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MCAS EL TORO
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Subject: OU-1 RCOMMENTS

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401 West A Street
Suite 1000
San Diego, CA 92101-7905

CLEAN II Program
Bechtel Job No. 22214
Contract N68711-92-D-4670
File Code: 0311

IN REPLY/REFERENCE: CTO-0080/0002

October 18, 1994

U S. Environmental Protection Agency Region IX
Hazardous Waste Management Division, H-9-2
75 Hawthorne Street
San Francisco, CA 94105-3901

Attention: Bonnie Arthur, RPM.

Subject: Review Comments on Marine Corps Air Station El Toro, California - Draft Operable Unit One Interim Action Feasibility Study Report including Draft Groundwater Modeling.

Dear Ms. Arthur:

Attached are review comments on the subject Interim Action Feasibility Study Report including Draft Groundwater Modeling dated 1 September 1994. Generally, this report meets the requirements for an acceptable CERCLA FS and it appears to satisfactorily evaluate and compare a number of potentially applicable alternatives. The final recommended alternative (4b) represents the preferred interim remedial action based on the evaluation presented. Because of the fact that ARARs fall within a very specialized category of agency review I have not commented on them. I have reviewed all other parts of this report..

There are numerous comments and concerns which should be addressed prior to agency acceptance of the final document. I have attached these comments in hard copy and electronic for your convenience. The electronic data are included as Word Perfect 5.1 and Word for Windows 6.0 files both in PC format. If you feel that this is assists you, I will provide electronic copy with all future comment packages. Let me know at your convenience.

I can be reached in Bechtel's San Diego office at (619) 687-8780; the facsimile number is (619) 687-8787 until October 19. I will be out of town until the Risk Assessment meeting on November 8 in San Francisco.

Sincerely,

BECHTEL NATIONAL, INC.

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

cc:

Larry Vitale, Remedial Project Manager
California Regional Water Quality Control Board - Santa Ana Region
2010 Iowa Avenue, Suite 100
Riverside, CA 92507-2409

Joseph Joyce BRAC Environmental Coordinator
Department of the Navy - Southwest Division
Naval Facilities Engineering Command
Environmental Division
1220 Pacific Highway, RM 18
San Diego, CA 92132-5181

Juan Jimenez, RPM Base Closure Branch
State of California Environmental Protection Agency
Department of Toxic Substances Control
245 West Broadway
Suite 350
Long Beach, CA 90802-4444

Attachments

To: MCAS El Toro BRAC Cleanup Team

From: Dante J. Tedaldi

Re: MCAS El Toro CH_2M-Hill Draft Operable Unit One Interim Action Feasibility Study Report including Draft Groundwater Modeling

Date 18 October, 1994

Summary of OU-1 Interim-Action Feasibility Study Report Comments

Generally, this report meets the requirements for an acceptable CERCLA FS and it appears to satisfactorily evaluate and compare a number of potentially applicable alternatives. The final recommended alternative (4b) represents the preferred interim remedial action based on the evaluation presented. Because of the fact that ARARs fall within a very specialized category of agency review I have not commented on them. I have reviewed all other parts of this report.

A great deal of effort was obviously expended on the groundwater model calibration and simulation tests. The modeling efforts form the basis for the support of Alternative 4 and appear to the reasonable.

OU-1 Interim-Action Feasibility Study Report

Executive Summary

Comment Number 1

Page ES-1

Is Volume I (the RI, HHRA, and the FS summary) available at this time?

Comment Number 2

Page ES-3

Figure ES-1 is missing.

Comment Number 3

Page ES-7

The distinction between when MCLs and MCLGs would be used is not provided but should be made here.

Comment Number 4

Page ES-9 through ES-11

The Executive Summary does not appear to be the place to summarize the screening of an alternative that was eliminated. Only the final alternative should be presented here.

Comment Number 5

Page ES-12

The text mentions the need/use of additional wells; however, Alternative 3 doesn't include these wells and this point is not clear in the description. Optimization of Alternative 3 occurs with Alternative 4 and the two concepts should be separated.

