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U.S NUCLEAR REGULATORY COMMISSION
DIVISION OF WASTE MANAGEMENT

REVIEW OF "THE BASALT WASTE ISOLATION PROJECT FIELD REACTIVE TRACER TEST STRATEGY"

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SD-BWI-PD-017

TECHNICAL ASSISTANCE IN HYDROGEOLOGY
PROJECT B - ANALYSIS
RS-NMS-85-009

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1.0 INTRODUCTION

FILE NO.: B-86/08--NWC-1993

DOCUMENT: "THE BASALT WASTE ISOLATION PROJECT FIELD REACTIVE TRACER TEST STRATEGY", by E.I. Wallick, Basalt Waste Isolation Project, Rockwell Hanford Operations. Copy dated 8/6/86. Document No. SD-BWI-PD-017.

REVIEWER: Mark J. Logsdon, Nuclear Waste Consultants MJL 7/11/87

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APPROVED:

KEY WORDS: BWIP; Basalt; Hanford; Geochemistry; Solute Transport; Tracer Test; Redox; Eh; Solubility; Retardation; Sorption; Adsorption; Distribution Coefficients; Selenium; Technitium; Lithium; Dissolved Oxygen

2.0 SUMMARY OF DOCUMENT AND REVIEW CONCLUSIONS

2.1 SUMMARY OF DOCUMENT

The purposes of the document are:

1. To outline a program for evalauting in-situ solubility of redox-sensitive radionuclides that could migrate from a repository in basalt;

2. To describe the relationship between the proposed field tests and laboratory-scale experiments of distribution coefficients and retardation factors for key radionuclides;

3. To describe the relationship between the laboratory experiments, the proposed reactive tracer tests, and potential tests of radionuclide chemistry which may be conducted from the ESTF at BWIP.

The goal of the proposed effort is to determine in-situ and at field scale if the geologic setting in the Grande Ronde Basalts is "effective in retarding the movement of key radionuclides that could be released from the waste package environment."

The subject document proposes a staged investigation that includes:

1. Phase 1:
   - o Development and calibration of single-well, injection-withdrawal tests
   - o Convergent tracer tests
   - o Laboratory analyses and experiments on water and core

2. Phase 2:
   - o Tracer tests in selected BWIP Hydrochemistry Boreholes
   - o Tracer tests in RRL piezometer nests

3. Phase 3 (Optional): o Tracer tests and laboratory analyses from ESTF

The proposed methods involve injection of mixtures of reactive and conservative tracers into Grande Ronde flow tops (Rocky Coulee; Cohassett;

McCoy Canyon). Reactive tracers under consideration include lithium, dissolved oxygen, selenium and technitium; conservative tracers may include thiocyanate, iodine, bromide, and organic acids. After a 30 - 90 day reaction period (single-hole tests) or completion of the convergent two-hole tests, the concentrations of tracers in recovered fluids will be analyzed and the behavior analyzed, with an emphasis on calculation of a classical Eh for the redox-sensitive species. A significant part of the technology development of Phase 1 involves an attempt to establish empirical relationships between results of single-hole and convergent tests that will allow a calibrated estimate of effective porosity from the single-hole tests. In addition to the tracer analyses, BWIP intends to collect core samples of the basalt and to perform laboratory analyses and experiments to assess adsorbtion of reactive tracers in order to calibrate laboratory sorption experiments to field-scale phenomena.

The document includes general discussions of the theory of reactive transport of solutes; theory and interpretation of both single-hole and convergent tracer tests; and identification of "key radionuclides for performance assessment mass transport modeling."

Because the document is a proposed testing strategy, it does not include any new site-specific data or draw any conclusions about site conditions of likely repository performance.

2.2 SIGNIFICANCE TO NRC WASTE MANAGEMENT PROGRAM

The proposed program is intended to provide three important types of information:

1. Performance-scale values of retardation factors for two radionuclides that are controlled under 40 CFR 191 (and which thus must be evaluated under 10 CFR 60.112).

2. In-situ testing of redox conditions and a method for calibrating laboratory-scale to field-scale geochemical (both solubility and sorption) data. These matters are addressed explicitly in 10 CFR 60.122 and may (in fact, likely will) be used to demonstrate compliance with 10 CFR 60.112.

