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WIPP PA

MEMORANDUM FOR: Ronald L. Ballard, Chief
Geosciences and Systems Performance Branch

THRU: Seth Coplan, Section Leader
Systems Performance Section, HGLP

FROM: Richard Codell
Systems Performance Section, HGLP

SUBJECT: ATTENDANCE AT WIPP PERFORMANCE ASSESSMENT REVIEW AND VISIT TO CNWRA

I attended the performance assessment update on the Waste Isolation Pilot Plant for the National Academy of Sciences and Engineering, held in Irvine, California December 11 and 12, 1990. From December 13 to 14, I met the iterative performance assessment team at the Center for Nuclear Waste Regulatory Analysis. The attached trip report summarizes these two meetings.

Richard Codell
Systems Performance Section, HLGP

Attachment:
As stated

cc: R. Browning
J. Youngblood
J. Linehan
N. Eisenberg

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TRIP REPORT
WIPP PERFORMANCE ASSESSMENT REVIEW AND VISIT TO CNWRA
by
Richard Codell, System Performance Section, HLGP

I attended the performance assessment update on the Waste Isolation Pilot Plant for the National Academy of Sciences and Engineering, held in Irvine California December 11 and 12, 1990. Most of the presenters were from Sandia National Laboratories or their contractors who were charged with the responsibility to develop the performance assessment for releases of radionuclides from the WIPP. While the WIPP site is unlike Yucca Mountain in many important respects, the demonstration of performance in the context of the EPA regulations in 40CFR191 are similar for both sites. I summarize some of the more interesting presentations and those most relevant to the NRC HLW program.

Rip Anderson of SNL is the project leader. He gave some of the perspective on the performance assessment of the WIPP, noting that they were initially somewhat overconfident about the ease with which they could perform a convincing demonstration. In practice, the demonstration was much harder, which caused them to fall behind schedule. In addition, unforseen scenarios such as gas generation caused them to reevaluate the models of the site. While the SNL-developed methodology existed for performing a PA they ran into several difficulties, including:

1. developing scenarios - This is essentially a deterministic process, and did not work well for a probabilistic analysis.

2. The processes at WIPP were very complicated

3. There were few data, partly because DOE initially thought that the salt site was so good that they would be wasting their time and money collecting data in the Culebra dolomite aquifer.

Previous to the present briefing, the SNL team had performed only a deterministic analysis using simple computer codes (NEFTRAN, I believe), which did not explore the complicated phenomena likely to control releases at WIPP. The present briefing was based on many more sophisticated models and was probabilistic in nature. They said they could now support a preliminary demonstration that the site would comply with the cumulative release limits of 40CFR191, and would present CCDF curves to demonstrate it. The work is in a preliminary stage, however and they do not consider it to be adequate to support a defensible comparison.

Anderson pointed out that in the licensing of WIPP, DOE was unsure who the regulatory body ultimately responsible for reviewing the site would be. EPA of course would be involved, but I think they were hinting the Nuclear Regulatory Commission should take a role.

Mel Marietta from SNL presented an update to the performance assessment modeling. The new model considers the distance to the accessible environment to be 3 km vs the 5 km used in the previous analysis, and has a direct release model for bringing contaminated cuttings to the surface from well-drilling. They do not consider individual packages or canisters as the waste form. Instead, the repository horizon is considered to be a contaminated monolith. The direct release model calculates that contaminated rock would be brought to the surface in drilling mud when the shear stress of the mud exceeded the strength of the rock. Marietta also noted that cuttings contribute mostly to the low consequence end of the scale, a conclusion incidently that we also reached for the NRC MOU Phase 1 demonstration.

They are working on a report on how the scenario probabilities are being generated. Robert Guzowski is the team leader.

In the present performance calculations they have replaced the NEFTRAN code with a model dubbed SECO. Other updated computer codes are STAFF and BRAGFLOW for considering 2-phase gas and brine flow and transport from gas pressurization in the repository and natural gas pockets.

