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MOFFETT FIELD
SSIC NO. 5090.3

UNITED STATES ENVIRONMENTAL PROTECTION AGENCY
REGION IX
75 Hawthorne Street
San Francisco, CA 94105-3901

December 8, 1995

Mr. Stephen Chao
Naval Facilities Engineering Command
Engineering Field Activity, West
900 Commodore Way, Bldg. 101
San Bruno, CA. 94066-2402

Re: *Draft Phase II Site-Wide Ecological Assessment Report*, dated October 24, 1995

Dear Mr. Chao,

The U.S. Environmental Protection Agency (EPA) has received the subject document and provides the following comments. In general, some areas of the report appear to be well put together. However, many other areas contain basic analytical and data interpretation problems. We understand that some data gaps exist because the development of TRVs are incomplete. But the many scoping meetings that occurred this past spring and summer between the Navy, your contractors and the regulatory agencies seems to have been only marginally useful. There are many concerns that Montgomery Watson may not be able to complete this document on time or in an acceptable manner. After reviewing the document, the consensus opinion of the technical reviewers is that it contains some serious deficiencies and that Montgomery Watson lacks the necessary experience to perform a scientifically defensible ecological assessment and this may be detrimental to a timely cleanup of Moffett Field.

It would be helpful, if time permits, to see work progress, including the Hazard Quotient calculations before the Draft Final (DF) is released, in order to avoid problems. As it now appears, the magnitude of changes that are necessary to correct the Draft document may cause the DF to be considered another Draft. If you have any questions, please call me at 415-744-2385 or Clarence Callahan for technical direction at 415-744-2314.

Sincerely,
Michael D. Gill
Michael D. Gill
Remedial Project Manager
Federal Facilities Cleanup Office

Attachment: Groundwater (Freeze, Cherry) Text (4 pages)

cc: C. Joseph Chou (DTSC)
Michael Bessette (RWQCB)
Ken Eichstaedt (URS)
Sandy Olliges (NASA)
Chris Petersen (MW) (Fax)
Peter Strauss (MHB)
Mike Young (PRC) (Fax)

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COMMENTS

Draft Phase II Site Wide Ecological Assessment Report, dated October 8, 1995

GENERAL COMMENTS

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} for 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.

2. There has been some misrepresentation 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.

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 areal 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.

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 retension 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 is 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 citiations 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.

SPECIFIC COMMENTS

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?

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?

7. Section 4.3. This section is very well written. It provides a clear summary description of a difficult technical subject.

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.

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?

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 dessication 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.

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?

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.

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.

14. Section 9. This chapter provides a verbal description of the data with regard to the potential effects of COPECs on the habitat and receptors. In order to provide a visual 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 retension 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 retension 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

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."

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"?

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."

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 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.

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) show 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% compared to sample SSNC-19 at 100%?

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?

20. Section 9.3.1.3, page 9-3, FETAX Bioassay. The results of this test actually had 17% mortality and 11 of the remaining embryos had an average of 13.3% $\pm$ 6.8% 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% and 13.3%, 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% in SSWL-22 and 4% 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% or more in the early life stages and then abruptly reverses to low mortality less than 1% for the remainder of the cohort's existence (Zug, 1993). This information in no way can be used to suggest that 17% mortality and 13% malformation is "...unlikely to be significant for frog populations." If the mortality was as high as 90% 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% 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.

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% survival, are significant and therefore represent a real problem for the sediment infauna.

22. Section 9.3.2, page 9-4, Derivation 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,)perlene	-.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.

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.

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.

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.

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.

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% and for a ratio of SEM-Cd/AVS between 1.8 to 44, had mortalities that ranged from 17 to 100% and was less than 4% 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.

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.

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 $NOAEC_a$ 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.

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.

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%;
SWWL-22 100%;
SWWL-22 10.2 mg/l,100%;
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%;
SWRP-27 9.49 mg/l,100%;

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.

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%; SSRP-27 = 41%; SSRP-28 = 56%; SSRP-29 = 50%; and SSRP-30 = 31%. These are hardly limited mortalities. Please explain the discrepancy.

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.

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.

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% cutoff point for determining the difference between a minor reduction and a major reduction for growth of crustaceans?

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.

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.

38. Section 9.4.4.2, page 9-26. Statements 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.

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.

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.

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.

