UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

BEFORE THE ATOMIC SAFETY AND LICENSING BOARD

In the Matter of	)	
	)	
U.S. ARMY	)	Docket No. 40-8838-MLA
	)	
(Jefferson Proving Ground Site)	)	

PREFILED SUR-REBUTTAL TESTIMONY OF A. CHRISTIANNE RIDGE

Under penalty of perjury, I, A. Christianne Ridge, declare as follows: I attest that the factual statements herein are true and correct to the best of my knowledge, information, and belief; and the opinions expressed herein are based on my best professional judgment.

Q.1 Please state your name.

A.1 My name is A. Christianne Ridge.

Q.2 Have you provided testimony previously in this case?

A.2. Yes, this testimony is in addition to my previous testimony in this case. In my previous testimony, I provided my professional qualifications, discussed general issues, and responded to the initial testimonies of STV witnesses. This testimony will now focus specifically on responding to issues raised in the rebuttal testimonies of the STV witnesses.

Q.3. Have you reviewed the rebuttal testimonies of the STV witnesses?

A.3. Yes, I reviewed the rebuttal testimonies of Dr. Henshel, Mr. Norris, and Mr. Pastorick.

Response to Mr. Norris's Testimony

Q.4 Does Mr. Norris's rebuttal testimony address the use of the Kd model? If so, what is your opinion of that testimony?

A.4 Yes. Mr. Norris's first main concern relates to the possibility that the local equilibrium assumption inherent in the Kd model may not be valid. In his rebuttal testimony, Mr.

Norris indicated that I did not "attempt to deal with the issue of peak suppression" that could result if the local equilibrium assumption is not valid. Norris Rebuttal at 47. As I describe below, I did not address "peak suppression" at length because it is the actual peak that is "suppressed" relative to the peak predicted by a Kd model if the local equilibrium assumption is not valid (i.e., a Kd model will overestimate the actual peak concentration if the local equilibrium assumption is not valid).

In my initial testimony, I stated:

> In general, if the local equilibrium assumption is not valid, the arrival of a contaminant will be spread in time so that the contaminant arrives for a longer period of time but the peak concentration is lowered (Valocchi, 1985).

Ridge Testimony at 8.

I stand by this initial statement. If the local equilibrium assumption is not valid, the contaminant will arrive for a longer period of time than predicted by a Kd model, and will have a lower peak concentration than predicted by a Kd model. Therefore, I did not, and still do not, consider the "peak suppression" that Mr. Norris noted, to be problematic because, if a Kd model were used but the local equilibrium assumption were not valid, the Kd model would over-predict the actual peak concentration. Thus, with respect to peak concentration, a Kd model would be more conservative than a model that accurately models non-equilibrium behavior.

In my initial testimony I indicated that the main deleterious effect that could result if the local equilibrium assumption is not valid is that the contaminant could arrive at a location earlier than it is predicted to arrive by a model based on the local equilibrium assumption. Ridge Testimony at 8. I addressed the implications of that possibility in my initial testimony. *See id.* at 8-9. Mr. Norris is correct in noting that I indicated that NRC Staff routinely evaluates peak concentrations that occur after the period of performance. NRC Staff also evaluates the uncertainty in any factors that could move a peak into or out of the period of performance, so a delay in the predicted time at which a peak concentration occurs is unlikely to affect a

decommissioning decision. In general, changes in the timing of the predicted peak concentration are less important to a decision about decommissioning than any effect on the predicted value of the peak concentration. Mr. Norris did not acknowledge my main point about the potential delay in the time of occurrence of the peak concentration. Specifically, the invalidity of the local equilibrium assumption will have a greater effect on when a contaminant arrives at a particular location rather than on the time of the peak concentration. *See id.* at 8. Because the NRC bases its decisions about decommissioning on the predicted peak dose, the potential invalidity of the local equilibrium assumption is not expected to have a non-conservative effect on a decommissioning decision.

In A.025 on page 47, Mr. Norris indicated it was unclear whether I was aware that the Soil Verification Study consolidated all of the soil series into just two groups. When I wrote my initial testimony, I was aware of that grouping. As described in my initial testimony, under-representing variability tends to increase the predicted peak dose (i.e., the predicted peak dose will be greater than the actual peak dose) by decreasing the predicted dispersion of the contaminant. *See* Ridge Testimony at 10. Therefore, I do not understand what specific harm the intervenor believes will result from grouping the soils into two groups.

In his rebuttal testimony, Mr. Norris also referred to a paper I presented at the Waste Management Conference in Tucson, Arizona, February 27-March 3, 2005. ("Ridge *et al*., 2005" ADAMS ML072680021). This paper describes thermodynamic sorption models ("TSM") and how they might be used to model radionuclide transport. The paper also describes situations, similar to the situation of an alkaline plume coincident with a radionuclide plume described in my initial testimony, in which it would be difficult to describe radionuclide transport with a Kd model and a thermodynamic sorption model may provide a more conservative estimate of radionuclide transport. *See* Ridge Testimony at 11-12. As indicated in my initial testimony, this type of complex chemistry does not appear to be occurring at JPG. The paper also describes why, in

some situations, a thermodynamic sorption model may allow a licensee to collect fewer samples to complete site characterization.

