## Delta Risk Management Strategy (DRMS) Phase 1

# Responses to IRP Comments on Risk Analysis Report

Prepared by:
URS Corporation/Jack R. Benjamin & Associates, Inc.

Prepared for:
California Department of Water Resources (DWR)

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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.

*This comment is primarily a documentation/editorial issue and will be addressed in the next revision of the report. The discussion of the development of the analyses (assumptions, methods, results, and interpretation of results and how they fit with the overall risk model) will be expanded. The Phase 1 Risk Analysis Report (and if needed, the TMs) will be reviewed and the text expanded to provide a complete description of the methods used and the evaluations performed. We will also provide a clear and consistent definition of the terminology we use in the reports.*

*A comment is made with regard to comparisons (spot-checking) of previous studies. We are not clear which other previous studies the comment is referring to. There are a few limited examples where we will add a discussion that compares elements of the DRMS risk analysis to other studies and highlight the reasons for any differences between our results and the results of the other studies. Examples of the previous studies that we will reference include the CALFED 2000 levee seismic vulnerability study, the Department of Water Resources 1992 summary of previous studies (this report contains a complete summary of relevant previous studies), the Jack R. Benjamin & Associates (JBA) 2004 risk analysis (which actually made use of the CALFED (2000) work), the USGS national hazard maps, and other applicable references to other topics presented in the report.*

*While we will modify the document as stated above, in our response to detailed comments on individual sections, there are a number of comments which suggest this work has already*

*been done or that parts of it have been done and were on the shelf and ready to be adopted in the DRMS analysis.*

*A study with a scope similar to DRMS has not been done for the Delta and Suisun Marsh.*

*The work done by others referenced in the detailed comments such as Torres, et al. (2000) and Mount and Twiss (2005) for example, are studies that could not be used in DRMS. These studies were out of date when DRMS was started (Torres, et al., 2000) or broad overviews of risks facing the Delta (Mount and Twiss, 2005, which cites the JBA 2004 risk analysis) and thus of no specific value to the assessments. We note that neither Professor Ray Seed, Dr. Les Harder, Mr. Gilbert Cosio or Professor Robert Twiss all active participants in DRMS (at least as members of the Steering Committee and as experts (with the exception of Professor Twiss) involved in the levee vulnerability analysis and the Torres, et al. study) suggested at any stage of the project that these studies should be used in any way, let alone be adopted. Thus we disagree with the characterization that is made in a number places in the review that work was available that could have been adopted by DRMS but was not.*

## 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.

*Again, we believe that when we provide proper documentation of how the analyses are carried out in the Risk Analysis Report, we will address these concerns. However, it would be helpful to get a more specific description as to what is meant by, "[T]he probability … [is] not integrated over the full range of possibilities."*

*As far as the specific areas raised by the IRP, we have provided some preliminary answers below. First, as a general response to this comment, we will add the necessary documentation of the analyses for the various topics, the treatment of uncertainty, and the limitations and the assumptions applied. We will also provide the justifications for the limitations and assumptions and their practical reasons. In places where we applied simplifications, we will fully explain and justify them (see some preliminary responses in item 3 below). In other places, we will conduct additional analyses as requested. For example, we will show the high-frequency, low-consequence hydrologic events. However, these events will not change the frequency of failures for the other analysis cases (moderate- to low-frequency flood events). The disruption of water supplies due to flood-induced single-island failure was found to be insignificant and will not lead to significant degradation of water quality and hence to interruptions of water export. Furthermore, we will also add probable life loss to our estimate of the population at risk.*

*As for the estimated probability of levee failure due to seismic events, we maintain our conclusions on the expected future probabilities of earthquake occurrences, the results of the probabilistic seismic hazard analysis (PSHA), and their impacts on the levees. We note that the PSHA is based on the USGS seismic source models for the major Bay Area faults, and the analysis was reviewed by both the USGS and the California Geologic Survey (CGS). Because many updates have been made to the seismic hazard models in recent years, we believe that the previous studies are no longer applicable. Specifically, the 2002 National Hazard Map ground motions, HAZUS model, and CALFED 2000 study (work done in 1999) do not include the recent updates of the seismic sources, the new attenuation relationships, the time dependency, or more recent site-specific data. We discuss these key points further below.*

## 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.

*The treatment of uncertainties is not highlighted in the documentation equally among the various topics covered. We will expand our presentation of this topic and be clear as to where uncertainties have/have not been addressed.*

*As we discussed at our meeting with the IRP in March, there are areas of the analysis where the evaluation of uncertainties (aleatory and epistemic) could not be evaluated due to the level of work that would be required to make a credible assessment. The reasons for this vary from one topic to the other. On this point, we note that during the meeting with the IRP, one of the panel members indicated that in his view the epistemic uncertainties in an ecosystem analysis are so great that assessing them and displaying them are counterproductive to decision making.*

*In the areas where the uncertainty evaluation could not be carried out (i.e., economics, hydrodynamic and water management, ecosystem impacts, and climate change), we attempted to mention it in the report. As we revise the document we will insure that this is done and discuss the simplifications. Where ranges in outcome are applicable we will use them.*

*The discussion of the uncertainty with regard to estimates of risk in future years is quite problematic and we internally debated it a great deal. In our response to comments in Section 14 we provide the rational for the approach we did take. This said, we disagree that "a false and potentially dangerous sense of inevitability and certainty" is presented. We are quite clear as to what we did (taking medium estimates of parameters). Further, we found no evidence that any of the key factors which influence risk will lead to a reduction in risk in future years under the business-as-usual approach we were directed to take.*

*In conclusion, we plan to add a more substantive discussion of the treatment of uncertainty where it has been evaluated and propagated in the analysis, and where it has not been addressed, we will explain why.*

## 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.

*As we discuss later in our responses we will be expanding the presentation of the risk analysis methodology and the quantitative methods that were used in the analysis. As reflected by the specific wording of the IRP comment, this is less about a "lack of integration of analyses" and more about completing and documenting the analyses and providing QA/QC for the integration. Our action plan is to provide a more detailed description of the integration of the various parts of the risk model and documentation on the QA/QC process that was followed.*

## 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.

*We agree that the methodology for assessing quantitative aquatic impacts can be improved. At this time we are working on a new approach that will focus on assessing the increase in the probability of extinction of selected aquatic species. The goal of this effort is develop a quantitative (or quantitative/qualitative hybrid) model that provides a best estimate of the immediate impact (and the range or uncertainty around that estimate) for any levee breach scenario for each fish species that we will be evaluating in the Delta estuary.*

*As we are going through this effort it is not clear that we will be able to develop a quantitative estimate of the epistemic uncertainty in the model results. While we believe it is preferred that such an estimate be made, it is not clear the experts and the science will be able support such an effort at this time and/or within the time frame of this work. Thus, our goal is focused on developing a simple model with a range about a best estimate.*

*Specifically, we plan to simplify and quantity the impacts in a simple, expert- elicitation-based approach. The main elements of the revised model are based on a simple cause and effect evaluation. The model will focus primarily on the impacts to the aquatic species from levee failures and entrainment. The failure mechanisms, timing of breach formation, turbidity, entrainment (percent of population entrained based on toe-net survey and density of population by region), island closing and pump-out models have been already developed by the DRMS technical team members. These will be defined and quantified in a manner suggested by the experts assembled to help with the development of the model. The quantification of impact (percent mortality and increase in the probability of extinction) will be developed based on input from the experts.*

*Currently the experts assembled for this effort include Professors Wim Kimmerer, Peter Moyle and Bill Bennett and Dr. Chuck Hanson. We are adding possibly two more experts on fishery from the DWR as suggested by the three experts.*

## *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.

*We appreciate the comments provided by the panel and believe they will be helpful in achieving an improved product. We are in the process of revising the Phase 1 analysis report and improving elements of the risk analysis. We believe these changes will satisfy the panel's concerns.*

## 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.

*Given the objective and scope of work that we were charged with for this report, it would be difficult to fully meet the intent of the comment by describing the methods, assumptions, and other such aspects in any detail.*

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.

*We appreciate the comments offered in this paragraph. In response to the points raised we offer the following:*

*With respect to the first and second concerns, we will be revising this document in light of the comments provided to improve the presentation. In addition, we will discuss with DWR and the Steering Committee to get additional guidance as to who the audience is that should be targeted and what approach should be taken to address that audience.*

*We do not agree with the third concern, regarding overstating the findings, for reasons stated in the response to the Tier 1 and 2 comments. We note that in past risk studies for critical facilities, such as nuclear power plants in the eastern U.S. (which have been performed since the 1980s), there was a similar reaction to the level of the seismic risk that was being estimated, even for plants located in quiet zones (outside New York City, Philadelphia, Chicago, etc.). Seismic risk is unique, due to the nature of the hazard that earthquakes pose. It is often not well understood. At this point, over 20 years later, seismic risk in the central and eastern U.S. is better understood and it remains a dominant contributor to risk. In addition, seismic risk analysis and the results it generates is an integral part of current engineering and regulatory practice.*

*With respect to the fourth concern we are a bit puzzled by the comments made here. The risk analysis methods that have been used to evaluate the Delta levee system are consistent with current practice. The statement, "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)" we do not understand, particularly the note in parenthesis. Consequences are part of the risk as indicated in the definition we provide and in the equation presented in Section 4. Lastly, we do not compute EMV's as a measure of risk.*

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.

*We appreciate these comments and will work to present a clear message in our revised report.*

*There appears to be conflicting guidance in this paragraph. It states, "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." Should smaller details be*

*presented as part of the big picture presentation? This would seem to contradict the point being made here of trying to address those with an eighth grade education or higher.*

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?

*The point made here is well taken. We were asked by the Steering Committee to produce a summary of the summary report. The expectation was the summary would be the 'short' document that saw the widest distribution. In producing the summary, some of the text in the summary report was used.*

## 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.

*This comment presents a difficult charge. If the comment means a full scientific discussion should be provided, we disagree. Further, we were offered guidance by DWR and our Steering Committee that presentations of this nature are not appropriate for this document. If on the other hand the comment means a simple sentence or two should be provided with a reference to the Climate Change TM, this could be done.*

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

*We will consider alternative titles when we revise this report.*

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.

*In the discussions with the Steering Committee and DWR we were not given the direction to write a scientific report. Rather, we were asked to tell a story (a very non-scientific notion) about the Delta and the risks. Further, it was suggested that a technical writer, experienced in matters related to California water, CALFED, etc. be charged with writing this document for the masses. This is what was done. As a result the summary report was not written with the notion of preparing a scientific document.*

*The table that is referred to is an attempt to provide probabilities for events which are familiar to most people.*

*We will take this comment into consideration as we revise the report.*

All references to "delta" should be capitalized.

*We will make this correction.*

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.

*The definition of risk provided in the box is a standard, common definition. It entails two elements – chance and a negative consequence. The idea of representing risk as an expected value is neither required nor, for purposes of this analysis, preferred. An expected value representation of risk is but one metric. It is our objectives in the DRMS analysis to estimate the entire distribution of a particular consequence (economic cost). This way one knows the full range of possibilities and their frequency. From this information an expected value could be computed. Alternatively an expected value can be computed without deriving the full distribution first. A more complete analysis derives the full distribution first.*

*The comment is correct; we do not calculate the expected monetary values in Phase 1. As we state in Section 4 of the Phase 1 report, we do not estimate (measure) risk in this way. In the Phase 2 analysis we do use the results of Phase 1 in this manner.*

*The results of this study are unique. No prior study, including the Torres, et al. (2000) work, estimates the frequency of islands flooding due to seismic or other events. No other study has attempted to quantify the impact of levee failures on water exports, or the economic impact or costs of these disruptions; to develop a systematic tool to quantify the cost and duration of levee breach and damage repairs, etc.*

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.

*We do not find this sentence in our version of the Summary Report.*

*The statement referred to is admittedly a bit vague. By the same token it is quite general, and not a very strong statement at that. We will attempt in our revision of the document to improve the presentation of the message we are trying to get across.*

*We were trying to make a simple point. The best way to state this is by example. For 2100 we have estimates of the frequency of earthquake ground motions and sea level rise. However there is no information we are aware that predicts the population of the state, the state of the ecosystem, etc. in the year 2100. As such, with no data, we consider it more difficult to estimate consequences when there are no available estimates of our exposure (i.e., population size, etc.).*

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"

*We do have information on the correlation of earthquake ground motions over relatively short distances (see Bazzuro and Baker, 2006). There is considerable uncertainty in the estimate of earthquake ground motions, which we model in the analysis. Note, the ground motion modeling is one area were we have a tremendous amount of data and modeling experience. Thus, the models are empirical. This said, there still remains a considerable amount of aleatory and epistemic uncertainty in the estimate of ground motions.*

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."

*We will consider this suggestion in our revision of this report.*

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

*We are unclear as to what is meant by "Explain why the frequency…" This is the summary of a summary report. This hardly seems the place to provide explanations – even brief ones.*

*For the third bullet we will revise the text so the meaning is clear.*

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.

*We will revise the text to be clear and consistent in our use of terms.*

*We will make a distinction between the Phase 1 and 2 activities.*

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.

*The primary combination of events that was considered were island failures due to any cause and wind waves that result in levee interior erosion on flooded islands.*

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

*We will revise this sentence in the revision of this report.*

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.

*The risk analysis does estimate the frequency of exceedance economic consequences and the frequency of exceedance of numbers of islands being flooded. These results are estimated for each hazard and they are combined to present a total.*

*We do not present ranges in Section 14. The reasoning for this is presented in our response to the Section 14 comments.*

*We certainly do not intend to mislead the reader. As we revise the report, we will take these comments into consideration.*

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.

*The point with respect to BAU is well taken. Certainly one cannot claim that the BAU analysis is unbiased in any sort of statistical sense. Our intent is to say that a BAU analysis is a known (reasonably understandable) basis for performing the analysis and can be used as a common baseline for the Phase 2 analysis. Further, the BAU is also consistent with the need to examine whether the Delta is sustainable in the sense that current and/or future risks may be considered by policy makers to be too high.*

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.

*The figure refereed to is a stylized figure. It is not intended to provide the details of the risk analysis, the interface between elements of the analysis, and the integration process. As we indicate in our response to comments on Section 4 we will be expanding the presentation of the risk analysis methodology and its implementation in the Phase 1 report. We do not however anticipate providing much if any of that information in this document.*

Page 13: How "unique" is DRMS? More comprehensive? More innovative?

*DRMS is unique in a number of ways. These include:*

- *No study of its kind for the Delta has been carried out.*
- *New computational tools were developed (ERR model, the WAM model) that evaluate parts of the problem that had not been addressed previously or in the manner that is done in DRMS.*
- *A GIS database was compiled which previously did not exist.*
- *An economic model was developed to estimate the impact and costs associated with water export disruptions*
- *Refined fragility functions by class and reach within each island using more than 2000 geotechnical borings.*
- *No other study has made an estimate of the frequency of island flooding due to seismic, flood and sunny-day events.*
- *Performing hydrodynamic calculations to evaluate the effect of sea level rise on the position of X2 in the Delta*
- *Development of a flood hazard model that estimates the simultaneous spatial distribution of flood stages in the Delta.*
- *Development of spatial wind model.*
- *Development of a hydrodynamic modeling tool that has proven to be efficient and accurate.*
- *Consideration of the risk to the ecosystem (this model is being revised as requested by the IRP)*

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?

*Our admonitions aside, we clearly have no means to prevent the misuse, misinterpretation, or misunderstanding of any of the results we have produced. This has and we expect will continue to occur.*

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

*Current climate change models which have made estimates out to 2100 do not predict significant changes in wind speeds in the future. As a result one can assume that current wind models remain applicable.*

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?

*The concern related to the estimate of the seismic frequency of levee failures we have addressed in our response to the Phase 1 report. Also see below in the response to comments for page 20.*

*In our opinion, the last sentence of the comment is unsubstantiated conjecture, and an unprofessional remark that should not be part of an objective scientific review. We are puzzled by the IRP observations on the seismic issues. No major earthquakes have truly tested the Delta in the last 100 years, hence no seismic-induced levee failures. The body of work on the seismic hazard in the Bay Area indicates that there is a 62% chance in 30 years that a large earthquake (M6.7 or higher) could occur in the next 30 years. Under such events our model shows substantial levee failures in the Delta.*

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

*Is this comment suggesting this explanation should be provided in this summary report? If this is the case, we would disagree. This level of detail is not appropriate for this report. The answer to the question is provided in the seismic hazard TM.*

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.

