Review of the Delta Risk Management Strategy Report, Phase 1

URS Corporation/Jack R. Benjamin & Associates, Inc., June 26, 2007

CALFED Science Program Independent Review Panel

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Review Summary

The *Delta Risk Management Strategy study* (DRMS), which comprises two phases, will underpin policy decisions regarding future infrastructure investments and water resource management in the San Joaquin-Sacramento Delta region for decades to come. *Phase I* results must establish a robust scientific and engineering foundation. This is essential for completing *Phase II*, the identification, and prioritization of strategies for reducing risk in the Delta. In short, *Phase I* is a vital first step in assuring the future sustainability and productivity of the Delta region.

The Independent Review Panel (Panel) found many technical problems in each section of the *Phase I Report*. Several of these emerged as major concerns because they may greatly influence the results and conclusions presented in the report. The major concerns which the Panels terms Tier 1, were: (1) lack of documentation and transparency of analyses, (2) limited actual analyses carried through to the end, (3) limited treatment of uncertainty, (4) lack of integration of single component analyses to produce the final results, and (5) lack of a clear, robust methodology for assessing impacts on aquatic resources. Other important technical concerns (Tier 2) were related to specific analyses in each section. The Panel believes the impact of these issues on the final analyses may be moderate to minor in nature.

For many components of the report, the general approach of the DRMS analysis is well done and consistent with standard practice. However, for other components, the science must be strengthened and most importantly, the implementation (coupling of the components and their models) must be fully transparent, which can only result from improved documentation and completeness to the analyses. As written, many of the analyses are generally incomplete and therefore inadequate to serve as a foundation from which to make reasonable policy decisions about future resource allocations concerning strategies for the Delta region. In other words, the Panel believes strongly that the inadequacies in some of the analyses may lead policymakers and others to erroneous conclusions and inappropriate decisions.

Tier 1 Issues

Lack of Transparency of Analyses

The report is poorly written, lacks transparent documentation of methods, including assumptions (and departures from assumptions), is unbalanced in terms of treatment of hazards and lacks consistency in how the risk analyses are performed. Probability, frequency, rate, likelihood, and even risk are used interchangeably and not consistently or clearly defined. It was difficult for the Panel, who are well versed in these topics and models, to piece together exactly what was done. One very important aspect of good scientific and engineering practice is clear and understandable documentation of assumptions, methods, results, interpretations, and conclusions. Indeed, the report is inconsistent to the point that what was described as having been done in the beginning sections does not match what was done in later sections. A few of the sections are better documented, especially when coupled with their associated technical memoranda (e.g., seismic and flooding), but most, including the critical sections that integrate the various analyses, suffer greatly from inadequate documentation. There is little comparison of results to previous analyses, and some spot-checking by the members of the Panel suggested that aspects of some of these new results are significantly different from the results of similar previous analyses. In fact, the entire project seems not to have followed standard review practices. As it is written, this draft report fails the adequate documentation standard, which necessarily means it fails the test of providing adequate information for public decision-making.

Limited Actual Analyses Carried Through to the End

Beyond the poor documentation issues, the fundamental technical problem with the report is that many of the critical analyses are simply incomplete. That is, what is promised in early sections of the report (complete probabilistic assessment of risk) is not delivered. The probabilities and consequences are not integrated over the full range of possibilities, from high-frequency, small consequence events to low-frequency, large consequence events. Human health risks, in terms of probabilities and consequences, are not provided. Only 18 earthquake scenarios are assessed for economic and ecosystem consequences, and even fewer flooding scenarios are assessed and they all correspond to low-frequency, large magnitude events. There is little if any attempt to evaluate the sensitivity of the results to input parameters and to assumptions in the modeling. This product at present is a major departure from the plan, from what was described at public presentations by the DRMS team, and even from what is described in the report itself.

Furthermore, there is an apparently unbalanced treatment of seismic versus hydrologic events in the risk analysis. For hydrologic events, consequences are only assessed for two scenarios of flooding. Consequences for the most frequent types of hydrologic failures historically, where fewer than ten islands are flooded, are completely neglected. Consequences due to water-supply disruption in the case of flooding from hydrologic events, even though it has occurred historically in a high-tide event, are neglected. Conversely for seismic events, consequences are assessed for eighteen cases of flooding, ranging from single to multiple-island failures. In addition, the estimated frequency for flooding from seismic events is much larger than what is supported based on available information. The return period for an earthquake causing at least one levee failure is estimated to be about ten years, while a single event of this type has not occurred in over 100 years of history. Even considering only the past 20 years of history in which the configuration of the levees has been more similar to that at present, the analysis predicts that there would have been two failures on average and only a 16-percent chance of observing what has actually been observed: no failures. This unbalanced treatment of risks provides a potentially biased result, especially when comparing between seismic and flooding effects in evaluating mitigation measures. It is a serious flaw in the analyses presented in the draft report, which would be best solved by completing the analyses the project team was initially going to undertake, which means simulating many additional and more representative scenarios or fully enumerating all the scenarios. It is critical to recognize that electing to limit the full range of scenarios considered is a subjective decision, and without clear documentation as to why the decision was made, damages the concept of applying a quantitative tool as a way of being more objective.

Limited Treatment of Uncertainty

The IRP found that the method proposed to treat uncertainty described in the assessment was not actually represented in the reported results. That is, the authors included uncertainty, which is admirable, but only in the originating analyses of seismic and flooding events. They then report this originating uncertainty as the *total* uncertainty, which implies much more confidence in results than is actually justified. For example, consider the climate change projections. In the *Climate Change Technical Memorandum*, the uncertainties in sea-level rise and temperature for the year 2100 are captured through a recommended set of ranges or probabilistic curves that should be used in the simulations. However, in the actual report these are simplified to single values for years 2050, and 2100. This creates a false and potentially dangerous sense of inevitability and certainty. It implies that this is what "will" happen in the future, when in fact what happens could be far worse or better based on the uncertainty.

Scientific and socio-economic uncertainty must be presented clearly and propagated through all analyses. The analyses performed actually show the sensitivity of results to uncertainty for a few selected parameters. Since this is not the uncertainty one would realistically expect in the entire analysis, the assumption that only a few parameters really influence uncertainty must be documented and empirically supported. Without a true uncertainty analyses or documentation of why only a few uncertainties actually matter, it is impossible for the Panel to be confident that the results are a reasonable presentation of the risks and uncertainties embedded in the system. At a minimum, the report text should reflect what has actually been done (as seen in the reported results), should clearly document and support procedures and critical assumptions, and should include simple numerical examples displaying the linkages throughout the empirical sections of the report showing how uncertainty is propagated.

Lack of Integration of Analyses

The Panel was unable to fully understand how the multiple models used to assess the risks were linked together and how robust the results are to assumptions made in linking them. In analyses that use multiple, linked models, the details of how information and computer files are transferred and maintained to ensure all analyses use consistent information is a major bookkeeping challenge. As such, it is important that the discussion is transparent in terms of how the pieces (models, assumptions, etc.) fit together, and how robust the subsequently estimated frequencies and consequences are. Documentation of the QA/QC procedures used with the modeling process should comprise a separate technical memorandum. More information should specifically be included with the consequences modeling, especially with the consequences to human health and safety and fisheries resources.

Lack of Robust Methodology for Assessing Impacts on Aquatic Resources

The Panel is concerned about the treatment of ecosystem consequences in the analysis. There is, again, a major disconnect between the introductory methodology description, both in the beginning of the report and the beginning of the ecosystem consequences section, and what ultimately seems to have been done. As currently structured, the ecosystem analysis is incomplete, difficult to interpret and potentially understates the ecosystem effects of the various hazards confronting the Delta. While the Panel was of the opinion that the simplified approach used for terrestrial taxa was reasonable, the simplified approach used for the fish was inadequate. A new “risk index” was introduced for assessing the risks to key fish species. No justification or rationale is provided for, what appears to be, a new method. The reader has no idea how the weights were determined, how the computed risk index behaves, and what levels of the index should flag concern. The Panel had no idea how to interpret the changes in the risk index under the few earthquake and flooding scenarios that were performed and the authors also seemed to have little idea on how to interpret their own risk index. While the Panel appreciates the complexity of performing such an analysis and the unsuccessful attempt to develop a quantitative metric, alternative approaches are available to provide information on this important category of effects. For example, the authors may wish to assemble an expert panel to evaluate a small set of scenarios, which encompass a wide range of outcomes. Something better than the risk index needs to be developed, evaluated, and implemented to understand potential ecosystem consequences.

Concluding Tier 1 Comments

Until the major issues presented above are substantively addressed and the analyses are completed as originally proposed, the results of the DRMS *Phase I Report* are of limited utility. The Panel seriously questions the usefulness of any Phase 2 analyses that relies on results reported in a Phase 1 draft report that is not significantly revised to address the Panel's Tier 1 comments. The Panel is also emphatic that simple responses to their major comments that do not involve changes to the analysis methods would be considered an inadequate response by the Panel. We understand the time pressures that have been placed on the DRMS analysis, but the results are too important and potentially too useful to be rushed to the point that the results are not trusted or that the generated results are unjustified. In reviewing the DRMS project team responses to previous comments on the *Phase I Report* and technical memoranda, there seemed to be an inconsistency in the way in which review comments were handled. Some comments appeared to be simply dismissed, despite raising valid concerns, while others received more thoughtful responses. In scanning the review comments, there seems to be a predisposition toward constraining the scope of the report to an inappropriate degree. The Panel raises this final issue so that authors of the draft report can address our major comments with thoughtfulness, and make the needed changes in the analysis to make the DRMS as useful as possible.

Detailed Comments and Tier 2 Issues

These comments are compiled from all the individual comments made by panel members on reviewing the DRMS *Phase I Report*. They were edited by the chair for readability and consistency, however, because they represent merged individual comments, there may be some inconsistency among specific statements. Each section of the report is covered, including the summary report. Individual editorial comments by the panel members are also included and tied to page numbers. For each section, we have summarized the Panel's comments into a general statement for each section. The specific comments following that statement cover all aspects of the report from writing through content. Although the Panel's charge was to review the report, we also read the various Technical memoranda. Where information in those documents were useful for the review we have added comments, but we considered the report as the primary document that would be read by the broad community interested in Delta issues, so concentrated on that document.

Summary Report (June 26th Version)

General Comments:

The following comments pertain to the "Summary" section that the Independent Review Panel (Panel or IRP) reviewed prior to the August 2-3 meeting. Some of these comments may no longer apply if the summary has been rewritten, but the general concerns raised here, and in the IRP's summary review of the entire document, should be addressed in any revision of this chapter.

As with most complex assessments done in support of public policy decisions, this report starts with an introduction followed by a lengthier "Summary" section (42 pages) describing procedures and results from the overall Phase 1 effort. Collectively, this "Summary" section is arguably the most important part of the entire *Phase 1 Report*, given that policy makers, stakeholders, and the public are unlikely to read the entire report. It is critical that this section represent clearly and concisely the nature of the problem (i.e., the charge as contained in AB 1200); the methods used to assess the charge; including assumptions, strengths, and weakness of the methods; the results of that assessment; and some cautionary overview of how these findings should, or should not be used in the public policy arena.

While the panel has sympathy for the authors of this report in terms of the complexity of their charge and the timelines under which they operated, we are disappointed with the original "Summary" section for several reasons. First, we find the quality of exposition uneven (we judge it to be among the most poorly written of the entire set of chapters). Second, and more important, we find the description of procedures to be confusing and misleading regarding what was actually performed in developing the findings. Third, we believe the authors are overstating the nature of their findings, giving greater weight to earthquake damages (and implicitly less weight to other hazards, such as low damage, high frequency events). The discussion in the current "Summary" section concerning the definition and treatment of risk and uncertainty in the assessment also implies a greater degree of precision than actually exists in the results. This combination of lack of balance in the hazard analysis and false precision in reporting the results is worrisome because it may encourage inappropriate use of the findings, particularly with respect to allocation of future resources to address Delta problems and by focusing attention on the risks to each island. Fourth, the overall risk framework used in the report and described in both this “Summary” section, and later individual sections differ from standard risk assessments that are familiar in the economics literature (e.g., the authors chose to combine risk and consequences, whereas their charge clearly distinguishes between them – see AB 1200). This is not necessarily a problem but it does call into question whether the economic losses are reported correctly (e.g., as Expected Monetary Values, EMV’s). Finally, the treatment of uncertainty in the assessment is confusing and unbalanced.

The writing was extremely uneven, with too much detail in some places, not nearly enough in others. It was very difficult to pull out the main messages of the report: What were the big results? Why are they important? The “Summary” section needs to present the big picture, not just smaller details, and at a level that can be read by anyone with more than an eighth grade education. The authors should be aiming for an Intergovernmental Panel on Climate Change (IPCC) like "Summary for Policymakers" product. Use of summary tables would help, as would a good edit of the bullet points as the language used was very repetitious and made no effort to distinguish between more important vs. less important results. The whole thing is in desperate need of a good edit to get rid of grammatical typos and repetitive sentence structures.

Furthermore, we were very confused between the overview and the actual summary (beginning on page 9). They contain much of the same information, word-for-word! What is the point?

Specific Comments:

Regarding sea level rise (SLR), absolutely no explanation why this report considers a wider range of future SLR than the IPCC. The summary must be a stand-alone document and in its present state, it is not.

"More winter flooding" is not the right title for the next paragraph.

Probabilities of different events (hole-in-one, cancer, etc.) are cute but don't really have a place in a serious scientific report. These are not the funny pages of the Sunday newspaper. Not to mention deceptive – many more people have hit a hole-in-one than one in 5000 – that's per shot, not per lifetime.

All references to "delta" should be capitalized.

Page ii: The box with a definition of risk is helpful (encourage even more sidebars in explaining key concepts and definitions) but it seems to be combining standard notions of risk with the consequences. This requires presenting results in terms of expected monetary values (EMVs), which we do not think is actually done. Also, the text in the adjacent paragraph claims that this framework is *unique*, in that it includes dimensions of the problem previously not treated. Is this really true? Since there was no original or new research performed in this study, it seems that what the study has done is bring together secondary information (including from other studies). The failure to cite important previous studies, such as Torres et al. (2000) on earthquake risks, along with a general lack of citations overall, is unacceptable.

Page ii: We do not agree with the statement “While estimating the likelihood of stressing events can generally be done using current technologies, estimating the consequences of these stressing events at future times is somewhat more difficult.” Why is it any easier to estimate the likelihood of an event than the consequences of the event? This perspective biases this study because a disproportionate effort was devoted to assessing likelihoods versus consequences.

Page iii. First bullet: Do you really have the precision in your analysis to make this sort of assessment of differing probabilities on such fine scale, given that it appears that inventories of levee integrity are lacking? Also, the paragraph on seismic risk is confusing. For example, what does the second sentence mean, “it is expected [...] could happen […] in next 25 years”

Page iv: Comparing the forecast risk of a flood event with the historical record is useful. We suggest that the authors add information on the historical frequency of the forecast risk to the discussion of other events, such as “sunny day.”

Page v. First bullet: Explain why the frequency is expected to increase by 12 %. Third bullet, the “combined effect” of what?

Page 2: In the first objective of the DRMS charge, note that “risk” and “consequences” are listed as separate parts of the charge, whereas in the assessment effort, risk is defined in terms of the consequences. The authors need to be consistent. Also, this report should note that items 2 and 3 are to be performed in Phase 2.

Page 6. Second paragraph: What are “appropriate” combinations? How treated in the risk framework? Our reading of subsequent chapters does not reveal how or if this was actually done.

Page 8. Under “future conditions:” The last phrase in the first paragraph is not a complete sentence.

