changes during decreasing gas pressure are not modeled.*

    b. *Deformation is modeled for a single room, whereas it will actually take place with respect to the entire repository.*

    c. *SANCHO cannot model spatial variations in gas generation within the room.*

    d. *SANCHO does not model the flow of gas from the room.*

    e. *Deformation in SANCHO is independent of brine flow, which is not explicitly modeled.*

    f. *Gas generation in SANCHO takes place at a constant*

rate.

g. *There is a bounding value for porosity which restricts BRAGFLO calculations (see p. 4-21).*

h. *Porosity is explicitly changed at time steps, but is not changed continuously during time steps (see 4-23).*

DOE should explain how these assumptions will be justified or replaced with more realistic modeling.

Response: a. Porosity changes during decreasing gas pressure in an undisturbed repository can be addressed by both SANCHO and BRAGFLO. See response number 3 for additional information.

b. Reconciliation of this is discussed in Section 3.1.2 of the position paper. To thoroughly model the entire repository is technically infeasible, as a result of the state-of-the-art in computers. The applicable EPA regulation requires us to define what is reasonably expected, and we believe we have done so.

c. The porosity surface approach has been checked against a fully-coupled method. No situation has been identified in the PA results where a "better" calculation would influence compliance. The fully-coupled model is not used for all the calculations because it is so time-consuming and resource intensive that it is not practical or prudent to waste these resources when it is possible to verify the SANCHO and BRAGFLO results against the fully-coupled model. It is important to realize that we must use fiscal responsibility as well as scientific soundness.

d. No situation has been identified by PA which would require the use of this more sophisticated analysis. See response c above.

e. Please see responses to c and d above and h below.

f. Please see responses to c and d above and h below.

g. Please see response to question 3 above.

h. All situations that PA has tested have shown that the present porosity surface approach is a reasonable approximation of repository response. If you have any credible scenario which you feel

is not suitably addressed by the present computational method, we shall be glad to calculate it both ways to allay your concerns.

20. The modeling of direct releases is not at an advanced stage. Based on the discussion in the white paper, certain concerns can be mentioned, but full treatment of the questions raised by the modeling of cuttings, cavings and spallings must await completion of the modeling effort. Questions at present are as follows:

20. There is a reference to a simple model of spall that approximates an upper bound for releases (draft at 45). Please describe the substance of that model.

Response: The simple model for spall referred to is the same as that described later in the position paper (See Section 4.2.4). One minor point: the model approximates an upper bound by assuming that the waste has zero strength and thus is unrealistically conservative.

21. The description of Delaware Basin drilling practices (at 46-47) requires supporting data and references. What is "typical" may be debated, and on some points there may be no typical practice.

Response: Drilling practices may have minor variation from driller to driller, but certain practices appear to be relatively standard. The practices quoted come from verbal descriptions given to one of the authors of the position paper by two drilling companies that are active near the WIPP - Ziadril Corp and McVay Drilling, both of Hobbs, NM.

22. Reliance on the text of Appendix C to 40 CFR Part 191 is a cause of concern, given the forthcoming promulgation of compliance criteria, 40 CFR Part 194, and guidances (at 47).

Response: As you are aware, the DRAFT 40 CFR Part 194 is still out for comment and may be modified based on those comments. Why this is a cause of concern is not apparent. The project will comply fully with 40 CFR Part 191. 40 CFR Part 194 merely describes how the project should show compliance. If you believe that the EPA is improperly adding additional requirements in this document, The CAO encourages you to address this to them during the comment period.

23. Carvings modeling is limited to the effects of fluid shear stress (at 47). What support is there for omitting to consider the other factors mentioned, e.g., drillpipe eccentricity, solids in the mud, extended contact between

mud and walls?

Response: The rotational eccentricity of the drill collars can affect waste erosion only at early times when the borehole is "gauge" and collisions between the collars and waste are possible. After the walls adjacent to the waste have receded as a result of erosion, the stiffness of the drill collars prevents further collar/waste contact and the erosion from this process is eliminated.

Solids in the drilling mud will increase the interface shear stresses and hence the wall erosion. This effect is accounted for by assuming a very low shear resistance to erosion for the waste.

In the shear stress model for erosion, it is assumed that sufficient time is available for the erosion process to reach equilibrium. Thus time, in this sense, does not play a role in erosion releases.

24. *The distributions of mud density, viscosity, and yield stress are not justified in the draft (at 48). The reference to volume 2 of the 1992 PA, table 4.4-1, cannot be located.*

Response: The referenced location of mud density, viscosity, and yield stress have been corrected in the SPM 2 Baseline version position paper soon to be sent out.

