RAS 14490

UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

BEFORE THE ATOMIC SAFETY AND LICENSING BOARD

BEFORE THE ATOMIC SAFETY AND LICENSING BOARD

In the Matter of	)	Docket No. 40-8838-MLA
U.S. ARMY	)	ASLBP No. 00-776-04-MLA
(Jefferson Proving Ground Site)	)	September 18, 2007

PREFILED SURREBUTTAL TESTIMONY OF CHARLES H. NORRIS, LPG IN SUPPORT OF CONTENTION B-1 OF INTERVENOR SAVE THE VALLEY, INC.

TEMPLATE = SECY-028

SECY-02

Q.001 Please state your name.

A.001 Charles H. Norris.

Q.002 Are you the same Charles H. Norris who previously prefiled initial and rebuttal testimony on behalf of Intervenor Save the Valley, Inc., in this matter?

A.002 Yes, I am.

Q.003 What is the purpose of your additional testimony at this time?

A.003 The purpose of my additional testimony is to identify and then address issues being raised or remaining in dispute by virtue of the surrebuttal testimony of the Army and the Staff of the Nuclear Regulatory Commission (Staff) with respect to the Save the Valley (STV) position and my rebuttal testimony regarding the hydrogeology components of the Field Sampling Plan (FSP), including its addenda.

I. Issues Raised or Remaining in Dispute

Q.004 Have you had the opportunity to review the surrebuttal testimony filed by witnesses for the Army and the Staff that address issues to which you testified in your rebuttal testimony?

A.004 Yes, I have.

Q.005 Broadly speaking, what issues do you see being raised or remaining in dispute bv virtue of the other parties' surrebuttal testimonies?

A.005 Army and the Staff discuss four broad issues that warrant reply:

1. The purported ability to characterize adequately the hydrogeology of the JPG DU impact area without collecting fundamental characterization data;

2. Issues related to sampling, analytical and interpretive techniques for DU detection and

migration within and from the JPG DU area;

3. The rationale for the appropriateness of the use of the Kd model to estimate uranium retardation in various earth materials; and

4. Apparent misunderstandings regarding the context and relevance of some technical issues addressed in my rebuttal testimony.

Q.006 Is there any convention you wish to use in your subsequent testimony?

A.006 Yes, there is. For purposes of brevity, in my testimony, I will refer to dose modeling as "RESRAD-Plus" modeling. It is explicitly understood and implied by such reference that the Staff indicates an as-yet unidentified model or procedure may be used to supplement RESRAD, as testified by Staff witness Peckenpaugh, A.7., pp 2-3 in surrebuttal:

> The FSP allows for sufficient identification of the karst features so they may be addressed properly at the decommissioning plan stage. The limitations on RESRAD are a separate known issue. As stated before, groundwater is part of the assessment and RESRAD may be used to address groundwater in areas without karst features.
>
> The karst features are only a portion of the site, and procedures or dose assessment models other than RESRAD can be used to calculate the dose estimates of the potential DU in the water and soil where karst features exist. The purpose of the FSP is to characterize the site by, for instance, identifying karst features. How RESRAD is, or cannot be, used on the karst features is separate from the site characterization provided by the FSP, and instead will likely be addressed in the decommissioning plan, which is informed by the results of the FSP.

Q.007 As a result of reading the surrebuttal testimony have you become aware of any misstatements you made in your rebuttal testimony?

A.007 Yes, in two cases. Staff witness Peckenpaugh (A.12., p 4) correctly points out I erroneously used the word *eastward* in my rebuttal answer A.023. The correct word, and the word I had intended to use, was *westward.* Staff witness Ridge is also correct in her

surrebuttal (A.4. p 1) that I misread her statement from p.8 of her initial testimony regarding *peak suppression.*

II. Issue 1. Hydrogeologic Characterization Data

Q.008 With respect to hydrogeologic characterization data, what issues do you see the Army and the Staff raising or reiterating in different terms?

A.008 The Army and Staff witnesses reiterate two previously advanced and closely interconnected positions in terms that are sufficiently different in nuance or content that further testimony is needed and not redundant. First, the FSP (with addenda) will collect sufficient additional characterization data to supply parameters required by RESRAD-PLUS and thereby justify the approval of the five-year alternative schedule. Second, if some data collected for the FSP (with addenda) as currently defined were interpreted as requiring additional site-specific data, rather than bounding limits, as input to RESRAD-PLUS, modifications to the scope of the FSP could be considered through further addenda or the data collection could even be deferred until the Army prepares its decommissioning plan.