Page 10: This is an important page, given that it contains the description of the risk analysis approach. We appreciate the authors’ use of sidebars. Note again that risk seems to be defined as the frequency of economic or ecological damage, instead of frequency of earthquake-induced levee failures, etc. Is this really what the authors intended to say? Also, the description makes some claims about including ranges of outcomes for all the dimensions of future risks. We do not see this in section 14, so we assume they are talking now in idealized terms? If the latter, then we think the summary report is misleading the reader as to what actually gets presented in the outcomes chapters.

Page 11: The scope of the analysis is helpful but we suggest the authors define “uncertainty” in a sidebar here to inform the reader as to how it will differ from the probabilistic representation of risk, which seems to also embed a type of uncertainty (the variability of outcomes). Also, in the last bullet, we agree with the challenge (futility) of trying to forecast many of these economic drivers out beyond 50 years but we are not sure we would say that the BAU is an “unbiased” measure, instead, it maybe less prone to error.

Page 12: This diagram is presented in chapter 4 and was also presented to the IRP in Sacramento. It appears to be a highly stylized portrayal of the integration process and does not help the reader much in terms of following through the step-by-step integration that goes from probabilistic-based information on certain events, to scenario-based states of nature, to the measurement of actual economic and other consequences. As we note in our review of chapter 4, a lot seems to be swept under the table.

Page 13: How “unique” is DRMS? More comprehensive? More innovative?

Page 14: Need a “be” between “cannot and reduced” in the middle of the page. The last paragraph makes an important disclaimer regarding results: they should not be used for decisions about any specific levee reach or island. However, in other places the authors present localized effects. Given that the authors present results that they feel should not be used, how do they then intend to prevent them from being used inappropriately?

Page 14. Figure 6: We do not have wind information out to 2100.

Page 16: Last paragraph notes that a levee has never failed in the Delta due to earthquakes. How does this square with the forecasts of a major failure within the next 25 years? Are the authors hyping earthquake risks because it is emotionally charged in California?

Page 17. Under “methodology:” Please explain how the analysis treats uncertainty in the forecasts of risks of earthquakes?

Page 20: How can one defend a forecast of an average failure rate from earthquakes of over one per year for the next 100 years when there has not been one in the past 100 years? Also, at this point, the risk analysis becomes scenario based. But the scenarios seem to be treated as equally likely; so at this point, the analysis departs from the described risk analysis framework.

Page 21: There is a lot of equivocating language here (“might be”, “usually will be”, “generally additive,” etc.), which differs from the tone of other sections. The authors need to be consistent, unless they have suddenly become more cautious?

Page 22: We suggest the authors use the word economic *damage*, rather than cost. Both terms can convey economic efficiency effects. This would apply to the subsequent tables in which economic “losses” are reported. Also, under “ecosystem consequences:” “the percent of the population” of what?

Page 23: Where is this "risk index?"

Page 24. Near top of page: what is "ruderal?" Also, at bottom of page the authors present information on probabilities of failure at each island and explain that table 5 is a "convenient" way for a landowner to assess their risk. This flies in the face of the earlier, and important cautionary note that this should not be done!

Page 27. First sentence: The seismic "risk" (the probability of an earthquake) is not going to increase, only the resources at risk will. This odd language is the outgrowth of the way the authors choose to define risk. Also, in table 6 and others that report economic damages, we believe that it is important to note that this is not an EMV, but some other type of estimate.

Page 28: How do the authors know that "non-historical floods" are a more accurate measure? Also, in the first paragraph, delete "the" between "may" and "cause."

Page 29: Last paragraph, insert "one island" between "than" and "fail."

Page 30: Under consequences of flood events, the authors again mix scenarios into the probabilistic analysis. Why not use probabilities of these three types of events?

Page 31: On the vertical axis of Figure 16, why not use "billions" instead of millions?

Page 32: The authors again report individual island failure projections. In view of earlier admonitions about why these should not be used, why present them? Also, in the last line, need a "the" before "historical."

Page 35. First line: authors should refer to this as the "*expected*" climate change (since they do not know what the change will actually be). Later in the same paragraph, "to be" is repeated.

Page 36. Methodology paragraph: "data *were*…". Also, this is the first use of scientific notation (need for consistency?). Under "Levee Failure" "[…] few available data" *sets*? *Points*? The next paragraph and following page have more equivocating language, e.g., "seems," "seemed to be."

Page 37: How are these problems calculated for the sunny day events?

Page 38. Middle of page: where are these conditional probabilities provided in the report and upon what are they conditioned?

Page 39: For perspective, it would be useful to provide the historical rate of failures from all causes.

Page 41. Last bullet: The combined effect of *what* would be a 240% increase?

Page 42: It would be useful for the authors to provide a definition of uncertainty here so the reader can contrast uncertainty with how the authors chose to define risk.

Sections 1 & 2 (Introduction & Sacramento/San Joaquin Delta and Suisun Marsh)

General Comments:

The *Delta Risk Management Strategy Phase I Report* (DRMS I) reviews the context for the report in the “Topical Areas: Risk Analysis” section and “Introduction.” It is not clear what the purpose of the first section is and could easily be omitted. The report lists the goals and objectives in section 1.1.2. One of the IRP’s objectives is to assess whether they met these goals. In general, this section does not lay a strong foundation for the report that follows. It states that much of the information supporting DRMS Phase I is in the technical memoranda. This created problems throughout the report, because arguments were commonly not developed in the report or substantiated with data, information, or citations where they could be easily evaluated. The “Introduction” also did a minimal job of describing a complex system and there were minimal citations of the established literature on the area, and problem. There are inconsistencies throughout this section. One place they say that they can make confident predictions 200 years out, in other place they say these predictions are limited by uncertainty. They need to state very clearly what was given to them by AB 1200, etc., and then establish what they can and cannot do. There is much inconsistency in this section and a large number of statements of “fact” that cite no references or data sources. Such statements as: “The scale and complexity of DRMS for the Delta and Suisun Marsh has likely not been attempted by another evaluation of risk from flooding.” Is not substantiated and not put in the broader context of work in many other areas or countries. This gives the impression that the authors have not “done their homework” on the topic. This feeling is enhanced by the lack of references throughout this section.

The presentation of the various working groups and advisory groups needs more clarification. How were these used and how were review comments incorporated into the final report? It is not at all clear how this structure worked and who exactly made comments and how those comments were considered and incorporated into the final report. Many comments from reviewers (listed on the DRMS webpage) appear to not have been incorporated into the final document when reading through the responses to comments; but it is not clear why and what process was used to determine what was modified and what was not.

These shortcomings are more common in “Section 2.” This is a very poorly referenced section. The authors make very specific statements and present information without citations to the source. There are many repetitions and in general, the section is very wordy and difficult to read. The authors present many conclusions without any substantiation. They offer no data or references for nearly all the statements made. Many statements are unconstrained and they present a large amount of material that is superfluous. This section contains a large amount of conjecture with no data or citations to back it up. There is no effort to present uncertainty, even when it is established in the published literature that the authors may have used (which they do not cite). Pages 2 through 7 are a severe example of this. These pages present conclusions about the Delta with no data presented and no references cited. This makes it appear that the authors have preconceived ideas about the system without justifying them.

The report very much needs a "previous work" section. As written, it is as if nothing has been done on the Delta when there is a huge literature base. There are vague references to other ideas but they are minimally cited. The authors need to do a much better job at establishing the framework for this work. They need a simple statement of the goals, past work, concerns, etc. They need a coherent description of the system (names, boundaries, etc.) so that the reader will be oriented for the information and discussion that follows. They have to cite where data comes from for statements, as well as for figures. They need to limit material to what is needed. There is too much extraneous information with no obvious need for it in the "Introduction" and then a lack of what is needed or has been done.

Specific Comments:

Section 1.2.3. Page 1-4: A consistent set of words should be used when discussing risk. Throughout the draft, the words frequency, likelihood, and probability, rate and even risk are used interchangeably. We would recommend frequency when talking about a measurable rate of occurrence (that is, the aleatory part) and probability when talking about how likely something is to happen (that is, including the epistemic part). We have been told by technical writers that the public is generally unfamiliar with the word "likelihood." We strongly recommend against using the word "risk" to represent frequency (as it is in the box labeled "Definition of Risk"): risk is an integration of the probability and the consequence of occurrence (as stated clearly elsewhere in the draft).

Section 1.2.4. Page 1-4: The title "Future Risk" is confusing. All risk corresponds to the future, whether it is tomorrow, next year, or 100 years from now. We recommend making the titles more descriptive, something like "Risk Under Present Conditions" and "Risk Under Future Conditions."

Page 1-5: The comparison with New Orleans seems out of place. Also, the statement that the study "needed to be completed in about 1 year using only readily available information" seems out of place. It calls into question the credibility of your results, which we do not think was the intent.

Page 1-6. Section 1.3.2 and 1.3.3: We suggest that you name the players. This same comment applies to pages 1-10 where you might name the Blue Ribbon
Task Force.

Page 1- 11: We did not find a Chapter 15 or a Chapter 16 as named on this
page.

Page 1- 12: Are we the Panel of "Independent Subject Matter Experts?"

<u>Section 2</u>

Page 2-1. End of second paragraph: A brief explanation of what is meant by “resource issues” is warranted.

Page 2-1: A graphical representation of the development of the Delta over the past 100,000 or 5,000 years would be very helpful to complement the narrative.

Page 2-3. Second paragraph: A figure would be helpful showing the locations of these water development features.

Page 2-3: We cannot find Locke, or Ryde on figure 2-2. Is it there?

Page 2- 5: What is the difference between wildlife viewing and bird watching?

Pages 2- 6 and 2-7: We like your bullets. They are clear and concise.

Page 2- 6: We suggest you look at your bullet that is next to the bottom and add a sentence or two about the need for future flood plain management and land use zoning in the area.

Figure 2- 3: The color scheme is hard to differentiate. We suggest the use of more contrasting colors.

Page 2-8. Top of page: Was there any evidence of liquefaction, either in the foundation soils or the levees themselves, in the 1906 earthquake? Has an analysis been performed to support the apparent hypothesis that this specific earthquake event wouldn’t have been expected to cause problems with the levee system as it existed in 1906 but would have caused problems today?

Page 2-8: bullet starting “CALFED is currently reevaluating…” we don’t know what “preferred alternative” means.

Page 2-8: Define what is meant by a 100-year, and a 1,000-year earthquake.

Figure 2-3: What does the phrase “Levee Fragility” mean in the title?

Section 3 (Risk Analysis Scope)

General Comments

Much of this section is repetitious and could be removed. It is difficult to see what the purpose of this section is. It appears that someone who had not read the sections before wrote this. The “new” information presented in this section should be moved to the two previous sections and consolidated into one comprehensive, coherent, and well-referenced introduction. As above, there are many speculative statements that are not referenced, nor is data presented to support them. The problem with this is that it makes it look like the authors have decided on what they will find before they present the results of their work. It is not clear what the authors did compared to past work and it is certainly not set in the present framework of knowledge (both on the Delta and risk analyses). There are no methods presented or even an allusion to methods. Through three sections there is no substantial information given, only very general statements that are not backed by data or citations. The problem has not been put in context of this area (previous work and other studies) or other areas. This in no way covers the information needed to put this work in a broader or even local context.

Specific Comments:

Page 3-1: The statement “By itself, this information will not be the basis for future decisions…” seems overly negative. We recommend saying something like “This work, together with other studies and information, will provide input to the decision makers…”

Page 3-2. Top of page: The statement “making an assessment of risk uncertain” is confusing. Risk includes uncertainty. Estimates for the frequency of occurrence or the average consequence in the event of an occurrence or the actual consequence in a particular occurrence can all be uncertain. However, the idea of risk is to integrate all of this information together into an expected consequence given all of the available information.

Page 3-2. Section 3.3, 1st paragraph: We suggest rewording to change “[...] exists given existing [...]” to read “[...] no single oversight is in place given existing regulatory [...]” or something similar.

Page 3-3. Second Bullet at the top of the page: We like it. It is clear and to the point.

Page 3-4. Second paragraph: What is the basis for saying that the “resources and funding required […] will clearly exceed the current and expected future available resources?” Have these costs been estimated?

Page 3-4: Defined “Primary” and “Secondary” Zones.

Page 3-5. Section 3.6: To be consistent, the first bullet should be phrased “Death and Injuries to Humans.”

Page 3-6: Since risk captures uncertainty, why is it "impossible" to estimate some aspects of risk 10-years into the future?

Section 4 (Risk Analysis Methodology)

General Comments:

This is a critical section in terms of understanding the mechanics of quantifying the risks of levee failure. It may assume greater importance, depending on what the authors chose to do with respect to the revision of the "Summary" section. As such, it is important that the discussion be transparent in terms of how the pieces (models, assumptions, etc.) fit together and the robustness of the subsequent estimated consequences. As we noted in our comments on the *Draft Summary*, we were unable from the presentation in the "Summary" to fully understand what is occurring in the risk assessment. Unfortunately, this section does not remedy that situation. Instead, it raises more questions than it answers.

This section is very opaque. In the original reading of this material, panelists had no idea what the project team was doing. It was only after extensive panel discussion that the IRP was able to piece together the elements of the analysis. This should not be the case. Anyone knowledgeable in the risk assessment area should be able to easily follow the method steps documented in the report. It also repeats much of the material presented in previous sections, including some of the same sentences, giving the feeling that it was written by someone who had not read the previous sections. There are again many unsubstantiated statements. They have slightly more references in this section – still not adequate – but some of them are not in the "references cited" section (e.g., Bazzuro and Baker, 2006). This shows a very poor effort on editing. Some of the references, particularly as they relate to risk analysis, are old and effort should be made to utilize new methods and common practices. There is a reliance on jargon instead of actually explaining the work conducted giving the reader a sense that the project team is not well versed in the methods they are applying. Given that the project team was supposed to rely on existing reports and studies, we would have expected an extensive reference list, particularly for this section.

There are many basic questions that need answering in this section. The authors do a minimal job of presenting what they used for seismic analyses. They would probably say "it is in the Technical Memoranda," but that is not a reasonable response. This is a report for the public and it has to stand-alone. It is fine to check details in the TMs but the basics need to be presented here. For example, there are a large number of tools to estimate earthquake hazard and damage built by the USGS (e.g., HAZUS Earthquake). What did they use, and why or why not?
There is also some sloppy use of terminology throughout this section. For example, there is a seismic hazard that produces a risk of levee damage and failure. It is not clear how seismic hazard, seismic fragility, and seismic event are used or meant in the authors' discussion. They again make many statements that are not corroborated.

In these four sections (or preferably combined in one section) they need to:

1) State the charge and objectives given.

2) Describe the Delta system (briefly)—what it is now, important underlying framework (e.g., stratigraphy, faults, land use, etc.) including geography and names used, size of islands, etc., making it all easily accessible and readable.
3) Describe the approach of the risk assessment with detailed information on individual and aggregated risk, etc. Then describe each process (e.g., floods, earthquakes, etc.) that levees can fail under and the potential effects (what is lost). These are independent of the cause of failure.
4) Give detailed methods used for each "process/forcing" analysis, separating the failure analyses from the response analyses. The two are mixed up in this presentation and it is very confusing and difficult to follow. Results, conjecture, methods, approaches – are all mixed up. They have especially mixed up both results and conjecture in this section, which is supposedly a methods section.

The authors definitely need to put all this in the context of previous work. Much of this has been proposed or done previously (e.g., Torres et al. 2000; Mount & Twiss 2005; Lund et al. 2007; etc.). They have cited none of this work, or how their approach is different, or how it builds on that previous work. It is also not clear why the authors did not just use available information (as charged). The USGS produces maps of ground motion predictions, etc. They could have used this for their impact analysis. They have not explained why it was important for them to redo all the USGS work (assuming they did, which is also not entirely clear). It appears that the authors have developed models for earthquakes on every fault (already done by USGS), but they have left off the foothill faults. Why not just use the probability for ground acceleration (PGA) maps constructed by the USGS? That is the only factor used and the maps they present later are very similar to the USGS maps. Again, it is not clear what they have done in the broader context of decades of work on seismic hazard and damage by the USGS and California Geological Survey.