Comment Number 6

Page ES-14

Table ES-2 includes a numerical ranking system for alternatives. The CERCLA FS process does not incorporate such a system. When the difference between values is large, e.g., 1 and 4, ranking may not be a problem, but when the difference is perceived to be small, ranking becomes quite subjective. Moreover, at this stage, two scores are missing - Community Acceptance and State Acceptance, and these two could change the final outcome on a numeric basis, but not on the basis of their importance described in the text. The uncertainty associated with Alternatives 4a and 4b seem identical. The BCT should consider the pros and cons of this approach and provide guidance to the contractor.

Introduction

Comment Number 7

Page 1-19

For ease of reading, consider aligning the numbers by the decimal point. This comment applies to all tables in this document.

Comment Number 8

Page 1-25

Contract required detection limits (CRDLs) are applicable under the CLP program to inorganics only. Contract required quantitation limits (CRQLs) should be used when referring to organic CLP analyses. The correction should be made here and in all other tables.

The correction should be made throughout the text of the document also, because this error appears repeatedly. For example, at the top of page 1-33, 1-40, and 1-41.

Comment Number 9

Page 1-47

Considering the large amount of effort expended in the RI Report on the description of the nature and importance of the inorganic aqueous geochemistry at MCAS El Toro, it is recommended that additional text be included here which identifies key geochemical parameters which may affect the remedial actions. This is particularly relevant because the desalter is designed to remove inorganics and it is being considered as a principal component of the remedial actions.

Comment Number 10

Page 1-48

Nowhere in any previous OU-1 documents has a presentation been made regarding the temporal change in the contaminant plumes. The third bullet item needs to be supported in some way by the data. Although the model predictions used as part of Alternative 1 seem to fill this need, inspection of the model figures does not strongly support the notion of widespread migration. In fact, after 20 years, not much migration would appear to have occurred when considering the 5 µg/L contours.

Comment Number 11

Page 1-49

The bold text for the first bullet items appears to be correct; however, the reason stated following it may not be as true. Restoration time, difficulty and cost (RT, D &C) could increase with increased sorption, however, dispersion of the contamination would also increase RT, D & C. The statement should not attribute cause and effect solely to partitioning.

Comment Number 12

Page 1-49

The second bullet implies that there is some negative aspect related to the increased presence of DCE and DCA. What would that be? These compounds are far less toxic than TCE.

Remedial Action Objectives and ARARs

Comment Number 13

Page 2-4

The entire risk assessment section needs to be updated with respect to the comments on the RI and RA.

General Response Actions and Remedial Technology Screening

Comment Number 14

Page 3-1

Table 3-1 should note that institutional controls, although traditionally associated with legally enforceable restrictions and agreements, could also include public education programs.

Comment Number 15

Page 3-3

Table 3-2 should state that "Natural process of biodegradation are not expected to be effective and are extremely slow." There is no evidence to support the assertion that natural biodegradation of VOC is potentially effective at this site. Consider the fact that earlier (see Comment Number 12) the IAFS implied that the by-products of biodegradation are more problematic than the original contaminants.

Comment Number 16

Page 3-7

Air stripping, not only steam stripping, may also result in scaling of the column and packing with the water quality at MCAS El Toro. This should be noted.

Comment Number 17

Page 3-11

The term, reinjection, is a misnomer that is commonly used. The groundwater was never initially injected, therefore, it cannot be reinjected. Simply use the term, injection, here and throughout the report

Identification and Screening of Remedial Action Alternatives

Comment Number 18

Page 4-5

The figure and those which follow show an arrow head dropping down from the plume. This is may be misleading to some readers since it seems to suggest that the remedial action would cause the plume to move in a new direction. The arrows should be removed.

Comment Number 19

Page 4-11

The discussion of this alternative and those which follow are noticeably brief with respect to institutional controls. These descriptions should be more detailed because it is clear that any remedial action proposed would include an institutional controls program.

Comment Number 20

Page 4-12

The pumping rate for Alternative 2 is not mentioned here, yet later on page 4-17 it is. This should be corrected.

Comment Number 21

Page 4-12

The text here and elsewhere states that the pumping rate for the Desalter would be 8,000 ac-ft/yr. Because this expression of units is not commonly used in feasibility studies, the authors should maintain consistency and use gallons/minute or gallons/days.