I disagree with Mr. Norris's characterization of my testimony as suggesting that "while thermodynamic equilibrium models can with a great deal of effort duplicate the Kd approach there doesn't seem to be any benefit to them." Norris Rebuttal at 48. In my testimony, I did not argue that there are no advantages to using thermodynamic sorption modeling. Instead, I addressed statements made about the Kd model in the intervenor's basis "J", and addressed the implication that the Kd model should not be used because it would not be sufficiently protective.

The paper I coauthored (Ridge *et al*., 2005) is not inconsistent with my testimony, contrary to Mr. Norris's statement at A025. The paper presented ideas for using thermodynamic sorption models at chemically complex sites. The paper states that a thermodynamic sorption model may allow a licensee to complete site characterization with fewer sorption measurements by providing a theoretical basis for predicting sorption based on water and soil characteristics instead of more labor-intensive sorption measurements. My initial testimony addresses whether a model based on a Kd approach can be sufficiently protective, and is consistent with the paper Mr. Norris references (Ridge *et al*., 2005), the conclusions of which are reproduced below.

> Thermodynamic sorption modeling is a tool that can be used to gain insight into the factors controlling radionuclide sorption and transport and to predict the effect of variability or temporal changes in site mineralogical or chemical characteristics on radionuclide transport. In addition, TSMs may be able to provide a means to characterize radionuclide sorption at complex sites more economically, facilitating more realistic modeling of heterogeneity in chemical conditions. Because of these potential benefits, the NRC is participating in projects to develop the use of TSMs in radionuclide transport modeling. Work is underway to develop practical guidance regarding the use of TSMs in conjunction with groundwater transport models and to develop software that integrates TSMs with code used to perform PA analyses. Future work may include efforts to quantify the costs of implementing a GC [Generalized Composite] model and a comparison of the utility of an uncalibrated CA [Component Additivity] approach implemented with conservative assumptions as compared to the common approach of using generic K_d values to represent sorption. Advancements in the fundamental understanding of sorption of compounds onto

natural particles are expected to improve both GC and CA approaches.

These conclusions also reflect the ongoing development of the use of thermodynamic sorption models, and the relative immaturity of efforts to apply this technique to performance assessments to support decommissioning. The complete paper includes a section about work that could be done to support the development of thermodynamic sorption modeling so that thermodynamic sorption models can be applied to performance assessments.

Q.5. Do you agree with all of Mr. Norris's statements about the possible implications of using conservative assumptions and bounding analyses?

A.5. No, I do not. My main areas of disagreement are in Mr. Norris's response to Q.011 and at A.015.

In his response to Q.011, Mr. Norris states that "the use of bounding for one element can cascade through the assessment process, sequentially eliminating or reducing other aspects of characterization." The implication of this statement is that less detailed characterization of one aspect of a natural system will lead to less detailed characterization of another part of the system. In general, the opposite is true. If conservative simplifying assumptions are made in one part of a model, the dose contribution from that part of the model will be overestimated (by definition of the word "conservative"). Thus other parts of the system will be able to contribute less dose before the dose limit is met, and the corresponding parts of the model may need to be more realistic (i.e., less simplified), not less realistic. In the specific example Mr. Norris provides in his response to Q.011, he hypothesizes that the use of a bounding assumption about uranium mobility as described in my initial testimony at A.20 could lead to elimination of the groundwater pathway from consideration. This concern is puzzling because the use of an upper limit of uranium mobility would increase the projected potential offsite dose from the groundwater pathway, thereby reducing the possibility that the pathway could be eliminated from analysis.

In his response to Q.011 and at A.015, Mr. Norris appears to be concerned that a

1 “bounding” result would be assumed to be zero in other parts of an analysis. This is not the
2 case. Instead, as Mr. Norris indicates at A.010, a bounded dose is a “no more than” projection
3 of (i.e., an upper limit on) the potential dose. Similarly, a bounding concentration is an upper
4 limit on the concentration at a particular location. That potential dose (or concentration) would
5 be used in other parts of the analysis. As a hypothetical example, suppose that the dose from a
6 particular pathway is bounded by 0.05 milliSevert per year (5 millirem per year). In the
7 calculation of the total dose, the dose from that pathway would be carried forward as 0.05
8 milliSeverts per year (5 millirem per year), and would not be set to zero or eliminated from
9 consideration. Thus, if a bounding or conservative value is used, it is more protective because it
10 will lead to an assumed higher peak dose to a receptor.
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I, A. CHRISTIANNE RIDGE, do declare under penalty of perjury that my statements in the foregoing testimony are true and correct to the best of my knowledge and belief.

/Original Signed By/

A. Christianne Ridge

Executed at Rockville, MD
This 25th day of September, 2007.