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☐ APPROVAL ☐ OTHER

Agency Comments "Draft Operable Unit 1 Remedial Investigation Report" Draft Baseline Human Health Risk Assessment"

ACTION REQUIRED

☒ WORK AUTHORIZATION (R. Ward)

☐ OTHER

TITLE: AGENCY COMMENTS "DRAFT OPERABLE UNIT 1 REMEDIAL INVESTIGATION REPORT, DRAFT BASELINE HUMAN HEALTH RISK ASSESSMENT"

AUTHOR: DANTE TEDALDI/BECHTEL

DATE: 08/22/94

CATEGORY: 3.4

CC:

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- ☐ Lead PjM (for Activity) _____*__
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Bechtel

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CLEAN II Program
Bechtel Job No. 22214
Contract N68711-92-D-4670
File Code: 0316

IN REPLY/REFERENCE: CTO-0048/0036

August 22, 1994

State of California Environmental Protection Agency
Department of Toxic Substances Control
245 West Broadway
Suite 350
Long Beach, CA 90802-4444

Attention: Al Arellano, P.E. Unit Chief Base Closure Branch

Subject: Review Comments on Marine Corps Air Station El Toro, California - Draft Operable Unit One Remedial Investigation Report including Draft Baseline Human Health Risk Assessment

Dear Mr. Arellano:

Attached are review comments on the subject Remedial Investigation Report including Draft Baseline Human Health Risk Assessment dated 1 July 1994. The overall impression is that the report provides a rigorous delineation and discussion of the nature and extent of trichloroethylene and its fate and transport. However, the report does not provide a supportable argument that DNAPLs are not present in the subsurface at El Toro. In addition, the general impression is that the authors have focused excessively on a micro-level approach toward understanding the issues rather than a macro-level approach. Specifically, assumptions and conclusions regarding contaminant fate and geochemical evolution of groundwater have been justified on the basis of specific micro-level issues which often appear to contradict other micro-level assumptions presented. This is especially noticeable when these issues are viewed within the scope of the entire presentation.

These concerns as well as technical comments are included in the attachments.

I can be reached in Bechtel's San Diego office at (619) 687-8780; the facsimile number is (619) 687-8787.

Sincerely,

BECHTEL NATIONAL, INC.

Dante J. Tedaldi, Ph.D., P.E.
Technical Quality Assurance MCAS El Toro

cc:

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Naval Facilities Engineering Command
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Attachments

To: MCAS El Toro BRAC Cleanup Team

From: Dante Tedaldi

Re: CH_2M-Hill Draft Operable Unit One Remedial Investigation Report including Draft Baseline Human Health Risk Assessment

Date August 19, 1994

SUMMARY OF RI REPORT COMMENTS

The primary contaminant of concern at El Toro is trichloroethene (TCE). The RI report provides a rigorous delineation and discussion of the nature and extent of that contaminant and its fate and transport. However, the report does not provide a supportable argument that DNAPLs are not present in the subsurface at El Toro.

The general impression is that the authors have focused excessively on a micro-level approach toward understanding the issues rather than a macro-level approach. Specifically, assumptions and conclusions regarding contaminant fate and geochemical evolution of groundwater have been justified on the basis of specific micro-level issues which often appear to contradict other micro-level assumptions presented. This is especially noticeable when these issues are viewed within the scope of the entire presentation. Details regarding this problem are noted below.

This review has provided extensive comments on the geochemical conceptual model involving oxidation of pyrite minerals. That model is given a great deal of attention in the text, although it does not significantly relate to the VOC contamination which is most important to this RI report.

The discussion of specific gravity promotes a common misconception that all TCE present in aquifers tends to sink. This is not true. Molecular TCE present in a dissolved form below the solubility limit will not sink, rather it will move as a result of advection and diffusion. The extremely low concentrations of TCE

measured at the site are not controlled by density limitations, and this point needs to be addressed in detail. The current discussion of TCE as a "sinker" is only applicable to pure product and free product mixed with water above the solubility limit. At present, there is no evidence to indicate that such a condition exists, although it will be assessed as part of the Phase II work. This is a significant issue with broad reaching influence over the conclusions of this report and it must be presented correctly.