3. Up to 19 additional estimates of effective porosity of Grande Ronde flow tops. As has been argued in other NWC documents (e.g., NWC, 1986, 1987), NWC considers that in light of available information on gradients and hydraulic conductivities, reliable evaluation of the range and distribution of effective porosity is perhaps the critical remaining data need for evaluation of compliance with the NRC's pre-emplacement groundwater travel-time (GWTT) criterion (10 CFR 60.113) and evaluation of GWTT as a siting criterion (10 CFR 60.122).

2.3 REVIEW CONCLUSIONS

The strategy document is well and clearly written. The goals and objectives of the geochemistry program are clearly stated, and the integration of the geochemical effort with the hydrology program - all with an eye to evaluation of repository performance against the EPA Standard - is to be commended. The proposed effort is clearly intended to develop the kind of data that will be needed to respond to the NRC's comments on geochemistry in the Staff's review of the BWIP FEA (particularly issues related to BWIP's estimates of solubility and sorption), and this responsiveness to NRC input should also be noted favorably. Additionally, it is worth noting that BWIP is the first of the three repository programs to tackle the difficult issue of translating laboratory-scale geochemical measurements to performance-scale measurements in a comprehensive (at least spatially) fashion. The technological difficulties that are likely to be encountered are significant, and this reviewer believes that subject document outlines an approach that has a possibility of moving the program significantly toward resolution of several important data needs, particularly the issues of in-situ redox conditions and effective porosity.

The general approach being considered - the use of coupled reactive and conservative tracers - is considered by this reviewer to be conceptually sound and, in fact, probably the only way that can be used to develop the data that BWIP has identified as needed. The reviewer notes that the use of reactive tracer tests is consistent with the philosophy of testing described in BWIP STP 1.1, i.e., that extrapolation of demonstrated performance from one scale to another is always to be preferred to predictions of ensemble performance from uncalibrated modeling based on theoretical relationships alone. The proposed staging of the effort is appropriate, and the reviewer considers that the NRC should encourage the Phase III activities which are apparently viewed as optional. (It is understood that additional details of Phase III activities are needed before it is possible to actually review that phase of the program. In particular, BWIP would need to describe how the relatively small-scale experiments that apparently envisaged would be conducted with respect to potential near-field phenomena such as radiolysis and relatively high thermal gradients which may affect both solution and solid phase chemistry.)

Despite a generally favorable response to the technical approach, the reviewer has a variety of reservations about the document and the strategy that it describes. The major reservations include:

1. The strategy that is developed in the document will not likely fulfill all the objectives that are stated.

2. Despite the reviewer's favorable response to the philosophy of performance-based testing, in the reviewer's opinion the proposed locations for Phase II testing are not chosen on an optimal basis from the point of view of the stated objectives.

3 Despite comments acknowledging difficulties with applying the concept of a unique Eh to the speciation of redox-sensitive solutes at low

temperature (e.g., p. 42 and 46), the proposed program seems to be based on the assumption that it is practical to establish a "true" Eh for the system.

4. The reviewer is not pursuaded by the selection of Se-79 as a key radionuclide, since it is based essentially on a strictly geochemical argument that Se is apt to exist as an anionic complex in solution, rather than being based on a performance assessment that indicates that Se-79 is significant to meeting the EPA Standards.

5. The reviewer has substantial reservations about the likelihood that the one-hole tests can be calibrated in any defensible fashion by reference to convergent tracer tests for the purpose of deriving effective porosity.

6. The reviewer considers that the so-called "swish test" may not be feasible at the scale described (and apparently required by BWIP's internal criteria). The effective radius claimed by BWIP depends on the full 990 m of borehole being available for mixing, which is inconsistent with key assumptions of the test. Based on a more likely mixing zone of 50 m, the probable maximum effective radius is approximately 8.5 m, which does not meet the Grisak et al. criterion of approximately 100 m.

These major comments are discussed in detail in Sections 3.0 and 4.0, below.

2.4 RECOMMENDATIONS

Based on the review of this document, NWC recommends that:

1. There be a joint meeting of the NRC's hydrology and geochemistry groups to discuss the comments and criticism of this testing program. It is clear that there will be increasing numbers of BWIP (and other DOE) proposals that cross disciplinary lines, and it would be well for the NRC's technical review groups to establish better communication on these matters. NWC would point out that one of our consultants, Professor J.I. (Tim) Drever of the University of Wyoming, has done pioneering work in the use of one-hole tracer tests for addressing combined hydrologic and geochemical concerns. We suggest that in joint hydrology-geochemistry sessions on tracer testing, Dr. Drever could provide a variety of theoretical and practical insights that would be helpful in focusing NRC technical concerns.