The authors present a very repetitious, incomplete, and incoherent description of the methodology used in their assessment. It is extremely difficult to determine what methods they used because they give very little detailed information. They cite very few references on methods, and so it is difficult to even place the approach in the broader context of accepted methodology. The technical memoranda help at some level but many of those are also poorly organized and it is not clear exactly what was used, and what was not in the final analyses. They seem to have used a risk model combining some aspects of the traditional concept of risk with other approaches. Any readers of this report need to understand how risk was assessed for the Delta.

Around page 4, the project team claims that the risk analysis can only be performed on an event-by-event basis. This statement is incorrect and should be rephrased to clarify that this was simply the approach taken by the project team. Currently it implies there is only one method for conducting the analysis.

There are different ways to consider risk. Classically risk is defined as "*Risk = Probability X Impact.*" The authors present a variation of this as the start of the section. In the Delta, this can be represented in the simplest form as breaching and flooding an island. Risk is simply the probability that any island will flood and the impact of that flooding. These are separate.

The impact of flooding for each island (houses, people, pipelines, wells, power lines, agricultural production, people affected, etc.) can be determined now. With projections of growth and development, impacts can be projected into the future for 2050, 2100, and 2200. These numbers have a certain uncertainty for the present that will increase in the future. The impact outside of the islands (Delta) will be some function of which and how many islands are flooded. It will range from small for one non-strategic island to very large for many strategic ones. This evaluation is straightforward, given the limitations of valuing goods, jobs, services, etc. now, with uncertainty increasing into the future. The authors need to present exactly what they did, how the analyses were done, and uncertainties carried through. It is extremely difficult to determine what the authors did to get to the final risk.

There is another way to think about risk. That people will not just stand around when something happens but will try to mitigate any potential risk. It is not at all clear the authors of DRMS Phase 1 have considered this but it seems to fit some of their discussion later in the report. This is a much more realistic but complicated approach. Under this approach, the system has warning and can respond with controls and mitigation. This will be the case for floods – there is a very good prediction system that will get better in the future – so this will definitely be part of any risk to the Delta. Response to a potential flood (control and mitigation) will have some effect on the final risk. It is not clear to what level this sort of response was considered in the risk analyses presented in the DRMS *Phase I Report*. It would make a difference in the final assessment and needs to be clarified throughout the report. In the end, there are three important questions that the report needs to answer:

1. What is the cost (all impacts) of *1 to n* islands flooding? Now, and in 2050, 2100, and 2200. What is the uncertainty of these estimates?
2. What is the probability of *1 to n* islands flooding from each hazard (floods, earthquakes, random, wind)? Now, and in 2050, 2100, and 2200. What is the uncertainty of these estimates?
3. What is the probability of *1to n* islands flooding due to a combination of hazards? For now, and 2050, 2100 and 2200.

The first part of the section (pages 4-1 to 4-6) does a reasonable job of describing the nature of the problem. However, in the discussion of the conceptual risk framework and its implementation, there continue to be gaps and inconsistencies in the presentation. As noted previously, a detailed example of the process, starting with one state of nature and one event, carried through to the calculation of the error bars on the economic damage function would be very helpful. Since one of the charges to the IRP is to critique the validity of the risk approach, we think this type of information is needed by the reviewers. We would note that at least one reviewer on the DRMS internal review committee (Kimmerer) made a similar request to have the authors lead the reader through a simple example showing how the analysis is actually implemented.

The use of vulnerability classes needs to be fully explained early in this section. The underlying assumption that the entire levee section breaches if in the same vulnerability class should have some sort of sensitivity analysis given that the assessment of vulnerability class is a somewhat subjective determination.

The scenarios generated for flooding are insufficient. It should have been a straightforward task to calculate the risk for a variety of scenarios.

Specific Comments:

Page 4-1. Last paragraph: This list is a confusing mis-match of different items (effects, failures, accidents, risks, etc.). Also, what is meant by, "Among numerous others?"

Page 4-2: Suggest re-wording "Each earthquake and the spatial field of ground motions it generates, is random and at the same time…" to "Each earthquake, including the spatial field of ground motions it generates, is variable and at the same time unique from one event to the next."

Page 4-3. Second full paragraph: Are events of levee damage between vulnerability classes assumed to be statistically independent?

Page 4-4. Last paragraph: A reference supporting the assumption that salinity intrusion is not significant for hydrologic events would be helpful.

Page 4-5. Third paragraph: Given that there has been an instance where "significant salinity intrusion and a noticeable water supply disruption occurred" when a single island failed, it seems inappropriate to neglect this possibility in the analysis. Since single island failures are the most frequent, they could very well dominate the risk, and more attention should be devoted to these consequences.

Page 4-6. First full paragraph: It is not clear how the time of year that an event occurs was included in the analysis.

Page 4-7. First paragraph under Section 4.3: Here is an example where probability and rate are being used in place of likelihood and frequency.

Page 4-7: The authors' note in the second paragraph that this section "combines all the elements of the analysis and calculates the risk for a range of consequences…" Thus, this is the heart of the effort and readers need to be comfortable with what has been done. One question we have relates to the distinction between risk and uncertainty in their approach. This is somewhat different than what is normally done in economic modeling, where risk and uncertainty tend to mean the same thing (for example, the variability captured in the prob. distribution of outcomes is a measure of the uncertainty). What the authors do in this report is not necessarily incorrect, but later on in the report, the link between risk and uncertainty gets blurred in presenting such things as an economic damage function with error bars. Also, at the bottom of the page, delete "the" between "estimated" and "rate."

Page 4-8: The first sentence defining risk on 4-8 is actually not quite correct and should be revised to reflect exactly how risk is being defined in the report. The sentence on page 4-9 is correct and should be used as a replacement.

Page 4-8. First full paragraph: The statement that the “distinction between what is aleatory and what is epistemic may be unclear” calls into question why so much effort was devoted to trying to distinguish them in the preceding discussion. Why not just describe all of the sources of uncertainty instead of trying to classify them in an “unclear” way? Furthermore, the introductions of the jargon laden terms, epistemic and aleatory, are completely unnecessary. And given that uncertainty is not carried forward (or estimated) in any reasonable manner, it’s ridiculous to introduce a concept that is never used.

Page 4-8: In the last sentence in section 4.4, it is not clear what “an event-based approach” means. It would be helpful for the authors to add a sentence that gives an example.

Page 4-9: In equation (4-1), we think that the “c” needs to follow the word “value”, to avoid having it look like a constraint or integrand on/over lamda.

Page 4-9: The implication is that a risk threshold has been set and events with impacts below a certain threshold are included in the summation. This makes sense, but what are the thresholds? How were they set for each consequence?

Page 4-9: The sentence defining instantaneous and variation in frequency is nonsense and given that variation is actually never modeled over time, makes no sense.

Page 4-10. First full paragraph: Suggest rephrasing “the performance of the Delta levees is random (due to variability in their response…) to “the performance of the levees varies spatially due to variations in the hazard and in the properties of the levees…”

Page 4-10. At the top: The correlation of ground motion between different levees is more than a function of distance. It is a function of site soil conditions, ground motion travel path, etc.

Page 4-10: Second paragraph, 3rd line from the bottom "is use" should be "is used" This is just a typo.

Page 4-10. Last sentence in first paragraph: We agree that incorporating these correlations is important but how are they measured? Do the authors know enough about levee integrity throughout the Delta to actually calculate these correlations? In the next paragraph, the text does a good job of defining the challenges in this effort, including the large number of outcomes to be realized. The text also notes that a decision-tree structure is employed. Unfortunately, the example provided in Figure 4-4 does not help much, for reasons noted later.

Page 4-11. Section 4.4.6: Under combination of events – did the authors consider the following series of events:

–1. An earthquake occurs. We get some levee failures, some levee damage and some good levee performance.

–2. Next comes high winds and waves. This generates possible additional failures or some additional damage.
–3. Next comes a flood, which generates some additional failures and some additional damage.

It is not clear to us that a series of events, over say a 6-8 month period, was analyzed. Was it? If the answer is no, it was not; then should authors analyze for such a combination of events? The authors say such an analysis is included as general exposure during the period a damaged, unflooded island is awaiting repair. Where is this discussed?

Page 4-11: Middle section of page refers to "Some technical people". Odd language – what are technical people? Also, the paragraph comes across as a speculation, given the use of "seems." Near the bottom of the page, need an "and" between "costs," and "environmental."

Page 4-11: Fifth paragraph: The statement "It is only considered as a general exposure during the period…" is not clear. A better explanation of how this aspect was modeled is warranted.

Page 4-13: Fourth line from top, delete "they" between "have" and "been".

Page 4-14. Top of page: The concluding statement implies that it is fundamentally easier to assess seismic hazard versus economic and ecosystem consequences. This statement is only true in the context of the team that performed this particular risk analysis. Also, it seems irrational to treat the input that is difficult to assess as deterministic.

Page 4-14: In the first two complete sentences on this page, the authors acknowledge (for the first and maybe only time in the report) the disconnect/disparity between the levels of robustness in the various components of the overall assessment. We encourage them to note this in the draft summary. The acknowledgement also raises questions about how the authors deal with the cascading effects of variability in each model.

Page 4-14. Second full paragraph: Define "Poissonian" for the general reader.

Page 4–14. Last bullet: This seems to contradict some of the above statements. Authors should be clearer regarding exactly what they mean?

Pages 4-16 to 4-17: This lengthy table is helpful in terms of understanding the components of the assessment. However, we repeat an earlier request to have an example of how they actually interface and result in the "consequences" damage function.

Page 4-18: By the word "total" under metrics, we assume this to mean all hazards combined. Suggest the authors say that.

Table 4-18: Why are National Costs not included in the economic costs?

Page 4-18. Table 4-2: Was loss of life included? Suggest they flag this table with an "*" or footnote.

Page 4–19: Are there no deer in the area? If yes, were they included?

Page 4-20: In table 4-4, under "Topical Area," the only component that is described as "probabilistic" is the seismic hazard. If all other risk factors (and consequences) are handled as scenarios or individual events, how does this limiting of probabilistic information to one factor square with the definition in the *Draft Summary* about the analysis being a comprehensive risk assessment?

Page 4-22: In the first box in this table, it would be helpful if the authors linked this box to some text in which it is explained how "frequency of failure" and "frequency of sequence" are measured?

Page 4-23. Figure 4-1: Should be expanded to show the same sequences for flood and sunny day.

Page 4-24: This schematic illustration appears several places in the report. However, we still are confused as to how the error bars around the damage function are obtained. Is it only from the probability of levee failure?

Page 4-24: There is an irregular dark blob in one sub-figure that we do not understand. Can you explain it in a footnote?

Page 4-25: This is the first place where the authors describe a type of density function (Poisson). Is everything modeled as a Poisson process? Does this only apply to earthquakes?

Page 4-26: This decision-tree figure is disappointing in that it does not make much sense. We had hoped that a decision tree would be presented showing the links (branches) connecting the states of nature, events, response variables, outcomes, etc. with some hypothetical probabilities at each decision node. As it is presented, it does not provide much help in understanding how one would solve the decision problem described at the beginning of this section. For example, it is not amenable to standard quantitative decision tools, such as stochastic programming, Markov processes, or similar tools. This reinforces earlier concerns about how the consequences (risks) in Chapter 13 were actually calculated.

Section 5 (State of the State & the Delta)

General Comments:

The purpose of this section is not clear. There are excellent reviews of the Delta (Lund et al. 2007; USGS fact sheets; CALFED fact sheets; books; etc.) that are not referenced nor apparently used for their “overview.” The authors present nothing on the “State,” so it is not clear why that is in the title. This section needs to present a very precise description of the infrastructure, ecologic resources, etc. in the Delta, itemized by island: also, the potential infrastructure outside the Delta that potentially can be affected by damage within the Delta. There is much extraneous information that does not inform the reader. Again very few references, even though lots of statements are made that require citations. They cite a personal communication (not in the references cited) when there are large amounts of information on this in the published literature and reports. This seems very weird. This section presents very little detailed data, only general statements. For example, the “Economy of the Delta” consists of two sentences. They cite one reference (PBS&J) that is not dated. This is not adequate. The Infrastructure section is somewhat better, but again it is not clear why some information is presented (depth of footings on transmission towers) and never mentioned or utilized again. The authors give names and sizes of pipelines but do not say what they transport. Again, they do not cite where any of this information comes from. The maps are interesting and would be useful if put into the broader context of the system (no references to origin of data on the maps). This needs to be a solid presentation of the essentials of the Delta and adjacent area, resources, and their evaluation with the uncertainties of those determinations. The authors need to present this in a detailed and accessible format, using tables and figures, for the Delta overall and individual islands. Readers need a simple way to determine what is in the Delta and what the situation is “now” (2005) as a starting point. All this should be combined with sections 1 and 2 into a readable “background” section. Describe the Delta, what work has been done, the major challenges, etc., then follow that with a detailed description of the resources (all of them). This has been done in many other reports and papers and could have been easily summarized in this report.

The section seems to be mostly an inventory chapter. However, it’s confusing because a lot of the noted inventory is never referred to again, even in the economic section. If this section is an inventory overview, title it as such and give context for what is used from the inventory, or why elements of the inventory were collected. Also, if this section represents a compilation of the inventory, it really should contain much more detail, and the GIS should be available for people to download and use.

We would have also expected a clear delineation of infrastructure between critical (or life supporting) and other. In the response module, there is no way to tell what infrastructure is considered and why. Also, in this section (if it is an inventory), we would have expected some age-related analysis. In other words, not all inventory matters and some is aged such that its loss may be mitigated with other options.

Specific Comments:

Page 5-3. First paragraph under Section 5.5: Defined “infrastructure assets.”

Page 5-7: Spell out the acronym MHHW.

Page 5-8. At the top of the page: We suggest you flag no loss of life costs.

Page 5-8. Third paragraph: Is the length of the scour zone very significant in assessing the risk? Figure 5-12 is not clear – how is a “scour zone” defined and how is it different from “scour limit”?

Page 5-8. Fourth paragraph: Define and describe the “GIS data.”

Figure 5-1: Showing Frank’s Tract as “Conservation Lands” instead of “Water” is confusing.

Figure 5-1: Discussion of this Figure presents a great future opportunity to flag the need for flood plain management and land use zoning.

Section 6 (Seismic Risk Analysis)

General Comments:

It is not clear how this approach (determining seismic hazard) compares or differs from the USGS information already available. The authors cite few references. From reading the technical memoranda it appears that this section has received the most resources and effort, but it is not clear why they did not just use the available information from the USGS and previous published reports (e.g., Torres et al. 2000). For example, there are available seismic hazard maps available from the USGS and the State of California: why not use those maps and then apply the ground acceleration predicted to the damage criteria for the levees?

Also, Torres, et al. (2000) have already done an analysis of the seismic risk to the levees. Why not just use that data? This report shows different faults in the area (compare maps in DRMS Phase I to Torres). Why are those different? Why are ground acceleration maps different from Torres and USGS and is that significant? This seems like a very simple effort (in many ways): use the available data to determine ground acceleration for the Delta region at some reasonable probability (or several probabilities). Then apply the failure criteria (probably the hard part) for that acceleration to determine what levees will fail. Again, Torres did this so the authors need to also show how their new analysis is different and better.