Anaerobic degradation of TCE appears to be occurring in the groundwater. This is supported by the distribution of DCE, the first expected degradation product of reductive dechlorination. Nitrate data also support the assertion that TCE is degrading anaerobically. As DCE increases, nitrate decreases in the same areas. This would be expected under more reducing conditions due to the denitrification or ammonification of the nitrate. This apparent fact, although interesting, does not significantly alter the approach toward long-term solutions to the groundwater contaminantion issue. As noted in the comments above, complete mineralization to carbon dioxide and water is highly unlikely; therefore, the end result of these degradation processes is likely to be vinyl chloride.

The anaerobic degradation of VOCs is contradictory to the assertion that highly oxygenated infiltrating water is resulting in the oxidation of pyrite minerals. Given the fact that VOC contamination is the major issue at the Station, the apparently erroneous conceptual model of oxidation of pyrite minerals may be a moot point, but if it remains in the document, it must be supported through referencing recognized literature citations.

Agricultural irrigation water return is not likely to supply enough oxygen to the subsurface to oxidize pyrite to the degree necessary produce the high levels of iron, manganese and sulfate observed in groundwater. The authors did not provide evidence that pyrite was present within the subsurface, only speculation. Our review indicates that iron and manganese levels present in groundwater are the result of reduction from their oxyhydroxy forms (Fe III and Mn IV) to the more reduced and more soluble forms Fe II and Mn II.

As the water moves along the flow path, we believe the following sequence of events occurs.

- Gypsum and other evaporite minerals are dissolved
- Iron and manganese oxyhydroxides are reduced and Fe II and Mn II are solubilized
- An unknown fraction of the TCE present degrades to DCE by (the commonly accepted) reductive dechlorination pathway producing the distribution displayed in the RI report figures.
- The concentrations of TDS, SO_4, Cl and other minerals increase due to the processes described above
- NO_3 decreases due to denitrification and ammonification.

We believe that this alternative conceptual model is much more consistent with conditions found at the Station. While it is possible that the current geochemical model may in fact be correct, the presentation provided in the RI report is not adequate to support it.

SUMMARY OF RISK ASSESSMENT COMMENTS

Procedures used to estimate risk were, in general, consistent with National and Regional EPA risk assessment guidelines. Deviations are identified below.

Used the arithmetic mean concentration to calculate average and reasonable maximum risk. EPA guidelines specify using the upper 95 percent confidence limits of the arithmetic mean to calculate reasonable maximum risk. Reason given for deviation is that only two concentrations were available for most wells. That reason is acceptable for not using the upper 95 percent confidence limit. In such cases, the highest reported concentration should have been used and worst case rather than reasonable maximum risk calculated so that the resulting risk range considers some upper-bound concentration rather than upper-bound exposure factors only.

Did not adjust oral toxicity values to account for gastrointestinal absorption when using the values to estimate dermal risk. For organics, which are almost completely absorbed via the gastrointestinal tract, adjustment is not necessary; however, for inorganics, many which are poorly absorbed, use of unadjusted values will tend to underestimate risk.

Calculated risk for each well rather than all wells combined. While the justification presented for this deviation is reasonable, the guidelines should be followed when there are sufficient data to do so. Other procedures or approaches may be used, but only in addition to the conventional one.

General RI Report Editorial Comments:

- Sections 1, 2, and 3, contain abrupt transitions which make it difficult for the reader to follow.

- There is inconsistency in citing figures and tables. Sometimes the title of the figure or table is cited and sometimes not. Section 3.5 Hydrogeology, is very technically oriented and should be simplified. Identify hydrogeologic units clearly and provide ranges of hydrogeologic properties for each unit. In general, this section contains a great deal of data without much interpretation and explanation of what they mean.

- There are an excessive number of references to other documents which may not be readily accessable. For example, on page 3-67 reference to a hydrograph of Well 18_TIC041 is presented for comparison with "Hydrographs for the other key wells (are) included in the Groundwater Modeling Report, Appendix A of the OU-1 IAFS (Volume VI)." These data should be included in the RI if the authors intend to present comparisons. Likewise, there should be at least a brief summary of field techniques used for sample collection and decontamination so that other documents would not have to be relied upon so heavily..

SECTION 3 PHYSICAL CHARACTERISTICS OF THE STUDY AREA

Comment 1

pg. 3-29

Figure 3-6 does not include a reference.

