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MOFFETT FIELD
SSIC NO. 5090.3

MONTGOMERY WATSON

June 28, 1996

Mr. Stephen G. Chao, P.E.
Engineering Field Activity West
Naval Facilities Engineering Command
900 Commodore Way, Code 1813SC
San Bruno, CA 94066

Contract No: N62474-88-D-5086 CTO 0326 File: 2738.1346/3.1.2/6.9

Subject: Responses to Agency Comments on the Moffett Federal Airfield Draft Phase II Site-Wide Ecological Assessment

Dear Mr. Chao:

Enclosed please find five (5) copies of the Responses to Agency Comments on the Draft Phase II Site-Wide Ecological Assessment Report.

If you have any questions, please contact us at (510) 975-3400.

Sincerely,

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RESPONSE TO COMMENTS FROM THE U.S. ENVIRONMENTAL PROTECTION AGENCY ON THE DRAFT PHASE II SITE WIDE ECOLOGICAL ASSESSMENT REPORT DATED OCTOBER 8, 1995

GENERAL COMMENTS

Comment No. 1:

There are mistakes of serious magnitude in the report concerning the presentation of the data, the analysis of the data, and the interpretation of the data.

The report is written without careful attention being paid to the techniques used and agreed to by all parties which results in serious deficiencies in interpretation of the data. This is most obvious for the echinoderm test results, where the consultant concludes lower impact to the developing larvae for the sample SSNC-18 compared to SSNC-19, i.e., the LC_{50} f or SSNC-19 showed that the pore water for this sample was more toxic. This was observed even though the chemical concentrations at sample SSNC-18 was higher than those for SSNC-19. This is the reverse of what would have been expected if the chemistry is causing the response for this test. The contractor uses this apparent "flip-flop" of chemistry and response results to state that there is little if any relationship between these two samples and more importantly, no relationship between the chemistry and the results of this bioassay. The original sample from SSNC-18 had a salinity that had to be adjusted with the addition of concentrated brine to perform the bioassay tests, whereas SSNC-19 did not. The chemistry was apparently completed on the original sample rather than the sample that was diluted. Comparing the chemistry results from the original sample to the bioassay results from a diluted sample is not reasonable with the expectation for explaining a cause and effect relationship.

Response:

Comment noted. The relationship between the bioassay data and chemistry data, in particular the echinoderm data and pore water chemistry data, was reevaluated, and the results were presented in the Draft Final Phase II Site Wide Ecological Assessment (SWEA) report.

See response to EPA specific comment number 19.

Comment No. 2:

There has been some misinterpretation of data in the report.

There is a progressive loss of severity each time the FETAX data and results were transferred to subsequent clients. The authors of this document appear responsible for the misrepresentation of the data produced by The Stover Group that clearly and obviously shows a significant impact to mortality (statistically and we believe

biologically) and to development i.e., malformation of the FETAX larvae over the duration of the test. More detail is provided in a specific comment.

Response: See response to EPA specific comment Nos. 20 and 31. The Navy believes that the FETAX data were, in general, accurately characterized in the document. In response to EPA's comments, the Navy's interpretation of the FETAX data was clarified for the draft final report.

Comment No. 3: **The report comprises a bare minimum of information with little or no interpretation of the potential risk, especially within the framework of the ecological risk assessment process.**
The test results are presented in Chapter 9 with few literature references to an interpretation of the results. Eisler documents are cited at least six times and AQUIRE at least once. However, no data are presented from the citations to bolster the position taken by the authors. The results are not integrated into the overall approach whereby they explain or attempt to evaluate the measurement and assessment endpoints. Nowhere in Chapter 9 are any results of bioassays mentioned in the context of addressing the measurement endpoints. It is in the characterization phase that we would expect to see an evaluation of the results of the bioassays and the chemistry with respect to the measurement endpoints in area extent and severity of impact. Once this phase is completed, statements can be formulated (through a weight of evidence) to support the level of impact to the assessment endpoints.

Response: The food chain modeling was incomplete at the time of the submittal of the Draft Phase II SWEA Report. Weight of evidence conclusions could not be developed without this information.

The Draft Final Phase II SWEA Report contains an expanded risk characterization in Sections 8.0, 9.0, 11.0, and 12.0. The interpretation of the bioassay results has been expanded and revised and the risk characterization now contains an evaluation of the results of the bioassays and the chemistry with respect to the measurement endpoints. This information is presented in Sections 9.1 and 9.2. Sections 11.0 and 12.0 of the document present a discussion of the spatial extent of effect, severity (in other words, magnitude) of effect, and the weight of evidence conclusions of the risk assessment. This discussion is organized by medium to facilitate decisions regarding remediation.

Comment No. 4:

There is a lack of supportive literature for an interpretation of the data presented.

There is an overall lack of toxicity literature presented to help explain the responses of the bioassay data, except for the publication Long and Markel (1992) and this report is very limited and often misused to support tenuous positions. In several sections of the document, there are references to an observed low abundance of benthic invertebrates except for dipterans, oligochaetes, and water boatman in the storm water retention ponds. Possible causes of this situation are offered as "the natural, annual, or seasonal environmental fluctuations at MFA render the habitat unsuitable for sustaining diverse populations of macroinvertebrates; and macroinvertebrate populations have been adversely affected by exposure to COPECs." The only possible explanation for this phenomenon is stated in the next paragraph: "In the San Francisco estuary, factors that are expected to influence the diversity of macroinvertebrates include grain size (SFEI, 1993) (no data presented on grain size effects), sediment and water quality such as the concentration of ammonia and hydrogen sulfide (EPA, 1986a [the only citation presented in EPA, 1986, Quality Criteria for Water 1986], SFEI [again without data], and ABAG, 1991 [Association of Bay Area Governments, Status and Trends Report on Wetlands and Related Habitats in the San Francisco Estuary][another citation without presentation of data]). The few citations that are offered do not relate to your position because the document fails to integrate the available data into the text to support the position being offered.

Response:

Comment noted. Please see Navy response to EPA general comment number 3. In addition, each of the main points referenced in EPA's *Framework for Ecological Assessment* (EPA 1992) are discussed in Sections 9.0 through 12.0 of the Draft Final Phase II SWEA report.

See response to EPA specific comment numbers 27 and 30.

SPECIFIC COMMENTS

Comment No. 5:

Section 2.3.4.2, Page 2-11. The criteria for receptor choices are listed here. One criteria that was in the workplan (page 2-11) is missing, specifically, that the receptor is a key component within one of the food chains. Was this intentionally removed?

Response:

The text in the draft final report, Section 2.4.2, page 2-23, was revised to include the criteria:

"a key component within one of the food chains (for example, an abundant prey organism for other species within the food chain)."

Comment No. 6: **Section 4.2.2.4, page 4-16, para 4. This paragraph states that the "flux ponds were constructed for the treatment of industrial waste water." Please describe the treatment technique performed here. Also, physically describe the ponds. Were they lined or unlined? What is NASA's schedule for completion of closing the ponds? Once the closure of the ponds is complete, what will be done with the land? Is the intention of the remediation to improve the quality of the habitat?**

Response: The flux ponds were unlined and were used to contain aircraft wash water prior to characterization and disposal. A pretreatment system was designed for the flux ponds but not used because the water was found suitable for discharge into the sanitary sewer. The Navy has closed the flux ponds and the closure activities are summarized in a report which has been sent to EPA and RWQCB for review. The report title is "Preliminary Draft Project Completion Report, Closure of Two Flux Ponds, Moffett Federal Airfield." U.S. Navy. December 1995. Closure activities included:

- Incoming lines were flushed and converted to storm drains.
- Water was pumped from the flux ponds and stored in holding tanks pending analytical results. Analytical results were within limits acceptable by the City of Sunnyvale for disposal into the sanitary sewer.
- Sludge and underlying soil were excavated and disposed off site. Excavation ceased when groundwater was encountered less than 1 foot below the bottom of the ponds.
- Clean fill was imported and placed in the former ponds. The former levees were graded so that the ground surface gently slopes away from the center of the former ponds.

Comment No. 7: **Section 4.3. This section is very well written. It provides a clear summary description of a difficult technical subject.**

Response: Comment noted.

Comment No. 8: **Section 5.1, page 5-2. It states here that only certain COPECs were chosen for figures because they are likely to be representative of contamination from anthropogenic sources and known bioaccumulators. This reduced data set should be increased to include all COPECs on figures.**

Response: The Navy is assessing the risk associated with exposure to all COPECs. However, the number of figures that can be included in the report is limited. The Navy presents a representative group of figures

in Section 5.0 to illustrate the nature and extent of sediment contamination. This set of figures was designed to facilitate the review of the Draft Final Phase II SWEA sediment data against the Phase I conceptual site models. The figures serve as a basis for text descriptions of the chemical contamination organized by geographical areas. Nature and extent information was reviewed for all COPECs in wetland sediment and upland soil. As a matter of practicality, the Navy included COPEC figures in Sections 9.0 and 11.0 only for chemicals that were identified as major contributors to ecological risk.

Comment No. 9:

Section 5.2.5, page 5-11, first bullet. Manganese appears to be the only metals COPEC that does not have data for .5 to 3 feet. What is the reason for this?

Response:

There are only six data points for manganese in sediment samples between 0.5 and 3 feet. As described in the attachment to Appendix G, data used for the SWEA were derived from multiple sources, including NASA. In general, sediment samples obtained by the Navy during the Phase I and Phase II SWEA were collected in the 0.0- to 0.5-foot-depth range. Samples collected by NASA were generally collected within the 0.0- to 3.0-foot range and at greater depths. The NASA investigations were purposive in nature and the target analytes for metals were typically lead and mercury, and occasionally silver, copper, and the suite of total or priority pollutant metals. Manganese was not typically analyzed. Therefore, the majority of the samples for manganese were obtained by the Navy during the Phase I and Phase II SWEA and were collected at the 0.0- to 0.5-foot-depth range.

Comment No. 10:

Section 5.7, page 5-20 through 5-22. The conceptual site model presented here does not consider the following factors which would profoundly influence the rate of flow of groundwater from the Northern Channel to the salt ponds:

a. Potential for underflow beneath the levee. It is standard practice to quantify seepage beneath a barrier, such as a dam or a levee during the design phase. The conceptual model presented here assumes no underflow or seepage, a situation that could not exist unless the levee was tied to an aquitard at depth. It is recommended that the Navy perform a flow-net analysis of seepage beneath the dam on the basis of the design of the levee and the hydrogeologic characteristics of its foundation. See the attachment to these comments from Groundwater by Freeze & Cherry for an elementary discussion of this concept.

b. **(i) Potential for discontinuities, such as desiccation cracks or root holes, that would act as conduits for groundwater flow and contaminant transport; (ii) potential for piping where upward seepage at the toe exceeds downward forces of groundwater flow through the dam. It is recommended that the condition of the levee be inspected to determine if discontinuities exist or if slope failures are present that would indicate piping.**

c. **There is a possibility for error in the assumption of the hydraulic conductivity of the levee materials. If possible, (i) test the hydraulic conductivity in the laboratory through the collection of soil cores; (ii) confirm the lab test through comparison of Unified Soil Classification System texture to published conductivity values and; (iii) perform tracer tests to further confirm (i) and (ii).**

Until these issues can be addressed, the assertion that the "migration of these COPECs pose an insignificant risk to the salt pond sediments" can only be considered an unsubstantiated hypothesis.

Response:

The Navy agrees that the analytical model presented in Section 5.7 would benefit by the collection of additional field data to address EPA concerns. However, collection of the requested data would be costly, would impact the schedule for completion of the Phase II SWEA report, and may not be warranted if groundwater flow across the levee is not in the direction of the salt pond. Because the fate of chemicals in Northern Channel sediment is entirely dependent on the hydraulic gradient direction, the Navy will monitor water levels in the Northern Channel and salt water evaporation pond (salt pond) quarterly for 1 year before resources are allocated for the collection additional field data. The Navy will also attempt to obtain historical records of water level fluctuations and water quality from the owner of the salt pond.

