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MOFFETT FIELD
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MONTGOMERY WATSON

May 28, 1997

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

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Subject: Response to Second Round of DTSC Comments on the Draft Final Phase II SWEA Report

Dear Mr. Chao:

Enclosed are five copies of the Draft Final Phase II Site-Wide Ecological Assessment (SWEA) Report Response to Second Round of DTSC Comments. This document is in partial fulfillment of Contract No. N62474-88-D-5086, Contract Task Order 0235.

In response to DTSC's comments, certain substantive changes to the Draft Final Phase II SWEA Report are discussed in the enclosed responses. Our understanding is that DTSC will review the enclosed responses and provide a final memorandum documenting any additional comments for the public record. Based on preliminary review of the responses by DTSC, we do not anticipate any additional substantive changes to the document. Therefore, we anticipate no changes to the existing schedule and the Final Phase II SWEA Report is due on July 25, 1997.

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

Sincerely,

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RESPONSE TO SECOND ROUND OF DTSC COMMENTS ON SITE-WIDE ECOLOGICAL ASSESSMENT REPORT, MOFFETT FEDERAL AIRFIELD

GENERAL COMMENTS

Comment No. 1:

For most of the substantive issues on the Draft Final Phase II SWEA raised by DTSC in our letter of August 23, 1996, The Navy response appears to disagree with DTSC comments. After careful review of the RTC and our previous comments, we cannot justify a change in our position stated in our letter of August 23, 1996, or our letter of January 23, 1996. Unless substantive changes are made in the calculation of HQ estimates for birds and mammals in the Final Phase II SWEA, DTSC will find the document unacceptable to quantify and describe risks to wildlife at Moffett Federal Airfield. Many of the issues are substantive, and directly impact upon the conclusion for describing ecological risks, and therefore may directly impact upon development of risk-based remedial goals. Specific substantive issues with which we disagree include:

a. the calculation of transfer coefficients using Moffett-specific data,

b. evaluation of toxicity for dioxin-like PCBS,

c. interpretation of the high toxicity reference value (TRV) as protective of wildlife, and

d. interpretation of impacts on the endangered Salt Marsh Harvest Mouse (SMHM).

Response:

DTSC is correct in noting that there is a difference of technical opinion surrounding a few remaining issues on the SWEA. The Navy has proposed certain substantive changes to the document in response to DTSC concerns. These modifications are described in the responses to specific comments. The Navy's responses to DTSC general comments are summarized below.

Regarding Issue a: In the Response to Comments (RTC) dated November 7, 1996, the Navy provided literature references to support the methods used. Since that time the Navy has reviewed the calculations of the TCs in a quality control effort. No errors have been found. In an effort to provide DTSC's project manager with additional information, a meeting on this topic was held on April 9, 1997 at EFA West. At that meeting the Navy presented a thorough review of the methods and rationale for the TC calculations. Additional references to support the methods used in the DF SWEA report (Thomann 1989, EPA 1993, Lake et.al. 1990, Linder 1997) were provided to the DTSC in a meeting held on April 21, 1997. Based on these references, the Navy believes that calculating TCs on a lipid/TOC normalized basis is an appropriate method for use at MFA. Therefore, no changes to the document are proposed in response to this issue.

Regarding Issue b: The Navy believes that there may be some utility

in evaluating congener-specific PCBs for the salt marsh harvest mouse in the future. However, the Navy believes that, if conducted, additional PCB monitoring should be done during the post-remediation monitoring not within the SWEA. As discussed in detail in the RTC (see Comment No. 1 p. 17 of RTC), the Navy believes that appropriate avian TEFs are not yet available.

The Navy believes that the most prudent approach to resolving this issue is to consider evaluating congener-specific PCBs for mammalian receptors during the development of the post remediation monitoring plan. The Navy will be developing the plan, in consultation with the regulatory agencies, based on the specific monitoring objectives. Therefore, the Navy anticipates that certain tools will be applicable, while others will not be used depending on the objectives of the monitoring.

Regarding Issue c: Please see response to comment no. 2.

Regarding Issue d: Specific issues associated with the interpretation of risks to the salt marsh harvest mouse are addressed individually in the responses below.

Comment No. 2:

Use and Interpretation of the Toxicity Reference Values (TRVs)

The Navy's position in the Draft Final SWEA and RTC is that the high TRVs are "protective", and thus only HQ1 and HQ2 estimates greater than one are indicative of adverse effects. DTSC disagrees with this interpretation of the TRVs based upon our active involvement with the Navy/BTAG effort, the development of the Moffett TRVs, and the development of the Regional TRVs. It is DTSC's position that HQs derived from the low TRVs (HQ3 and HQ4) are the best indicators of possible adverse effects for most contaminants.

The low TRVs were derived to be reasonable "low-risk" toxicity values. The low TRVs should not be viewed as overly conservative, since uncertainty factors were applied only when insufficient data were available (e.g. an unbounded lowest-observable-adverse-effect-level, (LOAEL)). No interspecies uncertainty factors were applied, nor were uncertainty factors applied to protect special-status species. HQ3 and HQ4 estimates less than one indicate there is low likelihood for adverse effects from the contaminant. HQ3 estimates greater than one indicate there is a possible adverse effect upon several individuals in the population since the dose is an average over the contaminated area. HQ4 estimates greater than one indicate there is a possible adverse effect upon individuals exposed to hot spots of contamination, or for species with small home ranges relative to the area contaminated. When the HQ3 and HQ4 estimates are greater than one, then more evaluation is needed to refine the estimates through either toxicity testing, laboratory studies, and/or field investigations.

If there is confidence in the major components used to estimate HQ3 and HQ4, the Low TRV is the appropriate toxicity value from which to derive risk-based cleanup numbers because it represents a reasonable estimate of a chronic no-observed-adverse-effect-level (NOAEL) (although the final cleanup numbers may be higher based upon the other balancing criteria).

The High TRVs were developed to provide estimates of dose levels at which significant adverse impacts can be expected on individuals and are also possible at a population level (since the endpoints of the High TRVs are generally significantly increased reproductive impacts or other systemic effects on a majority of the treated animals). The HQs derived from the high TRVs (HQ1 and HQ2) should be used to indicate contaminants which are at levels high enough to warrant expedited removal actions.

DTSC disagrees that using the High TRVs are the best point estimates of risk (HQ1 and HQ2). The Low TRV was developed as a "low risk" toxicity value. Levels above this low risk value imply there is potential risk to the receptor. The "High TRV" value was developed as a "significant adverse effect level". In a general sense for risk management purposes, the "Low TRV" is analogous to a 10-6 cancer risk level, while the High TRV" is analogous to a 10-4 cancer risk level (keep in mind we are not evaluating cancer risk in the SWEA, but adverse noncancer levels well above those predictive of cancer). Therefore, the Navy's emphasis on using the "high TRV" estimates (HQ1 and HQ2) "best point estimate" of risk is analogous to deciding the 10-4 cancer risk level is the benchmark of concern instead of the 10-6 cancer risk level. Such an approach is not consistent with the objectives of the TRV working group, nor can it be defended on a toxicological basis. We cannot overemphasize the importance of this issue in the interpretation of the SWEA report; it appears to be a major area of disagreement between the Navy and regulatory agencies/natural resource trustees.