Comment Number 22

Page 4-13

The description of Alternative 4 should include some detail of what can be expected from a system design standpoint. What are the expectations for flow conditions and extraction well locations and numbers?

Comment Number 23

Page 4-20

The nomenclature for the numbering of alternatives creates a bit of confusion later on because Alternative 2 seems to be missing, when actually it was simply screened out. A smoother presentation would have the final alternatives numbered 1, 2, and 3, rather than 1, 3, and 4.

Groundwater Extraction Options

Comment Number 24

Page 5-3

The production rate used in the model calibration should be noted.

Comment Number 25

Page 5-10

Terminology is confusing. What aquifer contains the 5 µg/L TCE concentration zone? Later in the paragraph a 50 µg/L TCE concentration zone is mentioned, but there is still no easily found definition of these terms. It seems that these concentration zones could be within any aquifer. The report should use only one system for description of contaminant zones.

Comment Number 26

Page 5-10

At the beginning of the paragraph the text states that "Under the Desalter-Only alternative...migration of contaminated groundwater within the Shallow Aquifer is reduced by pumping the Desalter wells." This language implies that the reduction is a beneficial result of pumping. However, later in the same paragraph the text states that "...highly contaminated groundwater from this zone may migrate into the Principal

aquifer." This implies that the downward gradient is a bit problematic from a restoration standpoint. Some clarity is needed here.

Comment Number 27

Page 5-11

The title of this figure indicates that it represents conditions within the shallow aquifer, but the figure shows projected flow paths within the shallow and principal aquifers. Apparently, the figure considers effects on the principal aquifer due to migration from the 5 μg/L TCE concentration zone in the shallow aquifer. If this is the case, it should be clarified.

Comment Number 28

Page 5-13

Similar to the preceding comment, the intent of this and other complementary figures should be clarified in the titles

Comment Number 29

Page 5-14

This figure conveys the essence of the comment above (Comment Number 12), i.e., Alternative 3 will draw more contamination mass into the deeper zones than Alternative 1. However, it would be helpful to see this in a table which clearly identifies the mass distribution between aquifers under different scenarios. Although we do not have an estimate of the initial source mass, a normalized representation would be helpful.

Comment Number 30

Page 5-22 through 5-31

Active source and former source scenarios both confirm that Alternative 4 would provide the greatest degree of cleanup; however, the distinctive components are the shallow on-station wells. The authors should confirm the predictions related to extraction rates from these wells because there have been indications that 600 gpm is far from achievable.

Comment Number 31

Page 5-34

Figure 5-8 is very difficult to interpret because in black and white there is almost no distinction between the flow lines and any of the other types of lines on the figure. It appears that this was an oversight in the copy room and the figure is actually a color illustration.

Comment Number 32

Page 5-38

It is unclear at this time whether the response to VOC migration in the northwest will be addressed by OU-2. Efforts between the OU-1 and OU-2 consulting teams will need to be managed with this objective in mind.

Development of Remedial Action Alternatives Remaining for Detailed Analysis

Comment Number 33

Page 6-4

Because institutional controls require administration and enforcement and there are costs associated with these actions, the cost for Alternative 1 cannot be zero.

Comment Number 34

Page 6-5

Units of expression should be consistent in the report; therefore, ppm should be replaced with mg/L and ppb with µg/L.

Comment Number 35

Page 6-19

It would be helpful and probably supportive of Alternative 4 to identify the estimated duration of time after initiation of Alternative 3 for which the implementation of source controls would be of little use.

Comment Number 36

Page 6-19

For comments on the cost estimates see Comment Numbers 90-92.

Comment Number 37

Page 6-21

An enhanced discussion of the viability of maintaining or even achieving 60 gpm/well should be included. The text should identify potential actions or consequences of the wells not being capable of maintain the predicted flow. Will the objectives of Alternative 4 still be met if the flow is less than predicted? These issues have been adequately discussed in meetings but they should be clarified in the text.

With the apparent uncertainty in the well yield, it may be appropriate to use a range of flow rates in the evaluations rather than a single value.