The purpose of the monitoring program will be to determine hydraulic gradient direction between the Northern Channel and the salt pond. A staff gauge will be located in the Northern Channel near the Navy fuel pier bridge. A second staff gauge will be located just across the levee in the salt pond. These gauges will be surveyed to a common datum and water level measurements taken quarterly as part of the groundwater monitoring program. In addition, water level measurements will be taken during storm events to determine the effect that heavy surface water runoff in the Northern Channel may have on hydraulic gradient direction.

The Navy will consider additional field data collection, to address factors a, b, and c of the EPA comment, if surface water level

monitoring or historical records indicate that the hydraulic gradient direction through the levee is toward the salt pond.

The text has been modified to emphasize the uncertainties associated with this simple analytical model including the lack of lithologic data from the levee, the possibility of secondary permeability features, and the lack of water level information for accurate hydraulic gradient information.

The following paragraph has been added to the discussion of model results in Section 5.7 of the Draft Final Report:

"5.7.3.1 Model Result Uncertainties

A few important assumptions about the properties of the levee material and groundwater flow were made for this modeling effort which can only be validated through the collection of additional field data. These assumptions are listed below:

1) The model assumes that levee material is homogeneous and isotropic with a uniform hydraulic conductivity. Several features, if present, would increase the rate of transport through the levee, these include:

- A stratigraphic horizon of higher hydraulic conductivity (silt, sand, or gravel) at shallow depth beneath the levee.
- The presence of secondary permeability features in the levee including desiccation cracks, root holes, animal burrows, etc.

2) The model assumes a hydraulic conductivity value typical of clay. It is possible that the hydraulic conductivity for levee material used in the model is lower than actual site conditions. Laboratory testing of the levee material would yield a more accurate value for hydraulic conductivity.

3) The model assumes a large hydraulic gradient in the direction of the salt pond. However, both the magnitude and direction of the hydraulic gradient between these surface water bodies is uncertain at this time. To better define the gradient direction, the Navy will install staff gauges in the Northern Channel and salt pond to monitor water levels for 1 year, including stormwater runoff events. In addition, the Navy will attempt to acquire historic records of water level fluctuations in the salt pond.

If monitoring or historical records indicate that the gradient direction is toward the Northern Channel, then the assumptions used in the model do not require further scrutiny, since chemical transport would not occur in an upgradient direction. If however, the gradient direction is toward the salt pond, as assumed in this model, then the parameters used in this model could be improved through collection of additional field data, specifically lithologic data from within and beneath the levee."

The last sentence of section 5.7.3 in the draft report has been removed. This sentence read as follows:

"The model results indicate that migration of these COPECs pose an insignificant risk to salt pond sediments."

Comment No. 11: **Chapter 6 was intended to present the dose calculations to be used with the yet to be determined TRVs in the HQ calculations. Why were these not provided?**

Response: Dose equations were provided in the draft report. Exposure assumptions were provided for agency agreement in the draft report and in a meeting prior to submittal of the draft report (August 21, 1995). Doses calculated using the described methods are presented in the Draft Final Phase II SWEA report.

Comment No. 12: **Section 6.2.3.3, page 6-12, para 2. Sentence 2 states "For the COPEC metals that were detected...." Please list these metals, as done for the metals that were not detected.**

Response: The draft report was substantially revised to produce the draft final report. The reference text has been deleted.

Comment No. 13: **Section 6.3.7. This section appears to be a well balanced description of uncertainties. We appreciate the fact that opinions were left out and only facts included in this section.**

Response: Comment noted.

Comment No. 14: **Section 9.0. This chapter provides a <u>verbal</u> description of the data with regard to the potential effects of COPECs on the habitat and receptors. In order to provide a <u>visual</u> description of the potential effects, a suggested improvement is to provide a series of maps of the ecological areas (per receptor) that indicate these potential effects to receptors at various sites. Different colors could indicate the gross types of effects: severe, adverse and no adverse effects. For example, adverse effects are unlikely for polychaetes in the Navy storm-water retention pond (page 9-13). This could be**

plotted on a polychaete map with a single color indicating no adverse effect in the Navy storm-water retention pond. The same method would be repeated for other areas on the polychaete map and then repeated per receptor:

a. Avian Receptors - Black-Necked Stilt, Great Blue Heron, American Kestrel, Mallard Duck

b. Special Status Receptors - Burrowing Owls, Salt Marsh Harvest Mouse

c. Sediment Receptors - polychaetes, echinoderms, FETAX, amphipods

Response: The Navy agrees that including visual aids improves the understandability of this difficult subject matter. The draft final report provides tables of all COPEC-specific and receptor-specific hazard quotients. As a matter of practicality, the Navy only provided maps of a subset of COPECs that appear to be responsible for the majority of the risks.

Comment No. 15: **Section 9.0, page 9-1. The Navy presents a very limited view of risk characterization (page 9-1, paragraphs one and two) compared to the Risk Forum (USEPA, 1992) definition: "Risk characterization uses the results of the exposure and ecological effects analyses to evaluate the likelihood of adverse ecological effects associated with exposure to a stressor. It includes a summary of the assumptions used, the scientific uncertainties, and the strengths and weaknesses of the analyses. In addition, the ecological significance of the risks is discussed with consideration of the types and magnitudes of the effects, their spatial and temporal patterns, and the likelihood of recovery. The purpose is to provide a complete picture of the analysis and results."**

Response: Comment noted. Please see Navy's response to EPA general comment number 3. In addition, each of the main points referenced in EPA's *Framework for Ecological Assessment* (EPA 1992) were discussed in Sections 9.0, 10.0, 11.0, and 12.0 in the Draft Final Phase II SWEA report.

Comment No. 16: **Section 9.0, page 9-1. Where in the test do the authors compare and contrast the "risk characterization" to "Hill's criteria for causality"?**

Response: Hill's criteria for causality were implicitly considered in the Draft Phase II SWEA report. For the Draft Final Phase II SWEA Report, the Navy included an explicit comparison of results with Hill's criteria for causality in Sections 10.0 and 11.0.

Comment No. 17:

Section 9.3.1.1, page 9-3, para 2, Polychaetes. Polychaetes were intended for use in evaluating the potential for bioaccumulation, not acute effects. See comments in correspondence, Appendix A, page 3, Polychaete Bioaccumulation, where a lengthy discussion is related to the question of "to depurate or not depurate" regarding the "polychaete bioaccumulation assay."

Response:

Section 9.2 is part of the risk characterization for the aquatic sediment receptors. The Navy commissioned a separate polychaete bioassay for growth and survival in addition to the bioaccumulation assay at the specific request of the regulatory agencies. See:

1) The Summary of Resolutions from January 9, 1995 Meeting Between Navy, Regulatory Agencies, and Montgomery Watson, Phase II Site-Wide Ecological Assessment (SWEA) Work Plan and Phase I SWEA Report, Moffett Federal Airfield. Growth and survival endpoints for polychaetes are listed.

2) Phone conference between the regulatory agencies, PRC, and Montgomery Watson on March 22, 1995 when the agencies specifically requested that a growth endpoint be included in the 28-day bioaccumulation assay.

3) Specific comment No. 3 from Laura Voloppi/DTSC Human and Ecological Risk Section (HERS) on the Draft SWEA Work Plan dated March 28, 1995; "The revised Table 3-14 includes a growth endpoint for this bioassay for the subchronic test; we agree. The laboratory..."

No changes were made to the document in response to this comment.

Comment No. 18:

Section 9.3.1.1, page 9-3, para 2, Polychaetes. The statement is made that, "The lack of observed bioaccumulation [for certain contaminants] suggests that the COPECs may not be bioavailable [i.e., capable of being detected in tissues of the particular receptor]". Because a contaminant is not detected in the tissue levels does not suggest that the COPECs are not bioavailable. Many compounds that are bioavailable are not bioaccumulative, but very toxic.

Response:

The Navy agrees with EPA's statement that many compounds that are bioavailable are not bioaccumulative, but very toxic. In sediment bioaccumulation tests with benthic invertebrates, tissue residues of compounds with a high bioaccumulation potential, such as PCBs, chlorinated pesticides, and some PAHs, would be expected if the chemical is bioavailable. The absence of mortality—a test requirement—and absence of significant COPEC tissue residues, as

observed in the polychaete bioaccumulation tests on MFA sediments, indicates that the benthic organisms may not be exposed to highly bioaccumulative COPECs. The information also indicates that ingestion of COPECs adsorbed to sediment particles may be a minor exposure route.

See also response to DTSC Comment No. 26.

Comment No. 19:

Section 9.3.1.2, page 9-3, Echinoderm. The authors state, "...a significant portion of the adverse effects observed was not attributable to the COPECs." The echinoderm test results are discussed in Section 8.4.2.2 where both of these samples from the Northern Channel were shown to be toxic for both acute and chronic endpoints (shown in percent of total tested):

	SSNC-18		SSNC-19		
	acute	dev.	acute	dev.	
LOAEC	68		34	25	12.5
NOAEL	34		17	12.5	6.25

Clearly, something in the pore water was toxic to the echinoderm larvae, as these results are very dramatic in the individual samples. The differences between these two samples may be the differences due to the method used to estimate the LC_{50} and the EC_{50}, a technique that does not use a straight line, as was necessary when the data are observed as shown. When the two adjoining concentrations produce a complete loss of organisms as shown, the method used to estimate the median value is very important.

The linkage to a chemical cause is more problematic. The data presented for the pore water and the bulk chemistry (Table 8-5) shows a generally confusing array of data. Whether or not one is more toxic than the other is immaterial; however the cause of toxicity must be related to some aspect of the sample. Page 8-8 states that, "A blind duplicate submitted for SSNC-18 showed excellent agreement." Where are these data reported?

The comparison of these data and toxicity results to amphipod data from Long and Markel (1992) may not be appropriate. More relevant data, i.e., echinoderm data, should be presented. Several questions about these data need to be answered:

a. Are these data validated?

b. What are the chances that the samples were inadvertently switched?

c. **Why is SSNC-18 shown with the top concentration at 68 percent compared to sample SSNC-19 at 100 percent?**

d. **Wasn't SSNC-18 diluted with brine to bring the salinity up to a required test condition?**

e. **From what sample were the chemical measurements taken? The original, undiluted sample?**

f. **Were the chemicals measurements made in the unadjusted sample (for salinity) and the bioassays performed in the brine adjusted sample?**

g. **Were any chemical measurements made in the diluted series of SSNC-18 or were these dilutions based on nominal concentrations?**

h. **Where are the ammonia and sulfide data presented?**

Response:

The EPA raises several important points about attempting to identify stressors responsible for adverse effects in sediment bioassays. The Navy agrees with EPA's comment that one or more stressors in the sediment pore water adversely affected the sand dollar larvae, and that relating the adverse response to the stressor(s) is a desirable objective. Relating adverse responses in bioassays to sediment chemicals is very difficult (1) when a sample contains multiple toxicants, and (2) when so few data (in this case, 2 bioassays had pore water chemistry) are available with which to determine an association. In the draft final SWEA, the Navy presented a weight-of-evidence approach for identifying stressors that might be responsible for adverse effects to the sand dollar larvae. Based on the synoptic pore water chemistry and pore water bioassay data, the possible stressors include PCBs, several heavy metals, ammonia, and sulfides. The weight-of-evidence indicates that ammonia, sulfides, and copper most closely explain the adverse effects.

EPA also asked several questions about specific aspects of this information. These questions are answered below:

a) The analytical data have been validated. The bioassays followed the QC procedures as outlined in the protocols (see the appendix of the Toxscan report).

b) The samples were collected two days apart which suggests that an error in the field was unlikely. The Navy is confident that a sample identification error was unlikely.

c) As noted in the draft document Section 8.3.2, page 8-5, first paragraph, the salinity of SSNC-18 and the duplicate were increased with brine which resulted in a dilution of 68 percent being the highest concentration tested. SSNC-19 did not require a salinity adjustment.

d) See c) above.

e) No chemical measurements were made on the dilution series. Additional text has been added to the draft final version to clarify that COPEC measurements were made in the unadjusted (for salinity) samples only. In the draft report, the single discussion estimating concentrations (Section 8.4.2.2, Page 8-9, last paragraph) clearly states that the concentrations were nominal. "The nominal concentrations of ammonia and sulfide at the echinoderm NOAEC for mortality were calculated. The concentrations..."

f) See e) above.

g) See e) above.

h) Text has been added to the draft final version, Section 8.3.1.3, to discuss the pore water ammonia and sulfide results shown on Table 4-3. All analytical results were presented in Appendix D of the draft report and are presented in Appendix C of the draft final report. The Navy elaborated on the role that ammonia and sulfide may play in the pore water toxicity tests.