One concern is that in Torres (pp. 23, 24) they present results on determining levee failures from earthquakes in the area that are different than the results presented in the *DRMS Phase 1 Report*. The figure 5-2 (below)

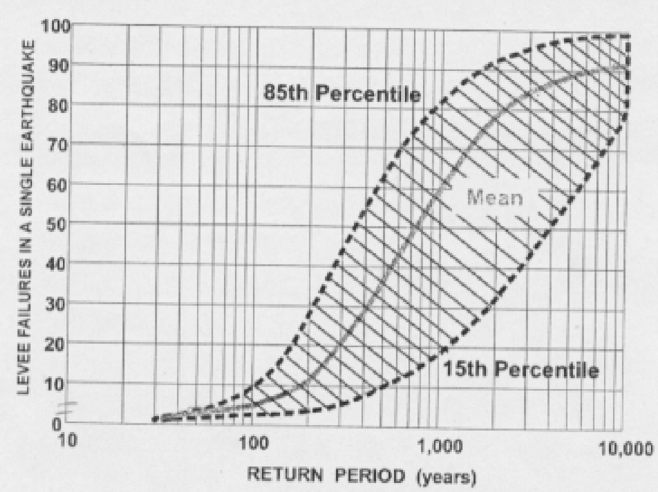

Figure 5-2: Number of Levee Failures in a Single Earthquake-Fault Models Combined
Note: Number of Levee Failures does not equate to Number of Islands Flooded

Figure 1: Figure 5-2 from the Torres report

shows that for a 50 year return interval (roughly out to 2050), we would expect from 2 to 5 levees to break (15-84 percentile). That changes to 3 to 10 at 100 years and about 4 to 29 for 200 years. This appears to be much lower than the values given in the DRMS *Phase I Report*.

The Torres figure (below, Figure 2) shows what is really needed. For example, at 90% confidence (typical statistics value) we see that in the next 50 and 100 years there is <5 failures expected (cannot read the 200 year plot because it was cut off in the copy received from CALFED). The even chance (50%) is about 5 to 7 failures for 50 years and 5 to 20 for 100 years. So given these plots and others in the report showing aerial response, it is not clear how the DRMS Phase I seismic hazard analysis differs, why it differs, and why they even did it with Torres and the USGS hazard maps available. There may be some value in redoing what is already done, but the authors need to lay out exactly what knowledge existed before, why they decided not to use it, and how their analyses differ from those of the past.

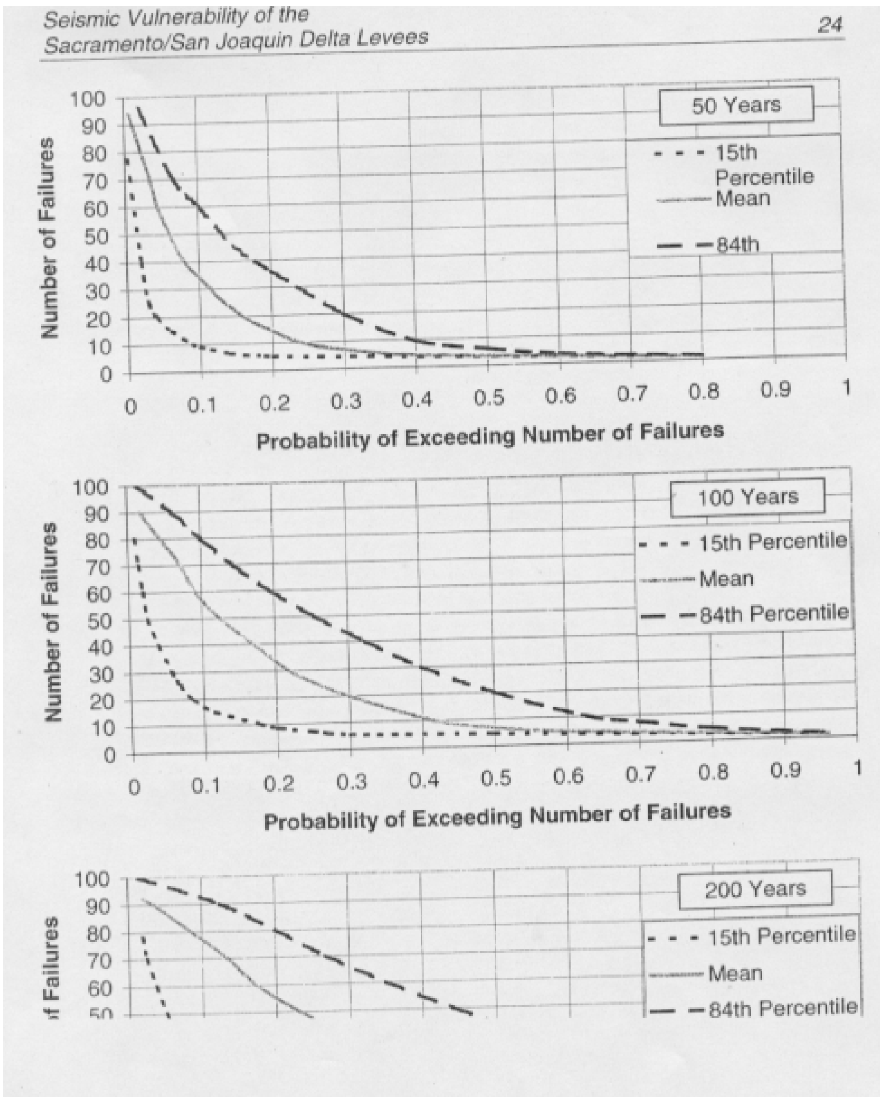

Figure 2

In this section as in the previous ones, the authors make statements without justifying them. There is a vast compendium of earthquake research in California so they should be able to cite anything done on this topic. They do a poor job of showing how they determined when (under what ground acceleration) a levee would fail. They need to give the details of this analysis. The authors make many statements (nearly all) without citations of where that information came from. Computer code is cited in the text but no reference is given for it: so, it would be impossible for anyone who did not already know what this was to find it or evaluate it. They make statements about levees failing in other areas and do not give references for those. The authors give minimal detail in nearly all the sections. They establish 'vulnerability classes' without saying where they came from, how they were developed, how they differ from those established by Torres, and why they needed new ones when they were already established. It is difficult to determine how their analysis fits into the broader understanding of levee engineering and failure. When looking at the technical memoranda, it appears that there was an inordinate amount of effort spent on the seismic section. Considering all that information outside the report, it appears that the analysis may be well founded. But it is not clear why it is different from previous analyses and why it had to be done.

There are no supporting discussions about what underlies assumptions made, nor does the project team carefully explain those elements of sunny day failures that carry through to the risk analysis.

Specific Comments

Page 6-1. Last paragraph: Risk is inappropriately defined again here as a probability instead of an integration of probability and consequences.

Page 6-1. Third paragraph: Spell out the acronym WGGEP please.

Page 6-2. Bullet 3: The statement that "the seismic hazard results are defined for a stiff soil condition" requires more explanation. We presume you are saying that these ground motions are for an outcrop of stiff soil or rock. It would help to explain that the effect of soft soils directly underlying levee in potentially amplifying ground motion is included in the levee vulnerability assessment, and, therefore, the ground motions characterizing the hazard correspond to the ground motions of stiff soil or rock that underlie the softer foundation soils of the levee.

Page 6-2: The first word after 1), 2), 3) and 4) has a printing error. In bullet 4) you say you are assuming a stiff soil site condition. Do you have data that the shear wave velocity in the area in the top 30m of soil is about 1000fps? If so, where are the data discussed? See page 6-5, where you talk about this but don't support it with any data.

Page 6-3. First paragraph under 6.1.3: The rationale that a lack of data precluded modeling all of the faults with a time-dependent occurrence rate is not very strong. It would be more compelling to state either that (1) it doesn't matter or (2) a time-independent model is a reasonable assumption (versus the only possible assumption because you could model it however you want) based on the available information.

Page 6-3. Second paragraph under 6.1.3: A list and qualifications for the experts should be provided.

Page 6- 3. Last paragraph: You have a missing word or something. See "[...] take into account various degree physics, date, [...]."

Page 6-4. First full paragraph: What is a "time-predictable probability?"

Page 6-4. Second full paragraph: This discussion about Reasenberg et al. (2003) and WGCEP (2003) is very confusing (such as referring to models A through F) and essentially requires the reader to go to the references to figure out what has been done.

Page 6-4. Section 6.1.3, last paragraph: Why is this paragraph in the report. What effect does this have on the results? More explanation would be helpful.

Page 6-5. First full paragraph: Again, it is confusing when you refer to the shear-wave velocity of the top 100 feet. We presume you are talking about the top 100-feet <u>below</u> the softer foundation soils that are below most of the levees.

Page 6-5. Section 6.1.5: More discussion is warranted about Figures 6-13 to 6-18, since these are the primary input to the seismic risk analysis. There is discussion about the spectral acceleration at a 1.0-second period – where is this information shown, is the natural period for a typical levee system around 1.0 second? The blind thrust faults below the Delta are significant contributors to the seismic hazard. In the earlier CALFED (2000) study on seismic vulnerability, the existence of these faults was questioned (in fact, the most recent information that they cite, Lettis and Associates (1998), concluded that they do not exist in the Delta region). Was the uncertainty in their existence accounted for in this analysis?

Figure 6-19: The colors on the map do not correlate with those on the legend.

Page 6-6. Last paragraph: The first sentence summarizing the review should be qualified as follows: "show that, if liquefaction occurs, then the earthquake-induced deformations…"

Page 6- 6: The first word after 1), 2), 3) and 4) has a printing error. In your second bullet "is" should be "was".

Page 6- 6: We suggest for consistency you change overtopping to overtop or breach to breaching – either is ok.

Page 6-7. Last paragraph: The statement that "The Levee Vulnerability team believes that levees that have granular materials with $(N1)_{60\text{-cs}}$ less than 15 would liquefy at a PGA of 0.05 g" requires more explanation and discussion. Based on the next paragraph on Page 6-8, the majority (about 75 percent) of the levees have $(N1)_{60\text{-cs}}$ values less than 15. Therefore, this statement is very significant. It warrants discussion for the following reasons:

- If $(N1)_{60\text{-cs}}$ is 15, then a cursory back-of-the-envelope check based on Seed et al. (1984) gives liquefaction for PGA values greater than 0.1g, not 0.05g. What is the average $(N1)_{60\text{-cs}}$ for sites where $(N1)_{60\text{-cs}}$ is less than 15?

- This statement is not consistent with the earlier CALFED (2000) study on seismic vulnerability. In that study, the worst class of levees (labeled Damage Potential Zone I) with a total length of only 20 miles in the 1,100-mile system (not 75 percent of it), was assigned a rate of failure of between 0.005 and 0.5 failures per 100 miles in the event of an earthquake with a peak ground acceleration of 0.05g. The resulting probability of failure for the most vulnerable stretch of levees is therefore between 0.001 and 0.1 for a PGA of 0.05g. This result is not consistent with the statement that levees with $(N1)_{60\text{-}cs}$ less than 15 would liquefy at a PGA of 0.05g.

Page 6-7. Section 6.2.2, 4th paragraph: These are not really verification runs in the formal sense. The results of two different calculation methods are just being compared. Verification over-states what was done.

Page 6-8. Section 6.2.3, 2nd paragraph: Authors pick a liquefaction threshold value of $(N1)_{60\text{-}cs}$ less than 15 but in Section 6.2.4 in the 4th bullet they divide the (N1)60 ranges up – 10.1-20 –. Why did they not choose a range that had a threshold at 15?

Page 6-10. First paragraph under 6.2.5: The statement that "[…] probability distribution functions of the input variables that exhibit random spatial variability were developed" requires more explanation. For which variables, over what spatial dimension, and how were these spatial variations modeled?

Page 6-10. First paragraph: Is it true that island side sliding surfaces control the deformations? Our guess is that it might control the downstream crest height.

Page 6- 10. Second full paragraph: We could not find the results discussed on Figures 6.2 and 6.3. Are they presented?

Page 6-10. Section 6.2.5: The first word after 1), 2), and 3) has a printing error.

Page 6-11. First paragraph: The logic behind relating the probability of failure during a seismic event to the ratio of the vertical deformation and initial free board is not clear. Isn't it the absolute difference between the vertical deformation and the initial free board that is important concerning overtopping and breaching (e.g., it would seem that a situation where the vertical deformation is 0.5 feet and the initial free board is 1.0 feet would be of more concern compared to one where the vertical deformation is 2.5 feet and the initial free board is 5.0 feet, even though they both have the same ratio of 50 percent)? Also, Dv and Ini-FB in figure 6-41 should be defined. Finally, the y-axis in figure 6-41 should be labeled frequency or rate of failure, not probability of failure, since it is an uncertain parameter.

Page 6-11. Start of 6.3.2: This discussion about the spatial behavior of the Delta levees is a stretch. The size of these "contiguous" zones will depend strongly on spatial variations in the geology and the properties of the levees in the Delta and will not necessarily be similar to other levee systems. The statement that "levee sections within a contiguous spatial zone around a given island with similar geotechnical properties are generally observed to behave as a single structural unit when subjected to a given earthquake" is not substantiated. What observations are available for this levee system subjected to an earthquake? How exactly are these “contiguous” zones defined for this levee system? Can they be shown on a figure?

Page 6-11. Section 6.3.1, first paragraph: add an "s" on need so that it reads “needs.”

Page 6–12. Top of the page: Typo - the word *breaches* should be *breach*.
Same page, third paragraph we think it reads better to say – *one and only one vulnerability class,* than *one and only vulnerability class*. Suggest adding the word *one*.

Page 6-12. First full paragraph: The assumption that levee sections across different contiguous zones behave independent of each other in a given earthquake seems extreme (although, it depends on how big these contiguous zones are relative to the total lengths of levees around each island and across the system). For example, if there are one hundred “independent” contiguous zones throughout the whole system, and the probability of failure for each zone in a given earthquake is only 10 percent, then it is essentially certain that there will be at least one breach in the system (99.999 percent). We are concerned that the system has been represented in the modeling with so many “independent” components that the results for the system are not realistic and are overly conservative.

Page 6-13. Section 6.3.6: This section is very confusing. What is “m?” How many independent contiguous spatial zones are in the model (that is, what is “n”)?

Page 6–22. Table 6-1, first two columns of the table: We suggest putting something continued here. It is presently blank.

Page 6-27. Table 6-5: More explanation about increasing PGA and 1.0 sec spectral acceleration with time is needed.

Figure 6-19: Suggest more contrasting colors. Some panel members have difficulty reading.

Figure 6-32: Do you have a problem on the far right margin with your printer?

Figure 6-33: The layers in the cross-section are not labeled and there are not units on the scales.

Section 7 (Flood Risk Analysis)

General Comments:

This section has all the shortcomings of the previous sections in minimal citations, poor justifications of statements, attribution of sources for data, etc. These omissions and problems extend throughout the section. There are some other concerns related to technical issues. Also, there are very detailed comments from reviewers on the technical memoranda for this section (see those from the USACE by Keer, Jensen, and Burnham) that very precisely identify problems that still seem to remain in the DRMS *Phase I Report*. The statements below are reproduced from these reviews (Jensen and Burnham) and address some of the critical issues:

1. The Draft *Flood Hazard Technical Memorandum* presents a means of:
   - Estimating the Delta total daily inflow for flood events and associated stages throughout the Delta.
   - Establishing existing or baseline frequency curves.
   - Adjusting those curves based on four climate change scenarios.

   The analyses are based on readily available data. To the extent that the analytical study constraints permit, the procedures adopted and applied are logical and accepted within the profession, with one exception: The climate change sections in which procedures used and assumptions made are not clearly presented in this Flood Hazard technical memorandum or in the Climate Change technical memorandum. Excluding the climate change analysis, the resulting procedures from the Flood Hazard technical memorandum can be used to conduct preliminary analyses in order to focus more detailed studies and identify reasonable alternatives.