Comment Number 38

Page 6-22

The addendum was received on October 6, 1994, "Addendum to the Draft Operable Unit 1 Interim Action Feasibility Study Report," dated 30 September 1994. No specific comments were identified.

Comment Number 39

Page 6-29

The text states that if Phase II data indicate that higher TCE concentrations in the source area exist, groundwater extraction for the source are may not be valid. This statement does not support the other arguments in favor of Alternative 4, namely that source control will prevent the migration of VOCs downgradient to the desalter wells. If higher concentrations are found at the source area, it would only emphasize the need to hydraulically control the source. Other technologies such as air sparging may be effective in removing VOCs from water, but the rest of this IAFS has lead the reader to believe that hydraulic source control is essential at this site. It would seem that other technologies, in addition to hydraulic source control would be appropriate if higher concentrations are found.

Comment Number 40

Page 6-39

Provide a definition of FRP (Table 6-5) and 8 in. WC (Table 6-6).

Comment Number 41

Page 6-41

MICR is not a common acronym and should be spelled out here. It is also missing from the Acronyms and Short Forms.

Detailed Analysis of Remedial Action Alternatives

Comment Number 42

Page 7-2

The maximum detected TCE value is missing from the text in the 4th paragraph.

Comment Number 43

Page 7-5

For Alternative 3, indicate which aquifer (the principal aquifer it would seem) would eventually be flushed.

Do not concur with the statement for Alternative 3 that "theoretically...no wastes would remain onsite, and therefore no residual risk would persist." Soil contamination in the vadose zone would be unaffected by the desalter system and therefore contamination would remain, even in a theoretical evaluation. See the discussion on pages 7-13 and 7-14 of the IAFS.

Comment Number 44

Page 7-5

For Alternative 1, consider changing text to "Does not actively reduce toxicity, mobility, or volume." This would eliminate the confusing combination of "...increase reduction..."

Comment Number 45

Page 7-5

For Alternative 4a, under Short-term Effectiveness. A hyperbola is a plane curve having two branches. Thus, the term "hyperbolic value" does not seem to be appropriate for the use provided in the text. A more accurate term would be "asymptotic value." Also, indicate where within Section 5 that the discussion of asymptotic values is found.

Comment Number 46

Page 7-9

See previous comments regarding the non-zero cost of No Action.

Comment Number 47

Page 7-9

Here and elsewhere in the report all large cost numbers (greater than $10M) should be rounded to the nearest million dollars, smaller costs should be rounded to the nearest hundred thousand.

Comment Number 48

Page 7-19

The statement related to the preferential sorption of VOCs onto fine grained material should be referenced.

Comment Number 49

Page 7-21

Arsenic is a carcinogenic metalloid.

Comment Number 50

Page 7-22

The meaning of the expression "...could require half again to twice as long..." is not clear and the sentence should be corrected to make sense when compared to the numerical values provided in parentheses.

Comment Number 51

Page 7-30

There is a typo error in the second to last sentence of the 4th paragraph. Alternative 3 is stated twice, but the comparison is between Alternative 3 and Alternative 4.

Comment Number 52

Page 7-36

The nine U.S. EPA evaluation criteria do not include uncertainty as a separate criterion.

Comment Number 53

Table 7-2

The table indicates that retardation has moderate effect on the contaminant concentrations. However, no retardation factor was used in the modeling efforts. The intent should be clarified.

Appendix A Groundwater Modeling Report

Comment Number 54

Section 3.2.3

Since the shallow aquifer is unconfined, the use of the term storage coefficient, instead of the term specific yield, seems to be inappropriate.

Comment Number 55

Section 3.3

Why the review of past ground water level data was restricted to the years 1957, 1965, 1983, and 1985 through 1990? A review of data from other years would provide further insight into changes in regional ground water flow direction and how they are affected by hydrologic conditions (dry vs. wet years) and ground water usage. Ground water level data for other years should be available from the Department of Water Resources.

Comment Number 56

Section 4.2.2

The report does not adequately explain how the hydraulic conductivity data presented in Section 3 were used to define the hydraulic conductivity zones presented in Figures 4-4a,

b, c. It would be helpful to present the hydraulic conductivity data on a map to facilitate the comparison of the hydraulic conductivity zones used in the model with the measured values. Including a Figure similar to Figure 3-22 of the OU-1 RI Report would help make the report more self contained.