*The logic implied by the comments suggests a result other than zero is incorrect. The primary reason there is a difference is the fact the last hundred years has been a seismically quiet period in the Bay Area and in the Delta in particular. As a result, there have been no significant earthquakes. This said, the USGS, the Torres, et al. (2000) study all estimate there is a non-zero probability of ground motions of engineering interest (PGA > 0.05g) that can occur in the Delta. Based on the estimate of the seismic fragility*

*of Delta levees, ground motions greater than 0.05g have a non-zero probability of producing a levee breach.*

*We have used a set of scenarios to evaluate the consequences for a given number of flooded islands. These scenarios represent a sample of the large number of cases that could involve levee failures. In principal these scenarios are not a departure from the risk analysis framework. They model the consequences given the specified number of flooded islands.*

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?

*The use of the equivocating language is not needed here. We will modify the text in the revision of this report.*

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?

*We will revise the text to use the appropriate terms.*

*The percent of the population refers to; the percent of the aquatic species population that is in the Delta.*

Page 23: Where is this "risk index?"

*The risk index is a measure of impact that was developed. We are revising this evaluation and will not be using this index in the next revision of the analysis.*

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!

*Ruderal is a plant that grows in rubbish, poor land, or waste. All scientific terms will be defined in the revised report.*

*The statement on the individual island risk to local land owners will be removed.*

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.

*We agree the first sentence is not clear. We will revise as part of our revision of this report.*

*The comment is not correct in stating only the resources at risk will increase. The frequency of earthquake ground motions increase, as does the seismic fragility of the levees, as do the resources in the Delta and the state.*

*The results in Table 6 are an estimate of the increase in the expected annual losses. This is the only place where we deal with expected values for risk results.*

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."

*The sentence referred to is a bit unclear. The point we are trying to make is that an historic record which is relatively short provides us with a limited set of realizations of the natural processes that contribute to flooding in the Delta. There are combinations of factors and events that are possible (they may have been observed individually), but as yet have not occurred jointly. The probabilistic analysis evaluates the probability that all possible combinations of events could occur and as a result this type of assessment, which is used for all types of natural hazards probabilistic modeling, gives a more complete (accurate may not be the best word to use) estimate of the frequency and magnitude of future events.*

*We will make the revision to the text that is suggested.*

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

*We will make this revision to the text.*

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?

*We are unclear as to the meaning of this comment. We have provided in the response to Section 13 a detailed answer and our proposed expansion of the consideration of the scenarios in the consequence modules versus the probabilistic analysis in the earlier modules.*

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

*We will make this revision to the axis label.*

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."

*We agree there is a bit of a disconnect with respect to the presentation of individual island results and our admonitions. On the one hand we have been requested to provide this information by DWR and the Steering Committee. On the other hand individual island frequency of failure estimates have their limitations – thus our admonitions. For instance, it can be argued that we have not done island specific assessments – DRMS is a regional scale study. As a result the scale and level detail in the analysis is different. Further, even if a island specific assessments were performed (more detailed island specific evaluations conducted for each island), individual island results are self limiting because of the inter-connected nature of island failures during major events (floods and earthquakes) and the consequences of these failures.*

*We will re-examine our presentation of the island results and our cautions for their use.*

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.

*The word "expected" actually implies to us a level of certainty that we do not want to convey. We will consider the spirit of this suggestion in our revision of the report.*

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."

*We will make the corrections noted and consider the revisions to the text that are suggested in our revision of the report.*

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

*Is this intended to say "probabilities"?*

*In our reading of this page sunny day failures are not discussed.*

*The evaluation of the increase in the frequency of sunny-day failures in future years is described in Section 14 of the Phase 1 report and is based on the estimated increase in the hydraulic head against the levees as a result of sea level rise and subsidence.*

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

*The discussion of future risks is discussed in Section 14 of the Phase 1 report.*

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

*This information is reported four times prior to this page.*

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

*All statements related to risks in future years are with respect to the base year, 2005, results. In the revision of this report, the presentation of the future risks will be revisited and improved, taking into account these comments and our new work.*

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.

*We can provide a definition of uncertainty.*

## *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.

*Section 1 of the report introduces the purpose of the study and the scope of the work. It introduces the topical areas compiled for the risk analysis, and informs the readers of the technical memoranda and their relationship to the risk analysis report. It introduces and briefly describes the main topics of the risk analysis. It also introduces the team and the program functional and organizational structure and their relationship to the project.*

*Upon review of this section we think this introductory section has relevant information and, as a result, will not be omitted in our revised version of this section.*

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.

*We can not bring up all the significant data and analysis developments from the TMs to the risk report. Doing so would make the report much more voluminous, complicated, and disproportionate. We will make specific references to the TMs where necessary on any source of data, model development, or results of analyses.*

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.

*The introduction was not intended to describe the Delta. This is addressed in Section 2.*

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.

*The comment suggests that the Phase 1 Draft Report makes a general claim where it says "One place they say that they can make confident predictions 200 years out," is incorrect. No such claim is made for the entire study. Perhaps we say that about some topics such as seismic hazard but that may not be true with other topics such as economics and ecosystem.*

*We do not see where the inconsistencies exist. We stated the requirements of AB 1200 and outlined the scope of work. Section 1 is not meant to address the methodology and assumptions used to carry out the risk analysis. In Section 4 we defined what can be done with the current state of the science and what is not possible to predict. Section 4 describes exactly how far the future predictions were carried out in each topic. Not all topics could project to 200 years from now. See Table 4-5 for the topical areas and their future projections.*

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.
*There is no statement of fact in Section 1 except the claim in the DRMS' unique scope. Our research of previous studies did not turn up a risk study with similar scope for a similar region. Please refer to our responses to comments on the Summary Report (June 26th version), and to page 7-Summary in particular, where we provide a list of reasons that the DRMS work is unique. We will reference any study with similar scope work the IRP can provide. The numbers (levee length, areas, etc…) cited in page 1-5 come from the project database used in the various GIS applications supporting the project. We will therefore add a reference to the project database.*

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.
*More description of the roles and review processes of the various working groups will be provided in the next version of the 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.
*To the best of our knowledge <u>we addressed every reviewer's comments</u>. Copies of the comments and response have been included herewith as Attachment 1. If for some reason we omitted a comment or a reviewer please indicate which or whom. It is our obligation to respond to every reviewer.*

These shortcomings are more common in "Section 2."

*See response to comments on Section 2 below.*

This is a very poorly referenced section. The authors make very specific statements and present information without citations to the source.

*See response regarding references above.*

There are many repetitions and in general, the section is very wordy and difficult to read. *The section has been edited as a response to the first draft. It will be further edited in response to these comments.*

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 are no conclusions presented in Section 1 "Introduction" except the claim about the unique nature of the DRMS project. We have provided an answer to this question to a similar comment above. We can not find where these comments apply to Section 1. Again, Section 1 introduces only the objectives of AB 1200, the scope of work, the project team structure, and other related studies.*

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).

*We have not addressed or talked about how uncertainties are characterized in Section 1. These topics are discussed in the other sections of the report. We will address those comments in their respective sections.*

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.

*We honestly do not see where conclusions about the Delta are present in Pages 1-2 to 1-7. These pages and their sections are descriptive. Section 1.1.2 describes the goals and objectives, Section 1.2 describes the overview of what will be addressed in Phase 1 and references the 12 TMs, Section 1.2.1 describes the hazards addressed in Phase 1, Section 1.2.2. describes the consequences of levee failures to be addressed, Section 1.2.3 describes the risk under present conditions, Section 1.2.4 describes the risk under future conditions to be addressed, Section 1.2.5 presents the limitations of the study, and Section 1.3 presents the project team.*

This makes it appear that the authors have preconceived ideas about the system without justifying them.

*We take exception to this statement. Only the findings from the analyses are presented.*

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.

*Section 1.1 discusses the purpose, Section 1.1.1 references AB 1200 and Section 1.1.2 describes the goals and objectives. Each section is one paragraph long.*

*Reference to any similar risk studies will be added. It should be noted that we reference any study/report we used, both in the risk report and the TMs. By mistake, we may have missed a few references we used, and those will be added.*

*There are no data, tables, figures (except for the program function chart), or other extraneous material in Section 1 "Introduction".*

## Specific Comments:

**Section 1.0**

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).

*Terminology will be defined clearly and consistently in the report.*

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."

*They will be changed to "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.

*As indicated in the comment, a comparison with similar large scale risk studies needed to be drawn. That was the intent.*

*The project schedule is part of the project definition (scope and schedule). It is as important to mention the scope of the work as it is to mention the schedule. The readers need to understand that this is not an infinite research project with infinite schedule. Scope and schedule are the critical project constraint. More work can be done given more time. The review should be made in the context of the scope and schedule (see the scope provided to the IRP) .*

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.

*The SC members, TAC members will be listed. The function of the BRTF and it members will also be mentioned and the same applies for the IRP members.*

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

*You identified an error in the Table of Contents. We will correct it.*

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

*No, the IRP is not mentioned in the report.*

**Section 2.0**

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

*An explanation will be provided in the revised report.*

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.

*A graphic representation of the Delta 5000 years ago will be added.*

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

*A map with water development features will be added.*

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

*The missing towns will be added in the revised figure/report.*

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

*They are the same.*

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

*No response required.*

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. This report is not the place to suggestions of the type recommended.

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

*The color scheme on Figure 2-4 (you mean 2-4 not 2-3?) will be changed.*

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?

*In 1906 the word liquefaction was not defined yet and hence was not used in any literature or eyewitness reports at that time. We do not know if liquefaction occurred. There were no specific reports in recorded testimonies to confirm or refute the occurrence of liquefaction.*

*Yes, an analysis of seismic stability was performed on today's levees in the Delta using a model earthquake similar to the 1906 earthquake. The calculations indicate that liquefaction has a high potential of occurring during an earthquake similar to the 1906 San Francisco earthquake.*

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

*This bullet will be expanded and better defined.*

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

*A 100-year earthquake has an annual mean rate of occurrence of 0.01 or, equivalently, a return period of 100 years. A 1000-year earthquake has an annual mean rate of occurrence of 0.001 or, equivalently, a return period of 1000 years. We will add these definitions to the revised report.*

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

*It does not belong to that figure and it will be removed.*

## *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.

*We appreciate the suggestions and will consider them in our revision of this section.*

As above, there are many speculative statements that are not referenced, nor is data presented to support them.

*On review of this section we do not find any speculative statements. However, we would agree there are some places in this section, such as the discussions regarding climate change, a reference should be cited.*

*We are a bit puzzled by this statement. It suggests there are "many speculative statements," yet not one such statement is called out in the specific comments provided below.*

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.

*We disagree with this statement. We find no indication in this section to suggest that we have apriori decided what we will find in the analysis.*

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.

*The purpose of this section is to discuss the overall scope of the analysis. It was not the intent here to discuss previous work, methods that would be used in the DRMS analysis, etc.*

*As we mentioned to the IRP members in our phone discussion after submittal of the written review, we did not ask the authors of the TMs or report sections to provide an exhaustive review of past work on all subjects related to the DRMS analysis and to provide a discussion of what was relevant/irrelevant, useful or of no use, etc. We did not have the luxury of time to carry out this sort of an effort (such as might be undertaken by a graduate student working on, and eventually writing a thesis). The report should document what existing information and data were used. Where this was not done, it will be corrected.*

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.

*As intended, this section defines a number of the factors that determine the scope of the DRMS Phase 1 analysis. For instance, the geographic scope of the study, the concept of Business-As-Usual, hazards to be considered, etc. all need to be defined and are discussed in this section. It is our view these topics should be defined early in the report. For instance, Business-As-Usual was an important topic for DWR and the Steering Committee and one that was not easily understood by most when they were first introduced to it. This section describes the scope of work not the framing of the work in the context of past studies. It is a standard practice to state of scope of work in any engineering report consults produce.*

## 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…"
*We appreciate the suggestion and will consider it in our revision of this section.*

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.

*The statement which is being quoted in the text says, "making an assessment of risk* ***is*** *uncertain." We agree, this statement is a bit confusing as written. The whole sentence is, "To the extent the present state of knowledge is incomplete, making an assessment of risk is uncertain." The point we attempted to make is; there is epistemic uncertainty in estimating risk due to our incomplete knowledge about the Delta. As we revise this section we will provide a better discussion of this subject.*

*The last sentence in this comment regarding "the idea of risk" seems to be a bit of a generalization and one that does not apply here. The suggestion that a risk analysis is intended to estimate "expected consequences" only is not the focus of the DRMS analysis.*

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.

*We appreciate the suggestion and will consider it in our revision of this section.*

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

*No response required.*

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?

*The simple answer is yes, these costs are generally known.*

*This statement is made in the context of the discussion of Business-As-Usual. We know (from DWR) what the current spending has been to maintain levees and to make minor repairs over time. Based on general experience as well as project specific experience and previous studies, work by CALFED, etc. we also know the current maintenance level of funding is not adequate to stay ahead of sea-level rise (raise levees substantially and maintain performance). This is self-evident, since maintenance programs were not intended to stay ahead of sea level rise.*

Page 3-4: Defined *(sic)* "Primary" and "Secondary" Zones.

*We appreciate the suggestion and will add these definitions in the next revision of this section.*

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

*We appreciate the suggestion and will make this change in the next revision of this section.*

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

*The statement is intended to reflect the fact there are areas where we have poor information about current conditions and limited or no information about the future. The use of the word "impossible" may be strong. Our experience is, given the practical realities of this project, it was* ***not possible*** *to evaluate these uncertainties. This was true for current conditions and certainly applies for future conditions. In addition, certainly it is difficult to make an assessment of risk in the future where there is no data and even more difficult to cite a source.*

*For example, as one of the panel members indicated in his comments at our meeting in March, the uncertainties in the assessment of impacts to aquatic species are so great, it is best not to evaluate/present them. We would expect similar concerns to be expressed regarding projections in the future where there is no data. However, in principle we*

*believe it is possible and appropriate to estimate these uncertainties and present them, for current and future conditions where possible.*

*We will review and revise this statement to better reflect the view we wish to express.*

## *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.

*We agree the Summary document does not describe how the pieces (model, assumptions, etc.) of the risk analysis fit together. Our directive from DWR and the SC suggests the Summary document is not the place for such a discussion.*

*We also agree this section does not provide the expected level of detail. It is our plan to expand the presentation of the risk methodology that is used in DRMS. This expanded presentation will be provided in this section as well as in appendices to this report.*

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.

*As indicated above, the discussion of the methodology will be expanded. We also agree there needs to be an improvement in the clarity of the presentation.*

*The reference to unsubstantiated comments is general and without reference to examples where this is the case.*

*The final sentence in this paragraph suggests there is an extensive list of risk studies for the Delta that we could have made use of and thus should have referenced. As we discuss below, there are no such studies that could be adopted.*

*As we noted previously, where we have used existing information and data, these will be referenced.*

*The reference to jargon is general and without reference to examples where this is the case.*

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?

*As noted above we will be expanding the presentation of the risk methodology.*

*The HAZUS methodology, or any other (a pre-packaged method or otherwise) for that matter, does not address all (if any) of the topical areas in a manner that were considered/required in DRMS. As an analysis tool (with built in modules and datasets) or simply as a software tool (calculator only) HAZUS is not suited for the DRMS risk analysis. Note, we did use some of the datasets available in HAZUS and the flood loss estimation functions as part of the Delta infrastructure part of DRMS.*

*Note, HAZUS was not developed by the USGS. It was created under the management of FEMA (now a part of DHS) and developed by companies working under contract to FEMA or their administrator.*

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.

*The statements made here are a bit surprising. We suspect this may be some of the "jargon" a previous comment was referring to.*

*The terms seismic hazard, seismic fragility and seismic event are standard terms in earthquake engineering and seismic risk analysis. We believe these terms were used consistently in the report.*

*In general, we will review the document with an eye to the consistent use of terms and where appropriate, provide clear definitions of terms that may not be known to the reader.*

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.

