STATE OF CALIFORNIA — ENVIRONMENTAL PROTECTION AGENCY

DEPARTMENT OF TOXIC SUBSTANCES CONTROL

REGION 2
700 HEINZ AVE., SUITE 200
P ELEY, CA 94710-2737

N00296.002621
MOFFETT FIELD
SSIC NO. 5090.3

December 8, 1995

Commander
Department of the Navy
Engineering Field Activity, West
Naval Facilities Engineering Command
Attn: Mr. Stephen Chao, Project Manager
900 Commodore Drive, Bldg. 101
San Bruno, California 94066-2402

Dear Mr. Chao:

DRAFT PHASE II SITE-WIDE ECOLOGICAL ASSESSMENT(SWEA) REPORT, MOFFETT FEDERAL AIRFIELD (MFA)

The Department of Toxic Substances Control (DTSC) and the San Francisco Regional Water Quality Control Board (RWQCB) have reviewed the subject document. Major deficiencies have been found in this document, some of the deficiencies relate to major components simply not being included, while others relate to incorrect procedures or lack of verification. Therefore we find this report unacceptable to adequately characterize risks to ecological receptors at Moffett Field.

In the last two years, the regulatory agencies have devoted considerable staff time to assisting the Navy in completing an acceptable document. Many technical meetings have been conducted between the Navy, the State and U.S. EPA. At these meetings, specific suggestions, recommendations and comments were provided. However, all these efforts have not resulted in a better report. Therefore, the State requests the Navy submit another draft document for the regulatory agencies' review. The attached comments reflect the State's major concerns, more specific comments will be forwarded to the Navy when the "critical comments" are responded. We look forward to discussions with you over these issues. If you have any questions, please contact me at (510) 540-3830.

Sincerely,

C. Joseph Chou
Remedial Project Manager
Base Closure Unit
Office of Military Facilities

Enclosures

2621

cc:

Mr. Michael Bessette
Regional Water Quality Control Board
2101 Webster Street, Suite 500
Oakland, California 94612

Ms. Susan Gladstone
Regional Water Quality Control Board
2101 Webster Street, Suite 500
Oakland, California 94612

Mr. Michael D. Gill
U.S. Environmental Protection Agency
Region IX, Mail Stop H-9-2
75 Hawthorne St.
San Francisco, California 94105

Ms. Sandy Olliges
Assistant chief
Safety, Health and Environmental Services
National Aeronautics and Space Administration
Ames Research Center
Moffett Field, CA 94035-1000

Ms. Laura Valoppi, M.S.
Dept. of Toxic Substances Control
Office of Scientific Affairs
400 P Street, 4th Fl.
P.O. Box 806
Sacramento, CA 95812-0806

Ms. Myrto Petreas, Ph.D.
Dept. of Toxic Substances Control
Hazardous Materials Laboratory
2151 Berkeley Way, Room 517
Berkeley, CA 94704

Mr. Barton Simmons, Ph.D.
Dept. of Toxic Substances Control
Hazardous Materials Laboratory
2151 Berkeley Way, Room 515
Berkeley, CA 94704

GENERAL COMMENTS

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.

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.

CRITICAL COMMENTS

Comments from Hazardous Materials Laboratory (HML), DTSC

1. <u>3.3.3.2 Ratio of the Concentration of PAH Compounds to Petroleum Hydrocarbons</u>

p. 3-17, fourth bullet: The last sentence should read "The average Total PAH value was 246 ug/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.

2. <u>Table 4-1</u>

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.

3. <u>5.0 Nature and Extent of Contamination</u>

Figure 5-1 is missing.

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).

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.

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.

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.

8. Nomenclature

It would greatly facilitate the reviewers if the PCB congeners were listed by their IUPAC number.

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.

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.

11. Tissue analyses

Section 5 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.

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.

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 Arochlor (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.

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 ground water?

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

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% 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% 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.

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.

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.

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.

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.

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.

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% 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.

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).

Comments from RWQCB

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.

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%, 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 was measured in the porewater samples. Unionized ammonia is generally considered to be the fraction responsible for toxic effects to aquatic organisms. This information should have been presented and discussed.

25. The terminology used to describe amphipod survival above and below 70% is inappropriate (Section 8.4.2.4, page 8-10). The Navy delineates the groups as high survival (greater than 70%) and medium survival (less than or equal to 70%). While I agree that 30% mortality appears to be an appropriate cutoff for the Moffett amphipod bioassay, the RWQCB generally does not consider 44% to 70% amphipod survival as "medium survival." The Navy should refer to survival as above or below 70%.

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.

<u>Section 9.0 Risk Characterization</u>:

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.

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.

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.

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).

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.

REFERENCE

Ahlborg et al. Chemosphere, 28: 1049-1067, 1994

McFarland & Clarke. Env Hlth Persp. 81: 225-239, 1989

PRC Environmental Management, Inc. August 8, 1994. "Mare Island Naval Shipyard, Vallejo, California, Technical Report Interim Site Description Report". Appendix F "Biology and Management of the Salt Marsh Harvest Mouse".

U.S. Environmental Protection Agency. December 1993. "Wildlife Exposure Factors Handbook, Volume I of II". EPA/600/R-93/197a.