Martin Tierney described the procedure for determining the input distributions of uncertain variables. He said that they recognized the confusion that existed in past analyses with spatial variation being mistaken for uncertainty. In the present analyses, the apparent scales of spatial variability are smaller than the computational scales, so they are using variance reduction formulas (probably referring to the work of Vanmarke from MIT). In choosing the distribution, they first ask the principle investigator of each part of the analysis whether they can assign a distribution for each parameter. If so, they use that distribution. If there were three or more actual data on the parameter, they assigned an empirical distribution. They are also making use of the maximum entropy formalism to assure an unbiased distribution for sparse data. There were 250 parameters defined in the analysis, but I think I understood that they used only 29 to 50 of those for the 1990 performance assessment. One of the important variables was the time for first intrusion. This was chosen to be a distribution with a mean of 3200 years. This is an interestingly large value considering the distribution we used in the Yucca Mountain performance assessment, and the fact that there are ample mineral resources near WIPP in the form of natural gas, oil, potash and water.

Tierney described the limitations of the 1990 performance assessment:

1. Use of lumped parameter models - The geologic media have highly distributed parameters, but the present models are based on spatial uniformity of the parameters. Spatial scales

are much smaller than the average volumes of the grids used in the computations.

2. Possible correlations between parameters were ignored.

3. Parameter ranges was limited in most cases to information provided by the principal investigators.

4. The waste form was taken to be uniform. The source term model was based on a solubility for all radionuclides in the waste taken from a single distribution ranging over 6 orders of magnitude (which doesn't seem too likely).

The panel questioned the wisdom of depending on a single uncalibrated expert to come up with the input distribution and not asking for any rational. Chris Whipple of the panel commented on the lack of expert elicitation for coming up with the input distributions, especially since there has been so much work developed right at Sandia on the subject, notably NUREG-1150.

The performance assessment was used to determine the sensitivity to alternative engineered designs and conceptual models. Marietta described the sensitivity of their results to the assumption of either single fracture flow and transport in the Culebra dolomite with sorption only on the clay layers in the fractures, versus the alternative model which accounts for matrix diffusion from the fractures into the rock matrix. The latter gave much lower consequences.

The most important scenario at WIPP in terms of high-consequence releases is pressurization of the repository either by natural gas or gas-generation by decay of the organic waste and metallic corrosion. The sequence of the processes in the scenario is important; for example, one scenario considers several drill holes through the repository, one of which penetrates a gas pocket, and is sealed. Subsequently the gas leaks into the repository causing pressurization and escape of brine up the second borehole. The sequence of drillings and plug failures would have to be correct before the brine would escape.

Their extreme dose calculations were based on an onsite farm, with exposure through stock watering, and direct exposure of the drillers to the contaminated cuttings.

The 1991 performance assessment will include the following refinements:

1. Scenario probabilities
2. Geostatistical module for dealing with spatial variability.
3. Culebra hydrogeologic properties by zones instead of uniform.
4. Two phase flow of liquid and gas.
5. Verification of models and conceptual models within the

Probabilistic System Assessment Code (PSAC) users group of NEA and the INTRAVAL project.

6. More time to devote to calculations.

Jon Helton from Arizona State University next described the sensitivity and uncertainty analysis used. Dr. Helton has been associated a long time with the SNL performance assessment methodology. He employed methods that we are familiar with, namely Latin Hypercube Sampling and linear regression. He presented the results of the LHS output in some interesting ways however. For example, using box and whisker plots, he showed that while Pu-239 accounted for the largest consequences in individual realizations, the median contribution of this isotope were much smaller than for some of the other radionuclides. Some of the key issues in the sensitivity analyses were the relationships between scenarios and sampled variables, adequacy of review of calculations, and the correct form of the conceptual models. In a later conversation with Dr. Helton, he described how he is calculating CCDFs for performance so that the distribution around the CCDF at any value of compliance could then be compared to the EPA criteria; i.e., at any value of compliance the CCDF would be a probability distribution, so one could make the statement about the probability of non-compliance. The normal way of calculating CCDFs actual finds only the mean CCDF, so such a comparison is not possible, only that the (mean) CCDF either satisfies of fails the EPA criteria. I asked him to send me his SAND report on this subject when it is released, I believe this would be an important improvement in our current application of the Sandia methodology.

Steven Hora from the University of Hawaii gave a presentation on the use of expert judgement for predicting scenarios, especially future societies and their impacts on intrusion. He used multidisciplinary expert panels and standard elicitation techniques to address the question "What are reasonable futures between now and the year 12,000?" There were three fundamental outcomes possible:

1. High tech and progress, in which there would be technological increases and a knowledge of the waste maintained. There was also the possibility that the waste would be rendered harmless by engineering or medical advances.

