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LETTER WITH NATIONAL OCEANIC AND ATOMOSPHERIC ADMINISTRATION COMMENTS ON MINUTES FROM PARTNERING TEAM MEETING FROM 12 NOVEMBER 2001 NAS PENSACOLA FL
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NOAA

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U.S. DEPARTMENT OF COMMERCE
National Oceanic and Atmospheric Administration
NATIONAL OCEAN SERVICE
OFFICE OF RESPONSE & RESTORATION
COASTAL PROTECTION AND RESTORATION DIVISION
c/o U.S. Environmental Protection Agency, Region 4
Waste Management Division
61 Forsyth Street, Atlanta, GA 30303

MEMORANDUM

TO: NAS Pensacola Partnering Team

FROM: Tom Dillon, Ph.D.

SUBJECT: NOAA Comments on Site 2 RI Addendum NAS Pensacola

DATE: January 23, 2002

CC: Lynn Wellman, EPA ETAG Coordinator

The U.S. Department of Commerce, National Oceanic and Atmospheric Administration (NOAA) appreciates the opportunity to comment on Draft Remedial Investigation Report Addendum, Site 2 Waterfront Sediments, Naval Air Station Pensacola, Florida, prepared by EnSafe Inc., Memphis for NAVFAC Southern Division, November 12, 2001. If you have any questions, please contact me at 404-562-8639, FAX 404-562-8662 or tom.dillon@noaa.gov.

1. The report's conclusions and recommendations (§5.0) are not technically supported.

Section 5.0 of the report concludes that, based on comparisons to 1996 data, conditions at Site 2 "continue to improve" as evidenced by: 1) decreasing surface sediment chemistry constituents, 2) no acute and minor sub-lethal effects in the sediment bioassays, 3) "vastly improved" benthic community, 4) SEM:AVS data. Based on these conclusions, the Navy recommends No Further Action at Site 2.

These conclusions and the ensuing recommendation do not appear to be supported by results in the report. Moreover, substantial uncertainties, not discussed in the report, greatly decrease the confidence associated with a No Further Action recommendation at this time. Technical results and important uncertainties are discussed below.

2. **Sediment Chemistry**

The conclusion that sediment chemistry at Site 2 "continue to improve" is not apparent from the results presented. Rather, the data suggest levels of sediment contamination in 2001 were more or less the same as that observed in 1996. This observation of minimal change between 1996 and 2001 is not unexpected for the following reasons: 1) the Industrial Waste Water Treatment facility was installed over 25 years ago, 2) major contaminant inputs presumably ceased at that time and 3) the system has likely reached some level of equilibrium.

a. **Table 3-1 "Chemistry Comparison 1993-2000"** (§3.1.1), appears to be the primary basis for the report's conclusion that site sediment chemistry has diminished. However, this table has limited value for temporal comparisons and may possibly be misleading for two reasons. One, in most instances, there are no data available for temporal comparisons; note the preponderance of "No Data" entries. Two, when data are available, the *highest* historical value was used. This comparison biases results towards a conclusion of *decreasing* sediment concentrations. (What station is represented by the data at the top of page 3-5, Table 3-1?)

Consider taking a broader, site-wide contaminant-specific approach for evaluating temporal trends in surface sediment chemistry. For example, one could compare all 1996 cadmium surface sediment concentrations to all 2001 surface sediment cadmium results. Then one could statistically test the null hypothesis that there is no significance difference in cadmium concentrations between 1996 and 2001. There appear to be sufficient data in both years to provide a rigorous statistical comparison. Repeat this comparison for all site COPCs including perhaps ER-Ms. One would then have a technically sound basis for concluding whether surface sediment chemistry at Site 2 is increasing, decreasing or staying about the same.

b. The **subsurface sediment chemistry** data in Appendix B are not presented nor discussed in the main body of the report. Not determining the vertical nature and extent is a major uncertainty in this (or any) RI. The lack of vertical nature and extent represents a very large data gap for risk managers who must consider the feasibility of remedial alternative.