Comment No. 20: **Section 9.3.1.3, page 9-3, FETAX Bioassay. The results of this test actually had 17 percent mortality and 11 of the remaining embryos had an average of 13.3 percent ± 6.8 percent malformations. The statement that "...no reduction in normal development was observed" is incorrect and a serious misinterpretation of the test results.**

To substantiate this point, consider these events. The FETAX data and results were presented to ToxScan as produced from the Stover Group as, "The combined mortality and malformation rates for sample RS0015 were 17 percent and 13.3 percent, respectively" (page 3 of letter from Dr. Douglas Fort to Dr. Ray Markel, Appendix F). ToxScan reported the data, "As a result of exposure to SSWL-22 sediment, there was no significantly decreased embryo growth nor was there significantly increased larval malformation when compared with embryos exposed to artificial (control) sediment. Larval mortality was 17 percent in SSWL-22 and 4 percent in the control sediment; this represents a statistically significant increase in mortality in sample SSWL-22" (page 14 of the ToxScan Final Report to Montgomery Watson, Appendix F).

Finally, this document states on page 9-3 that "A 17 percent reduction in survival and no reduction in normal development was observed." All three of the reports from each consulting firm had the data summaries for both mortality and malformations. The authors of this document should have spoken to Dr. Douglas Fort, if there was any question about the interpretation of the test results. There is a progressive loss of severity each time the information is transferred to each client. The SWEA Phase II author is responsible for misrepresentation of the data produced by The Stover Group that clearly and obviously shows the significant impact to mortality (statistically and we believe biologically) and to development, i.e., malformation of the larvae over the duration of the test.

The citation provided, Zug (1993) is misrepresented as presented in the material on page 9-3. From page 259 of the cited text, a discussion of population characteristics includes a discussion of survivorship and mortality with respect to a population structure. Four hypothetical survivorship curves are presented in the textbook where it is stated that most amphibians with indirect development and turtles have Type III survivorship curves, which is best represented by a "rectangular concave curve" where mortality is extremely high, e.g., approximately 90 percent or more in the early life stages and then abruptly reverses to low mortality less than 1 percent for the remainder of the cohort's existence (Zug, 1993). This information in no way can be used to suggest that 17 percent mortality and 13 percent malformation is "...unlikely to be significant for frog populations." If the mortality was as high as 90 percent for even a few cohorts of amphibians at Moffett Field and further increased by the presence of contaminants that produced mortalities as high as 17 percent or more, there would be few left after a few breeding seasons. In our opinion, the FETAX test results are significant and demonstrate a problem for reproducing amphibians at Moffett Field and therefore we do not agree with the interpretation of these data.

Response: The Navy generally believes that the FETAX data were appropriately characterized in the draft document on page 9-3. The Navy contacted Dr. Fort who confirmed that the discussion presented in Section 9.0 was an accurate summary of the results. While the Navy believes that the text was generally appropriate, a more specific discussion was added to the draft final report to address the EPA's overall concerns.

The following table was developed based on the conclusions beginning on page 3 of Dr. Fort's letter.

Endpoint	SSWL-22 (RS0015)	Blasting Sand Reference	Statistically Significant Difference?
mortality	17.0%	4.0%	Yes
malformation	13.3%	9.4%	No
growth	0.92 +/- 0.00004 cm	0.94 +/- 0.00005 cm	No

Text in the Draft Final Phase II SWEA was clarified to indicate that the reduction in survival was statistically significant, and that the increase in malformations was not statistically significant. Interpretation of the FETAX data, and conclusions drawn from the data, are based on statistically significant effects.

The Navy reevaluated the biological significance of the FETAX results. The Navy understands that the EPA's 30 percent criteria for significant effects applies only to interpreting the amphipod results (see letter from Michael Gill/EPA to Stephen Chao/Navy dated September 8, 1995; Appendix A of draft report). The Navy also evaluated the biological significance of the other bioassay results. The ecological significance of these results were also considered. The risk characterization in the draft final report was based on a weight-of-evidence consideration of these results and the food chain modeling results. As part of the weight-of-evidence, the strengths and weaknesses of each method and the biological and ecological significance of the results were considered.

Comment No. 21:

Section 9.3.1.4, page 9-4, Amphipod Bioassay. We agree with the general statement that amphipods exposed to MFA sediments would be expected to have reduced survival. Some of the observed mortality levels, i.e., as low as 44 percent survival, are significant and therefore represent a real problem for the sediment infauna.

Response:

Comment noted. The Navy agrees that amphipods exposed to certain MFA sediments showed reduced survival.

The significance of the amphipod as an MFA receptor and its potential exposure to and sensitivity to chemicals at MFA are part of the evidence evaluated in the risk characterization. The risk to sediment infauna as a group are evaluated through a weight-of-evidence approach that includes consideration of COPEC bioavailability, significant exposure routes, COPEC concentrations, and amphipod bioassay results.

Comment No. 22:

Section 9.3.2, page 9-4, Deviation of MFA NOAECs. Three potential Moffett Field NOAEC's are presented.

1 - amphipods in the Northern Channel;

2 - amphipods in the storm water retention ponds and diked marshes; and

3 - polychaetes site-wide

There is little, if any justification for using polychaetes over amphipods for the evaluation of sediment concerns at Moffett because these receptors provide a broader more comprehensive assessment opportunity. Furthermore, a single receptor species is seldom adequate for the evaluation of sediment habitats, especially in areas that could provide quality habitat for both resident and migrating wildlife. The measurement endpoint of mortality for sediment organisms must be evaluated along with biological uptake for food items utilizing the site. The strategy should include as many potential prey and resident sediment organisms in an effort to evaluate the assessment endpoint, in this case, aquatic resources of wetlands.

It is erroneous to try to justify the use of polychaetes as "more appropriate" representatives of the sediment habitat compared to amphipods using a limnological text (Wetzel, 1983) and an aquatic entomology textbook without presenting any data to support this position. Textbooks such as "Aquatic Insects of California" (Usinger, 1956) and "An Introduction to the Aquatic Insects of North America" (Merrit and Cummins, 1984) may have information about the distribution of insects in these habitats, whereas a literature search would definitely provide relevant literature.

The statement that the amphipod NOAEC "may not be appropriate because they are based on a reduction of amphipod survival that could not be correlated with COPECs" (page 9-5, para 3) seems to be contradictory to the data presented in Appendix F. Some of these data, however, are difficult to decipher. For example, it is not clear to us how the Spearman's rank correlation was set up and completed. What are the correlation matrices presented for PAH/SEDIMENT? With respect to percent survival and various contaminants, there are several correlation coefficients that appear to be significant at the 0.05 to 0.2 range of probability values. For instance, the Spearman's Rho and the P-value for several contaminants are as follows:

Contaminant	Rho	P-value
copper/AVS	-0.505	0.1106
cadmium/AVS	-0.591	0.0617
lead/AVS	-.568	0.0688
mercury/AVS	-.580	0.1551
nickel/AVS	-.618	0.0506
zinc/AVS	-.441	0.1632
benzo(g,h,i)perylene	-.406	0.1850
pyrene	-.420	0.1803
aluminum	-.509	0.1074
antimony	-.700	0.2252
beryllium	-.484	0.1258
magnesium	-.452	0.1527
mercury	-.530	0.0940

The mention of the need to have a "control sediment" and the inability to locate one (page 9-6) has been discussed at our scoping meetings. That is the reason we suggested performing a dilution series in order to establish a chemical gradient and a response that is below the no observable adverse impact level that can be identified along this gradient. Both the NOAEC for polychaetes and amphipods should be used in this assessment because they provide information that will increase the breadth of evaluation for the sediment habitat. These data are needed for different measurement endpoints.

When these NOAECs are compared to ER-Ls and ER-Ms, they may or may not be greater, which only demonstrates the site specific nature of the derivation of these values. If we wanted to use the ER-Ls as screening benchmarks, we would not have suggested performing bioassays at this time.

Response:

See response to EPA general comment number 3. The Navy has substantially revised the approach to the interpretation of the bioassay results. Because a large number of COPECs were included in the assessment and limited bioassay samples were collected, the bioassay results are now presented in the context of validation of risk estimates. A summary of the bioassay results with respect to impact on assessment endpoints is presented in Sections 10.0 and 11.0.

EPA's comments are worthy and underscore the difficulty of correlating bulk sediment toxicity data with bulk sediment chemistry to identify chemical stressors. This problem has been widely discussed in the scientific literature, and based on the current approaches for assessing sediment toxicity and developing sediment quality guidelines, the general consensus seems to be that this issue should be approached differently. For example, please see the December 1994 report

Determinants of Sediment Toxicity in San Francisco Bay, written by Erika Hoffman, Susan Anderson, and John Knezovich (published by the Lawrence Berkeley Laboratory). The authors reported few cases for which sediment toxicity was clearly attributable to increasing bulk sediment concentrations of chemicals. They concluded that future evaluation of sediment toxicity should incorporate measures of the bioavailable fraction of sediment contaminants. These findings are consistent with Long and Morgan's (1991) *The Potential for Biological Effects of Sediment-Sorbed Contaminants Tested in the National Status and Trends Program,* and the conclusions (1) embody the focus of EPA's Environmental Research Laboratory in Newport, Oregon, which is spearheading research on the marine and estuarine sediment toxicity (see, for example, Swartz and others [1995]) and (2) are consistent with the foundation underscoring the use by EPA (1993) of the equilibrium partitioning theory to develop national sediment quality criteria for the protection of benthic organisms.

For MFA, the attempt to derive NOAECs and correlate them with bulk sediment chemistry information was, at best, marginally successful and resulted in some conflicting results and equivocal conclusions. This may reflect the inherent difficulties described above. The Navy revisited the approach for characterizing the ecological risk of sediments at MFA. The Navy understands that there is no absolute method for evaluating sediment toxicity, so based on the available information, the Navy decided to (1) compute sediment hazard quotients for both bulk sediment and sediment pore water, (2) forego use of bulk sediment-based NOAECs, and (3) identify risk drivers from the synoptic pore water bioassay and chemistry data and identify correlations between pore water data and bioassay results. In addition, AVS data was used—in a general fashion—to evaluate the potential risk of metal COPECs. Implicit in the revised approach is the assumption that the COPECs in the pore water represent those COPECs that are bioavailable. The revised approach and the results were detailed in the Draft Final Phase II SWEA report.

Comment No. 23:

Section 9.3.3, page 9-6, Comparison of MFA Samples to MFA NOAECs. By the comparison of the NOAEC$_a$, we can see a range of results for each of the metals. It appears that almost ten percent of all of the samples were greater than this benchmark for manganese and nickel; up to almost 95 percent of the MFA samples were greater than the benchmark for antimony. The benchmarks should be shown on a distribution map for the contaminants as the results presented do not provide a sufficient visual summary. Based on these results, it appears that amphipods would be impacted (survival) at more than 25 percent of the sample locations. What amount of area does this represent? Although a smaller number of metals are indicated to be above the

$NOAEC_p$, what body burden is observed and how do these levels relate to the potential food chain effects?

It is not appropriate or logical within the risk framework to suggest that a single receptor should be emphasized because it presents a lesser risk; i.e., the polychaete vs. the amphipod. Rather, all important and relevant endpoints, i.e., acute amphipod effects and foodchain effects for the polychaete, must be evaluated.

Response: In the Draft Final Phase II SWEA report, bulk sediment hazard quotients and pore water hazard quotients, as well as bioassay-based COPEC no-effect-concentrations are presented, evaluated, and displayed on maps. Due to resource constraints, maps are presented showing only the risk drivers.

Comment No. 24: **Section 9.3.3.1, page 9-8, para 1. "Based on these comparisons, invertebrates more sensitive than polychaetes may be adversely affected by metal COPECs in the Northern Channel. The metals most likely to cause effects are cadmium, chromium, lead, mercury, and silver." This is in agreement with our understanding of the correlation coefficients presented in Appendix F as presented above.**

Response: Comment noted.