Response:

It is DTSC's position that HQs derived from the low TRVs (HQ_3 and HQ_4) are the best indicators of possible adverse effects for most contaminants. While the low TRVs were derived to be reasonable "low-risk" toxicity values, this objective may not have been met for certain contaminants.

It is the Navy's position that the TRVs should be evaluated on a contaminant-specific basis. While the Navy generally agrees with the DTSC's definition of each of the HQs, some cautions are advised in interpreting the results. HQ_3 estimates greater than 1 may or may not indicate possible adverse effects on several individuals in the population. In certain cases, the exposure point concentrations are skewed by a low number of very elevated concentrations indicating one or more hot spots. Thus, several individuals may not be at risk depending on the density of the population, size of the hot spot in relation to home range, etc.

DTSC maintains that the low TRV is the appropriate toxicity value from which to derive risk-based cleanup numbers. However, it should be noted that there may be doses greater than that NOAEL (represented by the low TRV) that also result in no adverse effect on receptors.

DTSC's defines HQ_1 or HQ_2 greater than 1 as being indicative of contaminants which are at levels high enough to warrant expedited removal actions. The Navy is concerned that this approach, applied without additional review of the risk estimates, results in an inaccurate characterization of risk. This concern is most clearly illustrated in

Figure 12-7. If HQ_1 greater than one is used, the soil covering the entire base poses risk. Clearly the risk estimates must be interpreted with an understanding of the nature and extent of contamination and the uncertainties in each of the variables used to estimate risk.

Risk estimates resulting from the high TRV may be more appropriate for certain contaminants (e.g., avian receptors and lead) (Please see response to comment no. 10). This likely results from overly conservative low TRVs and, in some cases, overly conservative transfer coefficients. To address this issue, the Navy will provide refined transfer coefficients, including rationale for the refinements in the Final Phase II SWEA report. The Navy will also include the resulting risk estimates in the report. Because the TRVs were the work product of a consensus workgroup, the TRVs will remain unchanged in the SWEA. However, the Navy would like to emphasize that there are substantial uncertainties in the selection of these TRVs as there are in the exposure variables.

It is also the Navy's opinion that HQ_1 should be emphasized in evaluating the specific risk drivers identified in the SWEA (with the exceptions noted in this response to comments). This position was supported in the Draft Final SWEA. It is the Navy's opinion that the scope of the SWEA is sufficiently broad to require a professional judgment about the single best estimate of actual risks at MFA. To resolve the issue with the DTSC, the Navy will provide text in the SWEA to represent the DTSC's concerns. The Navy will clearly document the difference of professional opinion so that the risk managers can evaluate the information provided and render an informed decision.

Comment No. 3:

Impacts on the Salt Marsh Harvest Mouse

Page 1, response to DTSC General Comment No. 2 states, "The Navy believes that the data and risk estimates suggest that the likelihood of impacts to individual salt marsh harvest mice is low..." DTSC disagrees with this comment. As indicated below, correct calculation of the sediment-to-pickleweed transfer coefficient (TC) for polychlorinated biphenyls (PCBs) will substantially increase the dose and risk estimates to the salt marsh harvest mouse (SMHM). We calculate the correct value for this TC to be 2.8 (dry weight food/dry weight sediment basis). This value is 4.4 times greater than the TC of 0.86 used by the Navy. Therefore, all dose estimates to the SMHM from total PCBs should be 4.4 times greater (2.8 / 0.86 = 4.4). Using the DTSC - calculated TC, the correct HQ3 is 3.7, and the correct HQ4 is 42.7. Since adverse reproductive effects were observed in laboratory animals chronically exposed to levels around 1 mg/kg-day, average dose estimates to the SMHM (HQ3) are close to this level, and localized areas of high contamination exceed these dose estimates (HQ4). The SMHM is likely to be exposed to high localized areas of contamination due to it's small home range. Thus, DTSC believes it is likely there are impacts on individual SMHM from PCBs.

Response:

Please see response to Comment no. 1, Issue a. The Navy has provided references to support the methods used in the SWEA. The Navy believes that calculating the TCs on a TOC/lipid normalized basis reduces the variability from sediment to sediment and is an

appropriate method for use at MFA.

Further support for a TC of less than one rather than 2.8 can be found in Table 5-22 of the SWEA report. As shown on this table, Aroclor 1260 was detected in two sediment samples from the stormwater retention pond, but was not detected in colocated pickleweed samples. Assuming that Aroclor 1260 was present in pickleweed at the detection limit, a TC of 0.04 to 0.05 would be calculated. The Navy believes that the few site-specific data that are available indicate that PCBs are not bioaccumulated in pickleweed to levels greater than that present in the surrounding sediment. This observation is consistent with the general understanding that plant uptake of chemicals from sediment or soil is directly proportional to water solubility (Travis and Arms, 1988; O'Connor et.al., 1990). Hydrophobic chemicals such as PCBs, therefore, would have a low tendency for plant uptake from sediment or soil. The only available evidence of plant accumulation of PCBs is for root crops such as carrots (O'Connor et. al., 1990); this observation is due to direct adsorption to carrot peel rather than translocation to above ground plant parts (i.e., leaves and stems). It is the Navy's position that a sediment-to-pickleweed transfer coefficient of 0.86 is protective. Therefore, based on the above information and the additional literature references provided to DTSC to support the methods used to calculate the TCs, no changes are proposed in response to this comment.

Comment No. 4:

We did not calculate HQ1 and HQ2, using the high TRV, because these quotients are of limited use in evaluating risks to the SMHM from total PCBs. As we stated in our letter of August 23, 1996, the small mammal high TRV of 84 mg/kg-day greatly exceeds the level at which adverse impacts in laboratory animals chronically exposed to PCBs were observed (1 to 2 mg/kg-day).

Response:

The Navy concurs with the DTSC's general concern regarding the risks to endangered species. The use of the low TRV may be more appropriate for the SMHM exposure to PCBs. The text in Sections 9 and 11 will be modified to acknowledge this point and PCBs will be identified as a risk driver for the SMHM. However, certain information providing context for that estimate should be noted. The HQ_4 estimate for PCBs and the salt marsh harvest mouse was 10 (Table 8-28). This estimate resulted from the maximum detection of Aroclor 1254 (6530 µg/kg). This sample was located in area of the site identified as SMHM habitat. However, this "hot spot" may be in an area that is unsuitable for the SMHM. Sample RP-H005 (Figure 5-3a) appears to be located in a drainage channel with a thick stand of cattails. This area is typically under standing water. Concentrations of PCBs resulting in an HQ_4 between 1 and 10 were detected in a second "hot spot", SSRP-25 (Figure 5-3a). The habitat quality in this location is not known but should also be reviewed. Therefore, in response to the DTSC's concerns, the Navy will modify the text as noted above, and walk the site to provide further description of the areas where elevated PCBs were detected. It should be noted that the Navy is considering remediation to address risks to a variety of receptors. The lateral extent of remediation for the other receptors will likely include these areas of elevated concentrations within the area depicted as SMHM habitat in the subject report.