Comment 2

pp. 3-39 and 3-41

Figs. 3-9a and 3-9b are presented in section 3.5.1; however, they are not discussed until section 3.5.2.1. They should be relocated.

Comment 3

pg. 3-46

Second and third paragraphs. These utility of these correlations towards the objectives of this RI is not clear.

Comment 4

pg. 3-106

Th water balance is based on very simple steady-state assumptions with estimates that are not supported by any references. There is no indication of the use of the calculated water balance values. Why was it not possible to perform a calculation of groundwater discharge from aquifer based on Darcy's law and compare this to the estimated value as a check?

SECTION 4.0 NATURE AND EXTENT

Comment 5

General observation: The depth of contamination should always be discussed for each chemical.

Comment 6

pp. 4-117

Discussion of toluene should consistently cite both site location and identify depth intervals.

Comment 7

pg. 4-120

There should be a discussion of the source of potable water at the base. What is the date and reference for the potable water samples which contained high fuel hydrocarbons? If potable water from the station was used in the drilling program, did the authors investigate the possibility that TFH-diesel contamination in the potable water supply may have resulted in the introduction of contaminants into borings?

Comment 8

4-119

Site numbers should be added to Figure 2-1 so that well locations can be related to sites. They should be in a format or font that makes them easily distinguishable from the wells.

Comment 9

pg. 4-119

In the last sentence please identify whether the petroleum occurences were detected in the first or second round. This is signficant because it may help to clarify the previous comment related to the possibility of the introduction of contamination through the use of potable water as part of drilling operations.

Comment 10

pg. 4-120

In the fourth paragraph, TFH diesel is not a compound but a group of compounds "lumped" and reported as one value. It should not be referred to as "the compound."

Comment 11

pg. 4-132

Expand general discussion of potential metal contaminant source areas. Include a discussion of the type of sludge and identify specific metals expected to be associated with source areas for other locations where plating occurred.

Comment 12

pg. 4-132

In the third paragraph. The concentration of aluminum in BGMW19B (22 mg/L) is too high to be soluble (dissolved) at this pH value (Hem, 1985). It seems unlikely that this sample was actually filtered. It also contains high concentrations of other metals that wouldn't normally be found at these levels in filtered samples. Please check the field notes for this sample to be sure it was filtered. According to HILL, all metals samples were field filtered (personal communication, Y. Chuang).

Comment 13

pg. 4-181

In the second paragraph. Arsenic, chromium, and cadmium discussions should include a reference to their use in plating.

Comment 14

pg. 4-191

In the second and third paragraphs. Oxidizing conditions are likely to cause Fe^{2+} to go to Fe^{3+} which could then precipitate as an oxyhydroxide. Is the authors assertion based on Eh measurement or other observations or field measurements? The data indicate that pH levels over the entire study area are approximately neutral (approximately 6.5 to 7.5) with remarkable stability. The oxidation of pyrite creates acidity at measurable levels. Thus, the proposed mechanism of geochemical evolution appears incorect. Likewise, at neutral pH manganese is typically more soluble under reducing conditions, not oxidizing. [See Comment number 57 - *General Comments about Manganese Speciation*]

The authors assertion appears repeatedly in the sections which follow and therefore, detailed references to other sources which provide support should be included.

Comment 15

pg. 4-205

In the third paragraph. Elsewhere in the report, the authors speculate that anaerobic degradation of TCE to DCE is occurring or has occurred at the site, and this seems reasonable. However, they also assert that at the same time in the same aquifer the oxidation of FeS to Fe^{2+} and SO_4^{2+} is also occurring. For reasons

stated throughout these comments, there is an obvious discrepency here which needs clarification.

Comment 16

pg. 4-192

In the third paragraph. Please identify the wells at Site 1 and confirm that concentrations are mg/L, not µg/L as stated in the text.

SECTION 5.0 FATE AND TRANSPORT

Comment 17

pg. 5-1

In the second sentence of the first paragraph. Edit to read as follows:"...released as a mixture of VOCs as free-product and as VOCs in aqueous solution. Thus, the VOCs may have infiltrated into the ground in either an aqueous phase or as a free-product liquid phase."

Comment 18

pg. 5-3

Figure 5-1 The temperatures under which the water solubility limits and vapor pressure are measured should be noted.