Comment No. 25: **Section 9.3.3.1, page 9-9. In addition to the misuse of the polychaete NOAEC, the authors compare the sample data for PAHs to ER-M levels which may not be protective of even 50 percent of the exposed population (depending on the quality and confidence of the ER-M). Setting protection levels at 50 percent is not sufficient protection for the aquatic resources (or even terrestrial) at Moffett Field.**

Response: The Navy agrees that adverse impacts to 50 percent of an exposed population would be undesirable. The Navy did not intend to suggest that a 50 percent protection level is appropriate for MFA. However, the Navy interprets the terms ER-L and ER-M as described by Long and others (1995): below the ER-L effects are rarely observed, between the ER-L and ER-M effects are occasionally observed, and above the ER-M, effects are frequently observed. The Navy understands that the ER-M is the median, or 50th percentile, of effects data for various species and does not indicate that 50 percent of a particular receptor population would be impacted.

In the Draft Final Phase II SWEA, the Navy computed bulk sediment COPEC hazard quotients to gain insight into the COPECs that might be responsible for adverse effects to benthic fauna, as well as those COPECs that probably pose little risk. To develop COPEC toxicity

benchmarks for the hazard quotients, the Navy focused on apparent effects threshold (AET) values for COPECs. AETs were identified from the data set used by Long and Morgan (1991) to develop ER-L and ER-M values. Priority was placed on bioassays with sensitive fauna—such as amphipods, bivalves, and echinoderms—on sediments from San Francisco Bay. In general, the AET-based toxicity benchmarks are about equal to the ER-L values.

Regarding the NOAEC values, the Navy presents recalculated NOAEC values for the polychaete, the amphipod, and the echnioderm using the synoptic bulk sediment or pore water chemistry data. The NOAEC values represent a line of evidence about the COPEC concentrations that may not adversely affect benthic fauna.

Comment No. 26:

Section 9.3.3.1, page 9-9, para 3. An example of an apparent exaggerated statement follows: "Invertebrates cannot metabolize PAHs (Eisler, 1987a)." The statements from Eisler (1987) are, from page 12, "Fish and most crustaceans tested to date possess the enzymes necessary for activation (Statham et al, 1976; Varanasi et al, 1980; Fabacher and Baumann, 1985), but some mollusks and other invertebrates are unable to efficiently metabolize PAHs (Jackim and Lake, 1978; Varanasi et al, 1985)." From page 36 of the same review, it is stated: "Authorities generally agree that: most species of aquatic organisms studied to date rapidly accumulate (i.e., bioconcentrate) PAHs from low concentrations in the ambient medium; uptake of PAHs is highly species specific, being higher in algae, mollusks, and other species which are incapable of metabolizing PAHs;..." The review does not make the sweeping statement that, "Invertebrates cannot metabolize PAHs." Please correct this statement.

Response:

Comment noted. Statements in the Draft Final Phase II SWEA have been revised to reflect current understanding of PAH metabolism by different taxa.

Comment No. 27:

Section 9.3.3.2, page 9-10. The statement is made, "In samples collected from this area of the Eastern Diked Marsh, the concentrations of copper, lead, mercury, nickel, silver, and zinc exceeded the $NOAEC_p$ by up to an order of magnitude." And the next paragraph notes that several metal concentrations are above the NOAEC and the ER-M, suggesting that "...adverse effects are possible." The text states that, at similar concentrations, reduction in survival of amphipods and bivalve larvae have been observed and although little information is available for polychaetes, "the expected dominant taxa", the "SEM/AVS ratio (1:3) exceeded one by a small margin." The interpretation of the SEM/AVS ratio may not be entirely correct, especially when considering the information presented in Pesch et al, (1995), page 133. Ratios of

SEM-Cd/AVS reported in the above cited paper that are less than 1.0 had mortality less than 4 percent and for a ratio of SEM-Cd/AVS between 1.8 to 44, had mortalities that ranged from 17 to 100 percent and was less than 4 percent for SEM-Ni/AVS ratios between 1.3 and 40. This is not conclusive evidence that the concentrations of metals found at MFA should be "considered unlikely" for producing lethal effects at SEM/AVS ratios of 1.3.

The ER-L was exceeded by 85 percent of the samples from the eastern diked marsh for low molecular weight (LMW) PAHs, whereas only three (30 percent) of the samples exceeded the ER-L for high molecular weight (HMW) PAHs.

Response: Comment noted. The SEM/AVS data has been re-evaluated and is presented in Sections 8.3 and 9.2.6. The Navy acknowledges that there is uncertainty associated with the interpretation of the SEM/AVS data at MFA. See the response to EPA specific comment number 23. See Appendix G of the draft final report for a re-evaluation of the PAH results. Hazard quotients calculated using bulk sediment benchmarks are discussed in Sections 8.0 and 9.0 of the draft final report.

Comment No. 28: **Section 9.3.3.2, page 9-12. The statement is made that, "Based on the potential concentrations of PAHs above the ER-M in the Western Diked Marsh, adverse effects to sediment invertebrates are possible." However, no samples were above the ER-M (NOTE: this is not the benchmark approved by EPA). Further, "Based on the detected and estimated concentrations of LMW and HMW PAHs above the ER-Ls in the Eastern Diked Marsh, adverse effects are possible for sensitive benthic invertebrates." The conclusion is then stated that because no samples had PAHs above the ER-M, and because "Invertebrates cannot metabolize PAHs...[and] PAHs were not detected in the polychaetes which suggests that the PAHs were not bioavailable," we should not expect a problem. Please clarify this discrepancy.**

Response: Comment noted. The likelihood that PAHs may cause adverse effects in benthic fauna was re-evaluated in the Draft Final Phase II SWEA Report. The re-evaluation of potential risk to benthic receptors is based on the susceptibility of sensitive fauna, such as bivalve larvae, echinoderm larvae, and juvenile amphipods. The potential effect of PAHs on polychaetes, which are generally less sensitive to toxicants, is also briefly discussed.

Comment No. 29: **Section 9.3.3.3, page 9-12. Again, the incorrect benchmark, ER-M was used and was based on a single NOAEC (for polychaetes). The suggestion is that, "adverse effects are unlikely for invertebrates with a sensitivity similar to polychaetes" however,**

sensitive invertebrates such as amphipods, "may have reductions in survival of 30 percent." Later, it is noted that problems may be expected with antimony, arsenic, chromium, mercury, nickel, copper, thallium, cadmium, lead, manganese, selenium, and zinc. The statement that, "No spatial pattern in the sample locations that exceed the $NOAEC_p$ or the $NOAEC_s$ was apparent" suggests that too few samples were taken to completely characterize the area. Would it be possible to complete this sampling effort during the remediation phase of the project? Finally, the DDTR ER-M benchmark is used to state that adverse effects to "less sensitive invertebrates" are not expected because polychaetes did not bioaccumulate DDTR, DDTR was not bioavailable. The final conclusion appears to be incorrect because it is based on the least sensitive sediment receptor, the polychaete.

Response:

See response to EPA Comment Number 28. Based on this comment, several different evaluations of the data were conducted to evaluate if there is any spatial pattern. This included evaluation of chemical data with toxicity benchmarks, in addition to the bioassay data. The Navy believes that sufficient samples have been collected to detect spatial patterns in the chemical data. The heterogeneous nature of sediments makes the task of finding biological patterns difficult, especially considering the various influences on toxic responses (see also response to EPA Comment No. 22). Based on the results of the additional assessments, the text was expanded and modified as appropriate. The modified and expanded text can be found in Sections 8.0, 9.0, and 11.0 of the draft final report.

Comment No. 30:

Section 9.3.4, pages 9-14, 9-15. Low abundance and diversity of invertebrates are not proof that contaminants above critical levels can be eliminated as potential causes for toxicity.

The authors offer the following factors that are expected to influence the diversity of macroinvertebrates:

a. grain size (SFEI 1993);

b. ammonia and hydrogen sulfide (EPA 1986a; SFEI 1993; ABAG 1991);

c. dissolved oxygen (Carpelan 1957);

d. changes in salinity (Carpelan 1957; Nichols and Patmatmat 1988);

e. interactions with algae (USFWS 1988);

f. unidentified stochastic processes (USFWS 1988);

The citation, USFWS 1988, is not listed in the References section, but should be presented to clarify the last two factors.

Response: The naturally occurring factors listed by EPA that affect diversity of benthic macroinvertebrate communities probably also affect the abundance of macroinvertebrate populations comprising the community. Hoffman, Anderson, and Knezovich (1994) discuss the role of these factors in the toxicity of San Francisco Bay sediments. The effect of these factors on benthic macroinvertebrate community structure is one line of evidence for characterizing potential impacts of sediments on benthic receptors. The possible effects of these factors on the structure of the benthic macroinvertebrate community at MFA are discussed in Sections 9.2.6 and 11.1 of the draft final report.

Comment No. 31: **Section 9.3.4.2, page 9-16. What are the chemical concentrations used in comparing these samples areas with the MFA NOAECs? What is the basis for making the statement, "...a diverse or abundant community of the invertebrates is not expected primarily because of low DO, seasonal lack of water, and lack of a source area for non-insect invertebrates (McCafferty 1983; Wetzel 1983; ABAG 1991)"? When examining the data sheets for these samples (Appendix B), the dissolved oxygen (DO) for the samples taken are listed as:**

SWWL-22 10.25 mg/l, 100 percent;
SWWL-22 100 percent;
SWWL-22 10.2 mg/l, 100 percent;
SWWL-23 9.55 mg/l;
SWWL-24 9.81 mg/l;
SWWL-25 9.81 mg/l;
SWWL-26 9.81 mg/l;
SWRP-27 100 percent;
SWRP-27 9.49 mg/l, 100 percent;

It continues to show that DO levels are high and sometimes completely saturated (no low levels). There is little data presented to justify the statement, "The potential for adverse effects is offset by the habitat limitations that will not support a diverse or abundant benthic invertebrate community." Please correct this statement.

Response: The text was modified to acknowledge that DO levels are expected to fluctuate in the surface water, but DO is not identified as a limiting factor. Regarding Appendix B, the Navy believes that the EPA was reviewing the Field Chemistry Calibrations section of the data sheets rather than the field measurements of DO shown on the data sheets.

Comment No. 32: **Section 9.3.4.3, page 9-17, para 2. The statement, "The responses observed in the bioassays refute the hypothesis that the lack of invertebrates is due to COPECs." Limited mortality shown from Table A-1, Appendix F show the following levels of mortality for the various bioassay results, SSRP-26 = 54 percent; SSRP-27 = 41 percent; SSRP-28 = 56 percent; SSRP-29 = 50 percent; and SSRP-30 = 31 percent. These are hardly limited mortalities. Please explain the discrepancy.**

Response: Comment noted. The discussion was modified to acknowledge the significance of the bioassay mortalities in the context of the benthic receptors inhabiting mud flats of South Bay.

Comment No. 33: **Section 9.3.4.3, page 9-18. The statements made in the second paragraph do not appear to be based on the data presented in this report and the two citations (McCafferty, Wetzel) have little relevance to the present study.**

Response: Comment noted. The text was revised to reflect site conditions, with an emphasis on the mud flat ecosystem common to San Francisco Bay.

Comment No. 34: **Section 9.3.5, page 9-20, para 2. The derivation of MFA NOAECs is highly uncertain because only one definitive test was performed on one sample and that was completed in the Northern Channel, generally believed to have higher "quality" sediments.**

Response: Comment noted. The Navy agrees that the NOAEC values for the amphipod, polychaete, and echinoderm are uncertain because of the limited number of bioassays. The values are also uncertain because there is no reference station for comparison, and because the NOAEC values do not account for risk due to background.

Comment No. 35: **Section 9.4.1.1, page 9-21. Algae are not "the most important" receptor for piscivorous predators, for carnivorous birds, etc. What literature is available to justify a 30 percent cutoff point for determining the difference between a minor reduction and a major reduction for growth of crustaceans?**

Response: Comment noted. Since no impacts to the algae were noted in the bioassays, the conclusion is that this trophic level does not appear to be impacted. The revised text uses a 25 percent growth reduction as a benchmark, based on Barnthouse and others (1986). They stated a reduction in 25 percent biomass in higher trophic level organisms would be considered significant to the community. Although a reduction in growth is not directly related to biomass, it is a relevant benchmark to assess potential community impacts.