2. The Staff transmit its comments and recommendations to DOE on this proposed testing program in a timely fashion. As noted in the review, the reviewer has substantial concerns about the likely success of the proposed effort, but believes that its merit would be enhanced by some procedural changes that could be incorporated. Clearly, the more quickly that these matters can be addressed by DOE,

the more likely the proposed program is to be initiated on a sound technical and practical footing.

3. The Staff initiate planning for a hydrochemistry workshop with DOE that would integrate hydrology and geochemistry input from both DOE and NRC. The basic argument for such a workshop is similar to NWC's point in recommendation 1 above: as integrated site characterization activities are planned and initiated, it is important that they be reviewed and discussed in an integrated fashion.

3.0 MAJOR REVIEW COMMENTS (PROBLEMS, DEFICIENCIES AND LIMITATIONS)

3.1. The strategy that is developed in the document will not likely fulfill the objectives that are stated.

The proposed effort will provide direct evidence on the reactivity only of selenium and technitium; a variety of other "key radionuclides" remain to be tested, and the issues of transfer of laboratory- to field-scale data will not be addressed for any of the radionuclides that are likely to move as cations (or zero-charge complexes) nor for the non-redox-sensitive anionic species. BWIP, of course, understands this, but the initial statements of purpose do not fully reflect the limitations of the actual strategy. This criticism is quite easily resolved by indicating (if it is true) that this program is viewed as the first in a set of reactive tracer experiments designed to assess the range of likely radionuclide behavior.

3.2. Despite the reviewer's favorable response to the philosophy of performance-based testing, in my opinion the proposed locations for Phase II testing are not chosen on an optimal basis from the point of view of the stated objectives.

BWIP Hydrochemistry Boreholes 1, 2, and 3 are located outside the accessible environment, and testing inside the RRL is planned only for RRL-2 (with possible tests at DC-22 and RRL-14) and is proposed only after completion of the LHS testing. There are two problems with this approach. First, an optimal testing program should attempt to collect information in critical areas first and move to less important areas only as time and resources allow; areas outside the accessible environment are surely not as important to either site characterization or performance assessment as are areas inside the accessible environment. Secondly, BWIP may be short-changing themselves in terms of their interest in establishing redox behavior in the important part of the geologic setting. There is good reason to believe, based on the dissolved gas data currently available, that the fluid phase in the vicinity of the RRL may be significantly more reduced than is the fluid phase to the east and southeast of the RRL (e.g., Terra Therma, 1986; Davis et al., 1987). Since this is exactly the condition that BWIP hopes to verify, it seems illogical to begin the field testing outside the RRL. (The reviewer understands that there may be other factors involved in optimizing the overall site testing program than the geochemical objectives presented in this report.)

3.3 Despite comments acknowledging difficulties with applying the concept of a unique Eh to the speciation of redox-sensitive solutes at low temperature (e.g., p. 42 and 46), the proposed program seems to be based on the assumption that it is practical to establish a "true" Eh for the system.

The BWIP approach assumes that there is a master variable for oxidation potential that is conceptually equivalent to pH as a master variable for acid-base potential. This is almost surely not true in low-temperature waters. In addition to the Thorstenson (1984) reference cited on p. 42 of the subject document, this matter has been addressed in Lindberg and Runnells (1984), references cited by Lindberg and Runnells, and many others, including NRC Technical Assistance contractors in geochemistry (e.g., in response to the BWIP EA). A theoretical basis for the lack of internal redox equilibrium (even in systems which may be approximately in equilibrium with respect to major dissolved species) is presented in Hostettler (1983).

There is, in fact, a growing consensus supporting the position of Lindberg and Runnells that

> The concept of Eh remains a valuable tool for theoretical and pedantic (sic) purposes. However, in the apparent absence of internal redox equilibrium, investigators should abandon the use of any measured master Eh for predicting the equilibrium chemistry of redox reactions in normal groundwaters. Our conclusions are most severe in the context of predictive computer modeling of the chemistry of natural groundwaters and wastewaters. In order to provide meaningful input for such models, it may be necessary to analyze the samples for the dominant ions of every redox element of interest.... If any measured Eh is used as input for equilibrium calculations, the burden rests with the investigator to demonstrate the reversibility of the system. (Lindberg and Runnels, 1984, p. 927)

In the context of the BWIP program, this position indicates that for waters that are poorly poised with respect to the redox-sensitive radionuclides (and one would anticipate that this might be the case for most or all of the radionuclides of concern because of the low concentration of redox elements), the extrapolation of redox behavior to other radionuclides based on the performance of selenium and technitium (that is, through the use of a master Eh or a general appeal to reducing conditions in the fluid phase) will be very difficult to justify.