*We appreciate the suggestions for making revisions to these sections and will consider them as we move forward.*

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.

*A risk analysis for the Delta such as has been carried out in DRMS has not been done before.*

*The work by Torres, et al. (2000) could not be used in DRMS since in all aspects of their analysis, the seismic hazard model and the fragility analysis are out of date. In addition, Professor Ray Seed (member of the DRMS Technical Advisory Committee and Steering Committee), Dr. Les Harder (Deputy Director of DWR), or Mr. Gilbert Cosio (consultant and member of the DRSM Technical Advisory Committee and Steering Committee), all members of the team that worked on the Torres, et al. (2000) study, never suggested the use of or adoption of any part of that work. Also, Dr. Norm Abrahamson (consultant, member of the DRMS Technical Advisory Committee and Steering Committee) who also worked on the Torres, et al. (2000) study and who performed all of the risk calculations for that effort, did not suggest we use that work.*

*The work of Mount and Twiss (2005), while interesting, is not a risk analysis, nor is it a detailed assessment of any of the issues/topics we are addressing in DRMS. This work looks to bring to the readers' attention enough information to make the case that the Delta is at risk. As a result, there is nothing in this work we can make use of. We also not that Professor Twiss, a member of the DRMS Steering Committee, never suggested there were elements of his paper with Professor Mount that should be used in any part of DRMS.*

*We believe the Lund, et al. (2007) work that is cited (full reference not provided) is the PPIC report. This work was published after the work for the TMs, the input to the risk analysis, were completed. In addition, it too is not a risk analysis.*

*All of this said, as we mentioned in a response in Section 3 we did not ask the authors of the TMs or report sections to provide an exhaustive review of past work on all subjects related to the DRMS analysis and to provide a discussion of what was relevant/irrelevant, useful or of no use, etc. We did not have the luxury of time to carry out this sort of an effort (such as might be undertaken by a graduate student working on, and eventually writing a thesis). The report should document what existing information and data were used. Where this was not done, it will be corrected.*

*We also note that DWR and our Steering Committee, agencies and/or individuals who are well aware of the work that has been done with regard to the Delta and the analysis of risks did not suggest that any of the references noted have any direct relevance to or should be used as part of the DRMS effort.*

It is also not clear why the authors did not just use available information (as charged).

*Indeed we did use available information. Presumably the suggestion here is that we might have used some of the studies identified above, which as we point out are not relevant to this work. We do note however, there are a number of cases where we did in fact gather new information. In these cases we spoke to and got the approval of the DWR project manager. Examples where this was the case included the collection of thousands of boring logs from a number of different sources, taking field measurements to update subsidence estimates, and the gathering of proprietary geophysical data which expanded our geosciences knowledge base as part of the seismic source characterization effort.*

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 USGS ground motion maps are of no use in modeling a spatially distributed system such as the Delta. These maps are a collection of individual site probabilistic seismic hazard results. The ground motions at these sites are computed on the basis that motions from the earthquakes that are modeled (in the integration process) are independent from site-to-site. If these maps were used, one would be ignoring the inter-event and intra-event ground motion dependencies that should be modeled in regional risk analyses (Bazzuro and Baker, 2006). That is, the maps provide no assessment of the joint probability of ground motions at different levees from the same seismic event. A failure to consider these correlations leads to an unconservative estimate of the risk.*

*We did use the USGS seismic source model for the major Bay Area faults (see the Seismic Hazard TM for more discussion of the seismic source characterization model).*

*It should also be noted the suggestion that the USGS and CGS have done decades of work in the Delta proper that would provide input to the seismic hazard model is erroneous.*

*Lastly, the USGS or the CGS did not suggest we use their ground maps for the DRMS effort.*

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.

*It is not clear what statement around page 4 is being referenced. We do not believe we say the event approach is the only way to perform the analysis and we do not intend to imply that an event-based approach is the only way the analysis can be done. It is the approach that we have taken since it offers an effective way to model the dependencies in the sequence of events and the consequences that result.*

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.

*We agree risk can be calculated as indicated. We did not use this particular approach. Our quantification of risk is presented equation 4-1.*

*In our revision of the report we will present a more comprehensive discussion of the risk methodology and its implementation.*

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 earlier suggestion, while interesting, is inconsistent with the Business-as-Usual approach that guided the Phase 1 analysis.

*In the expanded explanation of the methodology and its implementation, we will present a simple example.*

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 definition of vulnerability classes is provided in Section 7. We do not assume that the entire levee section breaches if it is in the same vulnerability class. Our assumption is that a*

*breach may occur somewhere within a levee reach that belongs to a vulnerability class and the probability of such a breach varies as a function of the vulnerability class.*

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

*Additional hydrologic studies will be considered as part of the additional work we are conducting. These results will be reported in Section 13.*

## 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?"

*What is provided is a simple list of events that put the Delta at risk. This is part of discussion to point out that DRMS does not address all events that put people, property and the environment at risk in the Delta. We will re-word the sentence as follows: "A partial list of events that put the Delta at risk includes:".*

*Among others might include vandalism, terrorist strikes, tsunami, upstream dam failures, meteor strikes, etc. We will add these as "for example".*

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."

*We appreciate the suggestion and will consider it in our revision of this section.*

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

*The performance of levees in different vulnerability classes are assumed to be (conditionally) independent, given the ground motion that occurs as a result of a seismic event or the flood stage from a flood event.*

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

*In the next revision of the report we will document the hydrodynamic calculations that are the basis for this statement.*

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.

*Single failures do not dominate the risk. Although such failures would be more frequent, their consequences are many times to orders of magnitude lower than a simultaneous failure of multiple islands. Hence their risk contribution is insignificant. The suggestion that single failures could well dominate the risk, fails to recognize the historic and prevailing flood experience with regard to levee failures and certainly ignores the potential for multiple, seismically initiated failures.*

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

*The time of year was considered in the flood hazard analysis (implicitly), in the wind analysis, and in the evaluation of the hydrodynamic response of the Delta to levee failures, in the aquatics impact analysis, and in the economic 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.

*In the last sentence in the first paragraph under Section 4.3 (and in other similar places), we will use "frequency of occurrence" in place of "probability or rate of occurrence".*

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."
We believe we have been consistent in our definition of risk, our definition of uncertainty, and our implementation of them. Our definition of risk includes two elements: likelihood (chance, uncertainty) and consequence. Thus, uncertainty is a component of risk. Note, we do not combine uncertainty and consequence, the ultimate blurring, by computing and presenting risk as an expected value. Rather, we make a clear distinction between uncertainty and consequence.

As the reviewer notes, a probability distribution of outcomes (fatalities) is a measure of risk. We would say this probability distribution captures the aleatory variability in the number of fatalities. There is also epistemic uncertainty in the estimate of this probability distribution since there is epistemic uncertainty in the estimate of the number of fatalities (as might be produced by different, credible models) and there is epistemic uncertainty in the estimate of the probabilities of different numbers of fatalities (even if the fatality models were not uncertainty).

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.

*We believe the definition on page 4-8 is correct. We are unclear as to which sentence on page 4-9 is referenced. Is it equation 4-1?*

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.

*We disagree with the sentiment/views expressed in this comment.*

*Making a distinction between the different types of uncertainty is an important part of a risk analysis. The argument as to what is aleatory and what is epistemic uncertainty has been ongoing. We simply recognize the difficulties (see the debates of Bohr and Einstein for instance).*

*The assertion that we do not use this concept is incorrect.*

*We model and propagate aleatory and epistemic uncertainties in the seismic, flood, and wind hazard analyses. We also use them in the seismic fragility analysis and the sunny-day levee failures analysis. Further, we propagate these uncertainties through the estimate of the frequency of island flooding. As stated in the report, we were not able to implement it in the consequence parts of the risk analysis.*

*The suggestion that we have in some ad hoc manner introduced jargon in this work is incorrect and the terms we used have been in common use in the probabilistic risk analysis vernacular.*

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.

*We will add a description and an example to the text describing what an event-based approach means.*

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.

*We will address this editorial suggestion in the next revision of this section.*

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?

*This comment is unclear. For example, the statement is made, "events with impacts below a certain threshold are included in the summation." The idea of the type threshold being implied would seem to indicate that impacts below the threshold* ***would not*** *be included in the summation.*

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.

*In the analysis of systems that are exposed to natural phenomena such as earthquakes, floods, intrinsic events (normal loading), etc., stationarity is commonly assumed and/or demonstrated to be applicable. That is, the frequency of occurrence of events is constant over time. Further, events are often assumed to be Poissonian. As a result, given an estimate of the frequency of occurrence one can make probability statements regarding the events being modeled for a specified future time period. This paragraph is making the straightforward point that we cannot do that in DRMS because the frequency of occurrence of the events we are modeling, over the time periods that we are analyzing, is changing. Therefore for 2005, we make an estimate of the frequency of occurrence of events of interest. This estimate is based on the information and conditions at and up to that time (i.e., no major earthquake has occurred (which would change the frequency of earthquake occurrences), given the current condition of the Delta levees, etc.). We refer to this frequency as an instantaneous frequency occurrence (in 2005). In 2006, 2007, 2008, etc. we could update the model parameters and re-run the analysis and get a new "instantaneous" estimate of the frequency of occurrence of events of interest. Of course in DRMS we are not doing this, we are making the estimates at 2005, 2050, etc.*

*The estimates we are making in the individual years we refer to as instantaneous frequencies since they are estimated for a given year, for the conditions at and up to that time. We point out the limitations of this estimate in making probability statements for a limited period of time.*

*In Section 14 when we do consider the change in risk over the time period of interest, we estimate the adjustment, the change in the frequency of the hazards, and the frequency of levee failure.*

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…"

*We appreciate the suggestion and will consider it our revision of this section.*

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.

*The statement is correct; all of the factors noted, soil conditions, and ground motion travel path are considered in the analysis. The point we were trying to make is the following; for a spatially distributed system, the ground motion correlation (given the other factors mentioned) due to distance (and the inter-event variability of earthquake events of the same magnitude) also needs to be modeled, which is not the case for 'point' systems.*

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

*We will correct this typo in the next revision of this section.*

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.
*We do have information on the correlation of earthquake ground motions over relatively short distances (see Bazzuro and Baker, 2006). There is considerable uncertainty in the estimate of earthquake ground motions, which we model in the analysis. Note, the ground motion modeling is one area were we have a tremendous amount of data and modeling experience. Thus, the models are empirical. This said, there still remains a considerable amount of aleatory and epistemic uncertainty in the estimate of ground motions.*
*The statement that we are using a decision-tree structure is not correct. We are using an event tree approach. Figure 4-4 is an event tree as the caption notes.*

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?

*We did not analyze the sequence of events described. When a group of islands is flooded and others are damaged, the repair priorities are set such that damaged, non-flooded islands are given the highest priority. These islands are stabilized first. It was assumed these efforts would limit the vulnerability of these islands to other events that could cause damage.*

*Wind waves that can erode the interior of flooded islands are modeled.*

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."

*We agree with this comment, this paragraph will be re-written to better describe the perspective that we are trying to present.*

*We will make the editorial corrections noted.*

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.
*The discussion as to how these events are modeled will be expanded.*

Page 4-13: Fourth line from top, delete "they" between "have" and "been".
*We will make this correction.*

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.

*There is no statement or implication that a probabilistic assessment of seismic hazard is easier than it is in the areas of economics and ecosystems. The statement is straightforward in stating there are "different levels of probabilistic modeling experience in different topical areas."*

*Probabilistic modeling has been done in the fields of economics and ecosystems. We would certainly agree it can be done. As noted by one commenter in our meeting with the IRP in March, the uncertainties in the ecosystem area are difficult to estimate and potentially so large their assessment renders the results useless (our paraphrase of that comment). This sentiment does not seem inconsistent with our statement or our experience in dealing with our TAC and team experts in the ecosystem area.*

*In the economics area we had a similar experience with our team members and in separate discussions with two economics professors from U.C. Berkeley. When addressing the subject of probabilistic modeling and in particular modeling epistemic uncertainties, the response from the U.C. professors varied from "not really doable" to "such assessments can be done." In neither case was there an expression that such assessments are within the normative practice of the profession or academia for that matter.*

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.

*We will expand on the different levels of maturity of the sciences with respect to conducting probabilistic modeling.*

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

*We will add a footnote or glossary to the document to define terms that are used.*

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

*The purpose of the risk analysis is to estimate the frequency of occurrence of events of interest (levee failures, island flooding, economic consequences, etc.). Based on current information we can make such an estimate. As part of the DRMS Phase 1 analysis we have also been asked to estimate the risk as it might change in the future, accounting for sea-level risk, the increased frequency of earthquake occurrences, etc. If we look ahead to 2100, we do not know what will occur in the intervening period. For instance, if a major seismic event occurs, it will relieve the strain build up on the causative fault, reducing the frequency of future events. If this event fails a number of islands, how will the island owners and the state respond; will some be abandoned and if so which ones? These "random futures" could not be modeled in this work.*

*As an alternative to modeling the random state of the Delta and the occurrence of future hazards and consequences that might be realized, we adopted the following approach:*

- *The configuration of the Delta will not change in the future with respect to the number of islands (no islands are abandoned). Note, their configuration does change due to subsidence.*
- *A major event does not occur that would initiate changes to the configuration of the Delta such as abandonment of some islands.*
- *A major seismic event does not occur which would change the strain accumulation on causative faults, thus changing the frequency of occurrence of future seismic events.*

*So, in 2050 our models will estimate the frequency of future earthquake ground motions, assuming in the intervening years (2005-2050) a major seismic event has not occurred (thus allowing us to use the current USGS model), and the potential for levee failures from these ground motions (accounting for subsidence and increased hydraulic head due to sea level risk) for all islands as used in the model for the present Delta.*

*We summarized the above by simply saying, "Assume that no major event (hazard or proactive policy) occurs in the intervening years that would result in a significant change to the integrity or configuration of the Delta system."*

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.

*As indicated in our previous response, we will be expanding the documentation of the risk analysis methodology.*

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

*The word total here refers to the sum of the In-Delta and Statewide costs that are estimated.*

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

*National costs were not included in the DRMS scope of work.*

Page 4-18. Table 4-2: Was loss of life included? Suggest they flag this table with an "*" or footnote.
*Loss of life was not explicitly evaluated in the analysis. However, the population-at-risk from island flooding scenarios was evaluated.*

*Table 4-2 refers only to economic risk metrics; therefore, public health and safety risks were not included in this table.*

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

*The deer densities are very low in the Delta even though there is some deer habitat in the Delta even though the range maps show the area to be devoid of deer. Tule elk are also found in Suisun Marsh, including a herd on Grizzly Island and are known to cross the Montezuma Slough and Suisun Slough to the east and west of the wildlife area.*

*Both deer and tule elk are non-listed species which are regulated for sport harvest by the California Department of Fish and Game. Neither deer nor tule elk were included in the ecological risk assessment. Species examined in the risk assessment were selected to obtain a manageable number of species/species groups, while representing the range of the types of possible consequences on wildlife that could be associated with levee failures. Species selection was conducted through the following screening criteria described in the Ecosystem Consequences TM.*

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?

*The hazards (seismic, flood, wind and sunny-day) and the performance of the levees were considered probabilistic in the analysis. In addition, the possible hydrologic conditions that might exist at the time of a levee failure were also considered probabilistically in the analysis.*

*The use of the word "probabilistic" solely in regards to the seismic hazard is misleading and will be corrected.*

*Best estimate (non-probabilistic) assessments of the consequences of island flooding and water export disruption (economic, ecosystem) were made in this analysis.*

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?
*We will provide the definitions of failure and sequence and the frequency of failure and frequency of sequence in the text.*

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

*We appreciate the suggestion and will consider it in our revision of this section.*

*We note the figure is essentially the same for flood events, with the exception there is typically limited non-breach damage; therefore, this box would be eliminated from the figure. For sunny-day events, the figure would be simpler still.*

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?