Comment No. 5:

In addition to likely impacts to the SMHM from PCBs, the SWEA

also indicates lead, selenium, and other metals may impact the SMHM. DTSC disagrees with the characterization of risk in Section 11.4.1.4 and 11.4.1.5 of the SWEA. The total hazard index is greater than one for all HQ estimates. Selenium and lead are significant contributors to the total risk. For lead, HQ3 and HQ4 estimated risks are significantly greater than one, even when compared to background HQs for lead. The SMHM HQ is 3 fold greater than background under average exposure conditions (HQ3), and 23 times greater than background when exposed to the highest concentrations of lead in sediment. Thus, localized areas of lead contamination above background are likely adversely impacting the SMHM.

Response:

The background risk estimates should be applied with caution as discussed in Section 11.3.4.9. These background estimates are based on upland soil samples selected as background concentrations for the MEW RI. The comparison with background risk estimates should not be used alone to conclude that contaminant concentrations are adversely impacting the SMHM.

The form of lead administered in the study is more easily absorbed than metallic lead, the form anticipated to be present in site sediments. The low TRV for lead is based on a NOAEL for systemic effects in rats fed lead acetate for 6 to 12 months. As discussed in Section 11.4.1.4, the high TRV was emphasized in evaluating potential impacts on the SMHM.

The Navy believes that the conclusions regarding risk to the SMHM resulting from potential lead exposure are appropriate. In general, the Navy agrees that localized lead concentrations may pose a risk to the SMHM. No changes to the document are proposed in response to this comment because the Navy believes that individual SMHM are at lower risk than is suggested by the calculations because of the limited areal extent of the lead contamination and the specific areas affected by elevated lead concentrations. As was the case with PCBs, the habitat offered by the area most affected by elevated lead concentrations is likely marginal. To confirm this understanding of the SMHM exposure, the Navy will walk the site to provide further description of the areas where elevated lead was detected. It should be noted that even if the habitat is found to be marginal, the Navy agrees that, in view of the special status of the SMHM, it is prudent to consider remediation of hot spots of lead contamination.

Comment No. 6:

The high TRV for selenium is based on a study using selenate in drinking water in which there was significant reduction (about 50%) in the number of offspring compared to controls (Schroeder and Mitchener, 1971). Therefore, levels below the High TRV would still be expected to have adverse effects on reproduction in the SMHM.

The Phase II SWEA dismisses the significance of selenium exposure to SMHM by speculating that the selenium at Moffett is in a less toxic form than at Kesterson Reservoir. This statement is made without any site-specific supporting evidence. In any wetland system, the biological cycling and mobilization of selenium from sediment into the foodchain is possible, and is expected to be a major exposure pathway. It has been shown that in aquatic systems, selenium is taken up by algae, plants, and zooplankton transform the selenium

into organic-Se compounds (U.S. Fish and Wildlife Service, 1987; Ohlendorf, 1989; Davis, et al, 1988).

It has also been shown that the organic forms of selenium are more toxic than the inorganic forms in birds (Heinz, et al., 1987); and that organic-Se accumulates in bird tissue more readily than inorganic Se forms (Heinz, et al., 1988). Similar to these effects in birds, studies on mammals have shown that organic forms of Se can accumulate and are more toxic than inorganic forms. Ferm et al. (1990) showed that organic Se is more toxic than selenite or selenate to hamsters. Pharmacokinetic studies in mammals have shown that organic Se can accumulate in mammalian tissue and be transferred to the neonate via lactation and intrauterine transfer (Hawkes, et al, 1994). Although in mammals the developing fetus may be protected from the reproductive effects of Se due to the ability of the reproductive systems to remove excess Se from the fetus (Hawkes, et al, 1994), maternal toxicity and embryotoxicity can still result in mammals exposed to excess Se (Ferm, et al, 1990).

The High TRV dose is associated with significant reproductive toxicity, therefore, we disagree with the Navy's position that the High TRV for selenium is "protective". We also disagree that the selenium is in a less toxic form at Moffett Field, since no evidence other than pure speculation is presented by the Navy.

For selenium hazards to the SMHM, all HQ estimates are greater than one, and are from 4 to 8 fold greater than background levels. HQ2, HQ3, and HQ4 are all significantly greater than their corresponding background HQs. For example, HQ3 risk to SMHM for selenium is 4.4 fold greater than the background HQ3 for selenium. HQ4, indicative of localized areas of contamination, is 8.4 times greater than the HQ4 background. Even HQ2 risks (representing localized areas of contamination with toxicity levels expected to cause adverse effects) for the SMHM exposed to selenium exceed one, and are double background levels. DTSC believes the majority of the uncertainty in the hazards estimates for Se come from the use of a literature-derived transfer coefficient from sediment to pickleweed. Therefore, we recommend that studies be conducted during the feasibility study (or post-remedial monitoring) to confirm the TC of selenium from sediment to pickleweed.

Response:

The Navy agrees with DTSC that exposure levels below the high TRV may potentially be associated with reproductive effects on the SMHM. However, it should be noted that the low TRV is based on hepatic changes in rats fed selenium in the diet over an entire lifetime. The low TRV, therefore, is not associated with reproductive effects. Therefore, the dose associated with a NOAEL for reproductive effects is most likely somewhere between the low and high TRVs. However, the Navy will provide text in the Final Phase II SWEA Report to summarize DTSC's concerns.

The Navy concurs with DTSC's observation that it has been shown that in aquatic systems, selenium is taken up by algae, plants, and zooplankton which may transform the selenium into organic-Se compounds. However, the degree to which this is occurring at MFA and the subsequent potential for toxic effects is what is most relevant. Lemly (1987) noted that in aquatic systems the degree of mobility and rate of selenium cycling determines whether toxicity occurs. As

shown in Table 5-22, selenium was not detected in plant samples from MFA. The Navy believes that these results support the discussion presented in Section 8.6.3 and Section 11.4.1.5. Moreover, these results suggest that mobilization of selenium from sediment into the foodchain via plant uptake is not an exposure pathway of concern at MFA. While mobilization and biological cycling of selenium may be a major exposure pathway relative to other pathways, the total exposure may still be below levels of concern.