Comment No. 36: **Section 9.4.1.3, page 9-22. A 28 percent reduction in growth of fish is a significant effect on the receptor and probably the measurement and assessment endpoints.**

Response: See comment response to No. 35.

Comment No. 37: **Section 9.4.3.2, page 9-25. The exposure to chlordane and Aroclor-1254 is probably better characterized by a chronic exposure, not an acute exposure as stated here. Whether or not the observed values are above the acute level is immaterial for these compounds that are highly bioaccumulative.**

Response: The surface water benchmarks for chlordane and Aroclor-1254 were revised for the draft final report and are presented in Section 7.1 and Table 7-1.

The Navy removed the chlordane detects from the surface water database because the only detectable concentrations occurred near the Lockheed outfall. There were no chlordane detects from surface water collected at MFA. Therefore, surface water ingestion does not include chlordane to food chain receptors.

Comment No. 38: **Section 9.4.4.2, page 9-26. Statements that are made without data to justify them. "DO is anticipated to be the most important parameter in the Eastern Diked Marsh..." is not true based on the data presented in this report. The statement "... the observations of abundant water boatman suggest that lethal effects are unlikely" is at best an unsupported hypothesis because there were no insect bioassays performed.**

Response: Comment noted. Please see Navy responses to comment number 22 and comment number 30.

Comment No. 39: **Section 9.5.1, page 9-29. We agree with the statement that COPEC concentrations in the sediment have resulted in adverse effects in the Northern Channel to algae, invertebrate, and fish populations. We do not agree with the statement that the habitat is of limited value to invertebrates and fish and if the COPECs were removed, that there would be no significant improvement in the receptors.**

Response: Comment noted. Section 11.0 of the draft final report addresses the potential impacts to the biota from the sediment and presents the Navy's estimate of the potential recovery of the channels and ditches.

Comment No. 40: **Section 9.5.2, page 9-29. We agree that the COPECs have the potential to adversely impact invertebrates in the northeastern corner of the eastern diked marsh. We do not agree that there is low DO and "unreliable surface water." We do not agree that if the stressors are removed, that the habitat conditions would still impede and limit an improvement of invertebrate and fish populations.**

Response: Comment noted. Please see Navy response to comment number 30. In addition, the Navy re-evaluated the likelihood for recovery in Sections 10.0 and 11.0 for surface water and sediment receptors.

Comment No. 41: **Section 9.5.3, page 9-30. We do not agree that the concentrations of COPECs in this area have a low adverse effect on the receptors. We do not agree that only the hardier types of invertebrates are the typical invertebrates for this area, nor do we agree that the removal of stressors would not improve the conditions for invertebrates in these areas. We disagree with the statement that the overall condition of these areas is of low quality and that no improvement would result from the removal of contaminants.**

Response: The Navy appreciates EPA's comments. Please see Navy response to EPA specific comment No. 40.

EDITORIAL COMMENTS

Comment No. 42: **Section 3.3.1, page 3-13. It seems that the first sentence after the bulleted item on this page should read "The COPECs added since the Phase I SWEA are presented in bold print on Table 2-1".**

Response: For the draft final report, the text in Sections 2.4.3 (bottom of page 2-25) and 5.0 (top of page 5-2) was revised to incorporate EPA's comment.

RESPONSE TO COMMENTS FROM THE
DEPARTMENT OF TOXIC SUBSTANCES CONTROL
DRAFT PHASE II SITE-WIDE ECOLOGICAL ASSESSMENT (SWEA) REPORT
MOFFETT FEDERAL AIRFIELD (MFA)
DATED DECEMBER 8, 1995

GENERAL COMMENTS

Comment No. 1: **We should consider some alternative formats for reports of this complexity. Some consideration should be given to electronic forms, such as a hypertext format, which could facilitate the review of data and text.**

Response: Use of electronic hypertext for report presentation, while a worthwhile long-term goal for projects of this nature, would currently restrict public access to the document. See also response to DTSC general comment No. 2.

Comment No. 2: **The draft document consists of two volumes. A great amount of information is contained in these volumes but its retrieval is problematic and makes the review process very cumbersome. A lot of the reviewers' time has been spent on navigating through the two volumes in an effort to locate pertinent information. It would greatly help the reviewers if volume 2 (Appendices) contained a list of Tables and Figures. Additionally, all tables with symbols for "validation results" should refer to a key-table where these symbols are defined. On many occasions the text in volume 1 refers to analyses without referencing the table or figure where these results may be found.**

Response: The following Volume 2 and 3 appendices have their own tables of contents:

- Appendix A - Technical Memoranda Documenting Agency and Navy Agreements"
- Appendix C - Results of Phase II SWEA Chemical Analyses
- Appendix D - Phase II SWEA Quality Control Summary Report
- Appendix E - Benthic Survey Results
- Appendix F - Chemicals Detected in the Wetlands and Uplands
- Appendix G - Discussion of Hydrocarbon Results and Calculation of LMW/HMW PAHs in Sediment

- Appendix H - Histograms- Frequency Distributions for Metals COPECs in Sediment and Soil
- Appendix I - Reference Information Supporting Exposure Assessment
- Appendix J - Toxicity Profiles for COPECs and Toxicity Reference Value (TRV) Development
- Appendix L - Reference Information Supporting Risk Estimation
- Appendix N - Reference Information Supporting Risk Characterization

These additions will facilitate retrieval of information from the SWEA report. The introductions to the major results sections include references to the appropriate tables, figures, and appendices; however, in order to increase report readability, references to appendices, tables, and figures are not be made for each result cited. All tables, figures and major sections that feature validation qualifiers refer to the table of qualifiers in the Quality Control Summary Report, Table D-8.

CRITICAL COMMENTS

Comments from Hazardous Materials Laboratory (HML), DTSC

Comment No. 1: **3.3.3.2 Ratio of the Concentration of PAH Compounds to Petroleum Hydrocarbons, p. 3-17, fourth bullet. The last sentence should read "The average Total PAH value was 246 μg/kg and the standard deviation was 330." If the mean and standard deviation are used in later risk calculations, the data should be examined for normality, and lognormal parameters used if more appropriate. Although the final process for calculating PAH concentrations is a bit complicated, I believe that the objective of determining reasonably unbiased estimates was accomplished.**

Response: Working with DTSC-HML, the Navy has developed a revised approach to calculating total low molecular weight (LMW) and total high molecular weight (HMW) PAHs. This approach was distributed to the regulatory agencies prior to submittal of the Draft Final Phase II SWEA. Therefore, the editorial portion of this comment is no longer applicable.

The Navy was directed by DTSC to use the 95 percent UCL on the arithmetic mean for calculating the "average dose" exposure (Appendix A, July 12,1995 meeting minutes, page 3).

Comment No. 2: **Table 4-1. The units for salinity in water samples in Table 4-1 are ppt (presumably parts per thousand), the units given in Table 4-3 are mg/L, and the lab reports in Appendix D list g/L. The units in Table 4-3 should be corrected, and consistent units should be used throughout the report.**

Response: The field salinity readings were initially recorded in order to provide the bioassay laboratories with an estimation of salinity of incoming samples. These readings were measured on a field instrument that provides measurements in parts per thousand (ppt). Surface water and pore water samples collected from each sampling site were also submitted for laboratory analysis for salinity by Standard Method 2520. The laboratory results were provided in g/L. Parts per thousand units of salinity are essentially equivalent to g/L. (Because the laboratory reading for salinity is a measurement of the conductivity of all anions and cations in solution, the results can be normalized to units of ppt only if the actual density of each sample is determined.)

Incorrect units were reported for salinity on Table 4-3. Table 4-3 has been revised to read the correct units of ppt with a footnote to indicate that laboratory data was reported in units of g/L which is essentially equivalent to ppt.

Comment No. 3: **5.0 Nature and Extent of Contamination. Figure 5-1 is missing.**

Response: Figure 5-1 was intentionally omitted from the report because estimated PAH values to be shown on the figure were still in the process of being calculated at the time of the report submittal. The draft final report includes Figures 5-1a and 5-1b for total LMW PAHs in wetland sediment and Figures 5-2a and 5-2b for total HMW PAHs in wetland sediment.

Comment No. 4: **PAHs in Upland Soils (5.1.1). As noted in the Montgomery Watson transmittal letter, this draft is incomplete pending the completion of toxicity reference values (TRVs).**

Response: Comment noted.

Comment No. 5: **7.0 Toxicity, 7.1.1. This section should clarify whether the bioassay PAH levels were lower than average PAH levels in sediment, or lower than PAH levels in specific locations near the bioassay sampling locations.**

7.1.1. The fractions of high molecular weight and low molecular weight PAHs are not given. The fractions and a summary of the data used to calculate the fractions is needed. The supporting data

should show the variability of the fractions among sediment locations.

Response:

Section 7.1.1 no longer presents the information relevant to this comment. For the draft final report, the PAH concentrations in sediment bioassay samples are presented in Table C-2.1.1. Locations of sediment bioassay samples are presented on Figures 2-5a and 2-5b. PAH results for sediments are displayed on Figures 5-1a, 5-1b, 5-2a, and 5-2b. The values displayed on these figures were estimated using the method discussed in Appendix G. Table G-3 presents the calculated concentrations for LMW and HMW PAHs in sediments.

As shown in Table G-3 of the draft final report, calculated LMW and HMW PAH concentrations for bioassay locations in the Northern Channel (SSNC-18, SSNC-19, and SSNC-20) were lower than the concentrations calculated for historical sediment samples from the Northern Channel. The calculated LMW and HMW PAH concentrations for bioassay locations in the storm-water retention pond (SSRP-26 through SSRP-32) were more variable and were within the range of concentrations calculated for historical sediment samples from the storm-water retention pond (Table G-3).

The fractions of HMW and LMW PAHs, and a summary of the data used to calculated the fractions, are provided in Appendix G of the draft final report.

Comment No. 6:

8.4 Bioassay Results and Discussion. The discussion of statistical analysis of chemical data and bioassay results should include some discussion of the statistical and biological significance of the results. There are principal component (e.g., SIMCA) and classification methods (e.g., CART) which do provide a statistical significance level as well as revealing relationships among the variables. It is difficult from the discussion to determine which, if any of the findings are statistically, let alone biologically significant.

Response:

The results of the bioassays were discussed at length at the August 21, 1995 meeting. The suggestions made by the agencies for analyzing the bioassay data were implemented. The Navy's toxicologists and ecologists believe that the effort was adequate to identify any significant patterns in the data. Hypothesis testing was used in comparing the bioassay treatment effects with control treatments and for the correlations between various measurements of COPECs and the observed effects in the bioassays. The application of the bioassay results (biological significance) is presented in Section 9.0, 10.0, and 11.0. Also see response to EPA comment 36.

Comment No. 7:

Samples for congener-specific PCB analysis. In numerous meetings and telephone calls, a lot of discussion was devoted to the merits of congener-specific PCB analysis vs. Aroclor analysis. The main benefit of congener-specific analysis is that it allows for bioaccumulation and toxicity estimation of specific chemical compounds rather than those of a group of chemical compounds with vastly variable toxicity and bioaccumulation potential among its members.

Only four samples were analyzed for congener-specific PCBs: one pickleweed (PKWD-2) and its corresponding sediment (SSRP-34), and one sediment used in bioassay (SSWL-22) and one polychaete after 28 days in the bioassay (SSWL-22-POLY).

The results of these analyses are in two different places of the Appendices (Table D-2.6.1 and Table D-3.4.1) with no clear reference to their relationship.

Response:

The Navy acknowledges the agency's concern regarding the variable toxicity and bioaccumulation potential of the various PCB congeners.

The locations, number of samples, and analyses to be performed were selected by the Navy and regulatory agencies during planning meetings. This information was then provided in the Work Plan which was subsequently approved by the agencies.