*In the Phase 1 results evaluated to date, the epistemic uncertainties that have been propagated through to the final results are the uncertainty in the hazard (e.g., frequency of earthquake ground motions) and the levee fragility. Only best estimates were made of the consequences (economic, ecosystem).*

*Note, the use of the term "error bars" is incorrect. This is a term typically used in the context of statistical studies. The dashed lines represent the quantification of the epistemic uncertainty in the result (at a certain probability level).*

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?

*The "blob" has two parts; one is a blue blob corresponding to a flooded island. The second part is a brown blob corresponding to an intact island.*

*We will revise this figure so the color coding does not result in the blob appearance in non-color printed copies.*

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?

*The DRMS risk analysis does model hazards as Poisson events (earthquakes, floods, and winds). However, it is recognized these hazards are not stationary Poisson events since the rate of occurrence will change over time. Therefore, we estimate the "instantaneous" frequency of events (see the response to comments on page 4-9).*

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.

*As labeled, the figure shows an event tree, not a decision tree. The Phase 1 of DRMS is intended to analyze risks, thus there are no "decision nodes" being evaluated.*

*The figure is intended to provide a "schematic illustration of an event tree" as indicated on page 4-10. The figure is used to illustrate the type of events that must be considered to evaluate risk. As an illustration, the event tree does show the links (branches) connecting the states of nature, events, response variables, outcomes. It does not show the branch probabilities as suggested.*

*Event tree analysis is a standard modeling technique used in the risk analysis of systems (see for example the following books; Baecher and Christian (2003); Hartford and Baecher (2004); Ericson (2005). Further, it is quite amenable to standardized event tree software or coding in a spreadsheet (ETA, by Item Software; Sapphire, by INEL; Relex Reliability Studio; ETA by SAIC).*

## *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 purpose of this section is to give a sense of what is at risk due to levee breaches and island flooding in the Delta. We do not intend to provide a complete Delta overview, nor a detailed inventory in this section, but we do want to provide summary information about the assets in the Delta and also activities outside the Delta that may be impacted by levee breaches. Because of the DRMS work, the Delta Vision process asked URS to produce the Delta Status and Trends report. The Status and Trends report is referenced by the Phase 1 Report and many of the other Delta inventories, overviews, summaries, and assessments are thereby incorporated into the Phase 1 Report. Readers who want extensive detail need to refer to this source. This will be explicitly stated in the revised introduction to this section.*

The authors present nothing on the "State," so it is not clear why that is in the title.

*We disagree with this review comment because it is untrue. The fourth and fifth paragraphs of Section 5.1 (Population) specifically discuss the relevance of the Delta to people outside its boundaries who depend on it for their water supplies. These paragraphs cite the importance of these water supplies to the state's economy and to practically all of the state's 37 million people. Furthermore, the following comments accurately reflect the reason that "the State" is in the title. A legitimate criticism has been overstated.*

*We do agree that the relevance of the Delta to the rest of the state needs to be better summarized. We need to say much more about the state's dependence on the Delta and what is at risk in the event of levee failures, especially related to the agricultural and general economies that depend on Delta water exports. We will further develop that aspect of the section in the next version of the Phase 1 Report. There is undoubtedly opportunity to take advantage of other Delta summaries that are available as we revise this section. We will do so and provide some direct citations even when they have already been summarized in the Status and Trends report, which we consider to be our comprehensive reference.*

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 detailed island-by-island inventory that is suggested has been created by DRMS, documented in the TM addressing Delta infrastructure and is used in the risk analysis. We note that much of this information was pulled together and put into a GIS system as part of the DRMS work. It did not exist previously in any unified accessible way. Much of the detail was pulled directly from the infrastructure TM. It will be summarized in the next version of the Phase 1 Report.*

*In the TM, infrastructure assets were itemized by island in tabular format (in "Excel" spreadsheets) for lookup in the context of the risk analysis. Infrastructure outside the Delta was considered within the economic consequences module where regional and statewide impacts were assessed. Infrastructure assets within the 100-year flood limits were considered for direct flooding impacts in Delta levee breach events.*

*The comments that are offered here are helpful and will guide us in updating this section. We agree that more work is required to summarize the detailed information and put it in perspective so a reader can discern its relevance to impacts from various scales of levee breach events. The details will be left in other documents, especially in the TM.*

*We note that another suggestion was made to combine this section with Sections 1 and 2. At the same time, this comment calls for greater detail. We find the suggestions to be ad hoc and somewhat random and they do not appear to be internally consistent.*

*We also note, other reviewers have suggested combining Sections 1 through 4.*

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.

*The information discussed and represented in the maps is used directly in the Delta Infrastructure part of the DRMS risk analysis – specifically in the assessment of costs and impacts when islands are flooded (see the Delta Infrastructure TM and Section 12.2.2).*

*The issue of availability of this data online was not a part of the DRMS scope. However, the database and the GIS layers will ultimately become available when they are turned over to DWR.*

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.

*No specific delineation of critical and non-critical infrastructure was made.*

*We are not clear what is meant by "response module." The infrastructure data were considered in the Delta infrastructure damage module and in the assessment of costs and impacts when islands are flooded. It was assumed that all infrastructure would be repaired to pre-flood status and the costs and schedules for repairs (including loss of use) were estimated, given protracted flooded conditions, resultant delays, and competition for resources. These analyses were performed separately from the Emergency Response & Repair (ER&R) Module (which focuses on levee repairs and marine resource constraints). However, the economic consequences due to infrastructure damage do consider the levee repair and dewatering times calculated by the ER&R module.*

*An aging analysis was not conducted for any of the assets in the Delta; it was beyond our scope. Criticality of infrastructure was implicitly considered in the assessments of loss of use and repair costs.*

## Specific Comments:

Page 5-3. First paragraph under Section 5.5: Defined *(sic)* "infrastructure assets."

*We will define this term or revise the wording in the report to explain our working scope for the term.*

Page 5-7: Spell out the acronym MHHW.

*The reader can refer to the list of Acronyms and Abbreviations provided at the beginning of the report. We will verify that the term was spelled out in its first use in the report – which is likely to be in an earlier section. After initially spelling out an acronym we use only the acronym.*

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

*This comment is not entirely clear, however we believe the suggestion is that we indicate that the asset values which are shown do not include loss-of-life costs. Assuming this is the case, we will make note of this in the next revision of this section.*

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"?

*The length and width of the scour zones are not major contributors to overall risk, but the location of the scour can be a significant part of loss-of-use and repair cost estimates, depending on the location of a specific breach. The scour zone for an island is defined as a perimeter band that is 2000 feet wide from the center of the levee. The scour limit uses the same 2000-foot distance from the levee centerline. However, scour limit is usually used in analyzing a specific levee breach. In such a case the scour limit is the edge of the scour zone; i.e., 2000 feet landward of the levee (perpendicular to the island perimeter/levee), 500 feet wide (parallel to the island perimeter/levee), and 50 feet deep. These dimensions are based on historical scour events.*

*We will review this section to ensure these terms are adequately defined and used properly.*

Page 5-8. Fourth paragraph: Define and describe the "GIS data."
*This statement refers to the GIS database that was compiled by DRMS from a number of sources, which was used to estimate the Delta infrastructure losses in the risk analysis. The GIS data includes attributes or characteristics of the infrastructure assets (which, in some cases, are missing). Attributes include pipeline diameters, number of stories of buildings, number of tanks in a tank farm, etc. These attributes are needed to develop replacement cost estimates for the various assets that may be damaged by flooding or scour. The initial GIS database and its augmentation with data from other sources is described in more detail in the infrastructure TM.*

Figure 5-1: Showing Frank's Tract as "Conservation Lands" instead of "Water" is confusing.

*Noted.*

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

*No response required.*

## 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?

*The USGS ground motion maps are of no use in modeling a spatially distributed system such as the Delta. The USGS and CGS maps are a collection of individual site probabilistic seismic hazard results. The ground motions at these sites are computed on the basis that motions from the earthquakes that are modeled (in the integration process) are independent from site-to-site. If these maps were used, one would be ignoring the inter-event and intra-event ground motion dependencies that are important to model in regional risk analyses (Bazzuro and Baker, 2006) and would be making an unconservative estimate of the seismic risk.*

*Lastly, the USGS model was not based on the most recent attenuation relationships. As such it is out of date.*

*We did use the USGS seismic source model for the major Bay Area faults (see the Seismic Hazard TM for more discussion of the seismic source characterization model).*

*We note, the USGS and the CGS did not suggest we use their ground maps for the DRMS effort.*

*The work by Torres, et al. (2000) could not be used in DRMS since all aspects of that analysis, the seismic hazard model and, the fragility analysis are out of date. In addition, neither Professor Ray Seed (member of the DRMS Technical Advisory Committee and Steering Committee), Dr. Les Harder (Deputy Director of DWR), and Mr. Gilbert Cosio (consultant and member of the DRSM Technical Advisory Committee and Steering Committee) all members of the team that worked on the Torres, et al. (2000) study, never suggested the use of or adoption of any part of that work. Also, Dr. Norm Abrahamson (consultant, member of the DRMS Technical Advisory Committee and Steering Committee) who also worked on the Torres, et al. (2000) study and who performed all of the risk calculations for that effort did not suggest we use that work.*

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)

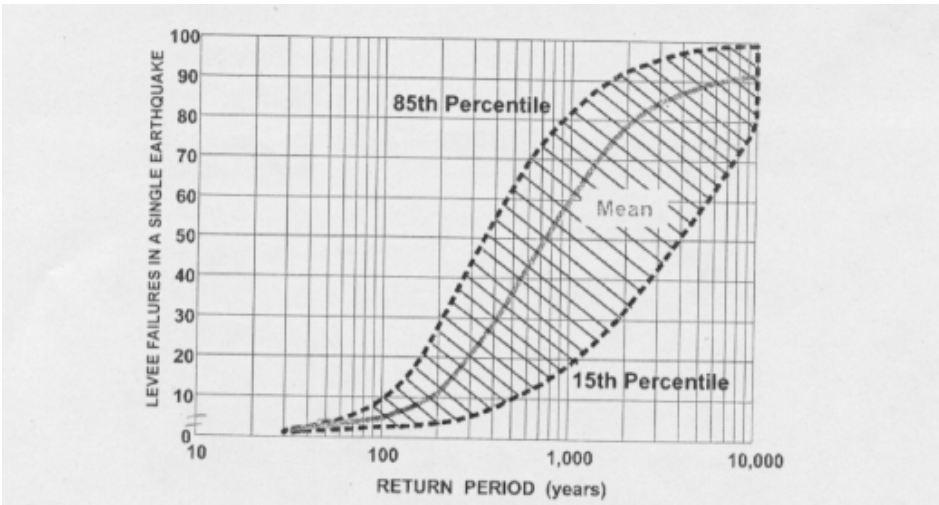


Figure 5-2: Number of Levee Failures in a Single Earthquake-Fault Models Combined
Note: Number of Levee Failures does not equate to Number of Islands Flooded

**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.

*The seismic sources in the Bay Area are updated regularly, as well as the attenuation relationships. For this study we used the most recent updates for both the seismic sources and the new attenuation relationships (NGA). This is customarily done for any PSHA*

*work in the area. The studies you cite are not current for use in this region and this project.*

*Comparisons are being made to other similar studies in the region and the results of the comparison will be added to the risk report.*

*The study cited above (Torres, et al. 2000) is being used in the comparisons we are conducting There are, however, differences both in the probabilistic ground motions (although small) and the way the Delta levee vulnerability was carried out. The Torres (2000) study groups the Delta levees in four regional groups while the DRMS (2007) defines vulnerability classes for each island and for each reach within each island. The Torres (2000) study calculates the number of breaches (with possible multiple breaches in one island) while the DRMS (2007) calculates the probability of an island being flooded (taking into account the possibility of multiple levee breaches on an island). Furthermore, the DRMS (2007) study includes the Suisun Marsh levees (more fragile) while the Torres (2000) study does not. Therefore a direct comparison with the chart shown below is not possible.*

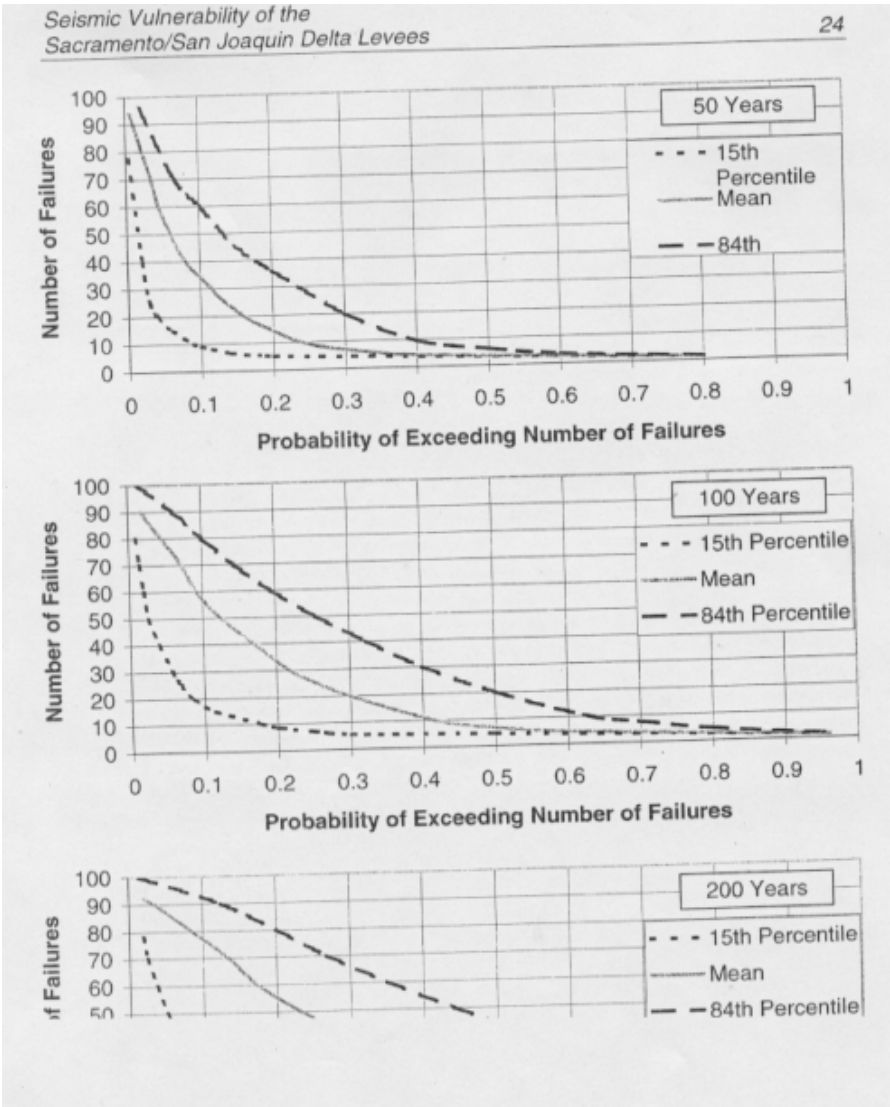


**Figure 2**

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.

*We made sure that any computer model or other work used in our analysis is cited. We are currently revising the report and will add any references that should be cited.*

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.

*We explained the difference between the DRMS (2007) and the Torres et al. (2000) study in a preceding response. In addition, the DRMS exhaustively used more than 2000 borings and cone penetration soundings to characterize the Delta and Suisun Marsh levee and foundation conditions to obtain a mesh (geographic discretization) that is small enough to be able to represent the variation of levee fragilities within each island. In the Torres et al. study (2000) the Delta was divided into four sub-regions. The description of the classes is included in the levee vulnerability TM.*

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.
*Sections 9 and 13 address the sunny-day levee failures and their risk.*

**Specific Comments:**

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

*The text will be revised to be consistent with the definition of risk given in Section 4.*

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

*The second paragraph, Page 6-1 spells out the acronym WGGEP as Working Group on California Earthquake Probabilities.*

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.

*The reference ground motions were developed for an outcrop of stiff soil. The dynamic site response analyses of levees were part of the levee vulnerability where the dynamic response of soft soils was explicitly considered in the 2-D finite element analyses.*

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.

*The downhole seismic survey data will be added in the revised report (we did not have permission from the authors to publish that data at the time).*

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.