The purpose of the comparison between Kesterson Reservoir and MFA was to (1) include, as a point of reference, field observations of mammalian toxic effects at a site known to contain selenium contamination that caused effects on other receptors, and (2) to identify similarities and differences between MFA and that site. As discussed in the subject report, effects on mammals were seen at Kesterson but could not be clearly correlated with selenium contamination (Clark, 1987, as cited in Ohlendorf, 1989; see Section 11.4.1.5 of the subject report).

Perhaps the most striking difference between MFA and Kesterson is the difference in selenium concentrations in environmental media (see Section 11.4.1.5). However, MFA data included in the subject report indicate other important differences between the sites. Certain soil properties are important in determining the form and availability of selenium in soil. These properties include soil texture, clay content and type, organic content, and the presence of other ions. (For a complete discussion of selenium chemistry in soils see Mikkelsen, et. al., 1989.) The following table compares relevant physical and chemical parameters between Kesterson soils and sediments collected at MFA.

Parameter	Average Kesterson Soils	Average Moffett Sediments
Silt and Clay Percentage	54.6	83.7
pH	8.2	7.2

Data on soil properties at Kesterson were obtained from the US Bureau of Reclamation (1986). MFA grain sizes are from sediments collected during the Phase I and Phase II SWEA; pH values are from pore water in sediment collected during the Phase II SWEA.

As discussed in the subject report, in well aerated, alkaline soils, selenate (Se^{6+}) is the dominant form of selenium. In acid and neutral soils, selenite (Se^{4+}) that is adsorbed to clays and hydrous oxides of iron predominates. Selenite solubility is lowest in pH range 4 to 7. The binding of selenium to clays and iron oxides results in extremely low solubility, and therefore, the selenium is largely unavailable to plants. Selenite solubility increases with increasing pH. The highest selenium concentrations occur in plants grown in the highest pH soils, and an increase in pH reduces the adsorption capacity of the clays. Thus, the lower pH at MFA not only influences the form of the selenium present but also the availability of selenium to plants. Further, there is a negative correlation between clay content and selenium content of plants. Therefore, the greater clay content at MFA compared to Kesterson suggests that selenium may be less available at MFA. These differences between MFA and Kesterson make the likelihood of toxic effects on mammals resulting from selenium exposure at MFA lower than at Kesterson. The additional

information provided here will be added to Section 11.4.1.5 to better support the statement about the form of selenium likely to occur at MFA.

The background risk estimates should be applied with caution as discussed in Section 11.3.4.9. These background estimates are based on upland soil samples selected as background concentrations for the MEW RI. The comparison with background risk estimates should not be used alone to conclude that contaminant concentrations are adversely impacting the SMHM.

The Navy believes that the interpretation of the selenium risks was sufficiently supported in the Draft Final SWEA report, with the exception noted above. However, the Navy agrees that uncertainty in the risk estimates for selenium result from the use of a literature-derived transfer coefficient from sediment to pickleweed. The Navy will present refined TCs and risk estimates in the risk characterization of the final SWEA report.

Comment No. 7:

In summary, DTSC believes HQ3 and HQ4 risk estimates greater than one are indicative of likely adverse impacts on the SMHM from primarily PCBs and lead (and possibly selenium and other metals). Interpretation of possible effects on the SMHM should emphasize HQ3 and HQ4 estimates due to the small home range and the special status protecting individual animals, and due to the significant adverse impacts using the high TRV (HQ1 and HQ2). Therefore, we disagree with the conclusion stated in Table 11-1B of the SWEA.

Response:

The Navy believes that the conclusions presented in Table 11-1B are accurate, but should be expanded to include PCBs as a risk driver for the SMHM. To better support the conclusions and to provide additional information to the risk managers, the Navy will make the changes outlined in responses to comments 4 and 6.

Comment No. 8:

Impacts on Avian Receptors

The Navy response and the SWEA claim that the High TRVs for avian receptors are the best estimates for evaluating risk to birds. We disagree, as we noted above. Our specific disagreements with the rationale provided in Chapter 11 of the SWEA for the contaminants contributing most to the hazard estimates are discussed below.

The example of the avian TRVs for DDT in the Navy response (page 16) and in the SWEA (p. 11-27-29) misrepresents the facts. There is no evidence that pelicans are "unusually" sensitive to DDT compounds. The U.S. EPA used the pelican study in their development of criteria for the Great Lakes (U.S. EPA, 1994) because it was the most appropriate peer-reviewed, long-term, field study with a dose-response for reproductive success. The pelican study was used by the U.S. EPA without any modifications for interspecies differences for the kingfisher, gull and eagle. Table 1-6 of U.S. EPA (1995) indicates this pelican study had the longest exposure duration of other species tested, and therefore, one of the lower LOAEL levels. Studies on other bird species cited by USEPA (1995) were conducted for significantly shorter time periods, were unbounded LOAELs, or were based on adult mortality instead of

reproductive effects. This does NOT mean the pelican is more sensitive; but rather experimental design was more sensitive (lower dose, longer exposure, more sensitive endpoints) than studies conducted on other species. Finally, a 10 week study on chickens found a lower LOAEL (unbounded) than the longer-term pelican study. Therefore claiming the pelican is unusually sensitive is without factual basis, and cannot be used as justification for choosing the high TRV as the "best estimate of risk". Further, the "low TRV" is only three-fold less than the "high TRV." Since the "high TRV" is based upon the lowest level at which adverse effects were observed, a three-fold lower value for the "low TRV" is not overly conservative. In short, there is no evidence to indicate the pelican is "unusually sensitive" to DDT compounds, or that the high TRV is a safe level, or that the low TRV is not an appropriate "low risk" value for bird species occupying Moffett Federal Airfield.

The SWEA also claims (page 11-29) that no impact is expected on the great blue heron because population declines of this species were not reported at the peak of organochlorine (OC) pesticide usage. First, this statement gives no information on the level of exposure to the great blue heron so that a meaningful conclusion can be drawn about the toxicity of DDT to the heron. Second, the statement misses the point that the great blue heron is used as a species to represent the exposure to a bird species feeding on amphibians, reptiles, fish, birds and mammals (tertiary consumer) in the aquatic habitats of Moffett Field (SWRP, EDM, NC). Other birds occupying this guild in aquatic habitat would include the northern harrier and the loggerhead shrike, as examples. Therefore, the hazards to birds occupying the tertiary consumer trophic level at Moffett Field aquatic habitat cannot be dismissed.

Response:

There is evidence that pelicans are among the most sensitive avian species studied to the reproductive effects of total DDT, principally DDE (Lundholm 1987; King, et.al., 1970; Anderson, et.al. 1975; Anderson, et.al. 1977; Schrieber 1980). The text stating that pelicans are unusually sensitive to DDT will be replaced by text stating that pelicans are among the most sensitive avian species studied to the effects of DDT.