To clarify the relationship of the various congener specific analyses, Table C-2.6.1 lists the results of congener-specific PCBs for sediment samples; Table C-3.4.1 lists the results for congener-specific PCBs for tissue samples. The relationship between the sediment and tissue samples for congener-specific PCBs is as follows:

- SSWL-22-0.0-0.5 (sediment) and SSWL-22-POLY (polychaete worm); sediment collected from the same location on 4/4/95. The polychaete sample was sent from the bioassay laboratory (ToxScan) to the analytical laboratory (PACE) on 5/16/95.

- SSRP-34-0.0-0.5 (sediment) and PKWD-2 (pickleweed); collected from approximately the same area on 6/12/95 (see Section 3.2.5.1 for details on sample locations).

The transfer coefficients calculated for congener-specific PCBs are presented in Tables I-47a and I-49a of the draft final report.

While toxicity and bioaccumulation of PCB congeners is variable, toxicity reference values (TRVs) were developed for total PCBs rather than specific congeners. Therefore, total PCB transfer coefficients were selected (see Tables 6-4 and 6-5 of the draft final report).

Comment No. 8: **Nomenclature. It would greatly facilitate the reviewers if the PCB congeners were listed by their IUPAC number.**

Response: The congener-specific PCB compound names that appear in Appendix C are similar to the IUPAC names but do not include "1,1-" in front of compound phrase "biphenyl." Table D-7 has been revised to included the exact IUPAC name and CAS Registry number for each chemical analyzed.

Comment No. 9: **Analytical method used and QA/QC. HML had repeatedly requested that we communicate with the laboratory selected to perform these analyses in order to ensure clear understanding of the data quality objectives. We did have a conference call with PACE chemists and PRC staff regarding PCB congener analysis. The outcome of that discussion was that PACE could not perform the analysis and another laboratory would be selected. No further exchange took place between HML and any other private laboratory staff. The report states that four samples were analyzed by Quanterra Labs, in Sacramento and the report cites the analytical method used as "DTSC Draft Method." We need to clarify that, even though we would have liked to, we have not provided Quanterra, or any one else, with our PCB congener-specific method. We never discussed this project with Quanterra staff, and therefore we had no opportunity to agree on any methodology.**

In addition to correcting the reference to the "DTSC method," we need information on the method actually used for the analysis and the QA/QC steps involved. Such information could not be found in the current draft report. QC samples (method blanks) are presented in two Tables (D-2.6.2 and D-3.4.2). One of the QC samples appears on both Tables. It is not clear what these QC samples represent.

It is unclear what is meant by "total homologue group" (total monochlorobiphenyl, total dichlorophenyl, etc.). Depending on the analytical method, one may sum all the individually identified congeners, or one may integrate all peaks within an elution window. From the reported values it is clear that the reported "totals" are greater than the sum of the identified congeners within each homologue group. The reporting scheme should be provided.

Response: The Draft Final Phase II SWEA Work Plan was submitted to the agencies on May 19, 1995. On June 6, 1995, the Navy requested that DTSC-HML review Table 3-9 (Reporting Limit Requirements for Congener Specific PCBs by High Resolution GC/MS) for the analytical list and reporting limits. Quanterra Environmental Services, Inc. (Quanterra) was identified as the contract laboratory in Navy

communication with DTSC-HML. DTSC-HML did not provide comments on the analyte list or request quality assurance/quality control (QA/QC) procedures.

The Navy apologizes for the misunderstanding regarding the distribution of DTSC's draft method for congener-specific PCBs. Quanterra's analytical method for congener-specific PCBs was developed using a congener-specific PCB method produced by EPA Region V (Duluth) and EPA Method 680 for total PCBs per chlorination level. References to "DTSC Draft Method" in the Draft Phase II SWEA Report will be replaced by "Quanterra In-House Method." Quanterra's Standard Operating Procedure (SOP) has been provided to DTSC-HML confidentially under separate cover.

QC samples other than method blank samples are not input into the Moffett Database. The method blank data presented on Table C-2.6.2 of the draft final report (formerly Table D-2.6.2) are associated with the sediment samples by date of analysis. The method blank data presented on Table C-3.4.2 (formerly Table D-3.4.2) are associated with the tissue samples by date of analysis. The method blank sample identified as 082225-001-MB was extracted with both the sediment sample (SSRP-34-0.0-0.5) and the tissue sample (PKWD-2) that were collected on the same day (6/28/95).

The data package that was submitted by Quanterra is stored at PRC in Denver, Colorado. As discussed in Section D.2.2 of Appendix D, the congener-specific PCB data was validated by an outside data validation firm (Triangle Laboratories).

A total homologue group is all mono-isomers of PCBs, all di-isomers of PCBs, all tri-isomers of PCBs, etcetera. The total was calculated by the addition of the following data:

- All individually calculated and reported isomers (these isomers are calibrated, and the concentration is calculated from the response factor of the individual isomer).

- All other isomers of that homologue are totaled (these isomers are not calibrated, and the concentration is calculated from the average response factor of the isomers in the homologue group that are calibrated).

There were two instances where the total homologue value was less than the sum of the individual isomers:

- Sample SSWL-22-POLY: The total nonachlorobiphenyls (12,000 pg/g) was less than the 2 individual isomers (1,800 + 11,000 = 12,800 pg/g).

- Sample SSWL-22-0.0-0.5: The total nonachlorobiphenyls (200,000 pg/g) was less than the 2 individual isomers (33,000 + 180,000 = 233,000 pg/g).

Quanterra confirmed that the above two discrepancies were the result of error introduced by rounding.

Comment No. 10:

Selection of PCB congeners. Had we communicated with Quanterra, we would have requested that, at a minimum, certain PCB congeners be included in the suite. They would have included the dioxin-like PCBs (coplanars, mono-ortho and di-ortho) that the World Health Organization (WHO) considers important for human toxicity (Ahlborg, 1994); PCB #128, 138, 153 considered important for their toxicity to mammals (McFarland & Clarke, 1989) and a number of congeners that are prevalent environmental markers (#28, 52, 101).

The report shows concentrations of 23 PCB congeners (2 mono-chlorinated, 2 di-chlorinated, 3 tri-chlorinated, 2 tetra-chlorinated, 4 penta-chlorinated, 2 hexa-chlorinated, 3 hepta-chlorinated, 2 octa-chlorinated, 2 nona-chlorinated, and 1 deca-chlorinated). The list does include the 3 coplanars (PCB #77, 126, 169). Of the mono-ortho PCBs, however, it only includes PCB #105, 118 and of the di-ortho it only includes PCB #180. There are, therefore, significant data gaps that may hinder the development of toxicity and bioaccumulation factors.

Response:

See response to DTSC comment number 7. The agency's general concerns regarding congener specific PCB analysis were expressed during Phase II SWEA planning meetings. However, the planning team, consisting of regulatory agency and Navy contractor technical staff, did not develop a specific list of congeners of concern. The Navy collected congener specific data consistent with the analyte list and reporting limits described in the Phase II SWEA Work Plan that was provided to the agencies for review. TRVs were developed for total PCBs rather than congener specific PCBs due to limited data. Transfer coefficients were also developed for total PCBs. Therefore, no impact to the development of the TRVs and transfer coefficients is anticipated.

Comment No. 11:

Tissue analyses. Section 5.0 discusses tissue concentrations. In many occasions (insect, polychaete, pickleweed) statements are made as to certain chemicals (PAH, OC, Aroclor) not being detected. There is no information as to the detection limit afforded by the amount of available sample for the respective analyses.

Response:

The reporting limits are provided in Appendix C of the draft final report where "U" qualified values are the given analytical result. When a compound is not detected, the U-qualified value is also the value of the reporting limit.

As described in Sections 3.2.6.2 and 5.6.5 of the draft final report, insect sample volume was not sufficient to permit all scoped analyses and to attain desired reporting limits. Reporting limits for insect tissue for OC pesticides/ PCBs were approximately ten times the desired reporting limit.

The volume of pickleweed and earthworm tissue sampled was sufficient to meet the desired reporting limits for all analyses.

The following subsection will be added to Section 3.0 for the final report to describe the polychaete tissue samples:

"3.2.6.4 Polychaete Worms"

Polychaete bioconcentration tests were conducted for sediment collected at the following points: SSNC-18, SSNC-19, SSRP-27, SSRP-29 through SSRP-32, and SSWL-22 (Figures 2-5a and 2-5b). The polychaete tissue samples are identified in the analytical appendices by a "poly" suffix at the end of the sediment sample medium in which they were grown, for example, SSRP-30-poly. At the conclusion of the bioconcentration test, the polychaete tissue (including the contents of the polychaete digestive tract) was submitted to PACE for analysis."

The following was added to the polychaete tissue results section (5.6.1) in order to clarify reporting limit discrepancies for polychaete tissue samples: "Reporting limits for PAHs in polychaete tissue samples are higher than other tissue because the results were reported on a dry weight basis, as opposed to other tissue samples, which were reported on a wet weight basis. In addition, reporting limits for congener specific PCBs in the polychaete tissue sample are higher than those for the pickleweed sample because the polychaete results were corrected for lipid concentrations in the sample."

Comment No. 12:

BAFs. BAFs are defined as the "tissue concentration in wet weight divided by abiotic medium concentration in dry weight" on p. 6-9. However, on p. 6-12, a literature citation defines BAFs as the "lipid adjusted tissue concentration divided by the organic carbon adjusted sediment concentration." In addition, Table K-2 lists "lipid adjusted BAFs" (with no information on the denominator). As there is no clear agreement in the literature on a standard way of defining BAFs, the report should clarify which definition is used and remain consistent.

Is there evidence that steady state is achieved within the 28 days of the bioaccumulation assays for the specific COPECs and organisms?

The whole discussion of BAFs for "total PCBs" and for Aroclor defeats the purpose of congener specific analysis. Congener-specific PCBs were needed to develop congener-specific BAFs. Such BAFs could not be found in the document. Instead, Table K-2 shows PCB homologue BAFs. Since the individual congeners were measured in both tissue and sediment, congener-specific BAFs should be reported.

Response: The text referenced in the comment was removed from the report. Table K-2 was revised and is presented as Table I-47a of the draft final report. Table I-47c presents the calculation of lipid and organic carbon normalized transfer coefficients (TC) for PCB homologues.

There is no site-specific information to indicate whether or not equilibrium was achieved in the bioassays. Equilibrium may not have been reached during the 28-day exposure resulting in a potential underestimate of the TCs.

The toxicological literature was insufficient to support the selection of congener-specific TRVs. TRVs were selected for total PCBs. For consistency, total PCB transfer coefficients were identified. The congener-specific analytical data (Appendix C) and empirical TCs for homologue groups (Table I-47a) were provided for comparison with the total PCB TCs.

Comment No. 13: **Literature cited. Table 4-7 cites a number of sources for sediment toxicity benchmarks for PCBs. It is not clear whether these benchmarks are for Aroclor (which ones?), Total PCBs (how defined?) or specific congeners (which ones?).**

A literature search is currently underway by HML and OSA staff to identify pertinent publications reporting BAFs for COPECs in terrestrial and aquatic systems. This information will be used to model exposures.

Response: Table 4-7 was deleted from the report. Table 7-2 of the draft final report provides a bulk sediment toxicity benchmark for total aroclor.

Comment No. 14: **VOCs in soil vapor, p. 5.18. Do the BAAQMD stations use the same methodology as the one employed on site?**

p. 5-19. What is the meaning of "TCE metabolites" in groundwater?

Response:

The BAAQMD monitoring stations use SUMMA-style canisters to collect air samples over a 24-hour period and EPA Method TO-14 to analyze the air samples. The following sentence was added to the second paragraph of Section 5.5: "The BAAQMD monitoring stations use SUMMA-style canisters to collect air samples over a 24-hour period. The samples are analyzed by EPA Method TO-14. The BAAQMD sample collection and analytical techniques are the same as those used to monitor the air in the burrowing owl burrows at MFA."

The sentence previously on p. 5-19 was modified to read "Although BTEX compounds were detected in the samples and appear to be elevated in the burrows in the flux ponds, the VOCs detected in the groundwater at this area are TCE and the TCE degradation products 1,1-DCE, cis- and trans-1,2-DCE, and vinyl chloride." This revision appears on p. 5-34 of the draft final report.