25. *As to drill bit diameter, should it not represent current practices as to oil and gas wells in the area, distributed in accordance with the frequency of such drilling?*

Response: As discussed in 1992 PA Volume 3, Pg 4-8, a uniform distribution of borehole diameters around the value used for deep gas wells (14") ensures a fairly large borehole is assumed. If as a stakeholder you desire that we represent current practices and distribute them according to frequency, we shall be pleased to do so. We feel, however, that we are bound to point out that to do so would be less conservative.

26. *The 1992 PA says that deep gas wells normally have a diameter of 17.5 inches in the salt; why then should the diameter of 13.97 inches be the median?*

Response: For deep gas wells a 17.5 inch hole is bored to the <u>top</u> of the salt section. Within the Salado, it is approximately 14 inches.

27. *Apparently, there are no experimental data on shear resistance of repository waste. This creates a concern (at 45). What is the justification for assuming resistance similar to saturated montmorillonite clay? The draft refers to the value for erosion shear strength as "speculation" (at 50), indicating the need for greater justification.*

Response: The values chosen for shear resistance are based on the erosive properties of clay and silt like materials found on ocean and lake bottoms. The numbers were chosen because they are believed to be very conservative.

28. *Releases by spallings create a significant concern. In analyzing such releases DOE should refrain from placing weight upon the Appendix C assumption that intruders will "soon detect" the incompatibility of WIPP with drilling (at 51). Any such assumption must be based on actual data concerning drilling practices.*

Response: Your comment is noted. However, the CAO will be constrained by the EPA regulations in effect at the time of the compliance submission. If they allow "soon detect" or other credit, we will take credit for these as so much that the EPA and CAO are doing is very conservative.

29. *The draft describes a "blowout" scenario and a "cautious drilling" scenario (at 51). How may the relative probability of each scenario be determined?*

Response: These scenarios require a search of the literature and additional discussions with drillers to define how reasonable each scenario truly is. These will be addressed in updates to the position paper.

30. *Does the assumption that gas driven waste transport ceases on placement of casing (at 51) assume too much as to the effectiveness of casing? Is there no possibility of leakage through casing joints or upwards through the annulus outside the casing?*

Response: The potential for leakage though casing joints or through the annulus is considered to be very remote. Your question asks "Is there no possibility ...". There is a possibility for almost any eventuality. But in this case we believe it to be, as we said, very remote. Damon Runyon once said "The race is not always to the swift or the battle to the strong, but that is the way to bet".

31. *In the second scenario there is reference to a gas pocket or plenum above the waste (at 51). The hypothesized presence of such a phenomenon needs explanation, especially in light of the anticipated closure of the repository and the presence of backfill.*

Response: The scenario hypothesized a crack or plenum above the repository caused by gas pressure in the repository approaching lithostatic. The plenum would be caused by the waste. No such plenum is known to exist anywhere in the Salado. The probability of the existence of such a plenum occurring is considered remote. See the response to question 30.

32. *There is discussion of characteristic responses to penetration of air pockets in salt (at 53); data support is needed here.*

Response: The occurrence of gases in the geological formations near WIPP, their composition, and outbursting events have been documented in report EEG-25 entitled "Occurrence of Gases in the Salado Formation" by Lokesh Chaturvedi.

33. *What is the justification for sampling entrainment rates between 0% and 10% of the gas flow rate? (at 53).*

Response: Solids entrainment in flowing gas is based on the assumption of dilute phase transport. Data on dilute phase vertical transport is available in the literature and indicates that the maximum rate of entrainment is approximately 10%.

34. *The statement is made that blowout preventers would probably be engaged within five minutes. (at 54). No citation is provided; moreover, the point is one on which a typical practice may not exist.*

Response: The five minute limit for closing of the blowout preventers was obtained from McVay Drilling of Hobbs, NM (J. Dyer). This time limit could possibly use greater clarification.

35. *Gas flow rate is another key variable. Our office does not have the technical expertise to examine all the assumptions in the draft (at 54). Logically, it would seem that the configuration of decomposed waste in the repository is highly variable, causing highly variable flow rates.*

Response: The State of New Mexico has a number of technically competent scientists capable of assisting you on this

subject. The Environmental Evaluation Group has written offering their services.

Technically, the gas flow rates depend primarily on waste permeability, waste gas pore pressure, and the geometry of the borehole. The values for permeability are computed based on waste porosity, which is a sampled parameter. The resulting waste permeabilities vary over a range from approximately 10^{-12} to 10^{-17}. The waste gas pore pressure is computed from a corrosion model that is driven by the pressure of brine.

36. *Release estimates are based on an assumed borehole surface of 1 m^2 (at 55). Is this estimate conservative? Is it not possible that the borehole surface will be larger? Should the borehole surface be measured similarly for cuttings, carvings, and spallings releases?*

Response: Only gas flow rates are based on an assumed borehole surface of 1 m^2. Refinements in this assumption are in progress. Please note that the refinements may make this less conservative and more accurate.