The Navy believes that including the general statement regarding the lack of documented population declines of the great blue heron is valid and supportable. Additionally, the Navy proposes no changes to the statement in the context of the great blue heron representing other species inhabiting MFA; the great blue heron is an indicator receptor and was clearly presented as such in the document. The brown pelican, and American peregrine falcon, both among the most sensitive avian receptors to DDT, are transient visitors to the site. The exposure of these species to MFA contaminants is likely much lower than the exposure of resident species selected as indicator receptors. A central tenet of the SWEA workplan was that adequate protection of the indicator receptors will provide adequate protection of the other receptors they represent, notwithstanding the uncertainty in the ecological risk assessment process. The Navy believes that tertiary consumers have been adequately considered in the Draft Final SWEA. The Navy believes that the conclusions for the indicator receptors in Table ES-2 are an accurate characterization of the results of the SWEA.

It is also the Navy's opinion that the high TRV should be emphasized in evaluating the specific risk drivers identified in the SWEA (with the exceptions noted in this response to comments). To resolve the issue with the DTSC, the Navy will provide text in the SWEA to represent DTSC's concerns. The Navy will clearly document the difference of professional opinion so that the risk managers can evaluate the information provided and render an informed decision.

Comment No. 9:

The SWEA (page 11-30) also states that the High TRV for birds exposed to PCBs is emphasized as the appropriate benchmark because the TRV was "...based on studies with chickens, a species that appears to be unusually sensitive to the effects of PCBs.... the mallard appears to be relatively insensitive to the reproductive affects of PCBs". This misrepresents the data set upon which the TRVs are based. The chicken appears to be one of the most sensitive species tested; the reason is largely because of limitations in the experimental design of the studies conducted on other species (exposure duration and sensitivity of endpoints). This does Not mean that the chicken is unusually sensitive.

Also, the mallard is used as a species to represent the exposure to a bird species feeding on aquatic vegetation at Moffett Field. Other birds occupying this guild are expected to have similar exposure as the mallard, and the TRV is chosen to have similar exposure as the mallard, and the TRV is chosen to evaluate the toxicity of the exposure. Finally, the High TRV of 131.5 mg/kg-day approaches the acute dose which is lethal to half the animals (LD50 doses), and levels significantly below the High TRV caused increased mortality in mallard ducklings after only 10 days of exposure to PCBs in combination with a virus. For this reason, the high TRV for birds exposed to PCB has been significantly revised as part of the Regional TRVs after evaluating the studies more thoroughly. The revised High TRV is 1.27 mg/kg-day, or two orders of magnitude lower than the High TRV for Moffett (Draft Regional TRVs, October 10, 1996). Even at 1.27 mg/kg-day, significant effects on reproduction were observed at only 9 weeks exposure in the chicken (hatchability decreased to 1.8% for Aroclor 1248, and 69% for Aroclor 1254 compared to controls of 95%).

Response:

In its statement, "based on studies with chickens, a species that appears to be unusually sensitive to the effects of PCBs..." the Navy was attempting to document the uncertainty in the use of the avian low TRV based on reproductive effects in chickens, and to qualify the risk estimates for non-gallinaceous species occurring at Moffett. There is evidence to indicate that gallinaceous birds, particularly chickens, are among the most sensitive avian species that have been tested to the reproductive effects of PCBs. Differential sensitivity in avian species to the reproductive effects of PCBs has been well documented in the literature (Ahlborg et al., 1994; Bosveld and Van den Berg, 1994; Metcalfe and Haffner, 1995). Endpoints frequently used in the evaluation of PCB toxicity in birds include in vitro and in vivo induction of 7-ethoxyresorufin-O-deethylase (EROD) activity (Metcalfe and Haffner, 1995). The relative potency of hepatic EROD activity induction by PCB mixtures has been proposed as an important indicator of potential reproductive effects in birds (Tillitt et al., 1993; Bosveld and Van den Berg, 1994). Kennedy et al. (1994) investigated the in vitro EROD induction potencies of coplanar PCBs in several

species of birds and concluded that the order of sensitivity of the species tested was chicken > pheasant > turkey > duck > herring gull. Thus, there is the potential for significant interspecies variability in the reproductive responses of birds to PCBs.

However, the Navy recognizes that the toxicity dataset used in the derivation of the low TRV for avians contains lowest observable adverse effect levels (LOAELs) for several species, including quail and kestrels, that are in the range of the LOAEL from the same study upon which the low TRV is based. Therefore, the Navy agrees with DTSC that the dataset upon which the low TRV is based does not support a statement that the chicken is "...unusually sensitive to the effects of PCBs...". The Navy will revise this statement to read, "...the low TRV was based on studies in chickens, a species that is among the most sensitive species that have been tested to the effects of PCBs".

In response to DTSC's comment, the Navy will modify the text in the SWEA to indicate that emphasizing the low TRV for comparison with avian PCB doses is appropriate. The resulting interpretation of the risk estimates will then be more protective of sensitive species and may be overprotective for certain species. This observation will be included as a uncertainty in the risk characterization.

Comment No. 10:

Lead exposure to birds is also a significant contributor to hazard. We disagree that the High TRV should be emphasized as the appropriate benchmark in judging the likelihood of impacts to individual birds, because it is not a protective dose. Significant reduction in egg production occurred at the high TRV of 8.75 mg/kg-day in the chicken after 10 weeks of exposure (14% reduction compared to controls). At doses significantly lower than the High TRV for lead, significant reproductive effects were observed in the quail: a dose of 0.12 mg/kg-day resulted in a 23% drop in egg production, and a dose of 1.1 mg/kg-day of Pb resulted in a 37% drop in egg production compared to controls, after only 5 weeks of exposure (Edens and Garlich, 1983). These results are corroborated in another study in which quail hatchlings dosed for 12 weeks at similar dose levels also had decreased egg production (Edens, et al., 1976).

Response:

It is the Navy's position that the high TRV is appropriate for evaluating lead impacts on avians. The form of lead administered in the study from which the low TRV is derived (lead acetate or lead sulfate) is more readily absorbed than metallic lead. Administration of metallic lead in kestrels resulted in a NOAEL approximately ten times higher. The NOAELs reviewed in the TRV database were spread over a range of approximately 2 orders of magnitude. Therefore, the Navy concludes that the low TRV is likely biased toward overprotectiveness and should not be emphasized in interpreting the risk estimates. The Navy proposes no revisions to the SWEA in response to this comment.

Comment No. 11:

We also disagree that the hazards to birds from chlordane are unlikely (p. 11-26, 27 and Table 11-1b of SWEA). We believe hazards from chlordane for birds at Moffett Fields are unknown, due to the lack of avian TRVs.

Response:

Comment noted.

Comment No. 12: **In summary DTSC disagrees with Tables 11-1b of the SWEA as the likelihood of adverse effect on individual birds and mammals because the analysis is based on the use of HQ1, which uses the High TRVs. We also disagree that adverse effects on populations is unlikely for many of the birds and mammals.**

Response: Comment noted. The Navy will modify the text as indicated in the above responses. The Navy will then re-evaluate the summary and conclusions presented in Table 11-1b, and modify the table as appropriate.