Comments from Office of Scientific Affair (OSA) Human and Ecological Risk Section (HERS), DTSC

Comment No. 15:

Food ingestion rates are incorrectly calculated, or values are presented without citation. Page 6-5 indicates "low dose" food ingestion rates were determined by assuming only 50 percent of the diet was from contaminated areas. However, Table K-5 carries this over to estimation of food ingestion rates, creating the absurd assumption that the animal ingests about 50 percent of the mass of food needed. The animal requires a certain amount of food, and may receive an additional amount of contaminant from background sources (e.g., metals) as well as from the contaminated site. Therefore there is a fundamental confusion on the part of the authors regarding the distinction between the amount of food ingested versus the concentration of contaminants in the food ingested in different locations. For the "low dose" estimates, use of the 95 upper confidence level (UCL) on the arithmetic mean detected in soil or sediment, along with an appropriate bioaccumulation factor, will account for the lower dose of contaminants received on average by the organisms. Therefore, HERS suggests that only one normalized ingestion rate (NIR) be calculated for each vertebrate species.

As another example, for the "high dose" estimates for burrowing owl and kestrel, an average metabolized energy of the diet for the animals are not calculated. Conversely, for great blue heron and mallard duck, it is assumed that the entire diet is only one organisms. Done correctly, a single value of NIR should be calculated for each animal by calculating an average metabolizable energy value for that species. Please refer to U.S. EPA (1993; Figure 4.7). Citations for gross energy and assimilation efficiency provided in Table K-5 must be referenced.

Response:

Table K-5, Section 6.0 exposure assumption tables, and subsequent dose calculations were revised to incorporate exposure to background metals. The revisions are presented in the draft final report in Tables 6-6 through 6-11 and Tables I-7 through I-9. Background concentrations for organics were assumed to be zero. Background values were obtained from the "Draft Final Site-Wide Remedial Investigation Report. November." PRC 1995, and PRC 1995 "Draft Final Operable Unit 5 Feasibility Study, Moffett Federal Airfield, California." January. These values were used as the exposure point concentration which comprised the "non-contaminated" proportion of the receptor's diet for the average dose determination.

In general, the Navy calculated the doses using the approach for estimation of "high and average" doses discussed during the April 24, 1995 BTAG meeting. For the calculation of the exposure point

concentrations, the Navy used the maximum detected concentration in a habitat for the high dose estimation, and the 95 percent UCL on the arithmetic mean for the average dose estimation (in accordance with the agreements reached in the July 12, 1995 meeting between the Navy and the regulatory agencies).

In cases where there were relatively low numbers of samples were taken and the 95 percent UCL on the arithmetic mean exceeds the maximum concentration or there are less than five detects, the arithmetic mean was used for the average dose estimation (this is analogous to the exposure point concentration used for the central tendency risk descriptor in human health risk assessment [Superfund's Standard Default Exposure Factors for the Central Tendency and Reasonable Maximum Exposure, EPA 1993]). See also response to DTSC Comment No. 18.

There are several variables that define the high and average doses. These variables are the food ingestion rates, exposure duration and body weights. It is therefore appropriate to calculate an average and high ingestion rate and not only one normalized ingestion rate as the comment suggests. Two components of the ingestion rate calculation vary with the average and high dose assumptions; the body weight and the proportion of the receptor's diet assumed to be contaminated. The body weights are used to determine a high and average field metabolic rate using allometric equations set forth in the EPA 1993 Wildlife Exposure Factors Handbook (WEFH). In addition, the varying proportions are considered when determining the proportion of the diet (P_L) times the metabolizable energy (EPA 1993, Figure 4-7).

The food ingestion rate calculation was clarified in the draft final report. Specifically, the average and high ingestion rate calculations were presented on separate pages and the text expanded to delineate each step of the ingestion rate determination, including an example calculation shown in the same format as Figure 4-7 in the WEFH. The text, specifically Appendix I, and tables were revised to show consistent use of the Normalized Ingestion Rate (NIR) term, as we agreed that the interchanging of NIR and IR terms results in a potentially confusing description of the methods used. In addition, Section 6.3.1 and Appendix I describe the rationale for using the NIR term and the impact it has on the presentation of dose equation. In addition, footnotes in Tables I-8 and I-9 describing the calculations and citations for gross energy and assimilation efficiency have been expanded.

Comment No. 16:

Bioaccumulation factors (BAFs) estimated for sediment-to-polychaete and sediment-to-pickleweed use units of wet weight divided by dry weight concentration in soil/sediment. The report then interprets the BAFs of less than one indicative that

bioaccumulation is not occurring (page 6-9). This is not an accurate representation of the data. For trace elements, dry weight concentrations of tissue should be divided by dry weight concentrations of media to accurately depict whether bioaccumulation is occurring. For organic compounds, the lipid normalized tissue concentration should be divided by the organic carbon-normalized media concentration to accurately depict whether bioaccumulation is occurring. Regardless of whether bioaccumulation is occurring or not, we are trying to determine if COPECs are bio-transferring to food items.

Response: For the draft final report, transfer coefficients were calculated consistent with the procedure outlined by DTSC in this comment. See Tables I-46 through I-49c.

Comment No. 17: **BAFs for higher trophic levels need to be obtained from literature sources since only select food items were analyzed for tissue residues. The report states, "Literature BAFs are being compiled as part of the TRV research efforts." This is not the understanding HERS has of the TRV effort, which is restricted to literature review of toxicity values, not biotransfer studies.**

Response: DTSC is correct. The Navy obtained BAFs and other transfer coefficients from literature sources. The text was revised to include transfer coefficients. Transfer coefficients and supporting rationale were provided to the agencies for review prior to submittal of the Draft Final Phase II SWEA Report.

Comment No. 18: **Exposure point concentrations for soil, sediment, water, and biota for the "low dose" and "high dose" estimates are not presented. Equations for estimating dose via ingestion of water and soil/sediment are not presented.**

Response: DTSC is correct; exposure point concentrations were not included in the Draft Phase II SWEA Report. The Navy provided draft exposure point concentrations (EPCs), along with draft transfer coefficients, to the regulatory agencies in a package from Bruce Narloch/ Montgomery Watson to Michael Gill/EPA dated February 13, 1996. The package was provided for agency review prior to submittal of the Draft Final Phase II SWEA Report.

Total dose estimates for each receptor consisted of the sum of the doses due to ingestion of water, prey, and soil/sediment, when appropriate. As shown in Section 6.3.9 and 6.4.8 of the Draft report, in general, doses due to ingestion of the various media are determined by multiplying the ingestion rate of the media by the respective concentration (the maximum for the high dose and the 95 percent

UCL on the arithmetic mean for the average dose) and subsequently by other exposure parameters.

Section 6.0 and Appendix I of the Draft Final Phase II SWEA Report include a description of the EPC calculations and dose calculations.

Comment No. 19:

Because items 1 through 4 are missing or incorrect, total dose estimates for the COPECs are not presented, nor is the basic format of the tables included. Estimates of dose are needed at this time in order to assist the regulatory agencies and PRC Environmental Management, Inc. focus our efforts in developing TRVs. The TRVs were to be included at a later date, since their development is independent of estimation of dose.

Response:

The Navy was in contact with the TRV development group via PRC and the Navy understands that the TRV development proceeded independent of the dose estimates. The exposure parameters and methodology were presented in the Draft SWEA report and the doses are included in the Draft Final SWEA report. The Navy's understanding is that DTSC is referring to DTSC comments 15, 16, 17, and 18 as missing from the draft document. Please see responses to these specific comments.

Comment No. 20:

It was HERS understanding that the Draft Phase II SWEA was to include development of the TRVs for volatile compounds to which the burrowing owl is exposed. These values are not included in this report, nor are they scheduled to be developed as part of the regulatory agencies efforts with PRC Environmental Management, Inc. for development of the bulk of the TRVs.

Response:

DTSC is correct that the original intent was to include the TRVs for VOCs for owls in the draft SWEA report. However, to ensure that the methodology was consistent with the development of the other TRVs, the preliminary VOC TRVs were not presented in the Draft SWEA report. The Navy developed the VOC TRVs and they are presented in the Draft Final SWEA report.

Comment No. 21:

HERS questions some of the exposure parameters listed in Tables 6-4 to 6-19. For example, why is incidental sediment ingestion for great blue heron not applicable? As another example, it is unclear why the "high dose estimate" columns have values for home range since the maximum concentration detected in the applicable habitat will be used. As yet another example, it is assumed that the salt marsh harvest mouse does not ingest water. However, PRC Environmental Management (1994) cite a Haines (1964) study which determined that the northern subspecies of the salt marsh harvest mouse (SMHM) drinks 19 percent of it's body weight per day (ca 2.4 g/day); when water is restricted, the SMHM can

survive on as little as 0.8 g/day. As a final example, pages 6-5 and 6-6 state that the highest estimate of home range will be used for the "average dose estimate," while the lowest will be used for the "high dose estimate." However, the larger home range is frequently under the "high dose estimate" column. This list is by no means exhaustive, but is meant to illustrate the type and frequency of inaccuracies.

Response:

The agencies were requested to review the pathways to be evaluated (Table 6-1 in the Draft Phase II SWEA report) at the August 21, 1995 meeting. The Navy understood that the Phase II SWEA planning team, consisting of the regulatory agency and Navy contractor technical staff, was in agreement regarding the pathways to be considered in the Phase II SWEA food chain modeling. The Navy further understood that those pathways were accurately presented in Table 6-1.

Incidental sediment ingestion for the heron was not included based on the anticipated diet of the heron. Ingesting fish, which is typically the majority of the diet in shoreline habitats (WEFH), is unlikely to result in any appreciable sediment ingestion. The WEFH does not discuss sediment ingestion for herons.

In the draft final report, home ranges were provided in Tables 6-6 through 6-11 to support the calculation of site utilization factors (SUF). In most cases, the SUF was 1.

As noted in Section 6.3.5.3 of the draft report discussing the exposure parameters for the salt marsh harvest mouse, "These estimates will likely be refined pending the receipt of data from a thesis project involving salt marsh harvest mice on Mare Island." The Navy has reviewed available literature, and water ingestion has been included for the salt marsh harvest mouse.

The transposed home ranges in Section 6.0 tables were corrected for the draft final report.

Comment No. 22:

Page 8-14 indicates that only analytes detected in at least 50 percent of the samples were included in the cluster and principal components analyses of the bioassay results. HERS questions this assumption since Phase II sample locations were NOT chosen randomly, and were specifically selected to have some areas with low contamination (in fact three of the eight locations in the storm-water retention ponds are "reference" locations). HERS requests that the cluster and principal components analyses be conducted on all analytes (excluding calcium, magnesium, potassium, and sodium).

Response: The cluster analyses were conducted on all analytes (excluding calcium, magnesium, potassium, and sodium). Page 8-14 of the draft report stated: "Cluster analyses were performed on the following three data sets using the software and methods of Ludwig and Reynolds (1988): all of the chemical data (metals, OC compounds, and PAHs); the chemical data excluding calcium, magnesium, potassium, and sodium because the cations were normalized in the bioassays; and the analytes detected in at least 50 percent of the samples excluding calcium, magnesium, potassium, and sodium."

Comments from RWQCB

Comment No. 23: **This section describes the bioassay test methods and interprets the results. To my knowledge, prior to the submittal of this document the Navy and their contractors did not provide the agencies with their final proposal as to how the bioassays would be interpreted (see Appendix A, July 12, 1995 meeting minutes). The tight schedule for completion of this report likely limited the time available to the contractors to make a presentation to the agencies. If there had been an opportunity for us to have interaction with the contractors on their proposed approach, I believe that the time required to review and comment would have been greatly reduced.**

Response: The Navy agrees with this comment. The Navy made every effort to coordinate with the regulatory agencies to minimize misunderstandings and facilitate rapid review of submittals.