2. The assumptions made and constraints used in the *Flood Hazard Technical Memorandum* limit its utility for more detailed studies. The primary reasons are as follows:
   - The daily time interval used is too long to capture the peak flows, tidal effects, timing effects, outflows from the Delta, etc.
   - The presented procedures do not take into account reservoir operations; bypasses, weirs, and diversion operations; other non-controlled diversions; pumping operations; levee failures; and with-project base and future conditions that effect flows throughout the system.
   - The procedures do not provide adequate hydrographs required for unsteady and multidimensional flow analyses and interior flood analyses with respect to the Delta.
   - The results presented are not accurate enough for the sizing and designing of Corps levees, or for FEMA levee certification analysis.
   - While the procedures applied for estimating flow-frequency curves associated with the four climate change scenarios are logical, the assumptions and data used do not enable consideration of different reservoir and system operations strategies to be studied. These strategies will need to reflect changes in the snow pack and runoff predicted by the climate change models (see *Climate*

> *Change Technical Memorandum*). The assumption that the 23 large watersheds' 100-year (or other) frequency flows can be added together to produce the 100-year Delta flow is invalid. Furthermore, there is no documentation of the assumptions, procedures, and results of the climate change analyses.

In the technical memoranda's comments and replies to comments, the authors of DRMS Phase I address these issues sufficiently. Other specific concerns and comments on this section follow:

There are much longer records for some of the gages in the basin than the 1955-2005 data the authors used. This is especially of concern because there were quite variable flows in some of the early 20th century records. If there is some reason for limiting the flow analysis to this shorter record, the authors need to explain why.

They state that, " […] it is believed that changes related to reservoirs and watershed development are associated with water supply and environmental flow releases from the reservoirs and have minimal impact on flood inflows into the Delta" (page 7-1). The Sacramento-San Joaquin watershed is one of the most regulated, large-scale watersheds in the world. The overall effects are shown in the figures below from Kondolf (U.C. Berkeley).

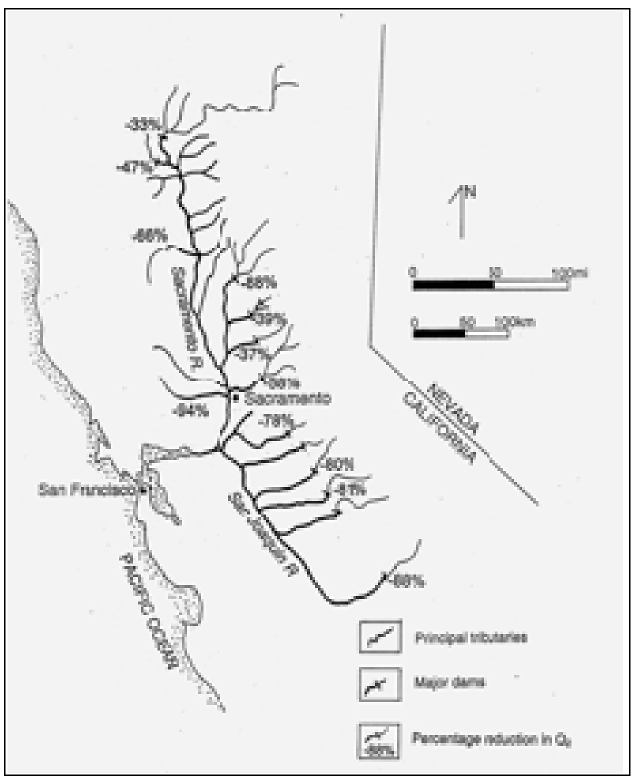

Figure 3: Watershed effects, Kondolf.

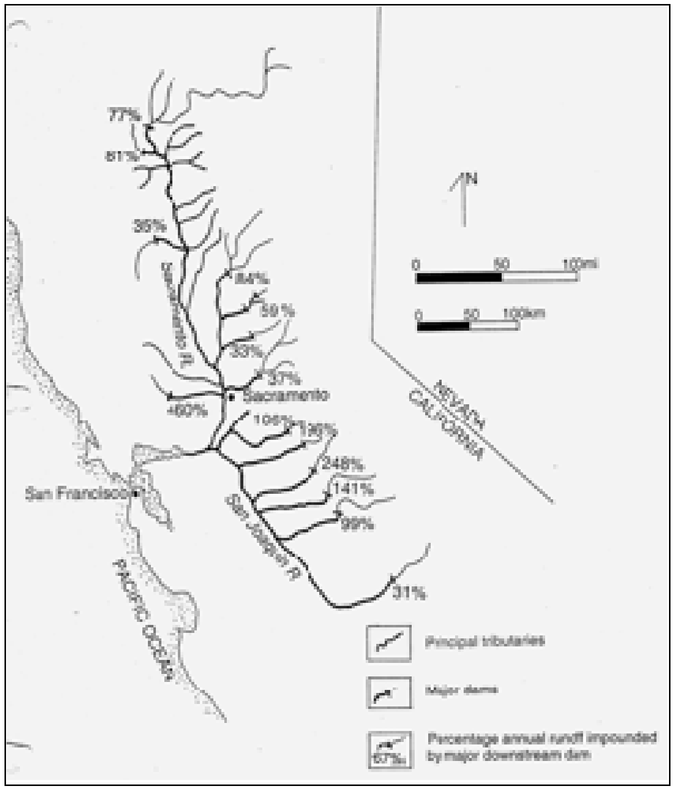

Figure 4: Watershed effects, Kondolf.

> These figures show that flows have been reduced in the main rivers from 33-94% and the percentage of annual runoff impounded behind dams ranges from 35-460%. That this large amount of storage and diversion does not affect flood flows seems highly unlikely. The analyses that they do on the Oroville Dam to show that dams do not effect the hydrograph is not convincing. The record of pre-dam flow is too short (12 years) to capture variability from potential drivers on flow, like ENSO and PDO. Also, looking at Oroville alone ignores the system. Shasta Reservoir is the 9th largest reservoir in the country. It was completed in 1945, so any effect it has on Sacramento River flow would be well before their records that start in 1955. Then there are the inter-basin transfers from the Trinity River into the Sacramento River. It is not clear how it is possible that the peak flows are not affected by all the dams and water diversions in the basin (e.g., look at the number of diversions on their maps in the DRMS *Phase I Report*).

The comments from a USACE reviewer (Kerr) of the technical memorandum also capture these concerns:

> Investigation assumes New Melones and Oroville dams have no significant impact on Delta inflows. This assumption will have a significant impact on the analysis – suggest either rethinking this approach or quantifying the impacts. If, “the average number of days per year with high Delta inflows from SJR is greater during current conditions [record reflected with regulation]” then NML is impacting Delta inflows (more comments below in Section 2.3, paragraph 4). This assumption appears to be in conflict with a statement made in Section 6.1 that “[…] estimated inflows into the Delta in some streams during some storm events may be significantly attenuated by reservoirs[…]”
>
> Section 2.3, paragraph 4: I believe the assumption that ORO and NML have no impact on Delta inflows is incorrect. The comparison made is over simplified and misleading. Simple comparisons between regulated and unregulated frequency curves contradict this assumption.
>
> Section 2.3, paragraph 5: the suggestion that “fewer peak daily inflows would be expected after the addition of reservoirs in the watersheds if the reservoirs were reducing flood flows” cannot be directly supported without a statistical comparison of reservoir inflows, storm patterns, and ungauged contributions.

The authors make another statement of concern, “although the total volume of available flood control storage in the watersheds during the flood events is not known, it is possible that runoff preceding the peak day filled whatever flood control storage was available and inflow into the reservoirs was not significantly greater than outflow on the peak day.” This is also an unsubstantiated statement. The storage in all the reservoirs in the basin is known (most can be obtained real-time). The paragraph that follows this is also unsubstantiated, that reservoirs only provide a portion of the storage in floodplains. It may have been true in the long-distant past that the Sacramento and San Joaquin rivers had vast floodplains (before European colonization) that stored tremendous amounts of water, but that certainly is not the case now. Nearly every river in California is separated from its floodplain by levees. This extends well into the upper reaches of the watersheds and certainly is the case for all the lowland river channels.

This section contains a large number of these types of problems. We will list them without explanation because of the lack of time:

Arbitrary 200,000 cfs cutoff to eliminate non-storm events – unsubstantiated and certainly arbitrary and effects the outcome of analyses (see USACE comments for details). Although they say in their reply to this comment that this has been removed, it is still in the report. This implies they have not made changes they say they have in response to reviewers.

Regression of total flow to individual river flows oversimplifies the system, e.g., assumption that Sacramento River always has 85% of flow. This is not supported by the data and plots presented.

It is not at all clear why they did not use existing work. Much work has been done by USACE, etc. on the flood stages of rivers throughout the region. They again cite no previous work and do not put their work in context.

The authors do not cite sources of data or have references to a website. They need complete references to all data used so that the reader can obtain it.
There is a major difference between the FEMA 100-year flood elevation and the authors determination. What are the causes of these differences? In general, their floods are much higher in about half of the Delta, especially the south end. They give no discussion of this. This is a very big deal. For example, Stockton is 0-10 feet from FEMA and 15-20 feet from their analyses. Those are huge differences and they need to be explained because they affect all aspects of their hazard (and ultimately risk) determination.

Throughout the report, the authors present information and make statements that are not attributed to a source. This is very frustrating because the validity cannot be determined without citations or sources.

Another very important aspect of long-term flow is the past (late Holocene) record. There have been major changes in flow over the last few hundred to few thousand years. There is no reason to not expect these to occur in the future, but there is no mention or discussion of this in the “flooding” section. This is as important (maybe more so because it is data and not model output) that the projections from climate models used to make future predictions of flow. This is a major oversight in this analysis that needs to be addressed or discussed.

Specific Comments:

Page 7-4. First full paragraph: What is significant about “1/34th of the difference in the natural logarithms of the total range of inflows considered?”

Page 7-4. Second full paragraph: Suggest revising "Because uncertainty exists in the estimate of the annual probability…" to "Because uncertainty exists in the estimate of the annual frequency…"

Page 7-4. Third paragraph under 7.3: What time corresponds to the coefficient of variation of 0.084 in flow in the Sacramento River: daily, monthly, annually?

Page 7-7. Last paragraph of 7.4.3 and figure 7-18: Some discussion is needed about the comparison in 100-year flood elevations between DRMS and FEMA. Why are they different? Why is there such a large difference near Stockton? What is the point of the third map in figure 7-18?

Page 7-7. Last paragraph: What does "attend to maintain stability" mean?

Page 7-8. Figures 7-21 and 7-22: They show two linear regression lines on figure 7-21 that are arbitrary. There is no substantial difference between early and later years. This division should not be used. The "difference" in the slopes of their regression lines is driven entirely by the 1903-1908 changes in earlier years. The "correlation" between storms (assuming they mean peak runoff, not actual storms) and failure was not measured statistically and does not appear to exist from the data presented. They present figures that do not show this relationship and do no statistical analyses to prove it. Peak discharge did not change substantially during any of the major increases in failure rates. The plot of "cumulative number failed" is not as useful as a plot of "number failed" and obscures the actual relationships. They have completely overstated this and there is no evidence to prove it, they make this statement without support from their own data. The fitting of lines to the data in figure 7-22a is arbitrary. The changes in the data are steps and show no linear trend. They maintain that there is a "correlation" between flow and failures, however, the "big" change in failures occurs more or less as a step from about 1979 to 1986 (the scale on the figures is very inadequate), while the one "big" flow event outside the previous typical highs was in 1987, after this failure increase. Their statements in the text are not justified by these plots. Similarly, during another "big" flow event in about 1997 (again the scales are inadequate to read the graphs easily), the failure rate was flat flowing a step up previous to the high flow during a time of very low peak flows (the droughts of the late 1980s to mid 1990s).

Page 7-8. First paragraph under 7.5.2: What does considering erosion and slope stability as a "fraction of total mode of failures" mean?

Page 7-9. First paragraph under 7.5.2.2: The description, "Often, water is seen exiting the landside slope of the levee, above the landside toe. As this increases, slumping of the levees slopes is often seen progressing from surficial slumps to complete rotation and/or translation of the levee prism and eventual breach of the levee," seems to indicate a slope failure due to seepage pressures lowering the effective stress in the soil AND not internal erosion. However, the discussion and the subsequent analysis of this mode of failure emphasize internal erosion (i.e., the vertical gradient) versus slope stability. Why?

Page 7-9. To end of section: This page has a large amount of unsubstantiated material that is critical for their final analysis of failure. They simplify the failure modes but do not say how and why. The second paragraph basically says they do not have the information to determine how levees failed and they based the allocation to a failure mode on “judgment and experience.” But they do not give any criteria on how that judgment was made. They need to give the reader the criteria used. If they had no criteria, than this is a major shortcoming of the approach. They have no information to support some of the statements made on this page. For example, they “[...] believe […] that both through-, and underseepage-induced failures occurred in equal numbers. The remaining […] failures can be attributed to overtopping.” They give no data to support this or any information of how they came to such conclusions. Yet, these are used to determine the potential failure later in the report. They need to give data, summaries of interviews with experts, reports, dreams, whatever they used to get this information. They make similar statements about permeability without any attribution to source or data. They make statements, “[…] because of their high permeability and layering […]” with no supportive information or data. This is partially the pervasive problem of poor referencing throughout the report, but is critical for knowing if their analyses are reasonable. It is not possible to determine that from this report. Again, the USACE reviews of the technical memoranda note similar or identical concerns and those were not addressed in the final report. They also make contradictory statements in this and following pages about “seepage model analyses.” Again, they talk about models but do not give the reader any information on the validity of those models. They give a list of “variables” or “classes” but say nothing about how, or why these were picked or cite references that would support this choice. This mostly looks like their “opinion” not a scientific analysis based on data. Without the presentation of data and support from previous work and substantiating research, this is mostly conjecture. It is extremely difficult to determine the validity of the failure analyses and response to floods, etc. Much of this is “conceptually” okay – that is it seems reasonable – but it is not backed by data or citations. It therefore becomes supposition not science.

Page 7-11. Second paragraph: What was the basis for assuming a 50 percent chance of occurrence for the presence of sediment in the slough and the presence of a toe drainage ditch?

Section 7.5.5: We have the following concerns about the approach used to model and analyze seepage-induced failures:

- The model predicts that the vulnerability to seepage-induced failures goes up if there is a ditch to control seepage flow and pressures on the inboard side of the levee. From the standpoint of water pressure and stability, it seems like the ditch would help not hurt. Also, the model predicts that for the same levee section, the deeper the interior of the island, the less vulnerable the section is. Again, this result seems counter-intuitive to me because the water pressures relative to the total overburden stress would be greater (smaller effective stresses, smaller shear strengths, greater potential for instability).
- We don’t think using the vertical gradient is necessarily a good indicator for seepage-induced vulnerability. An artificial and questionable set of conditions

(very high permeability for peat relative to lab measurements and very high ratio of horizontal to vertical permeability) is needed in order to come up with apparent vertical gradients that seemed high enough to the Levee Vulnerability Team to explain observed failures. We would like to see these observed failures analyzed in terms of stability using reasonable properties to see if the failures could be explained (e.g., showing effective stresses in addition to gradients and heads in the FEM results in the technical memorandum).

- It would be very helpful to analyze the available near-miss data for seepage failures. For example, how many times do boils appear at a location in years prior to a seepage-induced breach? How many times have boils appeared at various locations that have never manifested themselves as breaches? How have areas where they have consistently collected sediment from the inboard ditches (or seen gradual increases in sediment load with time) performed in terms of breaches? These data would be helpful not only for assessing the risk but for understanding how to manage the risk.
- If seepage-induced failures are related to internal erosion, then it seems like the model should account for conditions getting worse with time (which is one explanation for the "sunny day" failures). Again, an analysis of the near-miss data would be valuable.