Comment 19

pg. 5-7

The discussion of specific gravity promotes a common misconception that all TCE present in aquifers tends to sink. This is not true. Molecular TCE present in a dissolved form below the solubility limit will not sink, rather it will move as a result of advection and diffusion. The extremely low concentrations of TCE

measured at the site are not controlled by density limitations, and this point needs to be addressed in detail. The current discussion of TCE as a "sinker" is only applicable to pure product and free product mixed with water above the solubility limt. At present, there is no evidence to indicate that such a condition exists, although it will be assessed as part of the Phase II work. This is a significant issue with broad reaching influence over the conclusions of this report and it must be presented correctly.

Comment 20

pg. 5-7

Add sink "or float" to 1st sentence of the first paragraph, since preceding text included discussions about DNAPLs and LNAPLs.

Comment 21

pg. 5-7

In the fourth parqgraph. While it is true that the highest groundwater TCE concentration found at well 09_DBMW45 is not indicative of a DNAPL source, at this time there is insufficient information to strongly support the authors conclusion. The Phase II RI will attempt to verify this assertion; however, the observation of VOC concentrations less than 1 percent of the aqueous solubility limit does not preclude the presence of NAPLs (EPA, 1993). Additional wells and groundwater monitoring will be part of a comprehensive Phase II program to verify this conclusion.

Comment 22

pg. 5-18

In the first paragraph. The authors should reference a source for the mobility classification scheme. It appears suddenly without a reference or further explanation.

Comment 23

pg. 5-18

In the third paragraph. Include more discussion of determination of soil TOC content. How many samples were evaluated? What areas of the site were sampled and at what depths? Page 3-11 (3.4.3 soil properties) does not include mention TOC among the measured soil properties. This is important because the TOC range is used to generate Fig. 5-3.

Most significantly, the presentation needs to identify the fact that partitioning of VOCs into organic matter on soil is based on linear soprtion isotherms which are not applicable at the low organic carbon contents found at the station. Therefore, although the presentation and illustration (Figure 5-3) is interesting, the main point that, needs to be emphasized is that partitioning into organic matter is negligible within the subsurface. The authors should develop a new discussion which reviews the primary mechanisms for sorption at the station.

Comment 24

pg. 5-18

In the third paragraph. The soil porosity value used to calculate Figure 5-3 (porosity=0.4) is not the same as that used previously for calculation of average linear velocity. On pg. 3-71 the authors used 0.3. The same porosity value should be used in each calculation so that results are comparable.

Comment 25

pg. 5-18

The reference to Barbee 1994 is incomplete.

Comment 26

pg. 5-23

In the second paragraph. Capillary forces are approximately zero in the saturated zone, therefore, the statement made by the authors in not correct. Capillary forces are greater in the unsaturated zone than in the saturated zone. The unsaturated zone has negative pressure and the saturated zone pressure is greater than or equal to zero.

Comment 27

In the second paragraph. The discussion of molecular diffusion should be qualified with the statement *"...is generally ignored in large-scale flow systems where advection dominates."*

Comment 28

pg. 5-26

Figure 5-6 does not portray the reversibility of the sorption process and thus implies a false conception of the theory. Partition coefficients (Kds) are valid only if "the reactions that cause the partitioning are fast and reversible and only if the isotherm is linear." [Freeze and Cherry, 1979 p. 403] The process portrayed in the figure is not clear. The molecules of a VOC within a parcel of water represented by a numbered red dot must be conserved. This is not the case. For example, molecule 17 is part of the soil matrix at time zero, but at time one it is no longer part of the soil matrix nor is it part of the water parcel which still contains some molecules that were present at time one. The point here being that mass is not conserved in this presentation. If retardation is occurring and molecules are being retarded and rereleased then this should be shown in the figure.

Comment 29

pg. 5-30

In paragraph four. The first paragraph states that PCE is more susceptible to abiotic reduction reactions than TCE or DCE; however, Table 5-2 lists half-lives ranging between 2 to 3 orders of magnitude greater for PCE than TCE. This may be a moot point since most degradation is microbially mediated, but the erroneous statment should be corrected.

Unfortunately, the authors have not established Eh and pH status of the groundwater and therefore they cannot apply this information to predict the likely processes occurring. The pH of groundwater is not even presented in this report and this is significant because as noted in the earlier comments (see comment numbers 12 and 14), pH measurements do not support the alleged mechanism for geochemical evolution of groundwater.