Q.009 Please be specific.

A.009 The hydrologic characterization of the groundwater flow of a site, any site, necessarily includes determining where water flows, how much water flows, how fast water flows, the composition of the water, and how those individual elements change seasonally. Anything less than that is not a hydrologic characterization of the site. The more complex and varied the geology, the more data are required for the characterization. If one wants to simulate the fate and transport of any contaminant, whether under current conditions or speculative future conditions, one needs site-specific hydrologic characterization data for each of these

components.

Each component listed above is a necessary part of a hydrologic characterization, but none is sufficient alone and if any is missing, the characterization is incomplete. Individual activities or sources of data can contribute to hydrologic characterization of more than one of the above components. For example, borings that recover soil and rock materials can be completed as monitoring wells which can, in turn, serve multiple purposes such as allowing water level measurements over time, sampling water quality and quality changes over time, and measuring the hydraulic properties of the materials around the well. However, the installation of monitoring wells, as referenced in Mr. Peckenpaugh's response A.6, p. 2, does not constitute site hydrogeologic characterization unless the requisite data to characterize are collected. It is clear from subsequent testimony that such data may never be collected and will certainly not be collected during the initial phases of the FSP when, in my opinion, it should be.

Q.010 Please explain why you reached this conclusion.

A.010 The Staff, largely through the surrebuttal testimony of Staff's witness Peckenpaugh, clarifies the Staff's narrow concept of characterization as it applies to the JPG DU impact area. That narrow concept includes collecting site-specific hydrologic characterization data only when alternative sources to provide input numbers to RESRAD-PLUS cannot be substituted.

The expectation in the FSP is that karst-related features will include conduits that will dominate the groundwater flow system at the site (*e.g.*, the discussion in the FSP at 5.2 on

page 5-1.) Thus, as I see it, hydrologic characterization will necessarily include characterization of the hydrologic properties of that dominant karst system. But, the Staff sees no such need. As quoted above in A.004, Staff witness Peckenpaugh views the purpose of the FSP as much more limited: "The purpose of the FSP is to characterize the site by, for instance, identifying karst features." According to Mr. Peckenpaugh, the decisions as to which computer model in addition to RESRAD to use to simulate the karst system and whether to collect site-specific data to support that model can be deferred until the Army submits its decommissioning plan circa 2011.

In my opinion, this is a fundamental error. The karst system is central to the hydrology of the JPG DU site. Identifying karst features is a necessary part of the site characterization, but not sufficient in itself to achieve adequate site characterization. Collecting site-specific data to determine the DU transport capabilities of those features is also necessary. And, it very much matters what data is collected where. The few sampling wells installed at locations interpreted as having karst features and the limited data proposed to be collected from those wells under the FSP will simply not be sufficient to identify all the key karst conduits or provide the data necessary to model their DU transport capabilities.

Q.011 Does the Staff rebuttal testimony reveal or reiterate other differences of significance regarding hydrologic characterization data?

A.011 Yes, there is another significant difference regarding the need for and timing of hydraulic conductivity data collection. According to Mr. Peckenpaugh:

> ... However, the measuring or computing of hydraulic conductivity may be optional and is not necessarily needed for adequate site characterization. ... A conservative estimate of hydraulic conductivity may be made instead and will provide for the information needed for a decommissioning plan. ... (Peckenpaugh, A.5., p. 2)

The desire to avoid or defer collecting site-specific hydraulic conductivity data leads to some unusual, cart-before-the-horse decision cycles. For example, there is the response to Q.5. on page 2, in which Mr. Peckenpaugh states, in part:

> The Army must complete well installation and evaluate the groundwater system before they can even decide whether or where to perform the aquifer tests that will be used to determine hydraulic conductivity values.

The determination of the hydraulic conductivity is perhaps the most fundamental property of a groundwater system because it describes the capacity for the system's flow rates and volumes. The evaluation of a groundwater system begins with the determination of hydraulic conductivity; it is not something done here or there as an afterthought.