Comment No. 13: **Specific Comments on the RTC**

DTSC still recommends post-remedial monitoring in the eastern diked marsh, Lindberg Avenue stormdrain channel, and Northern Channel to verify remediation of the Lindberg Avenue drain remedial action was successful.

Response: Comment noted. The objectives of the post-remediation monitoring plan and the methods used to accomplish the monitoring will be developed with input from the regulatory agencies.

Comment No. 14: **Sediment to Pickleweed Transfer Coefficients (TC):**

DTSC's main point is that the Phase II sampling locations are not representative of the Phase I sampling data. The Navy's response (middle paragraph, page 6) centers on the Aroclor 1260 results, and ignores the complete absence of detected Aroclor 1254 in the sediments of SSRP-33 and -34 (Phase II sampling). This is done in spite of the fact that Tables I-4 and 5-11 (Phase I sampling) indicate Aroclor 1254 predominates in the SWRP and in the areas where SMHM will likely inhabit. A comparison of total PCB concentrations in Table I-4 (SMHM habitat), to Table 5-11 (SWRP habitat), to the collocated samples from Phase II (Table 5-22), reveals that the collocated samples are not representative of the SWRP or the sediments which to the SMHM is exposed. The absence of Aroclors in the SSRP-34 sample means that ratios between Aroclor data and congener-specific data cannot be obtained. We suggest that the SSRP-34 congener-specific data be used to estimate dioxin-like toxicity of PCBs to the SMHM (in addition to the risk estimates for total PCBs). Please refer to recommendations made in our January 16, 1996 memorandum. The preferred alternative to our recommendations for salvaging the PCB congener data is to resample the SMHM habitat areas during the feasibility study and do both Aroclor and congener-specific PCBs analysis at all sample locations.

Response: As stated in the RTC, the Navy agrees that SSRP-34 is not representative of average PCB concentrations for SWRP sediments and is therefore not necessarily representative of sediments to which the salt marsh harvest mouse would be exposed. However, the Navy would like to clarify the contents of Table 5-11. This table indicates that the most frequently detected PCB is Aroclor 1260 (hence the focus of the Navy's response). Aroclor 1254 was detected at higher concentrations in a few samples collected during previous investigations (e.g., the maximum detected concentration was from a 1992 sampling effort).

The sediment to pickleweed transfer coefficients calculated for the Phase II SWEA are adequate for the purpose of assessing risk to the salt marsh harvest mouse. Please see response to comment no. 3. No revisions to the SWEA are proposed in response to this comment.

Comment no. 15:

DTSC believes that there is considerable uncertainty in using transfer coefficient from Travis and Arms because they were developed for a human-food chain system that has not been shown to be applicable to wildlife habitat. The Travis and Arms regression equations cannot even be considered conservative estimates, since a regression equation would give a best-fit estimate, rather than an upper-bound estimate. The result is a significant gap in the data needed to develop site-specific transfer coefficients for organochlorine pesticides. Many of these compounds bioaccumulate, so transfer to food of the SMHM is possible. Although these pesticides may be detected infrequently in the SWRP as a whole, localized areas of contamination may be impacting individual SMHM. The preferred recommendation is to resample the SMHM habitat areas during the feasibility study (refer to above recommendation for PCBs) so that transfer coefficients for organochlorine compounds can be better estimated. If resampling is not done, we recommend it be included in the post-remedial monitoring.

Response:

Comment noted. The Navy will consider DTSC's recommendation during the development of the post-remediation monitoring plan. The scope of the post-remediation monitoring plan will be developed based on specific monitoring objectives. These objectives will be developed in consultation with the regulatory agencies. The objectives will then be used to determine the appropriate number of samples and types of measurements to be taken during post-remediation monitoring.

Comment no. 16:

The Navy's response compares the mean concentrations of contaminants in the collocated sediment samples to the ranges of contaminant concentrations in the SWRP sediments. DTSC maintains that comparing average to ranges is not a valid comparison. Average concentrations in the collocated samples are significantly less than the average concentrations in SWRP samples for the metals noted in our original comment. Similarly, maximum values detected in the collocated samples are significantly less than maximum concentrations in SWRP sediments. DTSC maintains that the collocated samples are not representative of levels found in the SWRP, and recommends that transfer coefficient determination be included in the feasibility study or the post-remedial monitoring. Because Phase I and Phase II data do not agree, and because TC are not necessarily linear across a concentration gradient, DTSC believes there is a high degree of uncertainty. This uncertainty is best addressed during the feasibility study or post-remedial monitoring.

Response:

Comment noted. The Navy will consider DTSC's recommendation during the development of the post-remediation monitoring plan.

Comment no. 17:

DTSC's comments regarding lipid-normalization of data in our memorandum of December 8, 1995 on the Draft SWEA was referring to evaluating bioaccumulation from transfer coefficient

data. This was in response to a Navy statement that bioaccumulation was not occurring. The Draft SWEA upon which our comment was based did not contain any calculations for estimating dose, and the food ingestion rate calculations were incorrect. Therefore, the manner in which the Navy intended to perform the dose calculations, specifically the estimation of contaminants in the diet based upon transfer coefficients, was not indicated in the Draft SWEA.

Response: Comment noted.

Comment no. 18: **The data presented in Table I-49b, indicate that the sediment to pickleweed transfer coefficient that should be used with the dry weight food ingestion rate is 3.8. We arrived at this value by dividing the pickleweed wet weight concentrations of 2.7 ug/kg total PCBs by 0.06 (the dry weight fraction assumed for pickleweed in Table I-49a) to derive a total PCB pickleweed concentration of 45 ug/kg dry weight. The transfer coefficient we determined by dividing the total PCB pickleweed concentration (dw basis) by the total PCB sediment concentration (dw basis). Using a sediment total PCB concentration of 11.9 ug/kg from Table I-49b, the transfer coefficient on a dry weight basis is 3.8 (45 divided by 11.9). This dry weight TC for total PCBs is 4.4 times greater than that calculated using a TOC and lipid normalized TC (3.8 divided by 0.862 = 4.4). To DTSC, this is a significant difference in transfer coefficients. Since dietary sources are the predominant contributors to the HQs for the SMHM, this will translate into approximately 4 fold greater hazard quotient (HQ) estimates than is represented in the Draft Final SWEA. We consider this a substantial difference.**

Response: See response to comment no. 1, Issue a.

Comment No. 19: **The use of a TOC and lipid normalized transfer coefficient with a food ingestion rate that is based upon dry weight is not correct. DTSC maintains that the transfer coefficient for total PCBs used in the Draft Final SWEA is not correct. The use of this incorrect TC will reduce the dose estimates which will result in HQs that are not truly reflective of the risk.**

Response: See response to comment no. 1, Issue a.