Comment 30

pg. 5-33

Figure 5-7. This figure indicates the potential transformation pathways for PCE, TCE, and TCA and implies that mineralization to carbon dioxide and water are the end products. However, vinyl chloride has been known to degrade very slowly, indicating that the final dehalogenation is the most rate limiting step (Chapelle, 1993). This limitation should be noted.

Comment 31

pg. 5-37

In the third paragraph. Provide a reference for the statement "These biological transformations are favored by reducing conditions, but can occur in mildly reducing or oxidizing conditions."

Comment 32

pg. 5-37

In the fourth paragraph. No evidence has been presented in this report to show that pyrite is present in the sediments. Marine sediments can contain marl, which is mainly calcium carbonate, they do not, by definition contain pyrite. Pyrite could be present in some marine sediments, but is generally found in igneous or highly metamorphosed rock. This section needs development to support the assertion that there is pyrite present in the unconsolidated material beneath the site.

Comment 33

pg. 5-41

In the fourth paragraph. A correction needs to be made within the paragraph that begins with "Lateral flow of contaminated GW off-Station from on-Station source areas. Delete "within the vadose zone beneath the water table" This statement does not make sense. The vadose zone lies above the water table.

Comment 34

pg. 5-41

Section 5.3.1 At first the text says that "The two primary transport pathways are..." Later, the text states that only groundwater is likely to be the primary mechanism for VOC contamination. The text requires some clarification on this point. Also, isn't the Phase II RI is scheduled to examine the second transport pathway in more detail; therefore, it seems premature to conclude that the first pathway is the principal mechanism.

Comment 35

pg. 5-42

In the third paragraph. Please supply an appropriate reference for the previous soil gas investigation.

Comment 36

pg. 5-44

The third paragraph is a good place to remind the reader that pumping from the lower aquifer and not density differences is most likely responsible for the downward migration of contamination.

Comment 37

pg. 5-47

In the fifth paragraph. Authors should edit the sentence to make clear that the hydrocarbons must exist as a separate phase in order to "float". Identify the aqueous solubility limit for benzene so the reader can compare to the measured values.

Comment 38

pg. 5-49

In the second paragraph. The text states that in Section 5.1 it was noted that the the shallow aquifer was under aerobic conditions. No such statement was found in Section 5.1. Moreover, while Section 5.1 states that anaerobic TCE degradation is most likely occurring it does not identify the aquifer.

Comment 39

pg. 5-49

In the second paragraph. State a range for groundwater temperature, not just that it is "relatively warm."

Comment 40

pg. 5-49

In the third pararaph. BTEX concentrations are so low it is unlikely that their degradation would have any measureable effect on groundwater pH and major ion chemisty.

Comment 41

pg. 5-49

In the fourth paragraph. A reference for the following statement should be cited. "...plumes reach a stable shape and size and eventually stop moving."

Comment 42

pg. 5-52

In the second paragraph. This paragraph needs references to support the assertions regarding geochemical evolution of groundwater.

Comment 43

pg. 5-52

In the fourth paragraph. This paragraph contradicts paragraph 3 above. It is unlikely to have a layer pyrite on top of iron hydroxides in the same soil. The authors may better support their main assertion by proposing that there is a layer

of oxidized Fe $(OH)_x$s on top of pyrite and the infiltrating water would contact an oxidized layer first.

Comment 44

pg. 5-53

In the first paragraph. The statement concerning preferential sorption of Ni on Mn needs a reference. This section needs to be referenced to some supporting scientific studies.

Comment 45

pg. 5-53

In the second paragraph. Please comment on the ratios of pyrite to Fe, Al, and Mn oxyhydroxides in these sediments.

Comment 46

pg. 5-53

In the third paragraph. It does not seem possible that a mechanism for preferential flow of recharging groundwater high in dissolved oxygen (DO) would provide for a retention time necessary to result in mineral solubilization and the production of water of high TDS. Please explain further (from a point of view of mineral dissolution) how these two processes can occur simultaneously to conform with the conceptual model.

Comment 47

pg. 5-54

In the second paragraph. What is rainfall (annual) in the Santa Ana Mountains? Please add a brief explanation on why chemical weathering proceeds to a lesser

degree in source rock in arid climates. The rock may by less weathered in arid climates but most likely this is because there is less water to weather the rock. See Singer, 1973.