III. Issue 2. Sampling, analytical, and interpretative techniques to establish DU migration.

Q.012 How would you describe your basic differences between the positions expressed by Army and the Staff in their rebuttal with respect to the sampling, analytical, and interpretive techniques necessary to establish DU migration for the purposes of the characterization needed from the FSP for purposes of developing a site decommissioning plan?

A.012 The fundamental difference is that I express a need for improved sampling, analysis, and interpretation techniques for the FSP beyond those demonstrated by the Environmental Radiation Monitoring (ERM) program from the outset of FSP data collection, whereas the Army and the Staff say I'm wrong on both the timing and the need. Neither the Army nor the Staff have stated expressly what changes in these techniques, if any, should be made but

instead deferred those decisions to later FSP addenda. But both object strenuously to my suggestions that what is observed from the ERM program indicates its techniques need, or could, be improved upon.

I maintain that it is worth substantial extra effort and expense as part of the site characterization effort to identify and quantify DU which is currently migrating within and out of the DU impact area, even at only low concentrations, and, thereby, understand the routes and mechanisms by which DU will continue to migrate in the future. This is because doing so provides data for one of the only independent verifications available for later fate and transport modeling. This is not a typical site. Dozens of tons of chemically active uranium are proposed to be left in place, on and below the surface of the ground. The site geochemistry is complex and, in part, conducive to uranium mobility. The hydrogeology is likely capable of extremely rapid transport of mobile uranium through conduits difficult to characterize, assuming such characterization will be undertaken. Thus, reliable migration data which can be collected now to guide the modelers later is worth the additional effort and expense it will require, in my opinion.

Based upon the limited data from management of the DU site to date, no high-concentration migration from the impact area has been observed in any medium. But, that doesn't mean DU isn't migrating from the site. It is. At a minimum, it is leaving atom by atom, molecule by molecule, indistinguishably mixed with atoms and molecules from natural uranium sources.

That high-concentration migration from the DU impact area has not been observed is a salient point with respect to compliance with the existing permit for both the regulator and the regulated. However, it is minimally significant with respect to the herculean task of reliably projecting peak and cumulative doses and toxicities for 1000 years into the future, with and without institutional controls.

In my opinion, what is significant and relevant is that by far the likeliest interpretation of ERM data from Big Creek collected at the point it crosses the JPG (not the DU impact area) boundary is that DU is identifiable – even if not at levels which are statistically significant using the Army's ERM analysis and interpretation techniques. If DU is present in these samples, that result is very important to reliable modeling in that it provides an existing value observation that the modeling must match. There are an immeasurable number of Kds and soil profiles and bounded estimates that can convincingly calculate that uranium from DU projectiles will never leave the DU impact area in measurable concentrations. Based upon such results, peak dose calculations are trivial. Any one of the no-impact scenarios could be right if there is no measurable DU in existing environmental data. But, if there are, already, measurable concentrations of uranium from DU projectiles leaving the JPG, then the entire modeling process is constrained by that empirical result and the adequacy of the hydrologic characterization does make a crucial difference to the reliability of the modeling's projected results.

Q.013 What is your response to the contention of the Army's witnesses that your interpretation of the ERM sampling results ignores the role of statistical uncertainty

in properly interpreting them?

A.013 There is substantial rebuttal testimony regarding the significance of the spring 2006 sample of Big Creek water where it leaves the JPG facility. The Staff and Army, in witnesses Condra and Anagnostopoulos, respectively, table-knock the issue of propagated uncertainty. In particular, Mr. Condra, when prompted by Staff Counsel in Q.13. on page 7, with my challenge to say there was no DU in that sample, chose instead to answer a statistical question. Mr. Condra concluded,

> The ratio and uncertainty that I calculated was 3.7 +/- 3.7 for SW-SU-002 SAIC05, not statistically above 1 and therefore not definitely identified as DU.

Mr. Condra's evaluation ducked the issue of whether there is DU <u>in</u> the sample and considered instead whether there was sufficient statistical evidence to identify the sample <u>as</u> DU. There are both theoretical and methodological points at issue here, and I disagree with Mr. Condra on both counts.