Section 7.6.1: This approach for modeling spatial variability is flawed, specifically, equations (1) and (2) are not correct. The probability of the union of failure events is theoretically bounded to be greater than or equal to the maximum probability of any one of those events. For example, if there were three reaches in an island and the probabilities of failure for these reaches were $P(F_1) = 0.9$, $P(F_2) = 0.05$ and $P(F_3) = 0.1$, then the $P(F_1 \text{ U } F_2 \text{ U } F_3)$ cannot be any smaller than 0.9 (the case where the events F_2 and F_3 are completely contained within F_1. However, equations (1) and (2) would give $P(F_1 \text{ U } F_2 \text{ U } F_3)$ equal to $(0.9^2 + 0.05^2 + 0.1^2)/(0.9 + 0.05 + 0.1) = 0.78$, which is not possible. If the intent is to include correlations between reaches, then the maximum probability for any individual reach provides a lower bound on the probability of failure for this case and is commonly used as a simplified approximation to more complicated relationships.

Section 7.6.2: Why are the events of underseepage and through-seepage treated as statistically independent? It seems that they would be highly correlated (e.g., both depend on the presence of sediment in the slough, the presence of a toe drainage ditch, the geology beneath the levee, the properties of the levee, etc.). Are failure events between multiple islands treated as statistically independent?

Section 7.6.4: This section is very confusing. If equations (1) and (2) were formulated adequately, then we suspect this scaling factor would not be needed.

Section 8 (Wind and Wave Risk Analysis)

General Comments

The authors make unsubstantiated statements throughout this section. They seem to limit their analyses to a very small subset (8.1) of the important factors causing levee failures or damage from wind and waves. They do not justify this omission. How can one get the data the authors refer to? Again, no reference or detailed information of how to get the data they used. Where the authors do cite references they are not in the references cited section. Most of the references cited in this section were not in the reference section, or they were in a different format. Extremely poor editing.

The authors assume deep-water wave conditions when all the “lake islands” would be very shallow. It seems that all waves would be shallow-water waves and interact with the bottom. They do not say why they assumed this. Again, the authors did not explain their methods, or give any citations.

In presenting the wind and wave model, the authors do not cite any references to the origin of this information or approach, why it is important, how it is to be used to determine levee failure, or how it fits into the overall determination of risk. Some of the terms are ambiguous, poorly defined, or of unknown importance. Why do we need to know “timing,” “met event,” etc.? This is all presented with no context.

Specific Comments

Page 8-1. First paragraph: It is not clear where this information fits into the overall risk model.

Page 8-1. Fourth paragraph: This paragraph is very confusing, particularly following the preceding paragraph where the important factors are outlined. Why wasn’t wave run-up considered during high-water events?

Page 8-2. Second full paragraph: What duration was associated with the peak wind speeds in the data set (e.g., gusts, 1-minute, etc.)?

Page 8-4. Description following equation 8-4: What is the basis for assuming that the spatial wind speed pattern is perfectly correlated in space? Does this assumption really matter in the results?

Page 8-10. Fourth paragraph: What does the following statement mean: “The 2-percent wave run-up height is not related to the probability of a given wind speed or wind wave condition”? Why wouldn’t the run-up height depend on the wind speed?

Section 9 (Sunny Day High Tide Risk Analysis)

General Comments:

This should be a very straight-forward section presenting the past data on failures and the probability of them continuing. But, it is poorly supported by references to past work, data, information, etc. The organization is difficult to follow and why they used certain reference elevations or databases is not discussed. They arbitrarily define “sunny day” failures as occurring from June through October but do not say why. They also do not give that definition until they have presented a bunch of undocumented data on failures. It is difficult to separate their conjecture and results. They again make statements without corroboration: “It seems like well engineered levees may be less vulnerable to failure than older non-engineered levees.” Seems to whom? It may be conceptually reasonable, but they should not make sure statements unless they back them up with some data (interviews with long-time residents, engineers, etc., something). They use terms like “unusually high tide” without defining them. Was this from a storm surge? Higher runoff corresponding with spring tides? What exactly? This section is very short and does not present any aspect of risk analysis.

Specific Comments:

Page 9-2. Top of page: The effect of cumulative deterioration is not necessarily captured by “sunny day” failures. If the levees are deteriorating with time, then they will be more susceptible to all failure modes with time, not just failures where there isn’t a flood or an earthquake.

Page 9-2. Second paragraph: What is meant by an “unusual” high tide?

Table 9-1: It seems like these failures could be included with the hydrologic events (they represent the left-hand tail of the fragility curve with probability of failure versus water level). This approach would both simplify the model and the presentation of the results.

Section 10 (Responding to Levee Breaches)

General Comments:

This relatively brief section describes the prioritization process (decision model) for responding to multiple levee breaches and the associated time and cost of performing those repairs. It also contains a good discussion of assumptions and the possible biases introduced by them. The material is generally presented in a reasonable fashion, although some specific questions do arise concerning what and how this module fits into other models/modules in the overall risk analysis. Also, this chapter contains some of the odd, equivocating language used in other sections of the report that is inappropriate for a document of this type (see below). As with other chapters, this section lacks references and citations. The last pages of the chapter list a bunch of speculations but it is not clear what they mean to the analysis.

Additionally, we note you ignored aftershocks. This needs to be emphasized.

Specific Comments:

Page 10-2: Is it un-conservative to assume no constraints on future dewatering resources? If yes, say so.

Page 10-1. Bullets at the bottom and top of page 10-2: We think the third and fifth bullets are not reasonable. In the fourth bullet the authors should emphasize this is not conservative.

Page 10-2. Section 10.4: So what? The report should state some finding or recommendation.

Page 10-3. First bullet: Although this section is describing wind erosion to the levees, we note that the analysis here divides each island into eight sectors, whereas some of the subsequent discussions concerning scour holes and their costs imply that levee vulnerability is treated as a continuous variable. If so, how is this reconciled in the linking of these modules? At the bottom of the page, we commend the authors for noting that any prioritization performed in the report is likely to be different than what actually happens.

Page 10-3. Section 10.4.4: Yes, the statement is true. How was this evaluated in the analysis/study? It is not clear what was done. More discussion of this topic “Secondary Breaches on Non-flooded Islands” is suggested.

Page 10-4. Bottom of page: The phrase “[…] the most important activity was thought to be controlling ongoing damage” is confusing. Do the authors not know what the current state response strategies are for these events? There must be some document prepared by some state agency defining this.

Page 10-5. Middle of page: What does the phrase “The scheduler looks through[…]” mean? Is this part of the optimization model, or are the authors talking here about an actual person? In the first line of the “Population” subsection, the word “only” seems redundant.

Page 10-6. Top of page: What is the source for the statement that flooding of McDonald Tract does not have a “crippling effect on the regional economy?” Under “Salinity,” what does the phrase, “based on the hydrodynamic modeler’s judgment,” mean? Does this mean that the model is programmed this way or is there some sort of interactive analysis whereby the modeler plays around with different orderings? Later in the paragraph, the text notes that multiple runs would be preferred but were not done due to time constraints. Does this mean that only one run was done with the repair module? If so, how then is this simulation outcome probabilistic? Later in the text on this page the authors use words such as “were thought to be” and “seems unreasonable” to justify what they did. We would prefer them just to say what they did and let the reviewers evaluate whether they are reasonable or not.

Page 10-7. Second sentence: Does this mean that the category C islands are not part of the risk analysis? Later, in the bullets, the authors again use words like “probably”, “may” etc. to describe situations where their assumptions may not hold. Since they are simply citing limitation here, we think they should just state what they are and not speculate as to whether or not some third party might interfere, etc.

Page 10-7. Section 10.6: Why is this component of the risk model treated as deterministic? The consequence of breaches will depend strongly on the response, and there is substantial uncertainty in the effectiveness of the response (as the bullets clearly highlight).

Page 10-7. Section 10.6, first bullet: The choice of no access constraints is not conservative and probably unrealistic. Last bullet same page: The state “will” have to make priority calls not “may” have to. They should start this process now.

Page 10–7. Section 10.6: The last bullet calls for planning, prioritization and management. Why not say so?

Section 11 (Salinity Impacts)

General Comments:

By examining the technical memorandum (TM) for this section, more information could be found (that was not presented in the report itself) that justified the approach. These documents showed that the WAM model forms the core for the salinity impacts. The authors acknowledge that they have only included salinity, and that other water quality parameters may also be important. This decision is understandable, considering the time constraints. The Panel also agrees that other water quality parameters are important but that doing a good job on salinity is a high priority.

It appears that the WAM collection of sub-models is reasonable, although there are aspects that are poorly documented so that definitive evaluation is difficult. The WAM is critical to the entire analysis because it the funnel point where the immediate effects of levee breach (flooding of an island) links to economic and ecosystem consequences. So the earthquake and flooding lead to island flooding, and the WAM follows the changes in water quality during the initial breach, repair and water management responses (e.g., reservoir operations, pumping), and then recovery. We presume these sub-models involving the water management and pumping decisions are reasonable, and they likely are reasonable based on the WAM TM and the accumulated knowledge we have about water dynamics in the Delta. One could question the rules built into the decision-making in these sub-models but it seems what was done is reasonable. For example, how consumptive use might respond to a major breach is debatable; but the authors have seemingly made reasonable assumptions and have used available models. This is an example of some “trust me” from the authors that the Panel grudgingly accepts as the price of doing this type of analysis in a short period of time.

The hydrodynamic and water quality (salinity) modeling is of particular interest. The wide range of temporal and spatial scales inherent in simulating local responses of salinity to rapid changes in water levels (i.e., flooding) is a challenge. The authors then want to be able to do this with relatively quick computer time. There are several models that simulate salinity in the Delta region. Indeed, the Panel’s first reaction to the conclusion reached by the authors that yet another hydrodynamic-salinity model was needed (page 11-7) was disbelief and frustration. However, this changed upon further examination of the revised WAM TM. The reason put forth by the authors was the need for performing many model simulations. On page 11-7, the authors state, “[...] provide sufficient accuracy while maintaining the computational speed needed to simulate many thousands of levee breach events.” As it turned out, only 18 earthquakes and even fewer flooding scenarios were actually done, so the authors could have used one of the existing models. But if an efficient model is needed for salinity simulation later (hopefully the problems and incompleteness of the analyses in the draft report are corrected), then the authors have a good tool available to them. But the Panel had to look at the new (revised) TM to find sufficient information to determine that the new, yet another, salinity model that had been developed, had been developed with careful thought, had been fairly well tested, and evaluated for its skill. The draft report was incomplete in its description and documentation of the new salinity model.

Based on the new information provided in the revised WAM TM, the authors have done a pretty good job in developing a reasonable and computationally efficient salinity model. This was quite a challenge and the developers of the new model should be commended for what appears to be a thoughtful approach. The draft report does not do the new salinity model justice. For someone who is not well versed in the WAM model, it would be good to show a network diagram of how everything works together and what feeds into what element that then determines the output.

Within the report itself, and not considering the associated TM, there is minimal citation and discussion of previous work is given. They present results and comment on their model but do not explain how it works or what past work it is based on. Many statements made were difficult to verify because there is no reference to previous work or data, etc. For example, they make the statement that the rush of water filling an island dominates Delta water flow. This is probably true in some situations, but certainly not all. It depends on the tide, runoff, etc. This is a general statement that is not always correct and not substantiated.

Overall, this section is very disorganized. Subsection 11.5 should be at the front. They again start with presenting conjectures without substantiation or explanation. It is not clear what the figures show and why they are important. There is no determination of risk in this section.

The section on “Other Water Quality Impacts” says little other than additional variables to salinity could be included at a later date.

The figures were readable, but having looked at the revised TAM, they do not reflect the amount of work that went into testing and evaluating the new salinity model.

Specific Comments:

Page 11-3: The text about WAM only using previous time step information, and the text about the mix of time steps that was used, is confusing. So, what was simulated on a daily time step and what was considered on a monthly time step? It seems from the example results presented that water quality (salinity) is predicted daily; yet, the authors state, “the overall results of Delta water quality […] are reported monthly.” The revised WAM TM helped here but the text in the draft report should at least be understandable.

Section 12 (Consequences Modeling)

General Comments:

One panel member found this section provided a good description of what the authors did, and did not measure in terms of consequences (resources at risk and the impacts of flood events to those resources) of flooding events. However, all other panel members thought that this section was disappointing: first because it is poorly written, and second because there are very important assumptions made and factors left out of the analysis. Understanding what is included or not included in the analysis is very difficult to ascertain because of the writing. The authors describe how various impacts were measured, along with caveats on the nature of those estimates. Of the three major categories of consequences (life and safety, ecosystem, and economics), the inventory of economic resources is most complete. It is unfortunate that more was not said or done with respect to the other categories. It is our understanding that there are standard safety models employed by USACE that could have been used to provide a better quantitative metric than simply listing populations. In that case, this section should be expanded to reflect such information. Similarly, treatment of ecosystem impacts could be revisited. The current “risk index” metric for species was confusing. Failure to say much about ecosystem impacts leaves a big hole in the overall risk assessment. Our specific comments relate primarily to the economic costs and impacts analysis.

As with other sections, there is a disappointing lack of citations, previous work, context, etc. There is much seemingly extraneous material, or least it is not clear why it was presented. It is not clear how this section is different than “Section 5”; the two sections should be combined. There is much repetition from previous sections within this section. The authors need to put all this information in the context of previous work and experience. Jones Tract flooded not long ago, so it seems like an excellent example to present, or at least test their concepts and models. It is not clear what they did exactly and how they did it. There is a long list of items, poorly grouped and organized, and it is very difficult to determine how valid their approach is. For example, there is a lot of information in the lookup tables for each island, but it’s nearly impossible to follow how the tables can be used. You’d expect that for each scenario, it would be easy to lookup impacts for each group of islands (rated by vulnerability if that’s their classification scheme).

Overall, it would be very helpful to show what the models estimate the consequences to be for some example breaching scenarios.

A large criticism of this section is that uncertainty really isn’t propagated through the analyses. In other words, despite claims that uncertainty is in fact incorporated in the analysis, it is not. Some elements of the impacts will have high uncertainty, others low – there is not stated or described methodology for how the project team handled the uncertainty throughout the analysis (and then uncertainty basically disappears for future horizon years).

There is a major disconnect between the introductory text to the report, and even the introductory text in this section, and what was finally done for assessing the risks to the ecosystem. Here, the focus of the Panel's comments is on the ecosystem impacts (aquatic species, terrestrial vegetation, and terrestrial wildlife species). In the subsections on ecosystem consequences (section 12.1), there are many examples of the authors saying words but not saying anything concrete. The authors spend most of the text in the draft DRMS report trying to explain how what was described for assessing risks to terrestrial plants, terrestrial wildlife, and fish in the *Ecosystem Consequences Technical Memorandum* was not ultimately done in the analysis. There is quite a bit of text in the already too brief "Section 12" of the draft report devoted to discussing stuff that was not used in the analyses.

Page 12-12: The approach finally used for terrestrial vegetation and wildlife is reasonable. Despite the authors not doing everything that was described in the TM (e.g., they dropped time to recovery), what was finally done for the terrestrial species was relatively simple and conceptually understandable. Due to the limited nature of the available data on vegetation distributions, presence was used to determine the fraction of the total area impacted (assumed all organisms lost). For terrestrial wildlife, habitat was defined from vegetation types and the same metric of percent of total area affected was computed. So, the effects computed for terrestrial vegetation and terrestrial wildlife are correlated to some degree.