This comment, clearly, is related to Comment 3.1 above: the proposed work may provide valuable information on the behavior of Se and Tc, but it will not, of itself, resolve the issue of the ability of the site to limit radionuclide flux due to geochemistry.

3.4. The reviewer is not pursuaded by the selection of Se-79 as a key radionuclide, since it is based essentially on a strictly geochemical argument that Se is apt to exist as an anionic complex in solution.

The BWIP criteria are not a particularly performance-based approach to identifying "key" radionuclides. Table 4 (p. 35) lists a set of radionuclides, their 1,000-yr inventory, and the EPA limit for each listed radionuclide. Note that the ratio of inventory/EPA limit for Se-79 is 0.35 at 1,000 years. That is, the entire inventory of Se-79 could be released instantaneously to the accessible environment at the end of the containment period without violating the EPA release limit for Se-79. One could, of course, claim that one needs to perform the assessment for a "sum of the fractions" evaluation or that one needs to assess Se transport for compliance with the groundwater or individual protection requirements, but BWIP has made neither of these arguments. Again, the concern here is one of establishing an optimal program. The Inventory/EPA limit ratios for Am-241, Pu-240, Np-237, U-238, and Zr-93 (and several other radionuclides that do not appear in Table 4; see Specific Comment No. 20 below) are all greater (some much greater) than 1, and all are redox-sensitive species. It would seem reasonable to have considered the possibilities of reactive tracer tests designed to address issues associated with their transport and with the scale problems of laboratory data; transport characteristics of uranium species (which may well be present as highly mobile uranyl carbonate) are an obvious example. The reviewer recognizes that there are practical constraints on the numbers, kinds and extents of tracer tests that could be accomodated by the BWIP program, but it seems well for BWIP to address these matters in providing the rationale for their proposed program, and specifically to have included a more robust selection procedure in terms of likely repository performance.

3.5. The reviewer has substantial reservations about the likelihood that the one-hole tests can be calibrated in any defensible fashion by reference to convergent tracer tests for the purpose of deriving effective porosity.

Consider the case of the DC-7/8 recirculating test. Based on the dynamic temperature logging results, the current BWIP interpretation of the test is that the tracer was transmitted through a highly transmissive zone of limited thickness but full connectivity between the boreholes within the packed-off interval of the test (Leonhardt et al., 1984). Because of the high hydraulic potential established between the two holes, the test yields no information about the transport characteristics in any direction except (essentially) directly between the the injection and withdrawal holes. Now suppose that a single hole test is conducted in DC-7. Presumably, dynamic temperature logging would identify the same "producing zone", but the lateral extent of the zone around DC-7 would be unknown. Consequently, the calibration of the two tests would depend on an assumption that the fracture zone is radially symmetric at the scale of the two-hole tracer test. In fact, this hypothesis cannot be tested without additional multiple-hole testing, and there will be a large (approaching infinite) number of geometric relationships and possible tracer behavior (conceptual models of the single-hole test, if you will) that would also "calibrate" with the two-hole test. Let us suppose, for the sake of argument, that the empirical relationship for the DC-7/8 tests is considered to be appropriate. Now assume that another location is selected and the logging identifies two significant water-producing zones: we still have no information on the lateral extent of the zones and we have no independent way to determine the transport characteristics of the two zones. Are we to assume that the zones are equivalent to each other and to the producing zone of DC-7/8? And so on.