First, as a theoretical matter, it is ineffective and inappropriate to deal with environmental samples as either DU or natural uranium. Virtually every sample of each medium has the potential to be a mix of DU and natural uranium. So, the appropriate question is not whether the sample is natural uranium or is depleted uranium, but whether and how much DU is in the sample.

Second, from a methodological perspective, I think that this is an issue of null hypothesis selection and testing, and balancing the risks of the Type I and Type II errors of that

selection. To me, the alternative hypotheses relevant for FSP purposes are:

1. There is no DU in the sample; or
2. There is DU in the sample.

The first issue is, which of these two hypotheses is the appropriate "null" hypothesis and which is the "alternative" hypothesis? To me, this is always a question for the investigator – and the answer depends upon the purpose of the investigation and the risks of associated errors.

As a generic process, sampling may be carried out in an attempt to disprove or reject a particular hypothesis, which is defined to be the null hypothesis. In doing so, we give the null hypothesis priority, so it cannot be rejected unless the evidence against it is sufficiently strong. Here, as I see it, STV is approaching this question from a different perspective from that of the Army and the Staff as to the purpose of the FSP sampling, as distinct from compliance monitoring. This in turn results in the (implicit) selection of a different null hypothesis.

I submit that for FSP purposes the null hypothesis should be that there **is** DU in the sample, while the Army and the Staff are assuming the null hypothesis to be that there **is no** DU in the sample. In particular, the Army and the Staff are (implicitly) saying that anytime their selected confidence interval for a sample analysis includes the assumed activity ratio for natural uranium in the particular medium being sampled, then their null hypothesis, that there is no DU in the sample, cannot be rejected. From their perspective, this means that no

additional testing for DU should occur unless and until there are multiple samples for which the null hypothesis may be rejected.

But, at low concentrations of uranium, the analysis technique they are using, alpha spectroscopy (AS), is simply incapable of ever identifying such samples because of the uncertainties inherent in that procedure, as performed using standard procedures at commercial laboratories. And, at least to my knowledge, neither the Army nor the Staff has ever defined a laboratory procedure to test for rejection of the alternative null hypothesis, that there is DU in the sample.

The role which uncertainty plays in AS results is not crucial when uranium concentration is high or the sampling is for ERM rather than FSP purposes (*i.e.*, to determine whether DU is present at levels sufficient to represent a present threat to public health). But, the role of uncertainty becomes crucial when the uranium concentration is low and the test is for FSP purposes. It is crucial because it increases the risk of a Type II error to unacceptable levels. In particular, it significantly increases the risk of a sample which does include DU, first, from even being considered at all and, second, if considered, from being considered as evidence for the presence of DU because the range of uncertainty in the sample's reported results is so large in comparison to the numerical values of those results. Consequently, I continue to maintain that a different set of sample collection, analysis and interpretation techniques from those used for ERM purposes is clearly required for FSP purposes.

The extensive dispute over the interpretation of the ERM data from where Big Creek leaves the JPG is the strongest argument that improved sampling, analytical, and interpretive techniques to be use as part of the site characterization. For something so important, it must not be left in ambiguity. If the FSP perpetuates the use of the existing sampling, analysis, and interpretive techniques, it will perpetuate the ambiguity of the results. Ambiguity at these low concentrations may or may not be important in compliance monitoring, but they rob the 1000-year projection problem of meaningful, constraining data.

Q.014 What is your response to Mr. Condra's and Skibinski's concerns that the analytical techniques you have suggested are "custom"and "not available" in domestic American laboratories?

A.014. The criticisms extended by Mr. Condra and Mr. Skibinski are referenced to analytical techniques used by others to assess aspects of DU migration that escape the methodologies used in the ERM. I have deliberately not proposed analytical techniques because, without appropriate sampling techniques and a common understanding of the purposes of the investigation, simply substituting one analytical technique for another may accomplish little of nothing. Purpose, sampling, analysis and interpretive techniques are tied to the extent that optimizing one without addressing the others does little.

However, with proper adjustments to the other elements for the analysis of environmental monitoring, the techniques of HR-ICP-MS (high resolution, inductively coupled plasma-mass spectrometry) for U-235 and U-238 isotope concentrations and AS for U-238 and U-234 comparisons should be able to provide the precision and analytical breadth to meet the

assessment needs. I have not surveyed the domestic laboratories for those capable and interested in contracting for those services. However, from my own laboratory experience, I would expect that the necessary adaptation of techniques could be secured by negotiated arrangement from one of them or, alternatively, from a European laboratory already employing the required technique. So, I would expect this to be a cost and convenience, rather than availability, issue.