Comment 48

pg. 5-54

In the third paragraph. Expand this section to include a discussion of the DO levels typically found in agricultural recharge. Irrigation return water would likely be somewhat depleted of oxygen after passing through fertilized fields and being in contact with high levels of organic matter in the root zone. There needs to be some reference to support the authors statement.

Comment 49

pg. 5-61

In the first paragraph. Please explain the depositional sequence of evaporites and pyrite which would result in them occurring in the same area. These minerals are normally found in two different geologic environments.

Comment 50

pg. 5-62

In the second paragraph. Calcite would precipitate only if the increasing TDS were due to calcium and/or carbonate. This would imply solution of Ca and/or $CO_3^{=}$. If ionic strength increases from other ions (which would accompany increasing TDS), the solubility of calcite due to activity effects would actually increase slightly with everything else being held constant (Blaedel and Meloche, 1963 and Garrels and Christ, 1965). Please elaborate further on the mechanism upon which this statement is based to explain how the above effect does not occur.

In the third paragraph. Although evaporative processes affecting infiltrating agricultural water would increase TDS levels in the return flow, these mass loadings are not expected to be significant with respect to the mass loadings which would result from dissolution of evaporite deposits in the subsurface. This is supported by the authors statement in the fourth paragraph regarding evaporites and the presence of calcium and sulfate. Calculations can be performed to evaluate semi-quantitively, at least, the relative contributions of each mechanism.

Comment 51

pg. 5-63

In the first paragraph. The valence of aluminum does not depend on the redox condition of the water, as it is always in the Al(III) valence state in groundwater. A major factor in controlling aluminum solubility in natural waters is the pH. Changing redox conditions alone would not increase soluble aluminum levels in groundwater. Delete aluminum from the last sentence of the paragraph.

Comment 52

pg. 5-63

In the second paragraph. Reference for statement regarding manganese remaining in solution for a very long time in near-neutral pH groundwater under either oxidizing or reducing conditions should be provided.

Comment 53

pg. 5-63

In the third paragraph. Fe precipitates according to the reaction $Fe^{3+} + 3H_2O \rightarrow Fe(OH)_3 + 3H^+$ and similarly for Mn^{4+}. They do not have to "encounter" oxyhydroxides along the flow path to assume this chemical form.

Comment 54

pg. 5-63

In the third paragraph. Please supply a reference for the relatively slow precipitation of Mn in groundwater.

SECTION 6.0 SUMMARY AND CONCLUSIONS

Comment 55

pg. 6-18

In the second paragraph. A check of other water quality data from well 05_DGMW67 does not support a correlation between high gross α/gross β and shale/siltstone. The presence of localized deposits of shale and siltstone which could produce unusually high levels of radioactivity would also be expected to produce higher than average levels of sulfate and certain metals; however, this is not the case. A review of the data in Appendix B supports this lack of correlation. Since the well is near the landfill which contains unidentified materials, please provide more evidence why it is more likely from natural sources rather than the landfill.

Comment 56

pg. 6-18

The fourth paragraph states that source of high gross α and gross β is due to natural sources but there is no real evidence to support this. See previous comment.

Comment 57

General Comments on Manganese Speciation

Issue: For this Station, the statement that dissolved manganese concentrations are caused by the oxidation of manganese oxides requires more support.

Rationale: Manganese speciation in the environment is sensitive to the redox conditions of the soil-water systems. Manganese occurs primarily as Mn(IV) under an oxidizing environment, and as Mn(II) under a reducing environment. Mn(IV) oxides generally are stable under oxidizing conditions. Thus, dissolved Mn levels in an aerobic soil-water system are typically very low or below most detection limits.

Generally, elevated dissolved Mn in an aquifer indicates that the system is under reducing (anaerobic) conditions. For example, elevated dissolved Mn is often observed in groundwater impacted by leachate from municipal solid waste landfills. Therefore, a possible explanation for the elevated dissolved Mn at El Toro is that these concentrations are the result of dissolution following the lowering of redox levels. The Draft RI does not provide information on the redox conditions of the aquifers and thus, each hypothesis is based on conjecture.