To fill this data gap, consider taking the broader, site-wide contaminant-specific approach described above for evaluating temporal trends surface sediment chemistry. However, instead of comparing 1996 and 2001, compare surface (0-6 inches) and subsurface (>6 inches) data. Differences in core lengths as well as speculations regarding compressed core samples can be discussed in the uncertainty section. Without this delineation, one

will not have any insight regarding potential self-capping processes, sediment mixing or if deeper material is more contaminated than surface sediment. These findings would also significantly affect the feasibility of remedial alternatives.

c. **Table 4-4** accurately describes the four categories of presumptive toxicity based on sediment guidelines developed by NOAA and FLDEP (Correct citation is missing in the report.). However, the Site 2 sediments appear to have been misclassified. For example, no comparisons to FLDEP guidelines are made. All Site 2 sediments have one or more chemicals which exceed FL TELs. For that reason alone, no sediments can be placed in Category I as the subject report has done. Table 4-5 shows ERM exceedences as well as Total and Mean ERM quotient. Similar tables should be constructed for FLDEP TELs and PELs as well as NOAA ER-Ls before Site 2 sediments can be properly categorized.

d. **Sediment organic carbon** was to be analysed in all samples. However, the data were "lost in the analysis process" (§3.1.4). A value of 0.1% TOC is assumed based on the 1996 data. If accurate, sediment-associated contaminants must be assumed to be highly available for uptake by biota. Consequently, the NOAA and FLDEP guidelines (which are generally based on silty sediments) become *less* conservative. Is a value of 0.1% reasonable given the low sand/high fine combinations shown in Table 3-3? Can we evaluate grain size results between 1996 and 2001 to compare differences/similarities and thus, the reasonableness of the 0.1% TOC assumption?

e. The **Total PAH** expressions in Table 3.8 of §3.1.3.2 in Appendix B should be brought forward into the main body of the text. These results may help explain some of the observed toxicity. Is there a land-based source for these elevated PAH concentrations?

3. **Sediment Toxicity**

a. Survival results in both the amphipod and mysid toxicity tests have been **"normalized"** for variations in the multiple control treatments (Tables 3-5 and 3-6). The "normalized" results are then carried forward into the sediment quality triad analysis. This "normalization" adjustment should be omitted for several reasons. One, this data manipulation was not specified in the Work Plan where statistical analysis of the toxicity data is described. Two, adjustments for control survival is typically not conducted for simple sediment bioassays. Rather, the control treatment is used as a performance standard to judge whether the test itself is valid. Normalization may be appropriate for very large programs such as EMAP but not for simple hypothesis testing like Site 2.

Three, "normalization" was not consistently applied. Survival data were "normalized" but growth and reproduction results were not. As a result, the triad analysis contains both "normalized" and non-normalized results ("apples and oranges"). Four, the normalization procedure diminishes the clarity of the report. "Normalized" survival is greater than 100% in a number of the sediment treatments. These anomalous results will likely elicit questions from readers.

b. No true **sediment control** was used in the Site 2 sediment bioassays. The control treatment for sediment bioassays consists of substrate the test organisms were either cultured in or collected in. The Site 2 bioassays used neither. Rather, an uncharacterised field-collected sediment was used to "dilute" sediments for the mysid bioassay and serve as the sole basis for all statistical comparison in both sediment bioassays. Almost nothing is known about the control sediment. No chemistry or grain size information is reported. Even the collection date and location are unclear. At a minimum, the location for collecting these sediments should be shown on a map. The lack of information regarding the sediment used as the control treatment is a potentially significant source of uncertainty.

c. **Multiple control treatments** were used in both the amphipod and sediment bioassays. As long as the variance among controls is small, the corresponding uncertainty associated with multiple controls is also small. This uncertainty should be evaluated in the subject report. In addition, control results for the both the amphipod and mysid bioassay tables are not reported (Tables 3-5 and Table 3-6, respectively). Results from the multiple control treatments should be reported in these tables as they are the sole basis for statistical comparisons.

d. **Growth** data for amphipods and mysids were not statistically analysed if survival was significantly affected. The triad analysis requires input from both the lethal and sublethal responses. Therefore, all sublethal data should be statistically analysed regardless of survival.