IV. Issue 3. The Kd model for uranium retardation.

Q.015 What is your response to Dr. Ridge's surrebuttal testimony regarding the applicability and appropriateness of using the Kd model to compute uranium migration rates through earthen material underlying the JPG DU site?

A.015 I disagree with several points raised by Dr. Ridge in surrebuttal.

First, I disagree with the opinion she reiterates in surrebuttal (page 3 in surrebuttal; page 10 in initial testimony) regarding the potential implications of the Soil Verification Study, which lumped the mapped populations of soil series into two series for purposes of determining Kd values to be used for predicting uranium retardation. Dr. Ridge asserts that "under-reporting variability tends to increase the predicted peak dose (. . .) by decreasing the predicted dispersion of the contaminant." This tendency is only true for under-reported variability along a particular flow path from a source to a receptor. Further, although it impacts the peak dose, it doesn't significantly impact the cumulative dose to an ecosystem over long periods of time. The difficulty with lumping the soil series into just two is that, on the site, each series exists in a location that is independent of other series. DU within each soil series migrates in response to the soil properties of that soil series by way of the

pathway connecting that soil series to its particular set of receptors. The existence of other soil series with other retardation properties is irrelevant to the performance of a single series. Eight soil series will, on site, have eight peak dose curves, but, at best, only two of which would be properly characterized by the FSP.

Second, I disagree with Dr. Ridge at several points with respect to her discussion regarding alternatives to the Kd model. In particular, I take issue with her statement on page 3 of her surrebuttal, "As indicated in my initial testimony, this type of complex chemistry does not appear to be occurring at JPG." This statement was made immediately following a discussion of the difficulty describing radionuclide transport with a Kd model in the situation of an alkaline plume that was coincident with a radionuclide plume. It is unclear why Dr. Ridge believes the JPG DU impact area site does not have the kind of complexity she has just described, both as it exists today and as it will exist indefinitely into the future. Today, the shallow soils on and within which the DU penetrators lie covers a carbonate aquifer that is itself a natural, continuous, extensive carbonate plume. Heads and water levels in the shallow portions of this aquifer are reported to rise quickly in response to rain events, providing the opportunity for alkaline aquifer water to episodically wash lower portions of the mantling soils. In the future, there is the scenario of the failure of institutional controls to consider. For the JPG DU impact area, that scenario involves a resident farmer on the DU impact area itself. Farming in this part of Indiana typically involves applying liming agents to the soils to raise and stabilize pHs. Anhydrous ammonia is applied as fertilizer, a strong base. Farming practices would generate an annual alkaline plume that would move

vertically through the DU-containing soils on-site. As Dr. Ridge does correctly point out, that is not the kind of complexity that lends itself to Kd modeling as a conservative approach.

V. Issue 4. Apparent misunderstandings by Army and Staff of previous direct and surrebuttal testimony.

Q.016 Please clarify and provide examples of what you mean when you indicate there may have been misunderstandings by Army and the Staff with respect to the context and relevance of your direct and rebuttal testimony.

A.016 There are several places in surrebuttal testimony where testimony indicates that, for whatever reason, what I said was apparently unclear. Several of these need to be addressed to make an unambiguous record.

There is apparently ambiguity in my discussion regarding the need to look for and, if found, characterize karst elements that do not coincide with vertical features that can be identified on aerial photographs and have resistivity anomalies where they cross roads at the perimeter or within the site. It is known that there are near-surface caves that were invisible to the photo/electrical technology pair used to select well locations in the vicinity of Big Creek. Comparable features away from Big Creek would be similarly invisible, and even more so if they occur at depth. These features may be forming contemporaneously, they may have formed millions of years ago, or they may be a hybrid of contemporaneous activity modifying old networks.