It seems to this reviewer that to develop a reliable empirical relationship between the single-hole and the two-hole tests will require a significant number of tests (perhaps a half dozen?) that converge on a consistent relationship between K and ne. Wallick has alluded to this issue in Section 2.1.2 of the document, where he points out that the calibration procedure assumes that "...the lateral variability of porosity and permeability of the hydrostratigraphic unit is not execessive..." (p. 25; par. 2; lines 8-10). The currently available data is certainly not encouraging in this regard: point measurements of lateral hydraulic conductivity in the Grande Ronde flow tops range over 8 orders of magnitude and estimates of effective porosity commonly assume a two-order of magnitude range for the 95% confidence interval (e.g., Clifton, 1986; NWC, 1987). One could argue that these data are not particularly relevant to the problem at hand, since the scale of the tracer tests (i.e., the effective radius) is such that one would expect some measure of correlation in hydraulic properties over relatively short distances. Unhappily, this qualitatively plausible situation for an equivalent porous medium may or may not apply because of scale issues that will be exceptionally difficult to resolve in any meaningful fashion: there is not only the macroscopic scale of the effective radius of the test to consider, there is also the mesoscopic scale of transport through (approximatley) discrete transmissive zones within the packed-off test interval of the flow top, and correlation structures of these two scales are unlikely to be consistent because of the qualitatively different nature of flow viewed in the two frames of reference.

In conclusion, it seems to this reviewer that extrapolation of empirical porosity relationships on the basis of perhaps two sets of one- and two-hole tests (DC-7/8 and the RRL-2 clusters) are very unlikely to be at all convincing. Because of the controversial nature of the effective porosity issue, a significant number of unconvincing (perhaps even unreliable) effective porosity numbers seems counter-productive, and BWIP would be well advised to significantly increase the number of two-hole tests before developing a licensing database that uses uncalibrated single-hole tests.

4.0 SPECIFIC REVIEW COMMENTS

Detailed review comments are presented in the following format:

- o Reference: page, paragraph, line(s)
- o Citation: the significant portion of the text (as appropriate)
- o Comment

1. 7; 1; 7-8 - **"Key radionuclides...present after a spent fuel waste package has been in place for 1,000 years."** From the NRC's perspective, this seems far too broad a definition, since the inventory of some radionuclides still present at 1,000 years will be below the normalized EPA release limit.

2. 7; 2; 9-11 - **"The NRC document 10 CFR 60.21...in the geologic setting."** While the citation is correct, this reviewer is puzzled by the selection of NRC requirements. It would seem more reasonable to have cited (and be focussing on) the performance objectives than on the mechanistic tools that will be used to demonstrate compliance.

3. 8; 2; 4-7 - **"The key radionuclides of concern..."** The selection depends on a specifically geochemical argument, which is not particularly robust with respect to performance. These are the radionuclides of concern to the geochemists because of their behavior in solution; it is not at all clear that they are the only or even the principal radionuclides of concern with respect to repository performance.

4. 9; 3 and footnote - **"...coefficient of hydrodynamic dispersion..."** Quite apart from the incorrect page citation, it seems peculiar for BWIP at this late date to be citing secondary sources such as Freeze and Cherry.

5. 10; 1; 1-4 - **"Single well...data will be modeled... (Clemo, 1984)."** The text never describes how the three BWIP codes will be used and only later (p.57; see comment # 31) do we learn that FRACSL is currently available only for homogeneous EPM models. The reference to modeling seems to be a bit of a throwaway.

6. 13; 2; 3-5 - **"Dynamic temperature logging...deep flow zones."** This is a very important practical matter, since the interpretation of both the hydraulic conductivity and the effective porosity depends on a knowledge (or assumption) about the thickness of the zone that is transmitting fluid, e.g., the interpretation of the recirculating tracer test in DC-7/8. This reviewer would like either a deeper discussion of this matter or a citation to the literature in order to be able to assess the likely impacts of the reolution question to interpreting the BWIP tests.

7. **16; 1; 4-7 - "The third model type...simulate field processes."** This is a very important point: Wolery in the documentation for EQ3NR has cautioned strongly against using master redox potentials in the computed speciation of redox sensitive solutes. This matter needs substantial further discussion. Does BWIP intend to use EQ3NR/EQ6 only for the modeling of species they have actually tested (e.g., Se, Tc), or do they intend to take an Eh value derived from the Se/Tc field data and apply that to the speciation of other radionuclides? In the opinion of this reviewer, the former is acceptable (though probably not very interesting, since they already will have the field data), but the latter would be very hazardous.

8. **16; 3; 2-4 - "...the third phase...in support of license application."** This seems a peculiar interpretation of the licensing process as this reviewer understands it. My understanding is that the site characterization phase is intended to provide all the data needed to support the license application and that the continuing activities are to be classified as "Performance Confirmation." The distinction, in the reviewer's understanding, is not trivial.