Another indication that the groundwater may be experiencing reducing conditions is the presence of dehalogenation end products. Since chlorinated VOCs are relatively stable in oxidizing environments (as suggested in the last paragraph on page 5-30) the dehalogenation of VOCs is not likely to occur if the groundwater is oxygenated. However the presence of the degradation products of these VOCs suggests that reducing conditions do exist. This contradicts statements related to the oxidation of managanese at the Station.

Comment 58

General Comments on Pyrite Oxidation

Issue: Oxidation of pyrite and other sulfide minerals is suggested as the cause of high TDS in groundwater. Current data indicate that the amount of dissolved oxygen in groundwater is not high enough to cause pyrite oxidation.

Rationale: A more likely explanation for the high TDS in the regional groundwater it that irrigation has resulted in the leaching of naturally present salts (Singer, 1973) and the introduction of nitrates from agricultural runoff. Adequate references on the discussion of pyrite oxidation and high TDS groundwater in the region are not provided.

Specific Risk Assessment Comments

Comment 59

Page ES-6, 3rd paragraph, 2nd sentence.

The sentence states, "For six production wells, there is no estimated excess lifetime cancer risk using EPA toxicity values, but there is estimated risk when using Cal/EPA toxicity values." The terms "estimated excess lifetime cancer risk" and "estimated risk" appear to be referring to some level of risk, probably 10^{-6}. The message would be clearer if the level of risk were identified. Example: "For six production wells, the level of risk was less than 10^{-6} when EPA toxicity values were used, but greater than 10^{-6} when Cal/EPA values were used."

Comment 60

Page ES-7, 2nd paragraph.

Please add that list of chemicals with risks $>10^{-6}$ is based on EPA CPFs. If that is not true, chromium should be added to the list.

Comment 61

Page 2-2, Section 2-2 and Page 2-17, Table 2-2.

Are any of the chemicals in the table TICs? Were TICs found? How were TICs handled?

Comment 62

Page 3-10, Section 3.2.2 and Table 3-1.

Section 3.2.2 and Table 3-1 identify and discuss potential exposure pathways and routes. They also identify the pathways that were and were not evaluated in the risk assessment. They do not identify the vegetable pathway (ingestion of produce sold at the market), whose absence the readers from the public sector are likely to notice even if not evaluated. Dermal exposure of swimmers in North Lake is also not identified or assessed.

Comment 63

Page 3-15, Table 3-2.

The equations that follow the table would be more understandable if the abbreviations for the exposure parameters were included in the table.

Comment 64

Page 3-18, Inhalation section.

No equation is presented for calculating inhaled dose. Does the assumption that the intake rate from inhalation of VOCs is approximately equivalent to the intake from ingestion of VOCs in the same water imply that the oral dose was multiplied by 2? A description of how the inhalation dose was obtained is needed. If the inhalation dose was assumed to be the same as the oral dose, the phase "approximately equivalent to" in the last sentence of the first paragraph should be "exactly the same as".

Comment 65

Page 4-19 and 4-20. Radionuclides.

Please state how the MCLs for gross alpha and beta were used to estimate potential risk. Section 5.3, page 5-77 suggests that hazard quotients were calculated; however, no statement is made.

Comment 66

Page 5-75, last paragraph, last sentence.

An explanation is needed for stating that groundwater ingestion is not likely to be a major exposure route for agricultural workers. Agricultural workers are likely to drink more liquids than the average resident because of exertion and heat. Is it assumed that most of the liquids consumed by the worker is trucked in from elsewhere?

REFERENCES

Blaedel, W. J. and Meloche, V. W., 1963, *Elementary Quantitative Analysis*, Harper and Row.

Chapelle, F.H., 1993, *Ground-Water Microbiology and Geochemistry*, John Wiley & Sons, p.369.

EPA, 1993, *DNAPL Site Evaluation*, Robert S. Kerr Environmental Research Laboratory, Ada, Oklahoma, EPA/600/R-93/022, February 1993.

Freeze, R.A. and Cherry, J. A., 1979,*Groundwater*, Prentice-Hall.

Garrels, R. M. and Christ, C. L., 1965, *Solutions, Minerals, and Equilibria*, Harper and Row.

Hem, J. D., 1985, Study and Interpretation of the Chemical Characteristics of Natural Water, USGS Water Supply-Paper 2254.

Singer, John A., 1973, Geohydrology and Artificial Recharge Potential of the Irvine Area, Orange County, California, USGS Open File Report 73-264.