e. **Mysid reproduction** was measured during the laboratory bioassay (see contractor report in Appendix C) but was not reported in the main body of the report (see mysid summary Table 3-6). Table 3-6 should report results of the statistical analysis of both reproduction endpoints; i.e., percent females and percent females with eggs. A statistical analysis of all mysid reproduction endpoints is required by the triad analysis (see top of page 4-2, §4.1.1 and Table 4-2).

f. **Feeding regime** for the two sediment bioassays requires clarification. The Work Plan indicates test organisms were to be fed (see Tables 1 and 2 in Appendix A). The contractor's reports in Appendix C are silent with regards to feeding. EnSafe has

verbally indicated mysids, but not amphipods, were fed. The contractor's report should be revised to indicate what feeding regime was actually used in both bioassays.

g. Considerable effort was devoted to evaluating the toxicity of **"diluted" sediments** in the mysid bioassay (Appendix C). What was the value of these additional test treatments and how are the results used? The DQO portion of the Work Plan indicates these data would be used to determine NOECs and LOECs (Figure 1, Appendix A).

h. **Control chart with the positive reference toxicant** suggests amphipods used to evaluate the toxicity of Site 2 sediments were the most resistant ever tested by this contract laboratory (see contractor's report in Appendix C). This finding, and its potential impact on characterizing Site 2 sediment toxicity, should be discussed in the report.

4. **Benthic Community Analysis**

Changes observed in the benthic community between 1996 and 2001 do not appear to warrant a conclusion of "vastly improved". The small differences observed may simply reflect normal seasonal variability. The 1996 samples were collected in winter (January 1996) while the 2001 samples were collected during the spring biological recruitment period (March 2001). This alternative alone could explain the observed differences and should be discussed in the report. Also, the potential influence of collection methodologies on differences observed in 1996 and 2001 should be noted and discussed.

5. **SEM:AVS**

The molar ratio of simultaneously extracted metals to acid volatile sulfides (SEM:AVS) can be used as a theoretical measure of bioavailability for five divalent metals (Cd, Cu, Ni, Pb, Zn). Theory predicts that at ratios less than 1, AVS in pore water will bind all 5 divalent metals sufficiently to reduce or eliminate biouptake and thus toxicity. At ratios greater than 1, insufficient AVS is present to bind all metal present in pore water and toxicity may be expressed.

The SEM:AVS model was developed largely with silty (high TOC) sediments. Because the report has not TOC results and has assumed a very low TOC (0.1%), some discussion should be devoted to appropriateness of using the SEM:AVS model in low TOC sediments.

The subject report is misleading when it concludes, "bioavailability of metals in the sediments is limited based on AVS SEM data" (page 5-1). An equally plausible alternative interpretation is that when SEM:AVS ratios exceed 1 (as they do in three locations, Table 3-2), amphipod growth or survival is adversely affected (Table 3-5).

6. **Other Comments**

a. A good **base map** should be prepared for this report. No figure currently exists that shows the relationship between the 1996 grid locations and the 2001 Decision Unit locations. Such a map would clarify temporal trend analysis. It would also greatly facilitate communication of results (e.g., where was toxicity observed). The base map must show the location of the submerged seawall. Once plotted, the seawall may emerge as an important feature for interpreting RI results and managing risks at Site 2.

b. **Be consistent in station location nomenclature**. Text, figures and tables all vary between grid designations (e.g., CD-23) and Decision Unit designations (e.g., DU 11) when describing station locations. This greatly diminishes the clarity and transparency of the document. This reviewer suggests grid designations are illuminating, especially when displayed on a good base map.

c. In Figure 1-1, indicate which location is reference station 18 and which is station 22.

d. In Tables 3-4 and 3-7, indicate the meaning of the range of values shown under "*Notes:*". Clarify what is meant by "* = Stations with species diversity overlap" in Table 3-7. Does it mean that stations with asterisks are different from other stations, or not different from each other, or what?

e. Report a variance term (SD or SE) for each mean toxicity value in Tables 3-5 and 3-6. Also, report means and variances for the multiple controls.