9. **24; 2; 3 - "...scalar requirements...of (less than) 100 m..."** The "less than" should be "approximately", based on Grisak et al. (see page 8, par 1, bullet 1).

10. **25; Table 2; note line 4-6 - "...H is the product of effective thickness times effective porosity."** H is the effective thickness or the product of total thickness times effective porosity.

11. **25; Table 3 and par 2 - "...effective radius of 3.8 - 38 m could be expected..."** The calculated volume is based on the entire 990 m nominal static water column being available as a mixing zone. It seems extremely unlikely that this could be the case without violating the implicit assumptions of no degassing and no cooling (p. 19; par 3; lines 11-44). The schematic diagram of Figure 6 (p. 23) is not very helpful in assessing the mixing zone that BWIP intends to use, but the implication is that the mixing would occur over a limited vertical extent close to the flow-top to be tested, perhaps within the superjacent flow interior. If this mixing zone were on the order of say 50 m, then the mixing volume in the borehole would be about 228 liters and the effective radius would be only 0.85 - 8.5 m. This effective radius is not consistent with the Grisak et al. criterion of approximately 100 m (though at the upper end it would be an improvement over lab-scale data).

12. **26; 4; 3-6 - "...the likelihood of success depends...within flow-top units."** While the reviewer agrees with the statement, the prospects are not good: look at the current range of data for Kh and estimates for range of ne.

13. **27; 1. 4-5 - "...should breakthrough of the reactive tracer occur..."** Based on the performance of potassium in the earlier DC-7/8 tracer test, it seems highly likely that there will be a breakthrough for reactive tracers,

though ion exchange and possibly matrix diffusion may confuse the interpretation. Perhaps the confounding factors can be dealt with through the use of multiple reactive tracers, as BWIP has proposed.

14. 30; 2; 4-7 - "Assuming a uniform distribution of selenium...core sample." The reviewer agrees that the arithmetic proposed is correct, but the critical assumption of uniform distribution of Se is at odds with the other goal of the experiment - namely to determine the solubility behavior of Se due to in-situ reduction. The Kd calculation depends on the assumption of constant chemistry (along the flow path, in this case). If reduction occurs, then the critical assumption is obviously not met. The best that can be hoped for is an "apparent Kd" on the scale of the sidewall core. A better approach might be to conduct two-hole tests with coring from both boreholes and subsequent interpolation of "apparent Kd" between the two sets of data. This problem is perhaps the key one that can best be addressed from the ESTF experiments, where it would be possible to overcore the flow path of a tracer test and get a truly large-scale sample for subsequent laboratory analyses (though only for very low permeability materials, since the breakout area of the ESTF is anticipated to be within the dense interior of the Cohassett).

15. 31; 1; 1-2 - "One independent variable...geologic structure." This is a somewhat peculiar way to state the matter and not strictly true, since it does not describe the scale of structures to which the author is referring. The reviewer agrees that there is good evidence from the dissolved gas data that there is heterogeneity in redox conditions of fluids. But, as has been presented elsewhere (TTI, 1986; Davis et al., 1987), the reviewer's interpretation is that there is a zone of vertical mixing in the vicinity of the RRL. This interpretation of an active vertical mixing zone is fundamentally different than the implication of Wallick that the structural feature of concern is the Cold Creek Barrier and that the RRL volume is a passive zone of reduced water.

Quite apart from this technical divergence, the document has two problems in this concern. First, BWIP personnel should be very careful about making sweeping statements such as this: it opens the door for endless and probably unresolvable inquries into the constitutive relationship between geochemistry and geologic structure at potentially any scale. Secondly, having identified a potentially significant field condition that deserves testing, BWIP doesn't pull the trigger and propose beginning testing in the area of greatest performance concern (and, not incidently, in the area that is most likely to support their qualitatively reasonable hypothesis).

16. 31; 2; 6-9 - "Testing during the second phase...possibly the DC-22 and RRL-14 borehole sites." The reviewer understands that the geochemistry program must be incorporated into the overall testing scheme and that there may be difficulties coordinating the proposed testing program with the LHS program. However, it is absolutely clear that the area of the RRL is the critical volume to be characterized with respect to performance, and the reviewer finds it disappointing that in the strategy document the paramount importance of data collected from within the controlled area is not acknowledged to the extent of proposing a program that concentrates on the critical data. Were the the data set to be limited to only RRL-2 from within the controlled area, this reviewer considers that the value of the overall program would be minimized.