While the Panel was of the opinion that the simplified approach used for terrestrial taxa was reasonable, the simplified approach used for the fish ("Section 12.1.1") was inadequate. A brand new method was introduced for assessing the risks to key fish species that appears for the first and only time anywhere in the draft report, and that which does not share the intuitive appeal of the simplified approach used for terrestrial taxa. The method is, for some reason, described in the following section that shows the base year results. Table 13-26a describes the calculations used to determine what the authors call the "Risk Index." The Panel sympathizes with the authors trying to wrestle with the very difficult task of assessing the risk to fish of levee breeches and island flooding. The broad scientific community is presently under fire to explain the recent declines in several pelagic fish species, and the explanations are not easily forthcoming and will likely be complicated. So it appears that the authors doing the DRMS analysis for risks to fish backed-off on their approaches described in the TM. But what the authors then did in place of the habitat suitability and other approaches in the TM is not very helpful. Their risk index is the sum of risk factors weighted by weighting factors. No justification or rationale is provided for, what appears to be, a new method. The reader has no idea how the weights were determined, nor how the computed risk index behaves. What levels of the index should flag concern, and to what degree should we be concerned. The Panel had no idea how to interpret the changes in the risk index under the few earthquake and flooding scenarios that were performed, and "Section 13" showed that the authors also seemed to have little idea on how to interpret their own risk index. This is clearly a challenging problem, and given the range of methods presented in the TM and then the final method that was used, the authors have wrestled with this problem without a satisfactory resolution. The high importance of being able to assess the risks to the ecosystem (especially fish), and method of risk index used by the authors, caused the Panel to elevate evaluating ecosystem effects as a major deficiency in the draft report that must be corrected.

The Panel discussed what approaches might have been taken to assess the risks to fish, and in doing so, noticed that the experts in this area were listed, in one form or another, as part of the DRMS overall organization (Steering Committee, Technical Advisory Committee, Risk Resources Group) or having made comments on the TM. Were these people conferred with by the authors? It would seem the right people were involved but it is not clear to the Panel if the risk index model finally used was a result of these people's input or not. It is easy to criticize the approach taken by the authors, and the Panels appreciate the difficulty inherent in computing the consequences to fish in the Delta. The Panel would normally recommend that the authors assemble a group of experts to derive a feasible and interpretable method that balances the needs of the analysis to be population-level oriented with the high uncertainty we have about what governs population dynamics of key fish species in the Delta. But if the authors of the fish risk index used the expertise that seems to be involved with the DRMS process and review of TMs, then the Panel is unsure what to recommend. Pending additional clarification from the authors of the risk index of how the risk index was derived and who was consulted, the Panel assumes that the risk index is not the collective wisdom of these other experts. The Panel therefore recommends that these experts, plus others, be assembled and tapped for their opinions of effects and methods for quantifying ecologically-meaningful metrics of fish responses. Something better than the risk index needs to be developed, evaluated, and implemented.

Specific Comments:

Page 12-1. Life and Safety Costs: Human life and safety should be treated the same as the other consequences. It is not true that "the quantitative models needed to assess these life and safety risks are not yet available." One example is the Corps of Engineers LIFE Sim model to estimate life loss in natural and dam-break floods.

Page 12-2: The text is confusing about what was actually done in the DRMS analysis versus what was described as going to be done in the TM.

Page 12-2: The selection of species to analyze is a good balance among life histories, specificity to the Delta, etc. The Panel believes that the spatial and temporal distribution information on the fish species was included in the analysis via the entrainment factor in table 13-26a; but this is not clear. The authors mistakenly state that the "the impacts of these mechanisms were quantified and normalized for a score between -2 and 2." What the authors finally did with the risk index was not a quantitative analysis. They also then say that a similar risk model was documented for terrestrial vegetation in the TM, but we could not find this. Then we think they later correct themselves again in the draft report and say but it was not used and a different risk model was used in the DRMS analysis for terrestrial vegetation. This is just one example of rambling and convoluted text. It continues later in the section as well. The authors were trying to relate what was described in the TM to what was finally done, but it gets very, very confusing. They should first say what was actually done, and then later can explain how it follows or differs from the TM.

Page 12-2: The authors recognize the difficulty in quantifying ecosystem effects. However, the Panel disagrees with the authors that the fish risk index somehow shows order of magnitude responses. The Panel could not determine what differences in the risk index mean and how to interpret high versus low values of the risk index across scenarios.

Page 12-3. Discussion of economic costs and impacts: We would encourage the authors to expand this discussion to help distinguish economic costs (efficiency effects) from impacts. We suspect that the lay reader will not fully understand the difference based on the terse discussion here. As an example, consider changing the definition of *economic costs* to read something like "In economic terms, the cost (damage) from a flood event is equivalent to the potential economic benefit of activities that eliminate that flood event (avoided damages). The more the authors can link the definition to examples (such as they do with *impacts)*, the more transparent the differences will be to the reader. The authors could borrow text from other economic studies meant for public consumption that spend more time on this difference.

Page 12-3. Economic Costs, and Impacts: Given all of these uncertainties in the economic impacts, why weren't these consequences modeled probabilistically?

Page 12-4: The authors decided to use the information in the TM but to simplify it for the DRMS analysis. Is there a particular reason this very significant strategic decision was made? The simplified version of the risk model for the fish species was considered inadequate by the Panel for assessing ecosystem risks. So the reasoning behind this decision should be provided.

Page 12-5: It is not clear how "season of breach" and "species and lifestage location in space and time" enter the risk calculation for the fish species. The Panel deduced that the location information entered in the entrainment on islands risk factor and maybe the authors were thinking about "season of breach" in terms of the different months in several of risk factors (table 13-26a). In section 12.1.1, the authors again explain the location aspects of the fish species but never say what was actually done and how the information on location was used.

Page 12-6: How do the items on this list of "things", such as species life histories, water temperature, etc., relate to the list of parameters on the previous page? So, the authors list water temperature and then say it was not used. This continues with many factors, some included, and most not included, until the reader gets lost as to what was actually done and why.

Page 12-6: How was the "level of suspended sediments" used in the risk index?

Page 12-7: The authors decided to group the possible factors under “Risk Model”, which we presume to mean was actually included in the DRMS analysis, “Further Refinements”, which we assume means was no included, and “Qualitative”, which we think means the factor was thought about but not included in the risk index. This was quite confusing, as not all of the factors listed in “Risk Model” show up in the risk index calculation, and there was almost no interpretation of the results in Section 13, so how the qualitative information was used remains a mystery.

Page 12-9: The text associated with many of the factors does not really say much in terms of concrete information. It is more that here is factor and it varies and its effects vary. A noteworthy example is the statement at the end of the discussion on “Succession after a Levee Breach […],” which stated, “Succession in newly created habitat was crudely estimated in the risk assessment model.” How? The entire discussion on contaminants culminates with the statement “[…] but these effects have not been quantified as part of this analysis.” At this point, the Panel was confused as what was actually done and why selected topics seems to be highlighted, some included in the risk index, some included but not clear how, and some dismissed.

Page 12-12: The model used for assessing risks to terrestrial vegetation was also simplified from that presented in the TM, although not to the degree that the fish model was simplified. The actual calculations done, as best as could be inferred by the Panel, was reasonable. Presence maps were used to determine the percent of total area of species presence in the Delta and Suisun Marsh impacted by the island flooding.

Page 12-13: Again, as with the fish discussion, the authors then go into further refinements, which are fairly vanilla descriptions that basically say things vary and things affect things and the authors ignored them.

Page 12-15: The risks to terrestrial wildlife were computed based on their habitat needs and the vegetation maps of habitat presence. The authors need to acknowledge that the risks to wildlife and risks to vegetation are therefore correlated.

Page 12-16. Middle of page: We believe this is the first time in the report an actual solution model, or algorithm is defined. We are not familiar with this model. We would like to see how the probabilistic information (presumably from earthquakes) interfaces with the discrete events. Is there a flow diagram for this model?

Page 12-16. Section 12.2: Please spell out the acronym ER & R.

Page 12-17. Bottom of page: How do the authors know where the scour hole will develop? This is a function of a host of factors and from earlier discussions in the report, it is not clear that the authors had the capability to define specifically where a levee would fail. This same comment applies to the second from bottom paragraph on page 12-18.

Pages 12-19 through 12-29: The remainder of the text describes the data and assumptions used to develop inventories of potential economic costs and impacts within the Delta. Unlike some of the other consequence categories, data on economic infrastructure and resources is abundant. The authors appear to have used the best available data to identify and quantify these potential costs and impacts on resources at risk. The assumptions employed in developing this inventory also appear reasonable.

Page 12-22. In the section on urban water users: It is not clear that this is correct. It seems like that any disruption of supplies has a “cost.” Just because they can replace it with water stored in aquifers, does not mean it does not cost them anything to replenish that storage, etc.

Section 13 (Risk Analysis 2005, Base Year Results)

General Comments:

This section is very, very important but fails to fulfill the standard level of documentation required in scientific and engineering reports. It took the collective expertise of the Panel, intensive discussion, detective work cross-referencing the TMs, and hypothesizing by the Panel to be able to deduce what was done sufficiently for the Panel to then intelligently comment on the technical aspects.

Because they have not defined their approach to determination of risk well, it is not clear what all this means. In this subsection, they say that “sunny day” failures will not have a forcing – they just multiply out the past rates into the future. This shows another problem with presentation. The probability of failure today (2005) is based on the annual frequency of events from the past. That is all they need to represent here. This is the “risk analysis” for the 2005 base year. There is no need, and in fact it is distracting, to present the number of failures in the next 50 and 100 years. These rates will change due to forcing from sea level rise, levee maintenance, etc. It is not clear why they make certain assumptions, e.g., no more than one failure on a high tide. They did not show that there was a significant correlation between tide and failure, so this seems arbitrary.

The seismic risk seems over-stated based on the historical performance. Figure 1 and table 1 compare the DRMS estimate with the raw data from the past 100 years. While there are no known incidents of flooding due to a seismic event in the past 100 years, DRMS estimates that there is a 100 percent chance of at least one seismic-induced flooding incident in 100 years, and a 95 percent chance of an event where at least 10 islands flood. Even if we assume that only the last 20 years are representative of the present-day conditions in the Delta, then we would have expected two events with at least one island failing due to an earthquake and there is only a 16 percent chance that we would have had no failures due to an earthquake.

The seismic risk seems over-stated based also on the previous CALFED (2000) analysis. This study estimated the annual frequency of at least one earthquake-caused levee failure to be three times smaller than the DRMS estimate (a return period of 30 years versus 10 years).

We would like to know about any geologic evidence of liquefaction in the Delta soils over the past 5,000 years. We would also like to see a hindcast of the site response from the 1906 Earthquake to see if widespread liquefaction is predicted. Just because the levees were lower, there still would have been obvious signs and reports of ground liquefaction if it did occur.

The failure rates shown by cause (seismic or flooding) appear to be very high (tables 13-3, 13-6, and 13-8), so the probabilities, by island, of failure in 25 years, and in 50 years are scary, but perhaps unnecessarily so. Given the historical record of much lower instances of failures, and the recollection of the Panel of previous studies showing lower failure rates, these high failure rates shown for many islands need further evaluation. Unfortunately, more insight by the Panel into possible reasons why the high failure rates were estimated are not possible without further investigation.

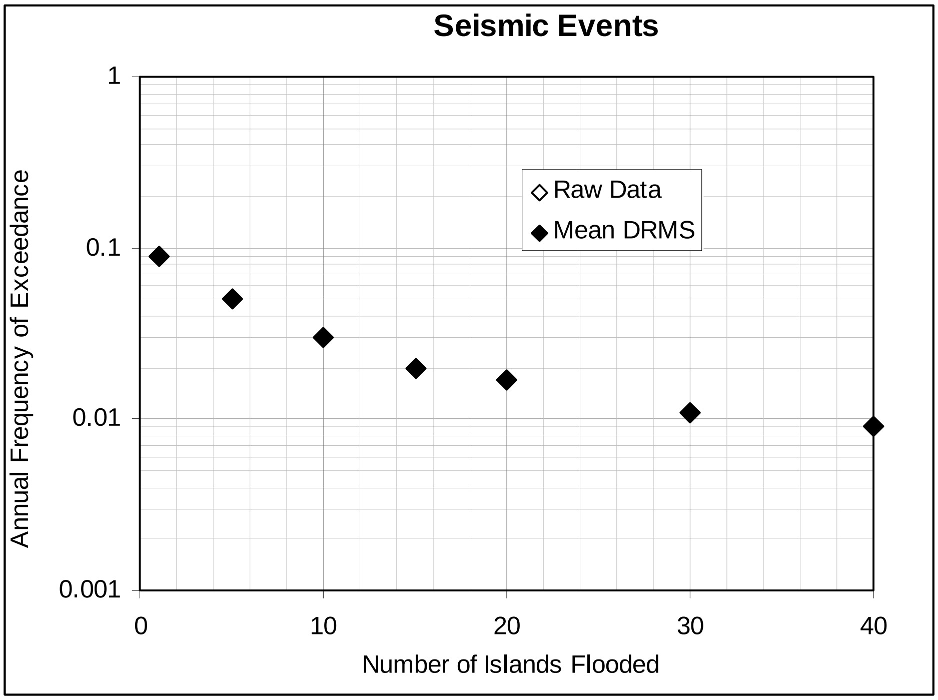

Figure 5: Comparison of DRMS Estimate and Raw Data for Seismic Risk

Table 1: Comparison of DRMS Estimate and Raw Data for Seismic Risk

	Seismic Events	
Number of Islands Flooded	DRMS Probability of Exceedance in 100 Years	Actual Frequency of Exceedance in 100 Years
1	***1***	***0***
10	***0.95***	***0***
20	***0.82***	***0***
30	***0.67***	***0***

There seems to be a very high failure risk of islands compared to previous work. For example, in table 13-3, Sherman Island has a annual mean number of failures of 0.043. That is about 4.3% chance of failure from an earthquake each year. Looking at the area of similar islands, this seems much higher than what Torres et al. (2000) found. In that region they simulated an M=7.1 earthquake on the Hayward Fault. That would be a very big event for this region. They determined that only 0.1 to 2 islands would fail in the region of Sherman Island. This is a different determination than in the report so it is difficult to compare, but Torres does present PGA maps. From looking at those maps, it appears that the probability of PGA of 0.2g (from 0.003-0.008 or 0.3-0.8% (depending on the model used). That is much different than the DRMS *Phase I Report* found. It is difficult to know if these probabilities are reasonable and why they are different from the Torres results because this was not discussed in the report. It is important to know how this affects their outcomes.

The tables presented in this section confuse the issue. Some make projections into the future when this is supposed to be 2005 probabilities and impacts. They need to either stick to 2005 or bring in the future for each topic and completely describe the probabilities and impacts. Also, they use years that do not meet the charge. They should stick to 2005, 2050, 2100, and 2200. If there is some reason to use other years, they should explain why. They do this throughout the report and it makes it difficult to compare across topics/sections.

Much of the information presented here should be (or was) in the "consequences" section. The organization of this is very confusing. It would be much better if they first developed the potential losses and then took each major topic as defined by AB 1200 and fully addressed it: probability of occurrence in 2005, 2050, 2100, and 2200.

Public health and safety consequences from earthquakes seem very minimal (two sentences).

For subsection 13.3.2 (Flood Consequences), they seem to have changed how they do scenarios. This has been a confusing issue from the beginning. They present in one figure in "Section 4" that they have this model-based, "continuous" system that determines probability functions for each process. But we find here and in the seismic section that they present scenarios. They spent a huge amount of resources trying to develop some probabilistic approach and in the end fell back on scenarios that could have been used effectively from the beginning. If they would have set out with this approach and used existing data and information instead of doing new analyses, they would have produced a much more useable and understandable document. The fact that they fall back on scenarios in the end shows that they cannot make the other approach work.

The estimated hydrologic risk in terms of the frequency of flooding events seems reasonable based on the historical data for events where up to 15 islands flood. However, the estimated frequency for events with 20 or more islands flooding seems high based on the historical data. For example, there is estimated to be greater than a 50 percent chance of at least one event with more than 20 islands flooding in 100 years, and yet there have been no such events in the past 100 years. Some discussion of the reasons for and justification for this discrepancy is needed. We would like to see a sensitivity analysis to understand what types of events are driving the cases where 30 or more islands are flooded.