2. Decline of civilization with loss of knowledge of repository and nuclear radiation.

3. See-saw, in which there would be multiple cycles of high-tech civilization and decline.

There would also be in the near term futures the possibility of unstable government in the U.S., or the WIPP site no longer being part of the U.S. Other possibilities would be weather modification, reopening of WIPP for additional waste, drilling for

resources, waste injection wells, underground weapons testing, and surface water impoundments. Another interesting point in the development of scenarios is that the expert panels seemed to weight consequences in the immediate future more highly than those long in the future, although there were no guidelines or rules to that effect.

Lokesh Chataverdi of the New Mexico Environmental Evaluation Group (EEG) commented that the retardation parameters used throughout the performance assessment were based on laboratory equilibrium data, which may not be reliable and could grossly overestimate the retardation in the Culebra fractures. Leonard Konikow of the USGS commented that the concept of a retardation coefficient was not convincing and was a poor indicator of the true behavior of transport in rock. Sandia commented that the dual porosity model is supported with laboratory tracer tests in which there was appreciable diffusion into the matrix. Konikow commented that laboratory experiments may not portend how tracers behave in the field and that there should be a program of field tests with sorbing tracers. Apparently such a program was proposed at one time for WIPP, but rejected by DOE with the support of the NAS panel. Elaine Gorham of SNL commented that DOE is establishing a thermodynamic data base necessary for more sophisticated modeling of retardation and source term.

I commented on the WIPP source term model that I saw no compelling evidence that releases of dissolved radionuclides should be solubility limited. The present model calculates release of radionuclides from the waste form as the product of the inflow and the solubility of the radionuclide. I referred the panel to the source term models for the Yucca Mountain site, in which the release rates might be controlled by the disintegration of the waste form, and only partially by the solubility of the radionuclides themselves. I think I caught them off-guard, and they did not have any good answers for me. I got the feeling however that they have not devoted much effort to a more-sophisticated source term model. In later conversations however, I got the idea that at the very least, the solubility limited model would be conservative, except perhaps for the generation of colloidal species.

Gas pressurization appeared to be one of the biggest concerns at the WIPP site, so they have spent considerable effort in exploring phenomena of gas generation and transport. Gas can be generated by corrosion of metal waste and packages and biological decay. Oxidation of metal would proceed to very high pressures, and it does not appear that the high pressures would inhibit corrosion as once thought. Although most of the metal would be corrosion resistant, it could become embrittled by hydrogen which would destroy the passivating layer, and also crushed by the lithostatic force of the collapsing salt.

Howard Adler of Oak Ridge Associated Universities discussed processes of gas generation by biological decay. Most rapid growth would likely occur within the first few years of burial. He suspected that most gas generation would have already taken place in any such waste before burial. He noted that while microbes can survive harsh conditions, this is not the same as being prolific. He also suggested that bactericides such as copper sulfate would kill microbes. His presentation was refuted however by several SNL people, who pointed to a series of experiments in the 70's that clearly showed the potential for gas generation. Someone also commented that the deliberate burial of strong bactericides would be a potential health hazard, which would have to be treated as hazardous waste.

One of the panel members asked if there has been any study of the possibility of sterilization of the waste before burial. SNL commented that this has been studied, but rejected as infeasible, both because of the high cost and the near-certainty of recontamination. Sterilization of the salt and rock itself would be nearly impossible. One surprising fact is that salt from the repository have bacteria counts of thousands per gram in undisturbed samples. Since the salt is many millions of years old, this is hard to believe, but apparently true. Even if sterilization of the nearby salt were possible, moving fluid inclusions in the salt would certainly bring new organisms into the repository.