17. 31; 4; 4-8 - "...RRL-2B...after water levels in the Rocky Coulee flow top have equilibrated." Since reactive tracer tests are proposed as part of the LHS testing, would it not be possible to extend the proposed program to include experiments that are part of this testing strategy? At a minimum, Phase I activities could, it would seem, be conducted at the RRL-2 cluster concurrent with developmental efforts at DC-7/8 (with the one-hole test being conducted prior to the LHS convergent tracer testing or even without the effort to attempt the correlation tests).

18. 32; 3; 3-6 - "...sorption can be quantified in much greater detail." This is true, but begs two questions. First, how will BWIP deal with post-emplacement conditions such as radiolysis and thermal effects (which would affect both the fluid phase and the solid phases). Secondly, to what extent will "much greater detail" be necessary and operationally significant, given that much of the area to be tested will be within the very near field of the repository. I imagine that BWIP has answers for both of these matters, but both are related to NWC's concern with "data needs": just because it is possible to collect additional data does not signify that it is necessary or even useful to do so. While I would strongly support the ESTF test phase, I would do so only if a coherent rationale and testing strategy were presented.

19. 32; 5; 4-8 - "The following relationship...granular flow medium and n is the dimensionless effective porosity." The issues raised by the tracer tests, such as the DC-7/8 test, address exactly this conceptual model: does the effective porosity concept have validity when the tracer flow is predominantly through a discrete zone? To date, the reviewer has not seen this matter addressed by BWIP (or anyone else); it will be the subject of a proposed NWC/TTI Technical Report under Subtask 2.5 of the current contract.

20. 34;2; 4-5 and 35; Table 4 - "Table 4 is a preliminary list of key radionuclides..." Table 4 is presented without reference. Calculations by NWC (based on data from NUREG/CR-3235, EPA 520/4-79-007C and Sinnock et al., 1984) note a number of other radionuclides that would meet BWIP's criterion that are not listed. Major radionuclides neglected include: Am-243; Pu-238; Pu-239; Pu-242; U-234; U-235; U-236. As discussed in Section 3.0 above, the reviewer does not find the mere existence of a measurable inventory a sufficient criterion for describing a radionuclide as "key". My concern is that BWIP could inappropriately allocate resources to matters that are of intrinsic scientific concern (e.g., the behavior of radioactive selenium) at the expense of other matters that are of substantially greater concern to protecting the public health and safety (e.g., the geochemical behavior of americium or uranium).

21. 36; 2; 1-5 and 37, Table 5 - "Approximate Kd ranges...safety factor of five." The cited FOS of 5 is not consistent with the data presented in Table 5. However, even if it were, I am unimpressed with a FOS approach. What is the rationale for any selected FOS? Surely one could make a statistical argument that would be fully defensible and still be implementable in performance assessment (and that would not be so restrictive on BWIP's performance modeling).

22. 38; 2; 3-5 - "Assuming an Eh of 0.3 V..." There are at least three potential problems with this statement: 1) it assumes what is to be proved (namely that the Eh is low); 2) it assumes that the Grande Ronde waters are at redox equilibrium; and 3) it assumes that the solubility would not be affected by temperature.

23. 39; 1; 1-4 - "Selenium, like sulfur...microbial selenite reduction." Probably valid statement of the research, but what did Rashid et al. determine and does it matter? This strikes the reviewer as another example of the author getting carried away with the intrinsic geochemistry of Se without asking himself whether it is of any significance to the repository problem.

24. 40; 2 and Figure 10 - Eh-pH diagram for Tc. In the final line, the reference should be to Figure 11. I would agree with the author that the diagram suggests that the kinetic problem in reaching Tc metal is probably due to the various phase transitions that are encountered, but it is not clear to me why, as a practical matter of geochemical reactivity, BWIP is so concentrated on the full reduction. Assuming that the Eh-pH diagram is at least schematically correct, it would seem to me that it would be the solubility of the most oxidizing Tc solid (or perhaps the second, if one wishes to be conservative with respect to redissolution) that would control the precipitation of Tc. In this case, the kinetic barrier to precipitation would not seem so extreme (at least qualitatively), and the reviewer wonders if BWIP is prepared to identify the intermediate Tc solids. (Note: based on the earlier Eh-pH diagrams of Brookins (1978), there is some likelihood that technicium sulfide could also be of importance or (perhaps even more likely) that Tc could co-precipitate with Mn or Fe oxides or oxyhydroxides).