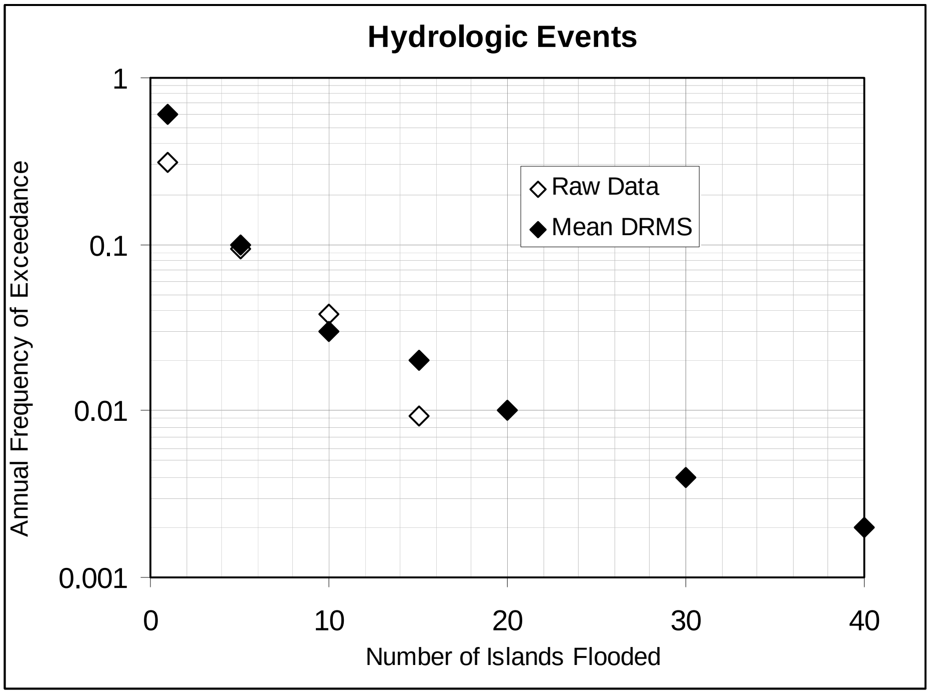

Figure 6: Comparison of DRMS Estimate and Raw Data for Hydrologic Risk

Table 2: Comparison of DRMS Estimate and Raw Data for Hydrologic Risk

	Hydrologic Events	
Number of Islands Flooded	DRMS Probability of Exceedance in 100 Years	Actual Frequency of Exceedance in 100 Years
1	1	1
10	0.97	1
20	***0.61***	***0***
30	***0.3***	***0***

Consequences: The treatment of consequences is not consistent with the treatment of the hazard. Why were the consequences estimated for only a handful of scenarios (18 for seismic cases and only 2 for hydrologic cases)? Why were the frequent, but smaller magnitude events (such as one island flooded), completely ignored for the hydrologic cases? Why wasn't uncertainty included in estimating the consequences? The consequence of $34 billion the worst-case scenario, 30 islands flooded due to an earthquake in a dry-water year, seems small relative to the significance that has been placed on this possibility. What are the associated probabilities with the wet, average and dry water years?

The consequences subsection ("Section 13.3") was disappointing, and especially for the ecosystem consequences. The panel presumes that the authors ran out of time, and not that the authors think this is a completed and documented analysis. Why so few earthquake and flooding scenarios were followed through to the end of the analysis is baffling. The authors failed to fulfill what was promised, and even what they described as coming in the beginning of the draft report.

- The results presented in this section seem out of step with what many of us would have expected. For example, failure rates seemed high (tables 13-3 through13-8) to many of the panel members given historical records. Failure rates are critical and should be fully justified.
- The eco risk index is incongruent with the methods presented in the earlier sections. How was this derived?
- We don’t understand why so few scenario runs for flooding scenarios (and really even for earthquakes) were conducted in the end? Maybe the earthquake scenarios cover at least the boundary conditions, but we are not even sure of that, and for certain, the flooding boundary conditions (based on frequency) have not been established. Without these, there really isn’t a way to make trade-offs for infrastructure investment decisions.
- The population risk measure borders on silly. Tallying the entire population for any given life and safety risk inflates the true life and safety risk. The project team should use a standard approach.
- It seems like there should be enough information provided that a person could draw a line and say “here is what is catastrophic.” There is just no way to do that with the current information.
- There aren’t really any integrated models; text referring to integrated models should be dropped from the report.

Specific Comments:

Page 13-4. Last paragraph: What does the following passage mean: “Because of irregularities in the levee crest elevations (singular dips and spikes) the probability of flooding by overtopping were (*sic*) modified to correct for these artificial conditions. Overtopping was allowed to initiate only between the two points bounding the 100-year flood event.”

Page 13-3. Table 13-2: We like this. We think it might be better (more useful) if you added a column for 10 years. Same comment for table 13-5.

Page 13-5. Table 13-7: In discussing this table the authors might just make the point that people who go to Las Vegas and gamble, place bets all the time on odds of 0.48 to .049 which is about the odds for failure of 20 islands in the next 25 years.

Page 13-8: The authors are unclear as to how the risk index “incorporates immediate mortality and as well as long-term impacts.” One cannot deduce from the index calculations how these are weighted or how they influence the index. Indeed, we have no idea what how to interpret this risk index.

Page 13-9: Why the risk index method is here is puzzling. Table 13-26a should be in "Section 12" as part of the methods for ecosystem consequences.

Page 13-10. Table 13-26a: This risk index to measure ecological impacts seem like a reasonable approach, however it is not described anywhere. Examples of scenarios should be provided to gain insight into its meaning.

Page 13-12: The authors do not seem to know what to do with the risk index results. While the simple approach for terrestrial vegetation and wildlife is satisfactory, the Panel (and apparently the authors also) had no idea how to interpret risk index used for the fish species. What does the risk index of -62.5 under one scenario, and 3.2 under another for the same fish species mean? The authors then go on to conclude that adverse impacts on fish species were nearly universal under flooding but a mix of responses occurred under seismic. They say none of scenarios resulted in an index value close to the worse case. The interpretation goes nowhere past these generic statements. How will the risk index be used when (presumably) the analysis is completed? If this small subset of scenarios is any indication, interpretation of the risk index will be a challenge, bordering on the impossible. This is quite important because the other consequences result in dollar values, and the ecosystem consequences can get lost and swept aside if their effects are expressed in uninterruptible terms of an index whose value has unknown ecological relevance and whose sensitivity to environmental effects is undocumented.

Page 13-13: The impacts on terrestrial vegetation and wildlife provides more hope for a useful metric that can be interpreted and not get lost when places side-by-side with the economic losses. A 42% loss of crane habitat is worthy of notice.

Page 13-14. Table 13-27: This table could easily be misinterpreted to indicate that the estimated number of fatalities in the case of one island flooding is 1,837 people. Additionally, flag no loss of life costs please.

Page 13-16. Section 13.3.2.3: Authors should take the opportunity to highlight public health, and safety MUST come 1st in priority.

Page 13-17: The poorly documented analysis that resulted in a very few scenarios actually being examined then culminates in the very dramatic statement "The population at risk and the economic and ecological consequences from a major event are expected to be severe." Where did this statement come from? Maybe one can go out on a limb and say the very limited analyses suggested economic costs would be such and such. That would be a large stretch. The portion of the statement related to ecological consequences is unsubstantiated by the analyses presented. In the end, the analyses in this report (with the gaps and details either taken on trust or filled in by the Panel), can only say that the ecosystem effects may be severe, or may not be severe (i.e., cannot say much of anything). The report seems to come to a sudden halt here prematurely.

Figure 13-1: The confidence bounds seem much too narrow given the significant uncertainty there is in predicting the occurrence, magnitude, and effects of potential earthquakes in this region. (Why does this figure show as many as 90 islands that could be flooded – we thought there were only 66 islands?)

Section 14 (Future Risk Analysis)

General Comments:

This section should be a solid presentation of what will change and how it will force the system. It starts with more unsubstantiated statements. Many statements are sloppy and so appear biased. For example, they state on page 14-1 that, “There are two factors to consider when evaluating future years – (1) the likelihood that an event will occur in any future year is increasing and (2) the likelihood that an event will occur at least once over a number of years grows even higher.” This is not true. The consideration is how will conditions change, therefore changing the likelihood of an event. It may increase or decrease. It is not foretold that it must increase. Statements like this run throughout this section. They need to be much more precise. It is not reasonable to make statements like, “[…] when exposure period of several years is also considered, the likelihood of an unwelcome event becomes high.” What is “unwelcome,” what is are “several years,” what is “high”? They also state on the first page of this section “[…] information is not available to conduct a comprehensive analysis of future risks.” This is amazing. Is that not what they have been doing for all this time? They are supposed to have done a precise, well-documented, statistical assessment of risk. Statements like these do not lead to confidence in their numerous figures.

This section reviews the assumptions embedded in the future analyses that the contractors were asked to perform as a result of AB 1200. This is very difficult charge to the contractors. This section outlines the various assumptions made concerning future events for 2050, 2100 and 2200, such as climate change, subsidence, population changes, and so forth. In general, the analyses here (mainly qualitative in nature) seem reasonable (to someone who is neither an engineer nor a hydrologist) and proper caveats are provided. However, we have serious reservations about whether these analyses can even be performed, given the large uncertainties embedded in any assumptions the analysts would make concerning the state of the world 50 or 100 years in the future. Imposing some limited, future conditions, such as climate change, on the current state of the world (i.e. 2005 conditions) is a more defensible approach than trying to forecast economic or other conditions beyond more than one or two decades. Whatever approach the authors chose to use, we encourage them to provide strong cautionary statements concerning their use in the decision process.

Again, this section lacks citations to previous work, substantiation of statements, etc., all the things seen in the other sections. They also do not give ranges of results or outputs to put this in the context of uncertainty. In fact, the presentation throughout the report does not emphasize or even really mention uncertainty. They need to develop a much more transparent and inclusive presentation of what they have found with uncertainty on it. In areas where they developed uncertainty, they do not use it in the final analyses (e.g., climate change).

The authors seem to assume no, or minimal, mitigation for any of this. Business as usual does not mean nothing will be done. It is not clear what they considered would be mitigated in any of their analyses.

They do not show how they merged all the previous information to come up with these combined predictions. This is a problem throughout, but severe here.

The authors continue to make general unsubstantiated statements when specific, detailed, and well-substantiated ones are needed.

They ignore previous work throughout. For example, they say that there is no indication that tidal amplitude will increase with time. That may be true astronomically, but there have been papers (e.g., at the CALFED Science Conference, by DWR scientists) that predict increased storms and increased storm surges and increased effective sea level in the Delta as ENSO events increase (article by Hansen in 2006 or 2007).

This entire section has too much repetition and not enough substantiation. They need consistency. They need to present only predictions for 2005, 2050, 2100, and 2200. And they need to say why they ignore 2200 (reasonable, but they need to justify it).

The authors need to consider the range in future climate change not just the median value. Parse out major sources of uncertainty and address each one.

The maps are very nice.

Ecosystems - what ARE the risks?

Specific Comments:

Page 14-2. Sea level rise bullets: The authors need to provide specific citations for their climate change-induced assumptions. For example, we believe a rise of .25 inches per year from 2005 to 2050 is several times higher than current rates reported earlier in the report. While this assumption may indeed be reasonable, some attribution would strengthen this, and other assumptions. This concern applies to many other sections of the report, where proper referencing is absent.

Figure 14-3: Shows salinity response to a 90 cm increase in sea level. However, they do not consider a 90 cm increase, they consider at 1 foot and 2.5 foot (again without uncertainty) 30 cm and 75 cm, respectively. Considering the cost of this effort it seems like they could do the analyses needed, not give some estimates around one that that was not needed or they happened to have.

Page 14-3. Bullets: More explanation is needed on why frequency of exceedance increases with time.

Page 14-4: They state that in 2050 there will be 50% more total runoff in the system. This seems very high. This increases even more for the 100-year predictions. Similarly, the predictions for changes in peak flow seem very high. Also, they present this data with no uncertainty. There is large uncertainty in climate predictions, especially when they get transferred to runoff, and that increases dramatically with time. They need to put these numbers in that framework. There are numerous concerns like this throughout "Section 14."

Page 14-4. First bullet: As noted in previous comment, some source citations here would be helpful. Also, in first paragraph under "Floods, Part 2," we believe "levee" needs an "s" and the "s" after failures should be deleted.

Page 14-5. Sixth line from top: Delete "-ment" from "improvement"

Page 14-6: Citations to sources for these bulleted assumptions would be helpful.

Page 14-8. Last sentence in first paragraph under subsection 14.1.6: This sentence reflects one of my concerns about mixing of "risks" and consequences in this report. We think this should read that increasing population "contributes to increased consequences of levee failure."

Page 14-9. Under "Business Activity:" Instead of saying "the entire state," we think, "the state as a whole" is more appropriate.

Page 14-10. Second complete paragraph: What does the sentence "However, as urban water use and tapping of local resources increase, demand hardening will occur" mean? By demand hardening, do you mean that demand becomes more inelastic because there are fewer possible adjustments? If so, then say that demand will become more inelastic.

Page 14-12. Sentence near bottom of first paragraph: Needs a "the" between "In" and "future." Also, in subsection 14.1.10, the sentence reads, "Other factors were not so easy to predict." Do the authors really mean that these other factors were "easy" to predict?

Page 14-13. Last subsection: In the first sentence, we think it should read "The risks *of* Delta levee […]" not "from." Also, how high is "high" in terms of risk. What does this mean to a state decision-maker in terms of scientific or engineering advice?

Section 15 (Assumptions and Limitations)

General Comments:

Although brief, this is an important section in terms of how to interpret and use the results of this study. The list of assumptions and limitations is helpful. We would like to see the list expanded to include an item dealing with the “methodology,” noting the problems inherent in using linked models of different structure and precision. Nowhere do the authors state that they do NOT consider the range of future climate change (!). This is a major assumption that we think is currently skewing the results.

Specific Comments:

Page 15-1. First bullet: Need a *the* before “ecosystem” and an *s* after “water export.” In the third bullet, it is noted that the engineering studies were conducted with “[...] a coarse data grid, hence carrying less site specific detailed, etc […].” In some sections of the report, however, much attention is paid to what appears to be fine scale forecasts of levee failure. This bullet then raises questions of whether what is reported in the sections is consistent.

Page 15-2. Second bullet, last sentence: Not clear what this is saying. In last bullet on page, need an *s* after “requirement.”

Other

Climate Change Technical Memorandum:

* Why use Knowles & Cayan snow pack projections when the PNAS (Hayhoe et al, 2004) are more recent?
* It's A1fi not f1 (table 1)
* Typo p. 20 - 2050 and 2050 for SLR (instead of 2050 and 2100)
* Needs an executive summary highlighting the main findings; also no conclusions to section 3.1 on slr!
* Doesn't include results from multiple model simulations for river flow (why not?? they are available! didn't take my comments from March into consideration - maybe lack of time/funding?)
* Wind analysis is pointless - not integrated with approach for wind/wave chapter. Should be using the same approach as the wind/wave analysis. Wind projections by regional climate models have not yet been tested and are NOT ready for prime time. Should NOT be used here.
* Does a good job of explaining what "should" be done in the final section but neglects the fact that a lot of that has already been done and you could do at least half of it with existing simulations but he does not.
* We were asked, "What is the refutability of the models and what is the degree of confidence that they can predict future conditions?"

For the wind projections – NONE

Levee Vulnerability Technical Memorandum

Page 44: The authors state that based on their judgment, they modified the permeability data of the peat by an order of magnitude and estimated the vertical permeability of the peat to be an order of magnitude less than the horizontal permeability. We do not necessarily disagree with the judgments, but it causes us to question the significant digits of the numbers in most of the tables.