Comment Number 57

Section 4.2.5 (page 4-10)

It would be helpful to expand the discussion on numerical dispersion to discuss the performance of CFEST in this respect based on past experience with the code.

Comment Number 58

Section 4.2.5 (page 4-12)

The discussion of the highest TCE concentration (1,000 µg/L instead of the observed maximum value of 2,000 µg/L) in the initial condition used in the model for the predictive simulations leaves some unanswered questions. Does the TCE distribution used as initial condition in the model provide the same total TCE mass as that estimated from the field observations? Does this mean that the discretization of the problem domain in the vicinity of the highest observed concentrations is too coarse?

Comment Number 59

Section 4.2.5 (page 4-13)

The statement "Although variations in TCE concentrations...suggest that multiple sources of TCE may be present, the modeling approach assumes only one source of TCE contamination" needs further clarification. Does the one source included in the model provide the same TCE mass as the combined total of the individual sources? Some of the potential sources are located at substantial distances from each other. How does the omission of potential sources, e.g., the Magazine Road Landfill source, affect the reliability of the model predictions?

Comment Number 60

Section 5

Flow Model Calibration: Besides minimizing the RMS error between observed and simulated heads, it is equally important to match the ground water flow direction. The model in general matches the observed flow direction, but with two notable exceptions.

The flow direction in the shallow aquifer at the base seems to be more to the west, while the interpreted flow direction is more to the northwest (compare Figure 5-1 with Figure 3-

10). It would be instructive to include a Figure superimposing the two sets of contours to illustrate agreement, or disagreement in flow directions in critical parts, in terms of contaminant transport, of the simulation domain. How does this difference in flow direction affect contaminant transport predictions?

The pumping center west of the base shown in the Fall 1992 data is not reproduced by the simulation (compare Figure 5-1b with Figure 3-11).

Comment Number 61

Section 5.1 (page 5-1)

The definition of the RMS error should read "the RMS error is the square root of the average of the squared differences of..." instead of the "the RMS error is average of the squared differences of..."

Comment Number 62

Section 6

Solute Transport Model Calibration: It is recognized that it is practically impossible to meaningfully simulate the migration of contaminants over the past 50 years with no documented information about contaminant sources and limited information about ground water flow conditions during this period. However, there are two areas of disagreement between simulated and observed concentrations that merit further discussion.

1) The simulated TCE plume is much wider than the observed plume. The report states that the lateral dispersivity used in the model calibration was equal to zero (page 6-3). This raises the question of why the simulated plume is so wide and calls for an assessment of the magnitude of lateral numerical dispersion in the model. It would be instructive to compare the simulated plume with particle tracks originating from the same sources as those used in the model calibration. The lateral extent of the simulated plume with the two \methods should be about the same. If not, the difference might be attributable to lateral numerical dispersion.

2) The calibration did not include any sources in the Magazine Road landfill area.

Comment Number 63

Section 6

Iit would be appropriate here to discuss the impact of using a steady-state flow field for calibrating the model over the last 50 years, in view of the fact that during this period

there were significant changes in the direction of ground water flow, as suggested by the maps presented in Attachment 2.

Comment Number 64

Section 6.2 (page 6-2)

The references to Figure 4-5 should be changed to references to Figure 4-4b. There is no Figure 4-7 in the report.

Comment Number 65

Section 7

Sensitivity Analysis: The sensitivity analysis presented in this section is focused on the model predictions, not the calibration. Therefore, this section should follow Section 8 which presents the model predictions for the base choice of input parameters.

Comment Number 66

Section 7.1.2 (page 7-3)

The discussion of the sensitivity analysis for the specific yield is confusing. Since the principal aquifer is confined, the use of the term specific yield for this aquifer (layers 3, 4 and 5) is inappropriate.

Comment Number 67

Section 8.1 (page 8-1)

Does the term "...time interval..." in the sentence "a 91-day time interval was used in these simulations" mean "time step", or does it mean something else?

Comment Number 68

Section 8.3.1 (page 8-7)

In the sentence "The Desalter extraction wells are..." the reference to Figure 8-9 should be changed to Figure 8-5.