Comment No. 20: **The Navy's response states that it is "likely that the majority of sediments in the SWRP are nondetect for Aroclor 1254 based on this low frequency of detection" (11.7% frequency of detection). We disagree that the available data demonstrates that PCBs are infrequently detected in the SWRP because very high detection limits for many of the analytes will bias the frequency of detection. DTSC has repeatedly commented on this problem throughout the Phase I SWEA.**

Response: The Navy is aware of DTSC's concern regarding detection limits. However, the Navy believes that the data collected are adequate to indicate a pattern of contamination associated with historical drainage channels. This is consistent with the Navy's understanding of the sources of contamination, and the conceptual site model regarding the nature and extent of PCB contamination.

Comment No. 21: **Using the data presented in Table I-47b (Aroclor 1260) and I-47c**

(PCB congeners) for calculating the sediment to polychaete transfer coefficients on a dry weight basis, we obtain TCs of 2.4 and 0.09, respectively. These are similar to TCs obtained using the incorrect TCs (TOC/lipid-normalized basis). This is coincidental in that the TOC/lipid normalized factor of 0.20 is equal to the dry weight fraction for polychaetes. Therefore, while DTSC agrees that the HQ estimates for birds are adequate, we maintain that the calculation is not correct and should be corrected in the Final SWEA.

Response: See response to Comment No. 1, Issue a.

Comment No. 22: **DTSC disagrees that assuming a transfer coefficient equal to one for PAHs and metals is likely conservative. Similarly, use of Travis and Arms equation for transfer coefficients have not been shown to be conservative for upland or aquatic habitats. The results of the SWEA indicate that food-chain pathways are dominant pathways for many metals. We continue to recommend that the risk characterization uncertainty sections discuss more explicitly how the various methods of calculating transfer coefficients impacts the risk characterization.**

Response: The Navy believes that the uncertainty discussions and transfer coefficient discussions in the SWEA report and the revisions proposed in the RTC (see response to DTSC's specific comment no. 2 in the RTC submitted on Nov. 7, 1996) adequately address this issue.

Comment No. 23: **DTSC agrees that the "...exposure point concentrations, dose estimates, and hazard quotients for DDTR may be potentially underestimated by as much as 21%" due to omission of o,p' -DDTR isomers from the sample analysis. DTSC however disagrees that it is likely the underestimate of DDTR (total of all isomers of DDT, DDD, DDE) is lower than 21%. The Navy response contends that the p,p' -isomer ratio to the o,p' to o,p' -isomer ratio in the environment will be less than that of the technical formulation. The ATSDR (1994, p. 35) indicates the p,p' -isomers account for approximately 85% of the total DDTR, which is approximately the same percentage of p,p' to o,p' - isomers in the technical grade DDT. ATSDR (1994, p. 92) cites a U.S. National Soils Monitoring Program on the overall pattern of DDTR that also indicates the proportion of p,p' to o,p' -isomers is approximately that of the technical grade DDT. Until site-specific information is available, no conclusions can be drawn regarding the proportion of p ,p' isomers to o,p' -isomers.**

Response: Comment noted. The DTSC states that p,p'-isomers account for approximately 85% of the total DDTR. The remaining o,p-isomers would then be approximately 15%. This percentage is lower than the 21% cited for technical grade DDT. Therefore, the Navy proposes no revisions to the statement that "it is likely that the underestimate of DDTR is lower than 21%". The Navy agrees that there is some uncertainty in the actual proportion of p,p' isomers to o,p' isomers.

Comment no. 24: **The Navy also contends that o,p' -isomers are less toxic than p,p' -isomers. We disagree that o,p' -isomers are less toxic. ATSDR (1994, p. 59, 60) states, "p,p' -DDT as well as o,p' -DDT can have adverse effects on reproduction after short term or longer term exposure of rats..." Further, regarding developmental effects, "These effects reflect the estrogenic activity of o,p' -DDT and o,p' -**

DDD on the developing reproductive system. These effects were not observed with p,p' -DDT, o,p' -DDT, or p,p' -DDE or o,p' -DDE." It is true that at much higher acute lethal doses of DDT compounds, that the p,p' - DDT is more toxic than the o,p' - DDT (Smith, 1991, p. 746). However, in this evaluation, we are primarily concerned with chronic exposures to lower levels of DDTR, and particularly concerned with reproductive and developmental effects.

Response: Comment noted.

Comment No. 25: **Finally, all of the TRVs are based upon reproductive effects (low and high TRVs; bird and mammal) using total DDTR or technical grade DDT, which contains both o,p' and p,p' -isomers. We recommend the proposed change to the SWEA simply state the hazard quotients for DDTR are likely underestimated due to analysis of only the p,p' -isomers. DTSC recommends that risk-based remediation goals be developed for total DDTR, including the o,p' -isomers, and that verification sampling methodology include all isomers of DDTR.**

Response: The Navy agrees that the risks may be underestimated because only p,p-isomers were included in the analysis. However, the Navy believes that it is important to provide an estimate of the approximate degree of underestimate. The Navy believes that the estimate provided is a good approximation and is useful to the risk managers. Therefore, no changes to the document are proposed in response to this comment.

Regarding the DTSC comments on risk-based remediation goals and verification sampling methodology, the Navy will address these issues in the Station-Wide FS or subsequent documents.

Comment No. 26: **Burrowing Owl Exposure to VOCs in the Subsurface**

There appears to be about 4 to 5 orders of magnitude difference in concentrations of VOCs measured in the 1988 soil-gas survey results around Site 5 (depth approximately 5 feet bgs), and the monitoring of the burrows conducted for the Phase II SWEA (it is unclear from the RTC at what depth bgs VOC sampling was conducted, or whether sampling 2 feet inside the burrow opening is at the terminus of the burrow). From the information presented in the SWEA, no remediation of the subsurface has been conducted around Site 5. The questions then becomes, what happened to the high VOCs in the subsurface? Are the results of the burrow samples representative? Are VOCs in the subsurface moving away from Site 5, and if so, in what direction? Because the discrepancy between the measured values is so great, DTSC recommends continued monitoring of the soil-gas around the animal burrows before conclusions can be drawn concerning potential ecological impacts on the burrowing owl. We continue to recommend that Professor Lynne Trulio evaluate her data on burrowing owls at Moffett Federal Airfield against the available contamination data (including soil-gas survey, burrow sampling data, and areas with surface soil contamination). Refer to our original recommendation in our letter of August 23, 1996. Appendix F, Table F-7 - "Chemicals detected in the Upland Soil Vapor" does not indicate that TCE or it's breakdown products were evaluated.

Response: The Navy has previously provided information to the DTSC to clarify the sampling methods used. Additional information is also provided below.