I did not presuppose that a deep feature is necessarily an old (paleo) karst feature, although

that seems to have been the understanding Mr. Peckenpaugh unfortunately gleaned (A.11.,p. 4) from my testimony. Similarly, the significance of the paleo-karst features I described that are observed at the surface is not that they are shallow where observed, but that they are hundreds of millions of years old. At other sites in that area, similarly-aged paleo-karst features are found at depth (*e.g.*, Plotnick, 2007, reference in my rebuttal testimony). I apparently did not convey this significance to Army's witness Skibinski who appears to perceive (p. 8, second paragraph) the significance as just the shallow depth, not the integration of contemporaneous (active) karst processes with paleo-karst systems.

In my rebuttal testimony I called attention to work with DU by the United Nations in central Europe which not only analyzed U238/U234 activity ratios but combined those with ICP-MS methods to compute a percentage of DU in a given sample. Mr. Skibinski (p. 4, A8) inferred from this testimony that I was implying ICP-MS was required to identify DU, when I intended to call attention to the fact that samples that were confirmed to have significant percentages of DU also had activity ratios substantially below the ratios reported in ERM samples which the Army and the Staff are willing to dismiss due to propagated uncertainty.

Mr Skibinski takes exception (p. 6, A9 (1)) to my testimony regarding a paper by a former SAIC employee given at a karst conference, in response to criticism by Mr. Snyder (A.38), regarding the use of GPR as a geophysical device for karst. Mr. Skibinski interprets my testimony as my implying endorsement by the author of the paper for the technique, whereas my comments exclusively focus on the endorsement of the GPR method by ASTM

consensus ranking, as reported in that paper.

Mr. Skibinski testifies at some length about the data to which I testified regarding MW-09, a well whose water elevations appear consistently below adjacent Big Creek. Mr. Skibinski correctly surmises I estimated the creek level from the USGS topographic quadrangle map of the area. Further, that methodology and underlying assumptions were described in the first disclosure materials provided by STV (Mws_DU_hds.pdf) to all parties, as well as in discussions to which the Army's witnesses were party. I would note that, contrary to the thrust of Mr. Skibinski's surrebuttal testimony, MW-09 was designated by SAIC after a survey of monitoring wells as a well that could appropriately be used for water elevations (SAIC, 2004).

In his A12, Dr. McLaughlin perceived my rebuttal testimony to tie DU to biota impairment described by other researchers in the cave system underlying the DU site. I have re-read that testimony and find only a reference to the location where the reported impairments were found. The researchers themselves suggested additional research to determine whether this impairment has resulted from groundwater contamination associated with the Army's past use of the site.

In his A14, Dr. McLaughlin provides a statement from my rebuttal testimony that he asserts is incorrect in its reference to and paraphrase of a portion of my initial testimony (p. 79, end of question 74), regarding fractionation. I have reread both the rebuttal reference and the

original testimony and I am at a loss as to how Dr. McLaughlin can read the original text as pertaining to anything other than the U238/U234 ratios associated with site data and how fractionation may affect them and their interpretation.

The Staff's witness Peckenpaugh apparently perceived my testimony regarding the need to reposition stream gauging stations as an issue related to data quality. Mr. Peckenpaugh is correct (surrebuttal, A.14. p.5) that, after appropriate rating curves are developed, the data quality will not be significantly different that the data quality at existing gauging locations. However, the need for relocated gauging locations is not that the data quality of the existing locations is poor, but that the data from the locations cannot properly be used for the stated purpose of establishing recharge rates for the aquifers if there are losing reaches in the drainge upstream of the existing gauging location.

Q.017 Does that conclude your surrebuttal testimony?

A.017 Yes, it does.

VI. References

Plotnick, Roy, 2007, *Exceptionally Well-Preserved Sub-Absaroka Paleokarst and Lower Pennsylvanian Fill: a Window into Early Pennsylvanian Paleoenvironments*, Paper No. 28-1, Joint South-Central and North-Central Sections, Geological Society of America, 41st Annual Meeting, at University of Kansas, Lawrence, Kansas, April 11-13, 2007.

SAIC, 2004, *Responses to the Nuclear Regulatory Commission May 20, 2004, Request for Additional Information Regarding the Environmental Monitoring Program Plan, Final*, Submitted

to U.S. Department of Army, Installation Support Management Agency, Aberdeen Proving Ground, Maryland, prepared by Science Applications International Corporation, November, 2004

VII. Verification

I declare, under penalty of perjury, that the foregoing testimony is true and correct to the best of my knowledge, information and belief.

Date, October 1, 2007