Comment No. 24: **The interpretation and conclusions of the porewater test results are suspect for several reasons. First, there were apparently two different dilutions for the two sample locations (a departure from the workplan), however, there is no discussion in the text (page 8-5) as to this fact. Because chemical concentrations were measured in the original PWNC-18 sample, but the highest concentration in the bioassay test for that sample is 68 percent, we cannot have much confidence in any positive or negative correlation of chemicals to effects. Secondly, there was no organic chemical analysis for sample location PWNC-19. Again, any positive or negative correlation of chemistry to effects in the two samples cannot be made. This results in an attempt to correlate organic chemistry to toxicity in one sample only, and thus we can have little confidence in the interpretation. Third, PWNC-18 chemical analysis revealed diesel and motor oil in the sample, yet this was not discussed in the text (see also Section 8.4.4 Summary of Bioassay Results, page 8-19, last paragraph).**

Toxicity to petroleum hydrocarbons has been observed in echinoderm porewater tests at other sites in the San Francisco

Bay. Again, the limitation is that we have data for one sample location only. Lastly, the effects in the echinoderm tests were attributed to ammonia or possibly sulfide, but the report does not indicate whether total ammonia or unionized ammonia is generally considered to be the fraction responsible for toxic effects to aquatic organisms. This information should have been presented and discussed.

Response:

The RWQCB comments pertain to (1) the possible effect of petroleum hydrocarbons, as diesel oil and motor oil, on echinoderm larvae, (2) the strength of the correlation of pore water chemistry with adverse responses exhibited by the echinoderm larvae, (3) and the toxicity of ammonia.

Adverse Effect of Petroleum Hydrocarbons

The Navy appreciates the opportunity to discuss the ecological hazards of petroleum hydrocarbons. During the preparation of the Draft Final SWEA, the Navy reviewed the scientific literature for information of the aquatic toxicity of petroleum hydrocarbons to evaluate the likelihood that adverse responses by the echinoderm larvae may have been caused by diesel oil and motor oil. Information indicates that hazard is due to aromatic constituents with some solubility in water. These generally include di- and tricyclic structures. Information indicates that polycyclic structures are relatively insoluble and would be strongly sorbed to the solid phase of the sediments.

The toxicity of water soluble components of petroleum hydrocarbons is relevant to the sediment pore water bioassays with the echinoderm larvae because pore water would contain the fraction of contaminants that are dissolved (soluble) in the sediments. In 1978, the U.S. Army issued the report entitled *Environmental Aspects of Diesel Fuels and Fog Oils SGF No. 1 and SGF No. 2 and Smoke Screens Generated From Them* (Liss-Suter and Villaume 1978). The authors reviewed literature on the acute toxicity of water soluble factions (WSF) of No. 2 fuel oil to several marine and estuarine taxa. Among the studies, different exposure regimes were used, and the concentrations of petroleum hydrocarbons are expressed in many different ways. The toxicity information on polychaetes and sand dollars is expressed in terms that are relatively comparable to the data from the MFA sediment pore water toxicity tests. This information is discussed below.

Rossi, Anderson, and Ward (1976) evaluated the effect of the WSF of No. 2 fuel oil on mortality of the polychaete *Neanthes arenacoendentata*. They reported a 48-hour median tolerance limit of 3.2 mg/L total hydrocarbons, a concentration representing 37 percent of the WSF of a mixture of 1 part oil and 9 parts seawater.

Nicol and others (1977) evaluated the toxicity of the WSF of No. 2 fuel oil mixture (1 part oil:8 parts seawater) to the sand dollar, *Melitta quinquiesperforata* (Leske). The effect of the WSF on several sand dollar endpoints were evaluated, including sperm viability, egg permeability, fertilization, cleavage, and development of embryos. The authors reported that WSF concentrations exceeding 4 percent (4 parts WSF:96 parts seawater) reduced fertilization, adversely affected larval development, and reduced the number of larvae produced. The authors noted that di- and tricyclic aromatic hydrocarbons were detected in the WSF in concentrations greater than 0.1 mg/L, and they speculated that these compounds may be responsible for adverse effects.

This information indicates that low levels of petroleum hydrocarbons in the sediment pore water may adversely affect echinoderm survival and development. The likelihood of effects would depend on the specific petroleum hydrocarbons and their concentrations in the diesel oil and motor oil detected in sample PWNC-18.

Correlation of Pore Water Chemistry and Bioassays

In the draft final SWEA, the Navy has revised the interpretation of the chemical stressors that might be responsible for adverse effects to echinoderms. The Navy agrees that confidence in the identity of the pore water stressors might be limited by the amount of synoptic data. Unfortunately, when the Phase II analyses were scoped by the regulators and the Navy, the amount of synoptic data was not discussed *per se*. In spite of the limited synoptic data, several useful conclusions were drawn from the pore water information. Among them is that adverse effects may be due to heavy metals, ammonia, and sulfides. The NOAEC values calculated in the draft final report account for the dilution of the samples. As noted by RWQCB, the lack of organics analyses is an uncertainty. However, based on the relatively high K_{oc} values of the organic COPECs, few COPECs would be predicted at detectable concentrations in the pore water. If organic COPECs were present in pore water, low concentrations would be expected given the COPEC concentrations in bulk sediment. While dilutions with brine was not mentioned in the work plan, it was required for survival of the test organisms because of the low salinity of the sample.

Toxicity of Ammonia

As described in the EPA ammonia water quality criteria document, ammonia toxicity is caused by the unionized form of ammonia. This is discussed in greater detail in the draft final SWEA.

Comment No. 25: **The terminology used to describe amphipod survival above and below 70 percent is inappropriate (Section 8.4.2.4, page 8-10). The navy delineates the groups as high survival (greater than 70 percent) and medium survival (less than or equal to 70 percent). While I agree that 30 percent mortality appears to be an appropriate cutoff for the Moffett amphipod bioassay, the RWQCB generally does not consider 44 percent to 70 percent amphipod survival as "medium survival." The Navy should refer to survival as above or below 70 percent.**

Response: Comment noted. For the draft final report, the text on p. 8-17 was changed to indicate the percent survival for each group rather than identifying them as less than or equal to 70 percent and greater than 70 percent.

Comment No. 26: **It is unclear what the relevance is of comparing amphipod survival and porewater COPEC concentrations (Section 8.4.3.2, page 8-12). While there may be merit to doing such an exercise, the Navy has not presented any rationale nor discussion as to how porewater chemistry would relate to a bulk sediment bioassay. Additionally, there is no rationale presented for correlating amphipod survival to COPEC residues in polychaete tissues. This seems inappropriate since amphipod survival is related to toxic effects, while polychaete residue is related to bioaccumulation.**

Response: In the draft final SWEA, the Navy again discusses the significance of the amphipod bioassay results with respect to pore water chemistry. This is relevant because *Eohaustorius estuarius* is a free-burrowing organism, and it probably is exposed to sediment pore water. Please contrast this with tube-building amphipods that have little pore water exposure because their tubes effectively shield them from pore water.

The discussion on the polychaete residue information in regard to amphipod survival has been deleted from the draft final SWEA.

<u>Section 9.0 Risk Characterization</u>

Comment No. 27: **This section presents a confusing comparison of both No-observed-adverse-effect-concentrations (NOAECs) for polychaetes and amphipods and also ER-Ls and ER-Ms to COPECs detected in abiotic samples. The comparison to the NOAECs and benchmarks does not seem to follow any particular logic. For example, Tables 9-1 and 9-2 list the ER-L as the Moffett NOAEC where a chemical could not be attributed to the observed effect in the bioassay, but the rationale for this approach has not been clearly described in the text. Further, in the text comparisons are made to the ER-M for a number of COPECs, yet**

there is no discussion as to why ER-Ms should be used instead of ER-Ls.

Response: The Navy agrees that the discussion on the approach for computing NOAEC values could have been clearer. In the draft final SWEA, the Navy presents new NOAEC values computed using a simple dilution approach instead of an apparent effects approach. NOAEC values have been computed using the polychaete, amphipod, and echinoderm bioassay results and the synoptic bulk sediment and pore water chemistries. In addition, the discussion about the significance of the NOAEC values has been simplified.

Comment No. 28: **Section 9.3.3.2, page 9-12, second paragraph misrepresents what an ER-M value is. The second sentence states that "No samples had PAHs detected above the ER-M *which suggests that adverse effects to hardier, pollution tolerant invertebrates, such as polychaetes, are unlikely.*" The ER-M (effects range-median) represents a culmination of the 50th percentile of expected effects for a number of different species, or where effects are considered probable; it is not a species-specific value.**

Response: The Navy appreciates RWQCB's comments about the ER-L and ER-M values. The approach for characterizing the risk of COPECs to benthic fauna has been revised. It is now based on bulk sediment toxicity benchmarks that approximate the Long and Morgan (1991) ER-L values (that is, some are lower, some are higher, and some are the same). The toxicity benchmarks were derived for very sensitive fauna used to test the toxicity of field-collected sediments from San Francisco Bay. With this approach, risk characterization is based on sensitive benthic fauna, regardless of whether they are able to inhabit sediments of the unique MFA environment.

Comment No. 29: **Some of the conclusions regarding risk characterization to benthic invertebrates (sediment receptors) are incomplete or premature. For example, Sections 9.3.3 through 9.3.4.3, which discuss how the biological and chemical results of the sediment bioassays compare with the remainder of Moffett abiotic samples and the potential effects observed, does not address chemical mixtures in sediments. While there is mention of this in the section discussing uncertainties (Section 9.3.5, page 9-20), conclusions have been made regarding exposure of individual chemicals to benthic invertebrates at each of the sediment habitats without taking synergistic or additive effects into consideration. Observations of synergistic or antagonistic effects from the literature (where available) should have been provided in the discussion of observed biological effects in Moffett bioassay results.**

Response: The concept of synergistic, additive, and antagonistic effects are documented in the toxicological literature. Risk assessment continues to wrestle with how to accurately characterize the effects of mixtures. In a conference attended by Montgomery Watson, "Chemical Mixtures in Quantitative Risk Assessment" sponsored by EPA Health Effects Research Laboratory (November 1994), the conclusion was that addressing the toxicity of mixtures is extremely important but cannot be effectively addressed with our current state of knowledge. However, to address RWQCB's comment, available information on the joint action of COPECs was included in the draft final report.

Comment No. 30: **Further, in each section discussing sediment habitat (i.e., Northern Channel, Eastern Diked Marsh, Stormwater Retention Ponds), the amphipod and polychaete toxicity test results are discussed in the context that one of the organisms will be representative of that site or habitat. This is an inappropriate use of the toxicity tests. The purpose of performing a number of toxicity tests is to determine potential chemical effects on various endpoints, such as survival or reproduction. The conclusions drawn from a suite of tests requires a weight-of-evidence evaluation as to whether or not the sediments (or other media) pose an overall threat to ecologic receptors. For example (contrary to what is implied in the report), no-observed-adverse-effects to polychaetes does not override observed effects in amphipods. The sensitivity of the test organism and the endpoint tested both need to be considered when evaluating risk to receptors at the site (see page 9-5, paragraph 3).**

Response: The Navy revised the risk characterization to present a weight-of -evidence evaluation of the available data. This is found in Section 11.0 of the draft final report.

Comment No. 31: **The NOAECs derived for both polychaetes and amphipods was purportedly to provide a range of responses. While this is an acceptable approach to utilizing toxicity test results, the approach was not followed in the discussion of potential effects observed (Section 9.3.4). In each case, the potential effects were presented as an "either or" situation, not a range of responses.**

Response: See response to DTSC comment 30.

References cited in Response to Comment No. 24

Liss-Suter, D., and J.E. Villaume. 1978. *Environmental Aspects of Diesel Fuels and Fog Oils SGF No. 1 and SGF No. 2 and Smoke Screens Generated From Them*. U.S. Army Medical Research and Development Command, Fort Detrick, Maryland. April.

Nicol, J.A.C., and others. 1977. "Chemical Composition and Effects of Water Extracts of Petroleum on Eggs of the Sand Dollar *Melitta quinquiesperforata*. *Marine Biology*. Volume 40, Pages 309-316.

Rossi, S.S., J.W. Anderson, and G.S. Ward. 1976. "Toxicity of Water-Soluble Fractions of Four Test Oils for the Polychaetous Annelids, *Neanthes arenacoedentata* and *Capitella capitata*." *Environmental Pollution*. Volume 10, Pages 9-18.