*As a matter of standard practice in probabilistic seismic hazard analysis, a stationary Poisson model (time-independent) model is used to estimate the occurrence of earthquakes. It is unique to be able to model faults with a time-dependent model.*

*The time-dependent data are only available for the seven major San Francisco Bay Area faults and thus time-dependent hazard can only be calculated for these faults. The other faults in the region lack such data and therefore only time-independent hazard can be calculated.*

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

*We will add a list of the experts and their qualifications.*

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

*The text will be revised.*

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

*Same as time-dependent probability. We will change the wording.*

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.

*The text will be revised.*

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.

*The text will be revised.*

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 below the softer foundation soils that are below most of the levees.

*Yes. This refers to the average shear wave velocity within the upper 30 m (100 ft) of "the" stiff reference site conditions for which the ground motions are calculated as an outcropping site. This site is indeed below the foundation peat and the loose sand deposits.*

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?

*The 1.0 sec spectral acceleration results were shown to indicate the long-period hazard in the Delta. The characterization of the Delta faults has been updated since 1998. This characterization was performed by Dr. Jeff Unruh who also did the evaluation in the 1998 (Torres, et al. 2000) report for Lettis and Associates. The uncertainty in the seismic source characterization for the Delta area sources was considered by assigning a probability of activity that was not equal to 1.0. That is, there is a non-zero probability these sources are non active. The text will be revised to make the presentation of this information clear.*

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

*This figure will be revised.*

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…"

*The text will be revised.*

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

*The text will be revised.*

Page 6- 6: We suggest for consistency you change overtopping to overtop

or breach to breaching - either is ok.

*The text will be revised.*

Page 6-7. Last paragraph: The statement that "The Levee Vulnerability team believes that levees that have granular materials with (N1)60-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-cs values less than 15. Therefore, this statement is very significant. It warrants discussion for the following reasons:

· If (N1)60-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-cs for sites where (N1)60-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-cs less than 15 would liquefy at a PGA of 0.05g.

*Additional analyses and details, explicitly showing the characterization and representation of the uncertainties of all random variables considered in the development of the levee fragilities are being prepared for inclusion in the revised risk report. Some sensitivities analyses are also being carried out. The results of these evaluations will be presented in the revision of the report.*

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.

*We will call them comparison runs.*

Page 6-8. Section 6.2.3, 2nd paragraph: Authors pick a liquefaction threshold value of (N1)60-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?

*As indicated above, additional analyses are being carried out to model the uncertainties around the (N1)60. Ground motions, residual strengths, CSRs, and the liquefaction potential have been added to the analysis of the levee fragilities.*

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?

*Additional explanation of these variables will be added to the report.*

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.

*The analyses that were performed show the island side moves more than the waterside during an earthquake. The crest settlement depends on the movement of both sides of the levee. The text in the report will be revised to clarify this point.*

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

*These two graphics will be included in the final report.*

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

*We will correct this.*

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)?

*In your example both cases have the same ratio of 50%; however, the case where there is 2.5 feet of deformation may indicate more serious damage than the case with 0.5 feet of deformation. Using the absolute deformation was tried in the Torres (2000) studies and was found insufficient to represent the damaged state appropriately, by members of that team who also served in the DRMS team (Prof. Ray Seed, Dr. Les Harder, Mr. Michael Ramsbotham (USACE) and Mr. Gilbert Cosio (MBK)). This approach was adopted as a refinement from the absolute deformation used in the past to keep track of the deformation with respect to initial freeboard and the likely breaching of the island.*

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.

*Concur.*

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?

*Contiguous zones were defined based on the variables that were used to define the vulnerability classes. These variables were: waterside levee slope, (N1-60) Fill, (N1-60) Foundation, and peat thickness. Each 1,000-foot reach of a levee was assigned to one and only one vulnerability class based on the categories of these variables. All contiguous reaches that were in the same vulnerability class were combined to define a contiguous spatial zone. To show these contiguous zones for all islands would result in a cluttered figure, which, we believe, may not be very helpful.*

*The judgment that behavior of contiguous levee sections with similar geotechnical properties in an earthquake is likely to be similar is based on the damage patterns observed in past earthquakes. In the Kobe (Japan) earthquake, for example, several miles of contiguous levee reaches (that were presumably weak) were damaged/slumped, while intervening reaches (that were presumably stronger) survived without significant damage. Four photographs substantiating extensive damage of levee failures during past earthquakes were presented in Figures 6-27 to 6-31.*

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

*Concur.*

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.

*Concur.*

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.

*The size of the contiguous zones is relatively large (several thousands of feet) and consequently, the average number of contiguous zones per island is relatively small*

*(about 3-12 is a general range). Therefore, we do not believe that the number of "independent" components per island is 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")?

*The number of independent contiguous spatial zones varies by individual islands. The discussion in this section is generic; it does not assume a specific number of zones. The discussion is meant to suggest that if the breach rate on a particular island is m/n (i.e., m zones are breached on the average out of n zones on an island in a given event), the damage rate is likely to be of the order of 2m/n (i.e., 2m zones out of n zones would be damaged on the average in the same event).*

*To simplify the discussion in this section, we propose to revise this section as follows: As stated above, when levees are damaged during an earthquake, the extent of damage spans a long distance, typically several miles. In the Kobe earthquake, for example, the length of slumped/damaged levees at various locations was 5 to 10 miles. An actual breach may occur at some location within a particular damaged zone. If an average levee contiguous spatial zone is assumed to be about 4 miles long, an event that causes a breach of one zone is likely to damage on the average about two spatial zones. Based on this assessment, the probability of damage on a given spatial zone was assumed to be twice the probability of a breach on the zone.*

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

*Concur.*

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

*Time-dependent hazard will always increase with time until the seismic sources controlling the hazard produce large earthquakes. This is simply the result of the elastic rebound theory, where strain accumulates with time on a fault until it releases that strain through a large earthquake. We will expand the discussion in the text.*

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

*Concur.*

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

*The figure looks fine in our report.*

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

*We will add labels on the axis.*

## *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:

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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.

*An unnumbered table summarizing climate change assumptions has been added to Section 6.1 of the Flood Hazard Technical Memorandum (TM). More detail is presented in the Climate Change TM.*

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 method was not intended for more detailed studies, but was designed for use in the DRMS risk analysis, where thousands of different simulations were conducted. Thus, the method needed to be simple and easily implementable.*

*The intention of the analysis was not to capture short-term or transient effects. The intention was to provide a reasonable estimate of the peak stage in the Delta for each of the scenarios simulated in the risk analysis. Hourly stage and tidal data were used in the analysis.*

- 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 method was meant to be simple enough to be implementable in real time for thousands of potential simulations. An analysis of the stage data collected in the Delta indicate that stage could be estimated with reasonable accuracy for purposes of the risk analysis. The analysis incorporates Yolo Bypass diversions. Operation of Delta Cross Channel is, in general, constant during the wet season*.

*None of the upstream facilities is explicitly included. They are, however, implicitly included in our approach of using the historic Delta streams inflow. The contributions of all the upstream facilities are reflected in the downstream flows. We need to stress that an important aspect of selecting this approach is that we never planned to perform a comprehensive analysis of the storms-watersheds-reservoirs-stream channel dynamics-levees along the streams etc. comprehensively all the way into the Delta. This work would be out of the scope of this risk study, and would require, in our estimation, 10 years or more to complete. Currently the USACE is working on this project deterministically and for today's condition. Think about the additional efforts required to capture the flow regimes and stage frequencies in probabilistic terms and do it again three more times for 2050, 2100, and 2200.*

- The procedures do not provide adequate hydrographs required for unsteady and multidimensional flow analyses and interior flood analyses with respect to the Delta.

*The analysis in the Flood Hazard TM was not intended for transient or multidimensional analysis. See the Water Analysis Module (WAM) TM for details on the modeling.*

- The results presented are not accurate enough for the sizing and designing of Corps levees, or for FEMA levee certification analysis.

*The flood hazard modeling was not intended for design purposes; it was only designed to provide input to the risk analysis. FEMA certification requires protection against a specific event at a specific location, not a specific inflow into the Delta.*

*It was never the intent for this study to support any design and we recommend it not be used for design. This is a risk study to assess the vulnerabilities of the system and estimate their probability of failure and the consequences of these failures.*

- 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.

*The Flood Hazard TM has been updated to provide a more accurate description of the procedure followed. Although future reservoir operations may be different than they are today, the purpose of the flood hazard analysis was not to analyze reservoir operations, but to estimate how the flood frequency curve may change in the future. It would be speculative to try and operate the reservoirs under future, uncertain conditions and would be unlikely to provide a better, more certain estimate of the future flood frequency needed for the Risk Analysis inputs.*

*We agree with the first point raised, we do not explicitly include reservoir operation for the reasons cited in the previous response on modeling upstream facilities.*

*We do not iterate the flood model for each flood event analyzed. We have rather used the first results from the flood model (frequencies and associated stages) and calculated the probability of levee failure. After the levees breach, then we use the WAM model to track the reservoir releases (CALSIM model) and the hydrodynamic changes (RMA model) in the Delta post- event and during repair.*

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 20$^{th}$ century records. If there is some reason for limiting the flow analysis to this shorter record, the authors need to explain why.

*The 50 years of data used in this analysis were selected because the data were readily available for all major delta inflows.*

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 1: Watershed effects, Kondolf.**

**Figure 2:** 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 text will be modified to better reflect the intention of the analysis of reservoir effects of flood flows into the Delta. During the 50 years of data used in the analysis several reservoirs were constructed on the Sacramento and San Joaquin river systems. If construction of the reservoirs had a significant effect on flood flows into the Delta it would not be possible to use the entire 50-year record. In that case we could only use that portion of the record that occurred after construction of the last significant storage project. This would eliminate about half the data. The intention of the analysis is to show that the entire data set could be used as is, without adjustment. The text will be modified to remove the statements that the reservoirs do not provide flood control benefits as that was not the intention.*

*The modified section of the TM now describes the statistical differences between pre and post- dam construction flows in the Sacramento and San Joaquin rivers. Results of an Anova analysis between the pre- and post- dam eras have been added to the report. The analysis indicated that at the 5% significance level there is no statistical difference between the pre- and post- dam construction peak annual flows. A figure comparing the temporal distribution of the largest events on record was also added, providing additional verification that the general nature of the flood flows into the Delta has not obviously changed over the 50-year period of record.*

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[…]"

*The discussion in Section 2 on the effect of reservoirs on flood flows into the Delta was used to decide if all 50 years of available data could be used in the analysis or if only data collected after construction of New Melones could be used. Before the analysis it was hypothesized that the reservoirs would decrease flood flows into the Delta and therefore there would be a noticeable decrease in the size of inflows into the Delta after construction of the reservoirs. As described in Section 2, that did not seem to be the case, so it was decided that all 50 years of data could be used in generating the frequency distribution of flows into the Delta.*

> 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.

*The analysis is simple yet it does indicate that the reservoirs have not had the effect on Delta inflows that might be expected. The purpose of the analysis is not to determine the level of impact of reservoir operations on flows in the tributaries to the Delta but determine if the use of 50 years of data that encompasses an era of dam building is reasonable. The analysis indicates that the use of the 50-year data record is reasonable for the purpose of the Risk Analysis.*

> 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.

*We disagree with this comment. It is not unreasonable to anticipate that the construction of reservoirs will reduce peak flood flows downstream of the reservoirs. That is often why they are built.*

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.

*It is possible to look back at the data and determine what the available storage was for a given historic flood event. It is not possible to look forward and predict what storage will be available for an unknown future event. It may also be true that nearly every river in California is separated from its floodplain by levees. But it is during the large flood*

*events that levees fail and floodplain storage becomes available. In many cases it is not the size of the storm above the reservoirs that determines the size of inflows into the Delta, but the capacity of the channels feeding the Delta to convey that flow to the Delta. The larger the storm the more likely levees will fail somewhere in the system and reduce the flows into the Delta. However, as we said, the intent of the analysis was not to describe the flood control capabilities of the reservoir system in California but to determine if it was possible to use the entire 50-year dataset.*

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.

*The 200,000 cfs cutoff was reduced to 80,000 cfs for purposes of calculating the distribution of flows in each tributary for a given total Delta inflow. Although a rigorous analysis was not undertaken it was felt that the distribution of flows in the major tributaries to the Delta could be divided into two populations; distributions that represent large storm events, and distributions that represent small storm events and non-storm periods. We were only interested in the storm event data and therefore wanted to eliminate from the dataset those flow distributions that represented non-storm periods.*

*Figure A, attached, shows a plot of daily average flow from October 1, 1955 to September 30, 2005. A line representing 80,000 cfs is also shown. Using a cutoff of 80,000 cfs captures all the significant storm events and excludes the small and less significant events. It is true that picking a value such as 80,000 cfs is arbitrary and could affect the outcome. But a review of Figure A shows that picking any flows from about 60,000 cfs to about 140,000 cfs would not have made a significant difference in the outcome. Not picking any cutoff value would have affected the outcome by trying to develop a relationship that represented both populations (storm and non-storm). This would likely result in a less reliable relationship for storm events than was used in the analysis.*

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 was not assumed that the Sacramento River is always 85% of the flow. It was stated that on average the Sacramento River provides 85% of the inflow to the Delta. The actual inflow used in any given scenario was calculated from the logistic regression that was developed as described in Section 4 of the Flood Hazard TM. The regression relationships have associated with them a mean square error for the regression so the inflow from each tributary could be calculated for any selected confidence limit.*

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.

*We are not aware of any other studies by the USACE or others on a probabilistic risk analysis of levee failure in the Delta. The flow and stage data and procedures developed in this study were specifically developed as inputs to the risk analysis. We did review the USACE Comprehensive Study. The purpose of that study was considerably different from the purpose of this study and therefore the information contained in the report did not appear to be relevant.*

*It is worth noting that the purpose of this study was not to develop frequency information on stages in the Delta. The purpose of the study described in the Flood Hazard TM was to develop a relationship for flood stages in the Delta for a given occurrence probability of Delta inflow.*

*For the given Delta inflow the stage everywhere in the Delta was predicted. The probability of those stages occurring (or of being exceeded) may or may not be equal to the probability of occurrence of the Delta inflow and likely would be different for different parts of the Delta. The procedures used in the risk analysis did not require the selection (or knowledge) of the probability of occurrence of a particular stage in the Delta. This is a departure from typical flood studies and that distinction helps explain why no other studies were identified as having relevant information.*

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.

*FEMA 100-year flood is a single deterministic water surface elevation in the Delta. In theis risk analysis each flood frequency (10-year, 20-year,…, 100-year etc.) have multiple surface elevations associated with it. Comparisons with Corps stage curves and historic data will be added in the revised report.*

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.

*Please provide the specific location of those statements so we can address them. All the specific comments below have been addressed.*

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.

*We are only considering flood risk in the next 200 years. In the thousands of years more changes will take place. In the late Holocene the hydrology was certainly very different from now when most of the rivers are damed and flow are regulated. These changes are beyond the scope of our work. We will attempt to describe the changes that have occurred in late Holocene in the Geomorphology TM.*

## Specific Comments:

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

*The analysis method developed for the risk analysis uses probability of occurrence of total Delta inflow rather than probability of exceedance. This requires that the probability distribution for total Delta inflow be discretized into bins with each bin representing an inflow range with an estimated probability of occurrence. Seventeen bins were considered sufficient to adequately represent the probability distribution for Delta inflow. The 1/34 is half the width of a bin (the range above and below the flow used to represent the bin).*

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…"

*We will take this suggestion into consideration as part of the revision of the report.*

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?

*The coefficient represents daily flows. The text will be changed to clarify this.*

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?