The burrowing owl typically occupies ground squirrel burrows (Trulio 1994). The ground squirrel burrows may extend as much as 200 feet laterally and often have many openings (Burt and Grossenheider 1976). At MFA, Dr. Trulio's observations indicate that the burrows used by the owls extend to a depth of 1 to 3 feet bgs, and run for several feet laterally. The owls use the 4 to 15 feet nearest the burrow opening (Trulio, pers.com. 1997). Sampling 2 feet into the burrow provides information on the area of the burrow commonly used by the owls, although this sampling method retains some uncertainty about the VOC levels further into the burrow laterally. Sampling at the terminus of the burrow is impractical as the burrow may extend so far laterally. Sampling at the terminus would also not be useful for the exposure assessment unless the terminus coincided with the area of the burrow used by the owls.

The Navy believes that the results of air sampling of the burrows are representative and appropriate for use in the burrowing owl exposure assessment. There are several reasons why the 1988 soil gas survey results and the 1995 burrow air sampling results would not be expected to match or even be on the same order of magnitude. For example, concentrations would be expected to vary based on the difference in media sampled. The soil gas was drawn from interstitial spaces between the soil grains. The burrow samples were drawn from inside burrow chambers approximately 1 to 2 feet in diameter with one or more open connections to the surface.

The difference in depth sampled might also be expected to affect the results. The owls use burrows constructed in the upper three feet of soil and the samples were collected from a distance of two feet from the burrow opening. The 1988 soil gas samples were collected from a depth of 5 feet bgs.

The burrow samples were also collected approximately 7 years after the soil gas samples. Contaminant concentrations would be expected to change over time.

The Navy contacted Dr. Trulio to discuss the current status of the burrowing owl population at MFA. Dr. Trulio has observed no overt differences in the owl population in the vicinity of Site 5. In her opinion, the fledgling rate at MFA is consistent with other sites in the South Bay (Trulio, pers. com. with Mr. Stephen Chao/EFA West, 1997).

In summary, the Navy believes that the risk assessment conducted for the burrowing owl is sufficient to support remediation decision-making. No changes to the document are proposed in response to this comment.

Comment No. 27: **DTSC disagrees that the high TRVs are the best point estimates of risk. Refer to our comments above under the heading "Use and Interpretation of the Toxicity Reference Values (TRVs)". Also, in our above comments regarding impacts on the SMHM and avian receptors, DTSC has indicated how toxicological data used to derive**

specific contaminant High TRVs are being misinterpreted by the Navy.

The Navy's example of TRVs for DDT and mercury, which are based directly upon the U.S. EPA (1995) document on the Great Lakes without modification, are two exceptions to the approach taken by the workgroup in developing TRVs for Moffett Federal Airfield. In general, the workgroup developed the High TRVs to be levels at which significant adverse effects on growth, reproductive success, or survival are expected to occur. As we have noted above in our comments, the "high TRV" are levels at which significant effects are expected. Essentially, the Navy is choosing only levels above the "high TRV" (i.e., HQ1 and HQ2) as being indicative of adverse effects. We do not support the use of "high TRV" values as the best point estimates of risk in the manner in which the Navy is using them in the SWEA.

Response: Comment noted. Please see response to comment no. 2 above.

Comment No. 28: **The Navy misinterprets our memorandum of January 16, 1996, and misrepresents that facts surrounding evaluation of TRVs for PCBs. The January 16, 1996 memorandum was in response to a lengthy discussion in a conference call held on December 7, 1995, in which DTSC disagreed that the toxicity data evaluated for several different mixtures and specific congeners adequately evaluated the dioxin-like PCBs. PRC staff requested that DTSC outline our recommendations, which we did in our memorandum of January 16, 1996, which was transmitted to the Navy. At that point, it was the Navy's responsibility to formally disagree with DTSC recommendations by raising the issue with the TRV workgroup. This was not done.**

Response: Comment noted.

Comment No. 29: **The Navy's response cites a Metcalfe and Haffner (1995) reference that suggests insufficient information is available to develop appropriate avian TEFs for risk assessment. However, in the same paper, those authors cite numerous examples in the literature, including application to avian species, where avian TEFs were used. These authors further state that tall the various studies using TEFs concluded that "...coplanar PCBs were responsible for more than 90% of dioxin-like toxicity".**

Response: The Navy appreciates DTSC's concern regarding the evaluation of PCB toxicity at MFA. Although avian TEFs have been used, the Navy maintains that there is not enough information at the present time to suggest appropriate avian TEFs for risk assessment purposes. Therefore, the Navy proposes no changes to the document in response to this comment. (Please see the Navy's response to DTSC's comments dated November 7, 1996 for a detailed explanation of the Navy's position on this topic.)

Comment No. 30: **DTSC acknowledges the limited PCB congener-specific data available at Moffett Federal Airfield. At the working meetings held to develop the Phase II SWEA work, DTSC wanted all the collocated samples to be analyzed for both PCB congener analysis and Aroclor analysis, but the Navy believed this to be too costly. The Navy wanted only one sample of each media to have PCB**

congener analysis done. If the Navy now believes these data are too limited, then additional collocated samples can be collected by the Navy prior to finalizing this SWEA. Otherwise, it is DTSC's position that these are the only data currently available, and they should be used to calculate dioxin-like PCB toxicity as described in our January 16, 1996 memorandum.

Response: Please see response to comment nos. 1 and 29 above.

Comment No. 31: **It is our recommendation (stated in our January 16, 1996 memorandum) to evaluate PCB toxicity using both the total PCB TRV and also dioxin-like PCBs using the approach outlined for the congener-specific data and avian and mammalian TEFs. The approach recommended by DTSC for evaluating dioxin-like PCB toxicity, in addition to the evaluation of the total PCB toxicity already evaluated in the SWEA (with modifications noted above), will provide a fuller description of potential hazards to wildlife at Moffett Federal Airfield. This will aid the risk managers in evaluating possible remedial actions. Due to the selective retention of PCB congeners by biota, and because certain PCB congeners are potent dioxin-like mimics, reliance on the Aroclor data alone would provide a misleading representation of the potential PCB hazards to wildlife. DTSC believes the TEF calculations are a useful screening tool to determine whether more extensive investigation, such as measuring EROD induction in field-collected specimens, are necessary. If the Navy chooses not to do these screening TEF calculations for PCB dioxin-like toxicity, then we strongly recommend that EROD-induction in field collected specimens be included in the post-remedial monitoring.**

Response: The Navy maintains that there is insufficient data to evaluate congener-specific PCBs in the SWEA in the manner suggested by DTSC. The Navy will finalize the SWEA with no additional discussion of congener-specific PCBs. The Navy will consider using congener-specific PCB analysis and the methods described by DTSC (letter dated January 23, 1996) in the post remediation monitoring program. The exact contents of the long term monitoring plan will be determined, in consultation with the regulatory agencies, based on the specific objectives of the monitoring.

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