25. 42; 2; 10-20 - "It is becoming increasingly clear...are carried out." Thorstenson's point is not so much that general thermodynamic equilibrium does not exist (after all, equilibrium thermodynamics produces very robust calculations of the non-redox, major-ion chemistry of natural waters), but that redox equilibrium is not reached. More fundamentally, the problem is not whether the groundwater is reducing, it is whether radionuclides would be reduced (and stay reduced) when they go into solution. This is a fundamentally different problem, as discussed by Lindberg and Runnells (1984) and many others.

26. **44; 2 - Discussion of Tc tracers.** The disadvantages in terms of understanding of using surrogate solutions seem to me to outweigh the potential problems with using a short half-life Tc isotope. Others will certainly disagree with this assessment, particularly in light of DOE/RL's record to date in handling radioactive materials.

27. **46; 6; 6-8 - "Additional work...true Eh values ultimately can be obtained."** While agreeing that it would be lovely if a master Eh obtained at low temperature, this reviewer considers that "you can't get there from here." The solubility and sorption behavior of radionuclides (or any redox species) depend on the valence state (and ligands, complex structure...) of the solute, but the valence state (and therefore the complex(es)) depend on Eh if and only if the system is in redox equilibrium.

28. **48; Bullet 1 Par 3 - "...excessively long monitoring times rule out the use of natural gradient tests."** The reviewer agrees that this is likely true for induced natural gradient tests in the time frame of the SCP program. However, it may be possible to evaluate natural tracers, particularly the movement of Cl-36 in the Pasco Basin. The reviewer also notes that NRC Research contractors have conducted natural gradient tracer tests elsewhere in the Columbia River basalts, and I consider that such tests should not be rejected out of hand as inapplicable (given appropriate scaling of hydraulic parameters) to the BWIP site.

29. **51; 5; 5-6 - "It is suspected that the error in determining the effective porosity will become greater as the effective porosity decreases."** While it is generally true that the precision of measurement decreases as the absolute value of a quantity approaches detection limits, I am not pursuaded by the argument presented. If one took a classical view of the tracer test/effective porosity problem, then as the effective porosity decreased, the average linear velocity would increase and both dispersion and matrix diffusion would decrease; additionally, since sorption should be proportional to surface area, sorption should decrease with effective porosity. What seems to me to be more likely is that as effective porosity decreases, it becomes a progressively less meaningful parameter: solute transport is either dominated by non-advective mechanisms (at extremely low permeabilities) or solute transport is effectively dominated by transport in discrete features that approximate pipes, in which "hydraulic conductivity" is very large but porosity equals one.

30. **56; 1; 4-7 - "The recovery curves...nearly coincident, suggesting there were no major changes in the dispersive character of the aquifer near the well bore."** This interpretation seriously overstates the conclusion that can be drawn. While this paper does not provide the data that would be necessary to calculate the effective radius of the Raft River tests, note that the volumes injected are orders of magnitude greater than the proposed volumes of the BWIP tests. Thus, it is not at all clear that the small-volume Raft River test is effectively testing the aquifer in the immediate vicinity of the well bore.

If this volume were injected in the Grande Ronde, the effective radius would be 14.4 to 144 m. As the author points out in the parenthetical comment that follows the cited statement, the result may be site specific. Another interpretation is that the Raft River experiments were all conducted at a scale that met or exceeded the Peclet number required to reach the so-called asymptotic dispersivity (Gelhar et al., 1979). While this latter case would be interesting, it would not be particularly helpful (at least directly) to BWIP, because of the site-specific nature of the dispersivity.

31. 57; 2 - Use of FRACSL. Having read earlier (p. 10) that FRACSL would be used to model the tracer tests, it was rather disappointing to learn that the current version is capable of treating only unfractured homogeneous reservoirs. Since this hardly seems likely to be the case for the proposed BWIP tests (e.g., DC-7/8), it is unclear how useful the simulations would be. While it is always gratifying to learn that a code has been successfully used elsewhere, the good performance at East Mesa is not particularly applicable, since the Salton Sea geothermal fields are predominantly developed in Tertiary sediments rather than in fractured rock.

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