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LETTER AND COMMENTS ON BEHALF OF SEACOAST ANTI POLLUTION LEAGUE
REGARDING DRAFT ONSHORE AND OFFSHORE CONTAMINANT FATE AND TRANSPORT
MODELING PHASE 2 REPORT NSY PORTSMOUTH ME
2/19/1999
LEPAGE ENVIRONMENTAL SERVICES

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February 19, 1999

Peter Vandermark
Seacoast Anti-Pollution League
P. O. Box 1136
Portsmouth, New Hampshire 03802

Subject: Review Comments, *Draft On-Shore/Off-Shore Contaminant Fate and Transport Modeling Phase II Report*

Dear Mr. Vandermark:

We are transmitting comments to the Seacoast Anti-Pollution League (SAPL) concerning the December 1998 *Draft On-Shore/Off-Shore Contaminant Fate and Transport Modeling Phase II Report* prepared by Tetra Tech NUS, Inc.. The Navy's intent is to use the modeling to evaluate current on-shore contaminant migration, based on data collected in 1996 and 1997, to the adjacent marine environment. The results of the modeling will be used in support of developing and evaluating alternatives for remedial actions. This report describes the results of the second phase of modeling. The bulk of the review was performed by Dr. Charles Hebson and his comments are enclosed. Additional comments are as follows:

1. Future Modeling Using New Data. The second phase of the modeling built upon Phase I results by incorporating site-specific groundwater and seep/sediment data collected in December 1996 and April 1997. Thus Phase II should provide a more realistic representation or simulation of actual site conditions, resulting in better input to the decision-making process regarding future actions at the Shipyard. The report states (page ES-1, PURPOSE, SCOPE, and OBJECTIVES) that "The intent of the modeling effort is to evaluate continuing [perhaps current would be a more accurate descriptor] on-shore contaminant migration to off-shore receptors." While we realize that Phase II is complete (with the exception of responding to comments and finalizing the Phase II report), what are the Navy's plans to incorporate new data generated during investigations (such as at Topeka Pier or Site 29) and/or future monitoring into the modeling effort before the modeling results are used to evaluate remedial alternatives? It would be helpful to include at least a general statement of the Navy's plans in both the Executive Summary and the Conclusions and Recommendations (Section 6) sections.

2. Description of OU5 Conditions. There are passages in the report that present confusing information about OU5. The model focused on groundwater-seep/sediment relationships at the shoreline. We understand that, for the purposes of the model, sediments were not considered at OU 5 because the shoreline is a seawall that presumably had no sediment immediately adjacent it

February 19, 1999

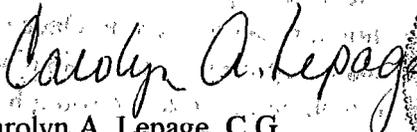
Phase II Modeling Report Comments

(and no seeps?). Both the Phase I and Phase II modeling (see pages 2-10 and 3-2, for example) considered that OU5 was not a **current** (emphasis added) source of heavy metal contamination to sediments. Yet contaminated sediments exist in the vicinity that may be attributed to releases from OU5. That fact is acknowledged on page 2-10, which states that metal contamination in nearby sediments may be the result of past releases from OU5, or from other past or present Shipyard sources or other non-Shipyard sources elsewhere in the estuary.

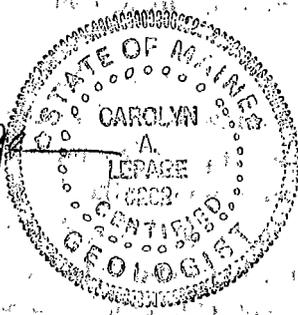
The confusion results from statements such as in the second bullet on page 6-2 that states, "Based on site conditions (seawall), OU5 sediments are not a concern.", or on page ES-2 (BASELINE MODEL RESULTS, first paragraph), that only surface water is of concern at OU5. For the purposes of presenting the model results, these statements appear to be accurate. However, they ignore the potential impact of OU5 on the quality of nearby sediments. Many readers, particularly those who are not modelers and especially those reviewing the Phase II report several years in the future when feasibility study and remedial action decisions are being made, are likely to focus on the Executive Summary and the Conclusions and Recommendations (Section 6) sections of the report. Therefore, we think it is critical that the statements regarding the lack of concern for groundwater and sediments in relation OU5 clearly state that they are made for the purpose of reporting the modeling results, and do not represent full characterization of potential contributions, particularly historic impacts, of OU5 to offshore media quality.

If you have any questions regarding the comments above, please give me a call at 207-777-1049.