EDITORIAL COMMENTS

42. Section 3.3.1, page 3-13. It seems that the first sentence after the bulleted item on this page should read "The COPECSs added since the Phase I SWEA are presented in bold print on Table 2-1".

Literature Cited

USEPA, 1992. Framework for Ecological Risk Assessment. (page 5, second paragraph) Risk Assessment Forum. EPA/630/R-92/001, U.S. Environmental Protection Agency Washington, DC 20460, 41pp.

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GROUNDWATER

R. Allan Freeze / John A. Cherry

is significantly less than that shown in Figure 10.11(b). Casagrande notes that uplift pressures are actually more effectively reduced by drainage [Figure 10.11(e)]. However, the presence of a drain induces even greater leakage from the reservoir than would occur under natural conditions. It is common practice now to use an integrated design with a grout curtain to reduce leakage and drainage behind the curtain to reduce uplift pressures.

The Grand Rapids hydroelectric project in Manitoba provides a grouting case history second to none (Grice, 1968; Rettie and Patterson, 1963). The project involved 25 km of earth dikes, enclosing a reservoir greater than 5000 km² in area, in a region underlain by highly fractured dolomites. A grout curtain up to 70 m in depth was emplaced from holes on less than 2-m centers over the entire length of the dikes. Grice (1968) notes that the grout curtain reduced leakage through the grouted formation by 83%, but it induced greater flows through the underlying ungrouted rock. He estimates that the grouting program reduced net leakage from the reservoir by 63%.

Steady-State Seepage Through Earth Dams

Failures of earth or rockfill dams can result from excessive leakage, from piping at the toe, or from slope failures on the dam face. All three can be analyzed with the aid of steady-state flow nets. For those rare situations where an earth dam is constructed on an impervious formation [Figure 10.12(a)], the flow net can be limited to the dam itself. Where the foundation materials are also permeable [Figure 10.12(b)], the flow net must include the entire dam-foundation system.

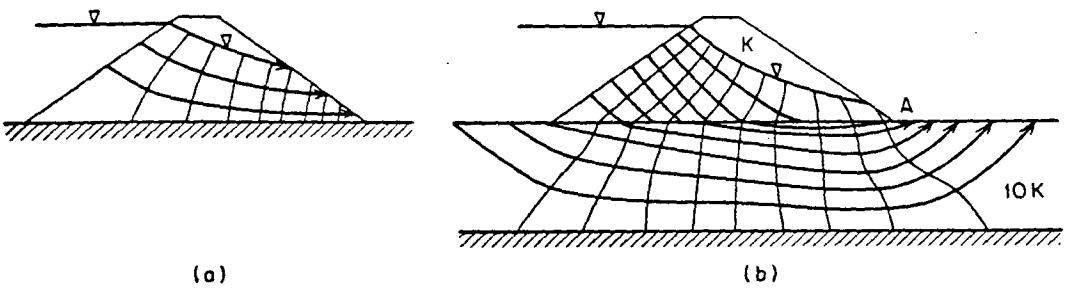

Figure 10.12 Flow nets for a homogeneous, isotropic earth dam on (a) impermeable foundation and (b) permeable foundation.

While it is recognized that a dam cross section constitutes a saturated-unsaturated flow regime, it is not common in engineering analysis to consider the unsaturated portions of the system. The free-surface approach outlined in Section 5.5 and Figure 5.14 is almost universally used. In Figure 10.13, flow is assumed to be concentrated in the saturated portion *ABEFA*. The water table *BE* is assumed to be a flow line. The specified heads are $h = h_1$ on *AB* and $h = z$ on the seepage face *EF*. The position of the exit point must be determined by trial and error. The flow nets of Figure 10.12 exemplify the type of flow nets that result. Engineering texts

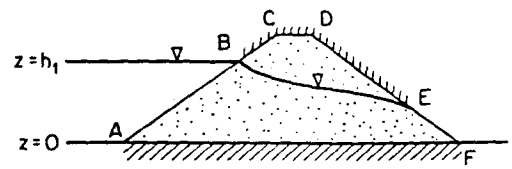

Figure 10.13 Boundary-value problem for saturated-unsaturated flow system in earth dam.

on groundwater seepage such as Harr (1962) or Cedergren (1967) provide many examples of flow nets for earth dams.