37. *The statement that blowout may occur at waste permeabilities below 10^{-16} m^2, but that this is a conservative limit, seems self-contradictory (at 55).*

Response: Since releases with waste permeabilities below 10^{-16} m^2 are in the range of gas erosion or stuck pipe, which have larger releases than blowout at this permeability, the assumption is quite conservative.

38. *The "stuck pipe" discussion places a limit of 5% of the mud flowrate on waste removal (at 55). Would such a limit consistently be applied by drillers during the extraordinary circumstances of a cleanout procedure? Would the driller be able to control the loadings of the drilling mud within such a limit?*

Response: It is probable that if the driller attempts to remove the sticking material (waste) at a rate greater than 5% the sticking problem will be exacerbated by additional material settling within the annulus.

39. *What is the justification for sampling the range of 12 to 24 hours for cleanout of a stuck pipe? (at 55).*

Response: Short, J. A. 1982. Drilling and Casing Operations. Tulsa, OK: PennWell Books, pp. 183-184.

40. The gas spall discussion (at 56) also requires justification of the limit of 5% loading of drilling mud.

Response: Above cuttings loadings of 5% the driller will encounter sticking and continuous drilling under these conditions will be difficult. Under these conditions, a sticking pipe procedure will be used.

41. How does DOE plan to establish the limiting volume of gas spall waste release (at 57)?

Response: It is not clear what is meant by this question. The spall model described in Chapter 4 is a first order model that will be used in SPM-2. It allows releases to continue indefinitely until stopped by some external event. It is <u>not</u> physically consistent with other similar phenomena, which indicate that material is ejected only when gas flow exceeds a certain "critical" velocity. Thus, in the real world, while initial flow in the hole is likely to cause release, the flow will eventually drop below the "critical" velocity, and release will naturally cease. This implies a limiting volume of release dependent on a critical velocity. SNL has recently demonstrated, with data from the literature for unconsolidated materials, that such a critical velocity reduces release by blowout to less than the first order model predictions. Furthermore, the waste will have some strength, even a slight amount of which would limit release further. Work on the "critical" velocity and calculations on compacted waste strength are ongoing and will be reported to the stakeholders as they become available.

42. The draft states that, aside from "severely reducing gas generation in the waste, methods to reduce surface releases include reducing waste permeability and increasing waste strength" (at 58). What engineered alternatives will be evaluated with these purposes?

Response: The engineered alternatives to be considered in SPM 2 are being defined at this time and will be briefed to the stakeholders in the relatively near future. In general, engineered alternatives are discussed in "Evaluation of the Effectiveness and Feasibility of the Waste Isolation Pilot Plant Engineered Alternatives: Final Report of the Engineered Alternatives Task Force," Volumes 1 and 2, DOE/WIPP 91-007.

43. Justification is required for the sampling of drill bit penetration rate and its relation to drill bit diameter (at 59).

Response: According to verbal information obtained from Ziadril Corp of Hobbs, NM the rate of penetration through the salt section is dependent on the diameter of the bored hole.

44. The statement appears that the role of spatially variable brine saturation and multiphase flow on spalling has not been investigated (at 59). These issues need to be investigated.

Response: We concur and agree that the tremendous decrease of spalling potential from reasonable waste strength also needs to be investigated.

45. There is reference to experiments to investigate spall in an axisymmetric geometry (at 60). Results of those tests should be made available and may raise further concerns.

Response: The results are not yet available and will be released as soon as they are. The results of these tests may also put concerns to rest.

46. There is reference to calculations with respect to the possibility of brine spall (at 60). Those calculations should be made available and, again, may raise further concerns. The same point applies to the brine slurry scenario (at 61).

Response: Brine spall calculations are planned and will be made available. Again, they may put concerns to rest.

47. It is evident that the direct-release models described in the draft are viewed as "simple-worst case computational models" (at 62) and will be further refined. Stakeholders must have the opportunity to consider the models, when refined, and to comment on them. Without the opportunity to comment on DOE's final position - as opposed to ongoing work - stakeholder involvement cannot be effective.

Response: As we have repeatedly stated at stakeholder meetings, stakeholder involvement is considered by the CAO as an ongoing process. We have consistently promised that as the project moves forward, stakeholders will have the continued opportunity to make their concerns heard. Having participated in our stakeholder meetings, we know that you have seen our commitment being carried out. While we agree with you that stakeholder involvement in the final position is important, we

continue to believe that only through continued involvement, from drafts all the way to the final product, can stakeholders be truly effective. We appreciate your interest and the work you have put into our draft position papers and look forward to your continued participation and cooperation.