Comment Number 69

Sections 6, 7, and 8

The Principal Aquifer is represented in the model by 3 layers (layers 3, 4 and 5). However, all the contaminant transport simulation results presented in Sections 6, 7 and 8

are described as contaminant distributions in the Principal Aquifer. Are these average (geometric mean) concentrations of the concentrations in layers 3, 4 and 5? What is the vertical distribution of contaminants in the Principal Aquifer predicted by the model? Are there any field data to establish the vertical extent of contamination in the Principal Aquifer?

Comment Number 70

Section 9

Model Uncertainty and Limitations: The discussion of the model limitations should address the potential impact of numerical lateral dispersion on the simulated contaminant distribution. The simulation results (Figure 6.1) suggest that the lateral numerical dispersion may affect the model results. This would affect both the lateral extent of the simulated plumes and the their peak concentrations.

Comment Number 71

Section 9.3

What is the uncertainty arising from hydrologic conditions (wet vs. dry years) and their impact on aquifer recharge. An evaluation of all past WDR ground water level maps in conjunction with hydrologic data for the same period could provide insight into any relationship between the temporal and spatial distribution of recharge and changes in ground water flow direction.

Comment Number 72

Section 9.4

The discussion of uncertainty in calculated concentrations should address ongoing contamination sources.

Comment Number 73

Section 9.5

The recommendations on improvements to the model should include an evaluation of other potential contamination sources.

Comment Number 74

References

The list of references is incomplete. For example, it does not include the references Cherry(1994) and Neuman (1987) cited in page 4-10.

Appendix B

This Appendix was not reviewed. Refer to introductory comments for explanation.

Appendix C

Comment Number 75

Page C-1

See Comment Number 37 regarding concerns about the selection of a single value for production from the shallow aquifer.

Comment Number 76

Page C-3

Although a great deal of environmental data are lognormally distributed, some readers may not know this and an explanation of the basis for the assumption of lognormal distributions and thus the reasons for the use of geometric means should be included here. In addition, because mathematically the geometric mean is always lower than the arithmetic mean, the authors should establish the validity of using this descriptor.

Comment Number 77

Page C-7

The assumed removal efficiencies are presented without a basis for the assumptions. It would seem that these efficiencies are the output of the model predictions based on a specific flow rate, temperature, packing type and size, column width and height, and other key design parameters. If this is indeed the case, then the sentence should read "...based on the following calculated removal efficiencies..."

Comment Number 78

Page C-7

In the last partial sentence on the page, it is not clear if the statement refers to combined flow of the on-station wells and the deep desalter wells or simply the on-station wells alone.

Comment Number 79

Page C-8

In the last sentence of the first paragraph. The statement regarding the 10 percent increase in TCE concentration from 3.2 µg/L to 3.6 µg/L is barely supportable and should be reconsidered. The calculation errors associated with the stripping model assumptions and round-off errors lend little support to such small differences. The authors should state the magnitude of the error associated with their calculations if they insist on maintaining a certain level of significant figures.

Likewise, in the last paragraph on page C-8 the text identifies a calculated TCE concentration of 32.7 µg/L and compares this value to the desalter design value of 21 µg/L. It would seem appropriate to round the value to 33 µg/L.

Comment Number 80

Table C-1

The geometric mean value is provided at the end of the first row of data from Round 1. Does this mean that the mean value is representative of only the Round 1 data? The mean should be calculated from all the data from both rounds.

Comment Number 81

Table C-1

This table includes several mean values which should be rounded to eliminate the presentation of unsubstantiated significant figures.

Comment Number 82

Table C-1

It is incorrect to present a mean pH. The non-conservative quantity of hydrogen ion concentration cannot be averaged. Temperature and conductivity should not be averaged. Present these values as a range, not a single mean value.

Comment Number 83

Table C-1

When a U value is presented, is it correct to assume that the detection limit (U value) and not 1/2 U was used in the calculation of the geometric mean?

Comment Number 84

Table C-2

Similar to previously stated concerns regarding use of significant figures, especially calcium, chloride and cyanide. Also, refer to previous comments regarding erroneous use of mean pH.