Sincerely,

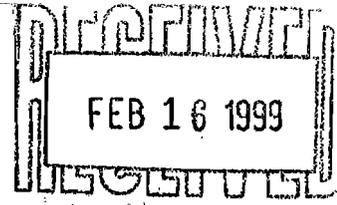


Carolyn A. Lepage, C.G.
President



Enc.

- cc: Iver McLeod, Department of Environmental Protection
Meghan Cassidy, Environmental Protection Agency
Charles Hebson, C.G. & P.E.
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26 January 1999

Carolyn Lepage
Lepage Environmental Services
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Subject: Review of PNS Draft Phase II Modeling Report

Dear Carolyn,

I have reviewed the PNS Draft Phase II Modeling Report by Tetra Tech NUS (TtN). The rationale for the underlying analysis has already been established and this report represents refinements to work presented in Phase I. I see the major improvements upon Phase I as being

- improved estimates of sediment K_d
- incorporation of deterministic and probabilistic sensitivity analysis for selected parameters

The writing is rather dense, to be expected of a highly technical report. I believe that a thoughtful and complete reading is required before attempts are made to extract, analyze and apply findings.

Major Finding: No Surface Water Impact, Likely Sediment Impact

The major finding to come from this analysis is that surface water impacts are likely to be less than the screening criteria, ranging from minimal to unmeasurable. TtN correctly point out that this finding will be compared to results from other avenues of investigation before any final decisions are made regarding remedial actions. Concomitant with no surface water impact is a likely impact on sediment quality. This impact is of at least moderate rank, on the order of sediment quality screening criteria. This would seem to call for follow-up work on sediment impact, once other investigations and analyses are completed.

Qualitatively, it is fairly easy to understand this outcome. "Conservative" (low) values of soil K_d were employed, which had the effect of producing high COC levels in ground water discharging to surface water as "porewater". These high porewater levels, coupled with "conservative" (high) sediment K_d values, removed relatively large amounts of COCs from porewater and onto the sediments. The resulting levels of COCs in surface water, in equilibrium with the amounts on sediment, were correspondingly low. These high sediment K_d values were based on recently acquired data and therefore have some factual basis.

The sensitivity analysis was useful in establishing the reasonableness of the conclusions of the modeling exercise. In particular, it is reassuring to see that the baseline cases correspond to the high end of the range of expected values. For surface water, even these high values are less than the screening levels. For sediments, these values as well as data, are on the order of the screening criteria.

Exposure Scenarios and Future Actions

The analysis suggests that even those parameters that would produce the highest impact on surface water still produce COC levels below screening levels. If other data support this contention, then future actions regarding surface water may not be necessary. As noted above, the sediment results are much closer to screening levels and further consideration is likely. No mention is made of soil and ground water levels. While this report is intended to address migration of COCs to surface water, the soil and ground water issues should at least be mentioned briefly. Some questions:

- are the calculated ground water COC levels used in any way, except as loadings for offshore contamination?
- has significant exposure to soil and ground water already been ruled out as a health concern?
- have remedial measures already been instituted to correct soil and ground water problems?

These questions are probably already addressed elsewhere. Yet typical of a large and complex project, this report lacks a sense of context. While not wanting to repeat what has gone before, this work ought to be tied into the larger context of contamination at PNS.

Comments

The analysis documented in the Phase II report is consistent with earlier work and reflects incorporation of planned improvements by TtN and previous review comments offered by SAPL and MEDEP. As such, there are no substantive comments to be offered. My biggest general concern is that of parameters or data that exhibit very large ranges of variation. TtN has been forthright in their treatment of these data items, in particular through sensitivity analysis. As a practical matter, focusing on the tails of the generating distributions for assessing "worst case" situations seems to be about the best we can do, given the study framework.

In addition to this general concern, I have several additional points that ought to be considered.

- Who did the actual work in this report? This is a major effort and the authors and analysts involved should be noted. An brief introductory section listing the contributors and their roles should be included. The implications of this report are too important to stand as an anonymous work behind the signature of a single project manager who may or may not have had a significant technical role in the analysis.
- In the Executive Summary, parenthetical references to sections where particular ideas are developed should be included. As noted above, this report is difficult to scan. These

references would be useful for follow-up study after a complete initial reading.