Let us now consider the question of piping. The mechanism of piping can be explained in terms of the forces that exist on an individual soil grain in a porous medium during flow. The flow of water past the soil grain occurs in response to an energy gradient. (Recall from Section 2.2 that the hydraulic potential was defined in terms of the energy per unit mass of flowing fluid.) A measure of this gradient is provided by the difference in hydraulic head Δh between the front and back faces of the grain. The force that acts on the grain due to the differential head is known as the *seepage force*. It is exerted in the direction of flow and can be calculated (Cedergren, 1967) from the expression

$$F = \rho g \, \Delta h \, A \tag{10.15}$$

where A is the cross-sectional area of the grain and ρ is the mass density of water. If we multiply Eq. (10.15) by $\Delta z/\Delta z$ and let A refer to a cross-sectional area that encompasses many grains, we have an expression for the seepage force during vertical flow through a unit volume of porous media with $V = A\,\Delta z = 1$. Putting the resulting expression in differential form yields

$$F = \rho g \frac{\partial h}{\partial z} \tag{10.16}$$

The seepage force is therefore directly proportional to the hydraulic gradient $\partial h/\partial z$. In areas of downward-percolating groundwater, the seepage forces act in the same direction as the gravity forces, but in areas of upward-flowing water, they oppose the gravity forces. If the upward-directed seepage force at any discharge point in a flow system [say, at point A in Figure 10.12(b)] exceeds the downward-directed gravity force, piping will occur. Soil grains will be carried away by the discharging seepage and the dam will be undermined.

The downward-directed gravity force is due to the buoyant weight of the saturated porous medium. A soil with a dry density $\rho_s = 2.0$ g/cm³ has a buoyant density ($\rho_b = \rho_s - \rho$) that is almost exactly equal to the density of water, $\rho = 1.0$ g/cm³. For this very representative ρ_s value, the seepage force will exceed the gravity force for all hydraulic gradients greater than 1.0. One simple test for piping

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is therefore to examine the flow net for a proposed dam design and calculate the hydraulic gradients at all discharge points. If there are exit gradients that approach 1.0, an improved design is required.

The ultimate failure mode in cases of piping is usually a slope failure on the downstream face. Slope failures can also occur there if the pore pressures created near the face by the internal flow system are too great. The limit equilibrium methods of slope stability analysis, introduced in the previous section, are just as applicable to earth dams as they are to natural slopes.

To avoid the hydraulic conditions that lead to piping or slope failures in earth dams, dam designers can incorporate many different design features. Figure 10.14(a) and (b) shows how an internal drainage system or a rock toe can serve to reduce hydraulic heads on the downstream slope of an earth dam. Figure 10.14(c) illustrates a zoned dam with a downstream shell five times more permeable than a central core. One consequence of such a design is a lowered exit point on the downstream face. If the contrast between core and shell is even greater, and drainage is added, [Figure 10.14(d)], the internal flow analysis is reduced to consideration of flow through the core itself. The shell and drain act as if they are infinitely permeable. Figure 10.14(e) shows the influence of a partial cutoff, or downward extension of the central core, on flow through a permeable foundation material. An extension of the core all the way to the basal boundary of the permeable layer would be even more effective.

Transient Seepage Through Earth Dams

Slope failures on the upstream face of a dam are usually the result of rapid drawdowns in the reservoir level. At full supply levels the effects of high pore pressures in the face are offset by the weight of overlying reservoir water. Following rapid drawdown, the high pore pressures remain, but the support has been removed. Unless the transient dissipation of these pore pressures is rapid, that is, unless the transient drainage of the dam face is rapid, instabilities may develop on the critical slip surface and slope failures can occur. Figure 10.15(a) is a schematic illustration of the transient response to rapid drawdown in an unzoned earth dam. Figure 10.15(b) shows the nature of the insurance offered against this type of failure by the presence of a high-permeability shell.

Freeze (1971a) pointed out that transient flow in earth dams is a saturated-unsaturated process; and, especially in the case of clay cores, the flow regime may be highly dependent on the unsaturated hydraulic properties of the earthfill material. However, it is not common in engineering practice to investigate the unsaturated properties of fill materials, so the free-surface approaches of De Wiest (1962) and Dicker (1969), which consider only the saturated flow, are of great practical importance in the analysis of transient seepage through earth dams.

There is another failure mechanism in earth dams that has transient overtones, and that is the triggering of slope failures by liquefaction during earthquake shocks. Cedergren (1967) notes that maximum security against liquefaction is