*We will remove Figure 7-18. The figure is misleading. The DRMS project did not develop 100-year water surface elevations. The figure showed predicted water surface elevations in the Delta for the case of the 50% confidence level estimate for a 100-year total Delta*

*inflow, with the flow distributed to all the tributaries at their mean values (for that total Delta inflow). This is not the same as a 100-year water surface elevation, so the comparison to the FEMA values provided in the report is misleading. The third map shows the predicted water surface elevations in the Delta for the same total Delta inflow as the second map but with a different distribution of flows to the tributaries.*

*For the risk analysis it was necessary to determine the water surface elevations throughout the Delta for any given flow condition. The prediction method used needed to be simple enough that it could be performed as many times as needed within a relatively short period of time (1000s or millions of times per day, for example). The probabilities used in the DRMS analysis are based on the probabilities of total Delta inflow, not the probabilities associated with any given river inflow or water surface elevation. Because the total Delta inflow is made up of contributions from several sources, there are multiple ways to represent the 100-year total Delta inflow (the last two maps in Figure 7-18 represent two of the ways). For a 1% probability of occurrence of total Delta inflow, the sum of the probabilities for each different distribution of flows is 0.01, the probability of a 100-year total Delta inflow. (It is worth noting that there is a distribution used to represent the 100-year Total Delta inflow. Each value in the distribution has associated with it a set of possible flow distributions in the Delta tributaries. This results in a larger number of possible 100-year events, each with its own probability of occurrence). The goal of the DRMS analysis is to incorporate all these possible conditions in the risk analysis.*

*None of the ways of achieving the 100-year total Delta inflow is more or less representative of the 100-year total Delta inflow than any other. Also, the water surface elevations predicted for each flow distribution do not represent the 100-year water surface elevations in the Delta. The risk analysis never actually calculates or needs to calculate the 100-year water surface elevation at any point in the Delta.*

*Because the analysis method used for the flood hazard in the DRMS project is different than is typically used in a flood or design study, it is an example of how the method can be used to generate a frequency distribution of water level at a point in the Delta.*

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

*The authors meant to say that if a stability-related problem arises, action will be taken to fix it. The text will be revised to be clearer.*

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).

*These figures show the raw data for the number of failures through time obtained directly from the database provided by DWR. The lines are simply trend lines averaging the total number of failures for the period of observation (slope of the line = number of failures/the duration of the period of observation). The time break (1980) was used to differentiate between the older and the current state of the levees (new geometry and the start of the funding program) and was requested by the Steering Committee Members. The readers are welcome to use the data and analyze it for any period of interest.*

*The data is provided only as a historic record of the levee failures and the available historic flow hydrograph. We will add to the discussion some details on how many and when a large number of levee failures occurred.*

*None of these data were directly used in the flood hazard model development. They were rather presented as historic data to compare to the model results.*

*In Section 7.5.1 we report some key observations directly from the data without interpretation or analysis. We revisited the reported information from the chart and could not find anything misstated, or a wrongly reported statement. The record of historic island failures obtained from the DWR data does not include days, months and time of failure. The only information available is the year of the failure. So, if failures occurred in 1986, we inferred that they were associated with the high Delta inflow during the storm of 1986.*

*As requested, we will plot the non-cumulative chart of historic failure with larger scale so it will be readable.*

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?

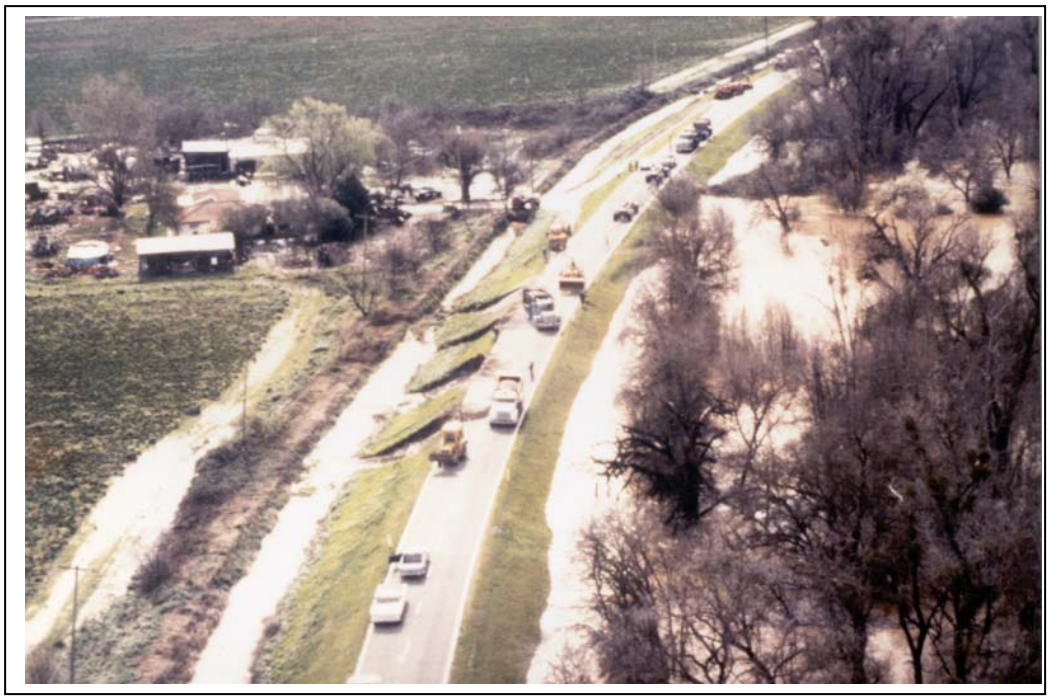
*Slope instability and erosion are addressed in other topical areas (Seismic and Emergency Response). The sentence will be revised appropriately.*

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?

*Slope stability and seepage are two separate failure mechanisms and are not necessarily correlated. However, one can lead to the other (for through seepage, enough material removed from the DS face of the levee and toes will lead to oversteep slopes which will fail ultimately by instability of the slope).*

*There are many instances where the long term steady-state effective stress analyses show a factor of safety greater than 1.4, and yet we experienced through-seepage and under-seepage failures (consider the case of the Natomas levees). Very low existing gradients (less than 0.4) have resulted in through seepage failures as documented by the 2007 and 1997 events (see pictures below). For through-seepage, low exit gradients can move particles from the face of the slope without much effort (Photo of through seepage failure in 1986 along the Sacramento River). The same holds true for through seepage.*

*Picture of sand boil at Staten Island (ejecting ~250 gpm) during the near levee failure 6-21-2007 (Picture by DWR 6-21-2007)*



*Picture of Sacramento River Levee Through-Seepage failure (USACE 1986)*
*Soil particles were observed moving down the landside slope while water was oozing from the slope face.*

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.

*We agree that more explanation and detail should be provided on this subject. All the mathematical models used to calculate expected performance are based on data and logical development of the model. The data, the interpretation and the model development are in the risk report and relevant TMs. When data and information were absent we used a formal expert's elicitation. Again, expert elicitation is a formal and accepted SCIENTIFIC process. When expert elicitation is used, we will provide a detailed description of the format and the process followed.*

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?

*The presence of slough sediment and a ditch affect the calculated seepage gradients and therefore valuable parameters to include in the seepage calculations. The shortcoming is that we do not have reliable data (i.e., per each slough mile by mile data). Also, during high velocity flows the slough sediment is removed and the reverse occurs during low velocity flows. No continuous survey of slough bottoms are performed regularly and every where along the Delta Sloughs. Based on the absence of that specific knowledge we assumed 50% chance for the slough sediment to be present or absent. The same applies to toe drainage ditches.*

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 disagree with this comment and maintain our prior statement. The local engineers and maintenance Districts report that the worst underseepage problems are at the location of the ditches. Sand boils form in the ditches, undermine the foundation sands, and ultimately result in formation of sinkholes through the levee or levee slumps.*

- 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 is not clear what this comment intends regarding the stability issue versus underseepage. We addressed the question of seepage versus stability previously.*

- 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.

*The reasons for past failures are not documented. Even the failure of Jones Tract (June 2004) is not documented and well understood. There is still debate on whether it was: high tide, rodent holes, a continuous pervious layer through the levee or through the foundation, human activities, or a combination of those. Most, if not all failures did not have witnesses or witness reports that we can refer to.*

- 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.

*Definitely! Conditions worsen with time as far as seepage is concerned. See the answer to the preceding comment.*

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 $P(F_1$

U $F_2$ 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($F_1$ U $F_2$ 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.

*The model we have used assumes that a failure occurs on the weakest link (i.e., the weakest levee reach). We just do not know which reach is the weakest link with certainty. Thus, if there are n reaches on an island, there is only one "trial (i.e., one unknown reach subject to a failure)" and not* n *different "trials." We are trying to find the probability. The outcome of the single trial is a failure. This is not the same as the probability that there will be a failure on at least one of* ***n*** *different trials. Each reach has some probability of being the weakest link. This probability is set proportional to the conditional failure probability for that reach given that it is the weakest link ($f_{ijk}$). The probability that a particular reach is the weakest link is given by $w_{ijk}$ in Equation 1. Equation 2 follows the total probability theorem. Each reach has a probability of being the weakest link (calculated from Equation 1). Furthermore, if a particular reach is the weakest link, it has a certain probability of failure $f_{ijk}$. Therefore, the total probability of a failure considering all reaches is the product of $w_{ijk}$ x $f_{ijk}$ summed over all reaches.*

*For the example noted in the review comment, let us assume that an island has 3 reaches with conditional failure probabilities of 0.9, 0.05, and 0.1. Then, the probability of being the weakest link would be 0.9/((0.9+0.05+0.1) = 0.857 for the first reach, 0.048 for the second, and 0.095 for the third reach. Finally, the total probability of a failure would be (0.857x0.9 + 0.048x0.05 + 0.095x0.1) = 0.783.*

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?

*The properties of the levee are different and uncorrelated with the foundation.*
*The events of underseepage and through-seepage are assumed to be* ***conditionally independent****. That is, given a levee reach with certain geotechnical properties subjected to a given hazard event and loading (e.g., water head), the two failures are assumed to be independent. For a vulnerable levee reach, probabilities of both events would be high using our model. However, the information that one is high would not change (the already high) probability of the second.* ***Unconditionally****, these events would be highly correlated; and that correlation is preserved in our analysis.*

*Failure events between multiple islands are again assumed to be* ***conditionally independent****. That is, they are assumed to be independent given a particular hazard event and loading. Given a high loading, the failure probabilities for vulnerable islands are*

*likely to be high using our model. However, the information that the failure probability for one island is high would not change (the already high) failure probability for another island*.

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.

*Equations 1 and 2 calculate the probability of a failure based on the vulnerability of each levee reach on an island. This calculation does not explicitly account for the length of an island. Some fine-tuning of the calculated failure probability based on the island length was considered to be necessary to distinguish between long versus short islands. That was the reason for developing the length-based scaling factor. Note that the scaling factor was relatively small for most islands in the range of 20 to 40 miles long.*

## *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.

*This section is to provide a brief overview and summary of the more detailed DRMS report on wind and waves and their relevance to the risk analysis as documented in the "Wind-Wave Hazard TM". In the process of creating this summary, material from the TM was abstracted rather than summarized and many citations were omitted. An explicit reference to the TM should have been stated to facilitate reader access to more detail, the relevant citations, and how to obtain the data. This will be done in the next revision of the Phase 1 Report and the presentation of this summary will be improved.*

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.

*The deep-water assumption is explained in somewhat more detail (but still inadequately) in the TM. It is correct to identify this as an important assumption that should be reviewed. It will be reviewed and more clearly explained and justified in the next version of the Risk Analysis report – or it will be changed.*

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.

*Again better context is presented in the TM. We will review the importance of these concepts to the present summary and either explain them adequately or delete them from this discussion.*

### Specific Comments

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

*The comment is correct. This will be clarified in the next version of the Phase 1 Report. Several potential impacts of wind-waves were candidates for inclusion in the risk analysis*

*and they were initially being addressed in the wind-wave studies. For information, the present risk analysis considers the wind-wave damage to the interior slopes of flooded islands from low level as well as major, regional wind events.*

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?

*The limitations stated in that paragraph apply only to the task of wind/wave development model. The application of the impacts of wind wave induced erosion and overtopping are discussed in their own sections. This statement will be revised to direct the reader to the proper sections of the report where the impacts of wind/wave are analyzed or discussed. The wind/wave levee erosion model was used in the emergency response module (Section 10 which will expand the discussion of the erosion model used). Overtopping is addressed in the flood hazard in Section 7.*

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.)?

*As stated in the subject paragraph, the DRMS risk analysis needed to consider region-wide, sustained winds. Three key regional meteorological conditions were identified and extreme winds with durations of several hours were of interest. Gusts were considered to be of limited relevance to potential wind-wave damage of levees. Additional detail on durations is provided in Figure 9 of the TM. The regional wind events considered relevant and their durations will be more clearly described in the next version of the Phase 1 risk analysis report.*

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?

*Correlation in space follows from consideration of major, region-wide wind events. Ultimately, this does matter because occurrence of a major, regional wind-wave events in the context of a levee breach event will have damaging effects Delta-wide, increasing the damage to already flooded islands and extending the overall repair schedule.*

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?

*The comment omits a crucial word from the above quotation. The accurate quotation is: "The 2 percent wave run-up height is* ***otherwise*** *not related to the probability of a given wind speed or wind wave condition." The prior sentence states that "The 2 percent run-up height was calculated for each wind wave height and period." These two parameters (height and period) fully incorporate the influence of 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.

*This Section is meant to present the historic record of failures as they are maintained in DWR files. No other reference was judged necessary. There are no references we are aware of that address the frequency of occurrence of sunny-day levee failures in the Delta except the database from DWR and discussions with the Suisun Marsh maintenance personnel.*

*Note, this section discusses the assessment of the frequency or rate of occurrence of levee failures and not "the probability of them continuing."*

The organization is difficult to follow and why they used certain reference elevations or databases is not discussed.

*NAVD88 is the reference datum that DWR preferred.*

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.

*The term 'sunny-day failure' is a commonly used term to denote the general class of failures of water-retaining structures such as dams and levees that fail under normal, non-transient load conditions. Further, in the Delta typically winter storm events and floods do not occur during the period June to October.*

It is difficult to separate their conjecture and results.

*Everything that is presented in this section is related to the historic record of levee performance and is based on available data provided by DWR. When details of an event are not fully known we state so. In interpreting some events, engineering interpretations are proposed to explain them. These interpretations are based on our experience working in the Delta. The reviewers can disagree and propose better explanations. We are open to any suggestions.*

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).

*We will revise the wording to indicate that we have a basis in review of the available data for making such a statement.*

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.

*This is related to the sunny-day events (the period between June and October) and is related to astronomical tides and/or remote ocean-induced storm surges that end up raising the tide in the Delta after some elapsed time.*

## 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.

*We agree that the effect of cumulative deterioration is not necessarily captured by the historic record of sunny-day failure only. A deteriorating levee will be more vulnerable to floods, earthquakes, and sunny-day conditions.*

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

*This phenomenon is associated with astronomical tides (coincident pull from both the moon and the sun when aligned) or remote ocean-induced storm surges that end up raising the tide in the Delta after some elapsed time. This was the case during the Jones Tract failure on June 4, 2004 and recently during the Staten Island near miss on June 21, 2007. In both cases the high tides were about 2 feet higher than normal.*

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.

*We kept the two models (sunny-day failure and winter-storm failure) separate because the frequency of these events and their corresponding stages are different.*

## *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.

*We will consider these general comments in the context of our specific responses, which follow these general comments and responses. We emphasize that this section was intended to be a summary. Any reader who wants more detail must refer to the TM. The TM includes appropriate references and citations; some may not have been carried through to this report. The references will be checked. Any that are missing will be added to the revised report.*

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

*It is standard practice in probabilistic seismic hazard studies and in seismic risk studies that aftershocks are not included in the analysis. In fact, if historic seismicity is used to estimate earthquake recurrence rates, one of the first tasks is to remove foreshocks and aftershocks from the catalog. Given that this is a standard practice, we did not feel it was necessary to identify or emphasize this fact.*

*In our revision of the report we will make note of the fact that aftershocks are not included in the analysis.*

### Specific Comments:

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

*We believe it is not un-conservative to assume that dewatering resources will be available as and when needed. Dewatering rates will be limited by levee stability considerations. With truck and rail transportation available in the region, the geographic territory accessible for dewatering resources is continent-wide. If this appears to be limiting, appropriate projection of needs should allow marine transportation to provide access to additional resources.*

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.