Fritz Seiler gave an interesting presentation on multiattribute analysis for the WIPP. This was used to construct a function that could be used to quantify the risks, and to serve as a basis for including personal evaluations into the exercise. He described how this analysis was used to initially rank the 5 HLW sites for the DOE civilian program, but in retrospect doubts that the technique is accurate enough to have distinguished one site from another. He proposed a better way to use the analysis that improved on the techniques used in the civilian program. John Garrick of the Panel commented that he did not like the multiattribute analysis, because it obfuscated the evaluation of performance. He preferred individual deterministic analyses, and commented that differences between sites or engineered alternatives would become obvious to the analyst without multiattribute analysis. Chris Whipple agreed, and felt that multiattribute analysis was not an aid to decision making. Seiler commented that the multiattribute analysis would help make defensible arguments in court to laymen. Chris Whipple commented that he had personal experience with using multiattribute analysis and eliciting expert witnesses in a program to reduce automobile emissions in California. His experience was that diverse groups will agree on values of proposed projects, but almost always disagree on the facts (e.g., the number of health effects caused by automobile emissions).

Charles Fairhurst summarized the Panel's findings:

- They were not very convinced by the studies of future societies presented by Steve Hora.

- With regard to chemical parameters of retardation and solubility, they urged DOE to flesh out these parameters with field data. There is a compelling need for sorbing tracer tests in the field.

- They thought that studies of engineered alternatives still being considered for the WIPP should be integrated into the performance assessments, rather than as parallel but separate analyses. Chris Whipple asked how a second reference design would fare in a performance assessment of the WIPP.

The next performance assessment review by the NAS panel will be March 20-21, 1991 in Washington D.C.

Meeting at Center for Nuclear Waste Regulatory Analysis, Dec. 13-14

I attended the latter part of a meeting at the CNWRA to discuss issues related to iterative performance assessment. I will touch on some of the highlights from my notes and recollections:

We started Thursday morning's meeting with a discussion of how dose assessment models should be used for the Phase 2 modeling effort. Unfortunately, the Center personnel appeared to be relatively unaware that this has been added to the Program Plan. Furthermore, they would need additional dollars or a shift in personnel to work on this new analysis. John Hageman from the Center staff appeared to be somewhat familiar with dose assessment models, although from a nuclear power perspective rather than nuclear waste management. Bob Neel presented an outline of the PATH code, which is part of the SNL methodology package and is in our possession. The modeling seems straightforward in some respects, but coming up with coefficients needed to represent typical land and water uses at the site could be very difficult. Bob pointed out that the PATH code seems to run slowly, and that we might need to optimize it on a mainframe computer. I offered to take care of this need by helping with the transfer to the INEL Cray and possibly getting a contract through IRM to have it optimized as we have done with several other complicated codes.

I discussed the development of the C-14 gaseous release and transport methodology with Ron Green and Bill Murphy. So far, they have only progressed to the literature review stage. I suggested that they follow the work already performed on this subject by Ampter, Behl and Ross draft report (1988). Bill Murphy pointed out that the Ampter model ignored the temperature dependence of the equilibrium coefficients. We rederived the transport equation, adding a term for the temperature dependence. The missing term acts a distributed source term in the model that depends on the change of retardation coefficient with respect to temperature. I

agreed to develop a simplified module for C-14 gaseous release for inclusion in the systems code, as part of the teams working on system code and gas transport tasks. This module would be extracted from the methodology that will be developed at the Center.

Bill Murphy brought out the fact that there might be other volatile radionuclides at the site. Technicium-99 is slightly volatile, but is present in relatively large amounts in fuel and has long halflife. This was the first I heard of any concern over gas phase Technicium, but it bears looking into. If it is important, it should come out in some simple order-of-magnitude calculations.

I spoke to the newest member of the Center staff, Berge Guhregian. Berge is well known in modeling circles, and has considerable experience in transport modeling, sensitivity and uncertainty analyses. I asked him if he could use one of the models he has developed for groundwater transport with chain decay in one and two dimensions to at least serve as a check to the NEFTRAN code that the Center will use for the Phase 2 iterative performance assessment. Berge also agreed to possibly working with the task I head up on sensitivity and uncertainty analysis. I was interested in acquiring his services because of his experience with differential techniques such as the adjoint method.

The final discussion at the Center dealt with the development of the system code. I contributed to this discussion by convincing Budhi Sagar and their team that the system code should not be a single large computer program. This would require using large mainframe computers for testing, and increase the quality assurance difficulties. Instead, I suggested having separate program modules for major tasks, e.g., flow, transport, sampling, CCDF generation, and sensitivity/uncertainty. The program modules would then be used in sequence with a batch program that calls the individual modules in their necessary sequence. They agreed that this was a better approach than their original plan to have a single large program.

Dick
Richard Codell