Comment Number 85

Table C-3

Remove mean pH and do not present a flow proportioned pH value. Similar comment for temperature.

Comment Number 86

Table C-5

It is not clear what the Removal (%) column refers to. Provide clarification of basis for removal calculation.

Appendix D

Comment Number 87

Page D-3

The need for radon measurement in groundwater as part of Phase II should be communicated clearly to the CLEAN II team.

Comment Number 88

Page D-7

Add the complete reference with date and source of publication for the article by Ford et al.

Comment Number 89

Table D-6

The footnote is not linked to the correct entry in the body of the table.

Appendix E

Comment Number 90

Table E-1, E-2, and E-3

The additions to the construction costs, represented as 5 to 20% multipliers, seem a bit on the high side in total; however, since these factors are applied uniformly this fact should not affect the cost comparisons between alternatives. In most cases, engineering design under CLEAN I and CLEAN II cannot exceed 7 percent of the capital cost.

Comment Number 91

Table E-1, E-2, and E-3

Any mechanical system that operates for a period of 40 years will undoubtedly require frequent repair of major components periodically during this period and probably may need complete replacement of most major components at least several times during this long period of performance. These additional non-annual O&M costs are absent from the estimate basis and these items will probably represent a significant cost when viewed as part of the present worth analysis. This issue should be addressed.

Comment Number 92

Table E-1, E-2, and E-3

As noted earlier, here and elsewhere in the report all large cost numbers (greater than $10M) should be rounded to the nearest million dollars, smaller costs should be rounded to the nearest hundred thousand. There is no reason to believe that a greater level of accuracy exists for these estimates.

Appendix F

Comment Number 93

Page F-16

Provide an explanation of Field's hydraulic calculator.

Appendix G

No comments.

Appendix H

Comment Number 94

Page H-3

Does V represent the total cell volume or the pore volume?

Comment Number 95

Page H-7

The computer model assumed no retardation, i.e., retardation factor equal to unity. Why was a retardation factor of 2 used for these calculations? The calculations are highly sensitive to the retardation factor and therefore a range of values should have been used as part of a sensitivity analysis, rather than a single value.

Comment Number 96

Page H-9

Third sentence of the first paragraph. What does the statement "...based strictly on soloing the mass balance equations..." mean? It appears to be an error.

NAS North Island

Restoration Advisory Board

Process for Conducting the November 1994 Community Co-Chair Election

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1. Prepare Co-Chair Applications and/or use previously completed RAB membership applications if already submitted.

2. Pass out the Co-Chair candidate applications to interested RAB members at the October 17, 1994 RAB meeting, as well as, make them available through Arno Bernardo, Navy Co-Chair and NASNI Public Affairs Office. The (suggested) deadline for submitting completed Co-Chair applications is October 31, 1994. (Note: the November RAB meeting is set for November 16.)

The Co-Chair candidate applications could contain the following questions/statements. This information was previously requested on the RAB membership application submitted by all RAB members.

- Please list any group affiliation (if private citizen, please state)
- What has been your experience working as a member of a diverse group with common goals?
- Briefly explain why you are qualified to serve as the RAB Community Co-Chair.
- Candidates may also submit other information such as a resume or write-up about themselves if they desire.

3. Candidate descriptions will incorporate the above bulleted information into ballots used at the Nov. 16 Co-Chair election. (This will be prepared much like the July 21, 1994 election handout.) The election handout will contain a bulleted description of the RAB Community Co-Chair responsibilities; candidates descriptions (1-2 pages per candidate); two pages for note taking; and the election ballot.

4. At the November RAB meeting, each candidate will have 2-3 minutes to speak to the other RAB members on their qualifications and interest in becoming the Co-Chair. Names of candidates will be drawn from a hat to establish the order of the speakers -or- speakers will go in alphabetical order.

5. After the candidate presentations, RAB members will vote. Ballots will be placed into a ballot box. Ballots will be tabulated by a volunteer RAB member, or a DTSC representative, or another agency representative. If the vote is a tie, an immediate runoff election between the runoff candidates will be held. If necessary, the runoff candidates could have an additional 1-2 minutes to speak again prior to the runoff election.