*Regarding bullet 3 – Trained labor, if insufficient locally, can be augmented via air transport. Gross availability of critical equipment was addressed in bullet 2. Other*

*equipment can be augmented by truck or rail. The critical material is expected to be rock loaded to marine transport, as described in the preceding paragraphs. Other material should be available via truck or rail. It is debatable whether the critical material (marine-based rock) or equipment (marine transport and placement equipment) will be pre-empted by other needs outside the Delta that occur due to the same seismic or flood event. We concluded that after an initial period of marine rescue and reopening of shipping channels, restoration of the state's water supply through the Delta would be the top priority need.*

*Regarding bullet 4 – Assuming that aftershocks are of less magnitude than the primary event, we concluded that other forces (extreme tides and wind/wave events) were more likely to result in new levee failures that flood additional areas. It is correct to say that not considering aftershocks is slightly unconservative.*

*Regarding bullet 5 – We addressed this bullet (potential limitations of dewatering resources) in the first specific comment response.*

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

*The single sentence following the Section 10.4 heading was intended as an introduction to the following four subsections, which address how assessment of ongoing damage and prevention of further damage are modeled. The next version of the Phase 1 Report will contain additional language to make this more obvious.*

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?

*Where a levee breach occurs in a failed reach is random. We have mapped the island sectors to the levee reaches that were modeled. The analysis identifies the sector where a levee breach or damage occurs. Dividing the islands into sectors was coordinated with the hydrodynamic modelers to ensure that the level of detail the WAM model required was passed along.*

*Note, to some level most if not all random variables in the analysis are discretized for numerical analysis.*

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.

*Under the BAU approach for emergency response, prioritization decisions are the responsibility of Incident Command. Obviously, higher authorities and political pressure will also be involved.*

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.

*Additional language will be considered for the next version of the Phase 1 Report. Damaged non-flooded islands are discussed further in this draft of the report in the next section, which addresses repair priorities. They are given the first priority for repair. Discussion on the secondary breaches will be expanded.*

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.

*The BAU response strategy assigns prioritization decision making to Incident Command. The authors have searched state documents for current state response strategies. We have also participated in a separate project addressing state "emergency preparedness and response to Delta levee breach events." We believe we are aware of relevant documents. Although some useful documents exist, they are not particularly helpful in providing guidance to Incident Commanders on prioritization decisions.*

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?

*"The scheduler…" means the scheduling function incorporated in the ER&R model. The model is not an optimization model. It is a simulation model that makes prioritization and scheduling decisions as necessary to proceed through any given event with a description of reasonable response and repair activities, given BAU assumptions and the needs of subsequent modules in the risk analysis (water analysis, economics, and ecosystem).*

In the first line of the "Population" subsection, the word "only" seems redundant.

*This language will be reconsidered in the revision of this section.*

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?"

*The source of the statement was personal communication with the leader of the DRMS Economics Team.*

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?

*This is not an interactive analysis. The priority order had to be established before the simplified hydrodynamic model was operational. Thus, the hydrodynamic modeler's judgment was used, based on a range of levee failure calculations that had been performed in earlier work using the two-dimensional Research Management Associates model.*

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?

*This statement is referring to the fact that we would have preferred to make a series of hydrodynamic calculations in order to establish the salinity priority. As noted above, we did not have the WAM model available to do this at the time.*

*Note, we do not state that the levee repair analysis is 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.

*This language will be reconsidered.*

Page 10-7. Second sentence: Does this mean that the category C islands are not part of the risk analysis?

*In the seismic cases analyzed to date, Category C islands have not yet been included. In the two flooding cases analyzed, flooded Category C islands were prioritized based on a consideration of acreage, flood volume, and apparent existence of flood easements.*

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.
*This language will be reconsidered.*

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).

*The initial task for the Emergency Response & Repair model was to create an analytical process for addressing any Delta levee breach event – a few breaches or many. Uncertainties are recognized, but considering uncertainty was delayed until a deterministic model was in place as a first goal.*

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.

*The word “will” will be reconsidered. Telling the state that it should start this process now is not a role we have.*

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

*Our contractual assignment is to assess the risks and consequences of Delta levee failures under BAU conditions and policies. In Phase 1, we have the added responsibility of highlighting the assumptions that are key to our assessment. Strategies to reduce risk are addressed in Phase 2.*

## *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.

*No response required.*

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.

*This is an accurate statement of the WAM function.*

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.

*We would be the first to say that the WAM would benefit from further review and refinement of some aspects of the submodels and especially of the decision-making rules. We hope to be tasked with this additional work and with facilitating the needed external inputs (particularly from water project operators) in order to provide improvements in the form of a "Version 2."*

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.

*We appreciate this assessment and also hope there is opportunity to perform the thousands of simulations that were initially envisioned. As the initial Phase 1 work was being completed, the WAM was used to simulate five specific levee breach sequences produced by the Emergency Repair and Response module at 909 start times within the CALSIM baseline simulation period (over 4500 individual WAM simulations of up to 5 years each). These results are discussed in Appendix D of the WAM TM. The number of simulations performed by the WAM and Hydrodynamic were sufficient (4500 individual simulations, it was the number of consequences simulations (economic and ecosystem impacts) that was limiting. We are currently adding a number of breach scenarios to cover a reasonable range of economic and ecological impacts (from frequent to less frequent.)*

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.

*The draft report was written before the revised TM had been completed. During the present revision of the report, additional information will be brought forward from the TM.*

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.

*We will provide a network diagram in the next version of the report.*

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.

*Additional detail and references to previous work will be provided in the next revision.*

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.

*Additional detail on variabilities will be included and care to avoid inappropriate generalizations will be exercised.*

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.

*We will consider these suggestions as we revise the report. In our view, the early sections (11.2 through 11.4) provide essential inputs to the hydrodynamic submodel and were therefore discussed first.*

There is no determination of risk in this section.

*This is as intended. Risk results are presented in Section 13.*

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

*This is as intended, although specific concerns about organic carbon and toxics were highlighted.*

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.

*The additional figures needed were not available at the time the report was prepared. They will be added in the next revision of the report.*

## 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.

*The text on page 11-3 will be revised.*

## *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.

*We agree with the comment. The discussion on the life safety is being expanded to loss of life estimation using appropriate and applicable models. We recognize that ecosystem impact was complex and not well defined in terms of impacts metrics. We are currently revising and simplifying it using expert elicitation.*

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.

*Any redundancy between sections will be removed and the sections will be more focused. The ecosystem impact analysis is being revised to be simpler and clearer. We will add simple tables summarizing the main impacts for selected island flooding 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).

*As we stated at the meeting with the IRP in March and in the report, we were not able to incorporate uncertainty in the consequences. The reasons for this and the limitations we faced were discussed in our response in Section 4. We repeated our response here.*

*We believe such assessments can be made in the consequence areas we addressed. As noted by Professor Rose in our meeting with the IRP in March, the uncertainties in the ecosystem area are difficult to estimate and potentially so large their assessment renders the results useless (our paraphrase of Professor Rose's words). This sentiment does not seem inconsistent with our statement or our experience in dealing with our TAC and team experts in the ecosystem area.*

*In the economics area we had a similar experience with our team members and in separate discussions with two economics professors from U.C. Berkeley. When addressing the subject of probabilistic modeling and in particular modeling uncertainties, the responses varied from "not really doable" to "such assessments can be done." In neither case was there an expression that such assessments are within the normative practice of the profession or academia for that matter.*

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.

*The ecosystem impact is being revised and simplified as indicated below in the specific comments section.*

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.

*Many of the experts listed in the Steering Committee and technical review groups did not have the chance to work on the ecosystem impact TM. We are currently taking a very different approach in estimating and quantifying the risk to the aquatic species. The approach used is focusing on simplification and the use of expert elicitation.*

## 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.

*The statement in the text is in error. We recognize and are aware that this part of the analysis needs to be developed further (we just did not have enough time to complete the expected life loss part of the model). We are familiar with the LIFESim model.*

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.

*The response to this comment was provided above to the general comments.*

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.
*See response on revised ecosystem impact model below.*

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.

*See response on revised ecosystem impact model below.*

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.

*To begin, economists attach different meanings to "cost" and "impact." The following changes are proposed:*

*The definition of economic cost has developed from the guidelines for analyses performed relative to federal water resource projects. Economic cost is the monetary value of resources or benefits that are dedicated, consumed or lost. Benefits are people's willingness to pay for goods or services, and economic costs are often a loss of these benefits. As examples, the cost of rebuilding a home and the loss of recreational willingness to pay when the Delta is closed to boating are both legitimate costs.*

*Economic impacts are measures that people often ask to see – the values of output, employment, labor income and value added that are changed by the flooding event. (Value added is the sum of wages and salaries, proprietors' incomes, other property income, and indirect business taxes.) However, even these economic impact measures can be misleading. For example, if Delta flooding were to prevent harvest of a local asparagus crop, that would have impact on local output, employment, labor income and value added. However, if this shortage of asparagus caused prices to rise and Imperial Valley farm net income to increase substantially, the adverse impact could result in positive economic benefit when considering the state as a whole. As another example, the cost of rebuilding homes can result in a positive economic impact through construction expenditures, but this depends on where the money comes from to pay for construction.*

*In summary, the economic costs are the net costs to the state economy without any consideration of who within the state bears the cost.*

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

*For most of these values, obtaining estimates of any sort was difficult. No information was available about probability distributions associated with these estimates. However, with more time, scenario analyses could be performed to investigate how alternative assumptions (such as groundwater availability, and availability of transfer water) might have resulted in a range of estimates.*

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.

*The simplification was part of the effort to automate the evaluation scheme. However, This approach will not be used as we are revising the ecosystem impact model as described below.*

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.

*See response on revised ecosystem impact model below.*

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.

*See response on revised ecosystem impact model below.*

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

*See response on revised ecosystem impact model below.*

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.

*See response on revised ecosystem impact model below.*

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.

*See response on revised ecosystem impact model below.*

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.

*See response on revised ecosystem impact model below.*

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.

*As a response to the above comments, the ecosystem is being revised completely. We plan to simplify and quantity the impacts in a simple, expert- elicitation-based approach. The main elements of the revised model are based on a simple cause and effect evaluation. The model will focus primarily on the impacts to the aquatic species from levee failures and entrainment. The failure mechanisms, timing of breach formation, turbidity, entrainment (percent of population entrained based on toe-net survey and density of population by region), island closing and pump-out models have been already developed by the DRMS technical team members. These will be defined and quantified in a manner suggested by the experts assembled to help with the development of the model. The quantification of impact (percent mortality and increase in the probability of extinction) will be developed based on input from the experts.*

*Currently the experts assembled for this effort include Professors Wim Kimmerer, Peter Moyle and Bill Bennett and Dr. Chuck Hanson. We are adding possibly two more experts on fishery from the DWR as suggested by the three experts.*

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.

*We concur with this comment.*

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?

*The model will be described completely and clearly.*

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

*ER & R refers to Emergency Response and Repair. It will be spelled out in the report.*

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.

*We do not know where the breaches are going to occur. We apply a probability of occurrence anywhere along a levee, differentiated only by the variation in the ground motions, flood stages, levee vulnerability, etc.*

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.

*No response required.*

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.

*The report did not mean to imply that there was not a cost to these agencies. The language of the report should be changed to make this clear, by including the following explanation:*

*These basins had largely recovered from overdraft conditions in the 1960s, and the agencies could be expected to be able to mine water from the basins over an extended SWP outage with very little effect. They are not expected to experience shortages or incur shortage costs. However, there will be costs associated with the reduction in Delta export deliveries. First, the agencies and society as a whole will SAVE the incremental cost of transportation of the water from the Delta – that is, there will be a savings because of the reduced water transport costs. However, these savings will be more than offset by the increase in pumping costs because the water levels in the aquifers will remain lower than they would otherwise be. This net cost was felt to be small enough compared to the modeling effort necessary to estimate it that it would be best ignored in order to have the time to complete other parts of the analysis. It should be noted that these agencies could not maintain their water supplies during an indefinite closure of the Delta.*

## *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.

*We agree this is a very important part of the report. We also agree this section needs revision and expansion.*

*The presentation of the probability of failure in the next 50 to 100 years can be confusing. Further, it does change as suggested by the comment. We will revisit the presentation of this information in our revision of this section.*

*Historically we have not had simultaneous sunny-day failures. While the Delta as a whole might experience a common high tide. There are other reasons why these failures occur that relate to the levee and its foundation and not to the tide. Our analysis assumes that levee performance is independent from one island and even one reach to the next, given a common high tide. As a result the joint probability of 2 or more sunny day failures occurring simultaneous (same day, week or month), while not zero, is very small.*

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).

*This concern is noted and we have responded to it in a number of places.*

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.

*We have responded to the question of evidence or lack of evidence of liquefaction during the 1906 earthquake. There is no information available to state either way. We do not believe that in the context of the DRMS scope and schedule we could conduct paleo-liquefaction investigations in the Delta. This task is a large investigative undertaking that may not yield any answer given that the Delta was mostly wetland and tidal marshes where silts and sand deposits may not be distinguished from liquefaction-induced sand boils. We have performed analysis of the levee and foundation responses using a 1906 earthquake and the information will be presented in the revised report.*

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.

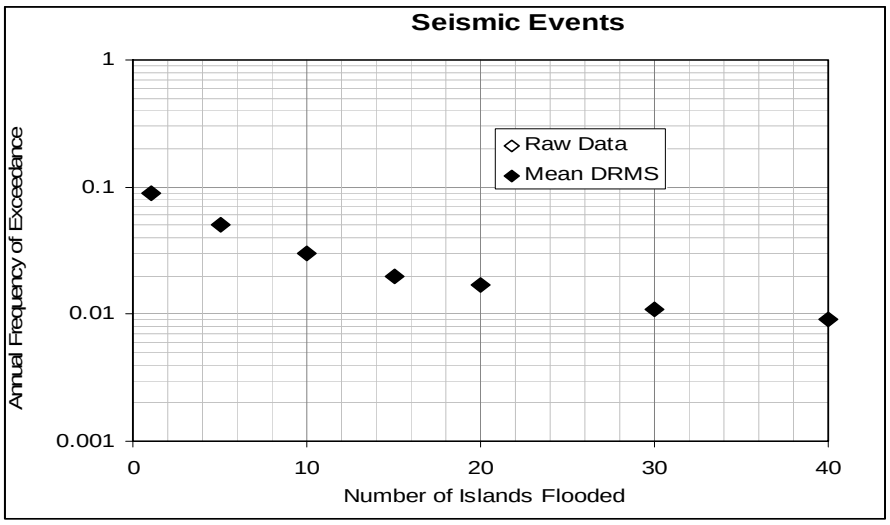



**Figure 1:** Comparison of DRMS Estimate and Raw Data for Seismic Risk

**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.

*We are in the process of reviewing the levee seismic fragility work and conducting additional evaluations to verify the work that we have performed. We note that our conclusions, while numerically different from the Torres, et al. (2000) work, are the same. Further, our conclusion is the same as all other studies over the last fifteen years.*

*The quoted estimates from Torres, et al. (2000) which we are aware of, are not real clear. For instance, how does one fail a fraction of an island? Since they are working on the estimated number of breaches, this result does not compare directly to our assessment. Nonetheless, we are looking into the differences between these two studies.*

*Please note that we have offered answers to this question in response to comments in previous sections where we highlighted the main differences in the two studies.*

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.

*As noted above, the presentation of the probability of failure in the next 50 to 100 years can be confusing. We will revisit the presentation of this information in our revision of this section.*

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.

*We will consider this suggestion in our revision of the report.*

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

*We will be expanding the assessment of public health and safety consequences and including these results in the report.*

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.

*We disagree with the characterization of the approach that was used in the risk analysis. When we expand the presentation of the risk analysis methodology and its implementation we believe the reader will have a clear understanding of the analysis that was performed.*

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.