- In the Executive Summary (P. ES-7) general reference is made to distinction between site-related and non-site COCs, but the particular COCs are not specified. Some specificity should be included in the Summary and reference to the detailed section should be included. This is important since it seems that this idea may be employed to avoid PNS responsibility for certain COCs.
- P. 2-3, 1st paragraph: "... was not intended to be used as a human health or ecological risk assessment". This statement needs some clarification, as clearly the model is used in some fashion as a means for assessing exposure (and thereby, risk). Is this statement a categorical rejection of using the modeling for exposure and risk assessment? It is stated (P. ES-6) that the modeling is intended to support on-shore/off-shore feasibility studies, but ultimately there would seem to be some implicit connection here between the modeling and exposure/risk.
- P. 2-8 and other locations: The reasons for using two different sensitivity analysis approaches needs to be clearly stated. Obviously, the authors concluded that ground water source conceptualization was best attacked through the deterministic side, while model parameters were treated by Monte Carlo. This should be explained: why couldn't everything be done via Monte Carlo (or for that matter, deterministically)?
- Regarding deterministic sensitivity analysis. It appears the rate of increase in the ground water source term is a particularly sensitive aspect of the model. Now that it has been identified, is there any way (independent of the data that suggests steady state) to assess whether such increase is likely? This is an important point, applicable to all of the critical, sensitive parameters.
- P. 3-3, Table 3-1. This table is missing concentration units.
- P. 3-5. Throughout the report, the term "porewater" is used. It took me some time to figure out that "porewater" was water in the pores of the sediment at the bottom of saline intertidal areas, and not ground water. (The term "porewater" can also be used to describe soil water and ground water, and thus my confusion.) Therefore, this term needs to be clarified early in the report. Likewise, the meaning of seep water needs to be stated clearly and related to ground water.
- P. 5-22, last paragraph. Reference is made to "assumed distribution functions...". Some mention should be made of the basis for these distributions, whether assumed according to findings in the literature or fit to available site data. Apparently, lognormal distributions were fitted to a number of the parameters (see Appendix D for K_d fitting). The text should also say how good a fit was achieved. Special attention should be paid to the tails of the distributions, as they drive the simulation of the extreme events. What is the assumption of uniform distributions based on?

- P. 5-47, last paragraph. This is wishy-washy. The Phase II sediment K_d values are improved in the sense that they are based on real porewater and sediment concentrations. But what exactly are these "limitations and/or weaknesses"? Generic statements such as these do not belong in this report. These weaknesses are either worth identifying and documenting, or they are not worth mentioning. I suspect the former, in which case the implications of these weaknesses need to be identified. Presumably, the Monte Carlo procedure compensates for the uncertainty in K_d estimation. But this needs to be made explicit in any discussion of weaknesses in estimating this critical parameter.
- Appendix A, p. A-2. It is stated that since most of the COCs are lognormally distributed, it is reasonable to use geometric means to calculate baseline K_d values. This seems to infer that the ratio of two lognormally distributed quantities is also lognormal, hence the use of geometric means. This can be demonstrated analytically (mathematically) if it is true, or empirically, if this particular data set justifies it. All too often in environmental work, the lognormal assumption is employed without much thought. This point should be clarified. Also, brief reference is made to statistical analysis in Appendix D. At least a very brief summary of the findings should also be included, to provide some context for this chapter. As it stands, too much "flipping back and forth" is required to provide context for various statements (this is true of much of this report).
- There seems to be a huge range for sediment K_d values. Is this to be expected? Even though the K_d values have been statistically analyzed (for which a better discussion is required), no attempt has been made to explain this variation. This variation has been addressed through the sensitivity analysis, but are there any further implications of this variation for the general modeling approach employed?
- Hydrogeologic parameters were not subject to Monte Carlo analysis, since it was felt that they were known with relative certainty as compared with other model parameters. However, given the "simplicity" of model and the model's "suitability" for Monte Carlo analysis (characterizations given on p. 5-22, first paragraph), Monte Carlo analysis of the hydrogeologic data (flow velocity, or conductivity, hydraulic conductivity, porosity, and gradient) should also be performed for completeness. The additional effort would be relatively small and it would add to the completeness of the analysis.
- Appendix D. This requires some general discussion of results of the statistical fitting. Right now just the raw analysis sheets are included. Just how good is the fit? Also, the data points and fitted distribution curve should be graphed on probability scale paper for each fitted distribution, with appropriate parameters and numerical results posted on the graphs.

Concluding Remarks

The Phase II Draft Modeling Report is carefully prepared, complete and for the most part well written and organized. I found nearly all of the information I was looking for, with greater or lesser effort. Given the agreed upon modeling strategy and available data, the findings are reasonable. The

principal findings, those of possible sediment impact and no surface water impact, will be verified by other avenues of investigation. I hope this review is of use to you and SAPL. I would be happy to discuss this with you further.

Yours truly,

Charles S. Hebson
Professional Engineer and Certified Geologist