*We are doing additional work on the hydrologic hazard analysis and the risk calculation. As part of the reporting of this work we will provide information about the events that contribute to the probability of levee failure and island flooding.*

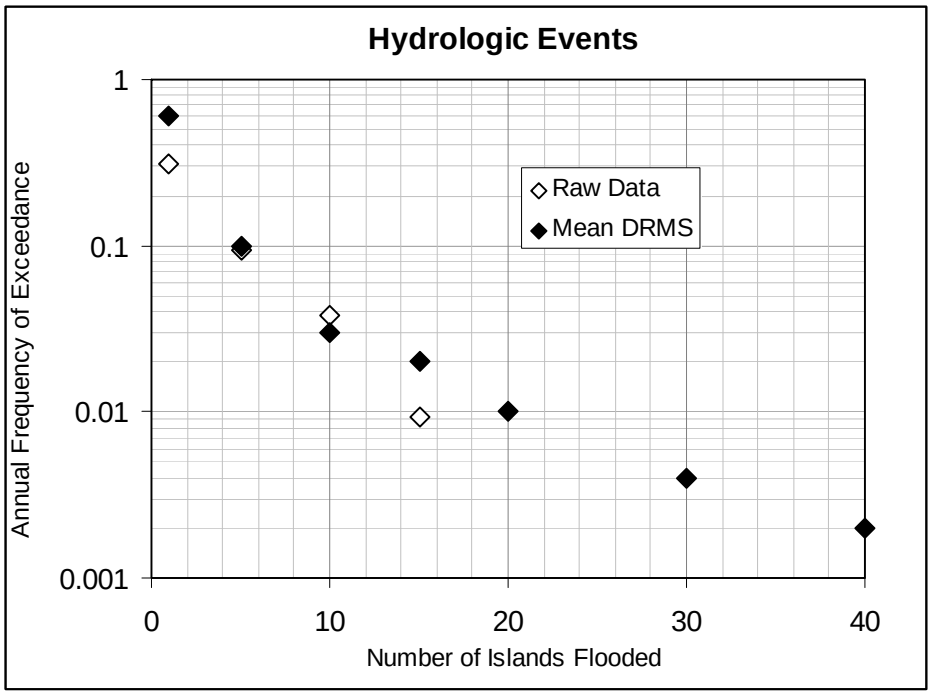



**Figure 2:** Comparison of DRMS Estimate and Raw Data for Hydrologic Risk

**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 issue dealing with the uncertainty (in particular epistemic uncertainty) in the assessment was discussed in our responses in Section 4.*

*Also as discussed previously in the response to comments in Section 12, we agree additional scenarios should be considered for the hydrologic cases.*

*The reason the dry water year consequence results are as reported is due to the fact there already was limited water available, therefore the occurrence of the levee failure event was not as significant as might be anticipated.*

*The number of scenarios considered is being reviewed to insure a reasonable range has been considered.*

*We will report the probability of the different hydrology combinations in the revised report.*

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 through 13-8) to many of the panel members given historical records. Failure rates are critical and should be fully justified.

*We will present further detail and the basis for the results that are provided.*

- The eco risk index is incongruent with the methods presented in the earlier sections. How was this derived?

*We are revising the ecosystem assessment and moving away from indexing methods.*

- 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.

*Based on the hydrodynamic calculations that were performed, there are no water export impacts for flood events (disruptions are less than 3 months, which was judged by our economics team to be a level of disruption that was not necessary to quantify) even in the case of 20 and 30 flooded islands. Thus, for cases involving fewer islands there will also*

*be no water export impact. As a result, the only economic costs and impacts are those that occur in the Delta.*

- 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.

*We are revising the public health and safety modeling and the revised report will reflect this work.*

- 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.

*The perception that a line can be drawn in the manner suggested is erroneous. First of all – what is the definition of a catastrophe for California? What are the parameters for defining a catastrophe: are they economic, public health and safety, legal, etc. and what are the limits that need to be crossed?*

*Technically, the answer to these questions is not a part of the Phase 1 analysis.*

*We have asked ourselves this question and have work ongoing that started in the summer that will at least lay out the framework for what might be considered a catastrophe in California. We have two professors, in economics and law, from U.C. Berkeley working on this.*

- There aren't really any integrated models; text referring to integrated models should be dropped from the report.

*This statement is incorrect. Our detailed presentation of the risk analysis methodology will show the elements of the risk analysis and how they are integrated.*

## 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."

*The IFSAR survey of the crest of the levee has been a continuous source of artificial nodes that needed to be carefully removed so they do not present source of error in the calculation of overtopping. As an example the radar survey (IFSAR) with low grid resolution could miss the crest of the levee and shoot points on the side of the slope. These points can be misread as crest elevations wrongly and would cause early overtopping. On the other hand a point can be shut on the top of a structure and would show a higher crest elevation. We filtered out all these singularities for all crest surveys*

*before using them. These corrections were made by hand or were removed by developing a 1000-foot running average for each levee crest survey.*

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.

*We will consider this suggestion in our revision of the report.*

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.

*In this report we do not believe it is necessary to make such a comparison. Further, when we provided this type of information which can be helpful in presenting information to the public, we note in the review of the Summary Report the panel had the following comment:*

> *"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."*

*It appears the panel members have mixed views on this matter.*

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.

*We agree, the indexing approach is confusing. We are revising the ecosystem assessment and moving away from indexing methods.*

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.

*We agree, this discussion is not required in this section.*

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.

*As we have stated previously in our response, we are changing the ecosystem aquatics analysis. As such, the presentation of the ecosystem risk will be significantly revised.*

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.

*As we have stated previously in our response, we are changing the ecosystem aquatics analysis. As such, the presentation of the ecosystem risk will be significantly revised.*

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.

*No response required.*

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.

*We agree, this table can be misinterpreted. As indicated above, we are revising the public health and safety modeling and the revised report will reflect this work.*

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

*While we agree with the prime importance of public health and safety, we will not express a measure of the importance on this or any other consequence that is assessed.*

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.

*We agree this presentation is overly dramatic and will revise the discussion accordingly.*

*As we have stated previously in our response, we are changing the ecosystem aquatics analysis. As such, the presentation of the ecosystem risk will be significantly revised.*

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?)

*The reason the "confidence bounds" are narrow is due to the fact the uncertainty in the seismic hazard, which typically dominates the uncertainty in seismic risk results, is relatively small at the low ground motions that are causing levee failures. These hazard curves with their uncertainty are shown in the seismic hazard TM.*

*Note, the fractiles that are presented are not "confidence bounds". The notion of confidence bounds is used on the context of statistical analysis – which this is not.*

*A total of 104 islands (analysis zones as we refer to them, but there are more 104 small islands and tracts that are mostly wetlands in the Delta and Suisun Marsh that we did not include in the analysis) in the Delta and Suisun Marsh are considered in the analysis. Note, this is one source of the difference between our seismic results and that of Torres, et al (2000).*

## *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.

*Agreed.*

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.

*We disagree that the referenced statements are unsubstantiated, sloppy, or appear biased. The comment seems to be applying a narrow standard and concept of appropriate style – one applicable to original scientific research reported in peer-reviewed journals. The work and the audience in the present case are different. This is not original research; it is a technical compilation (supported by the Climate Change TM) and analysis of available information for use by scientists, engineers and policy staff in support of decision makers and decision-making. One feature of such a report is to begin a section with an overview/ summary that is then substantiated by the details in the following subsections. As is indicated in the subsections of Section 14, although it is not preordained that only increases can occur, no example was found of expected future-years "Business as Usual" change that would decrease the likelihood or consequences of levee failures.*

*We disagree that the quoted statement is "not true," although we can accept that the suggested alternative statements are also true. However, those statements recognize only part of the story. Other important aspects (besides the likelihood of an event) are (1) the consequences of the event, and (2) the implications of an extended exposure to the risk of an event. One exposure to Russian roulette has a specific probability of an unfavorable outcome (0.167). However, 20 exposures have a dramatically different probability of an unfavorable outcome (0.974). Similar mathematics applies to 1 year of exposure to levee breach risks versus 20 years of risk exposure.*

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"?

*Again, this does not appear to allow for the purpose of this introductory overview and summary.*

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.

*We would have liked to provide a more-precise, better-documented, probabilistically-rigorous analysis of future risk. However, our scope of work was explicit. We were to use available data and projections and to provide a resulting assessment within a defined schedule. In the following paragraph the comments seem to recognize this context of our work.*

*As a point of clarification, the DRMS Phase 1 analysis is not a "statistical assessment of risk"; it is a probabilistic risk analysis.*

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.

*This is what we were trying to achieve as an initial assessment of the risks associated with levee failures under future conditions.*

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.

*We have these concerns and reservations as well. Our internal debate on what approach to take concluded that a "partial analysis" (e.g., addressing sea level rise and other drivers individually, including substantial discussions of uncertainties) would not be adequately helpful to decision makers. Instead, we concluded that a presentation centered around a medium future expectation for the many drivers of change taken together would be more useful as an initial assessment of future risk. Although we recognize the uncertainties, we concluded an intensive discussion of uncertainty would detract from an important message – namely that a reasonable, medium expectation for each driver indicates risks related to future levee failures are increasing and, when all these drivers are combined, there is certainly increased risk in the future.*

*We should also note here, a lower bound assessment of risks in future years would also indicate that risks are increasing. Thus, contrary to a reviewer's suggestion above, there are no indications, given Business-as-Usual in the Delta that risks may be decreasing.*

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).

*References to the TMs were assumed to be implicit. In the next revision, we will make the appropriate citations. The TMs do cite available literature, data and projections, and uncertainties.*

*With regard to the final comment, see the discussion above regarding the approach we decided to take. This said we are looking at alternatives to the analysis and presentation of future risks.*

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.

*Business-as-Usual was carefully defined as a basis for the baseline (Phase 1) analysis (see Section 3.4 of the Phase 1 Report). In revising Section 14, we will include a more explicit statement of what this means relative to each type of future condition analyzed. This said, for the most part, Business-as-Usual means very little will be done with regard to current practices in the Delta, and that is the point. The case where we will keep up with sea-level rise will also be included in the revised report.*

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.

*As indicated above, we are re-examining the analysis of future risks. In revising this section we will consider these comments and provide the appropriate level of detail, rational and substantiation.*

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

*We will reconsider each statement made and the degree of substantiation that should be provided.*

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).

*We disagree that previous work is ignored throughout. The example given on tidal amplitudes specifically considered astronomical tides and past work on astronomical tides (see the Water Analysis Module TM, Appendix H3). Additionally, we performed analyses of prospective increases in surges in the Delta, given a simulation of future tides at the Golden Gate that was part of the climate change scenario adopted for the DRMS analysis of future years (see the Climate Change TM). Although there were some indications of potential increases, and no indications of decreases, the indications of increases were not regarded to be strong enough to merit the label of a medium expectation of future conditions. We will reconsider our assessment with explicit reference to the cited papers. Thank you for the references.*

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 apparent repetition occurred in the context of a specific effort to be consistent in addressing changes that can be expected (relative to 2005) in 2050 and 2100. The present conditions (2005) are addressed elsewhere in the report and the risk results for 2005 are presented in Section 13.*

*The reason for not giving more attention to 2200 is explicitly addressed in Section 14.1.10.*

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.

*Our rationale for not providing an intensive discussion of each driver's range and uncertainties is provided in our response above. We are considering applying ranges in analysis outcome for the climate change.*

The maps are very nice.

*No response required.*

Ecosystems - what ARE the risks?

*To directly answer the above question, the present (2005) ecosystem risks are addressed in Section 13; they are not within the scope of this section. The change in the risks to ecosystems due to levee breaches is within the scope of this section and is addressed in Subsection 14.1.8. The relatively large uncertainties in present risks to ecosystems due to levee breaches and regarding the expected viability and health of ecosystems in the future make it difficult to say how future ecosystem risk consequences from a given levee breach event are likely to change. The obvious risk of concern is species extinction. The statement in*

*Section 14.1.8 indicates the absence of a basis for saying that such a consequence from a given levee breach event would be less likely in the future. This subsection and the related subsections in section 13 are being reviewed and revised as appropriate.*

## 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.

*We will provide an explicit reference to the Climate Change TM and explain the derivation of the estimates used as a medium expectation.*

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.

*The analysis of salinity response to an increase in sea level (Water Analysis Module TM, Appendix H3) was performed prior to our establishing "a medium expectation" for 2050 and 2100 sea level rise. The alternative to including the 90 cm illustration was to include none.*

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

*More explanation will be provided in the upcoming revision.*

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."

*The statement relative to 2050 does not say "there will be 50% more total runoff in the system." It says "There will be approximately a 50% increase (over 2005 conditions) in the frequency of the total Delta inflow discharge that presently has an annual frequency of exceedance of 0.01….." Our decision relative to not providing detailed discussions of uncertainty was explained in the response to general comments.*

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.

*The implicit reference to the Flood Hazard TM will be made explicit. Thank you for pointing out our error with the "s's."*

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

*Thank you for pointing out our error with the "ment."*

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

*References to TM's or other documents will be provided.*

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."

*We have considered overall risk to be a combination of the probability of failure and the consequences given failure. We will consider this comment as we revise the report.*

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

*We will consider this suggestion in our revision of the report.*

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.

*You have interpreted the statement correctly. Your suggested revision has the same sort of "jargon" limitation as the original. We will explain the concept in the types of generally meaningful words that are incorporated in your question.*

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?

*We meant "easy" as a relative term rather than an absolute term.*

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?

*One measure of risk could be the failure of the system itself. Other measures of risk can consider the consequences of the levee failure. We will consider this comment as we revise the report.*

*The second point expresses a valid point. The sentence suggests an evaluation of the risk relative to some standard, which of course does not exist. We will consider this comment in our revision of the report and attempt to avoid judgments or advice with respect to the level of risk. The main points from this section are that risks are increasing and that multi-year exposure periods are a major consideration.*

## *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.
*This is a very broad comment and it is not clear what point is intended by it.*

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.
*Climate change is but one of the inputs to the risk analysis that has a range of prospective quantitative realizations. Since climate change is relevant to future conditions, it is discussed in Section 14. In Section 14 we noted that a range of prospective futures could occur due to climate change and other variables, including subsidence, population, land-use and the future state economy. Section 14 is being reviewed and consideration is being given to incorporation of a range of conditions for each of these future-condition variables.*

### 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.
*The editorial corrections are noted.*

*The studies performed by DRMS require a difficult balancing of coarse data (representative of conditions in the region and analyses that are based on specific inputs to estimate levee failure and island flooding. The bullet is meant to highlight the fact that regionally coarse (but generally appropriate) data were used in the analyses. But in considering a specific location, more precise data and finer resolution would be needed to calculate prospective results of specific projects.*

Page 15-2. Second bullet, last sentence: Not clear what this is saying. In last bullet on page, need an *s* after "requirement."
*We assume the comment refers to the CalSim limitation that "Also, the historical record includes less than half of the 125 potential 3-year sequences of water year types." This will be clarified in the next version. Briefly, there are five types of water years and, in a three year sequence, they can occur in 125 different orders. In the 83-year historical record some sequences occur more than once so less than half the possible sequences have occurred.*

## *Other*

### Climate Change Technical Memorandum:

Why use Knowles & Cayan snow pack projections when the PNAS (Hayhoe et al, 2004) are more recent?

*The projections of Knowles and Cayan were not used as inputs to any model or projection. Rather, we showed them in an introductory section to illustrate that future loss of snow, although uncertain, is likely to be very significant. For this purpose, it is not necessary to use the latest projections. The figure caption clearly states that the Knowles and Cayan results are merely an illustrative example: "This is a typical result based on one model … other models would give qualitatively similar but quantitatively different results." Thus, for our purpose, there would be no significant benefit to using newer projections.*

It's A1fi not f1 (table 1)

*This error does not appear in the latest version of the TM (that on the DRWM web site: http://www.drms.water.ca.gov/docs/Climate_Change_TM_Revised-updated07.pdf; thus, it seems that the IRP was reading an older version.*

Typo p. 20 - 2050 and 2050 for SLR (instead of 2050 and 2100)

*Noted.*

Needs an executive summary highlighting the main findings; also no conclusions to section 3.1 on slr!

*The request for an executive summary is noted. The conclusions and recommendations on sea level rise trend were presented in Section 3.1.4, which was then followed by a section on short-term sea level variability. This apparently led to some confusion.*

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?)

*Because water levels in the Delta are strongly influenced by daily-timescale variations in river flows, we felt that it was desirable to have daily-timescale simulated flows for DRMS. We know of only one simulation of river flows that uses daily time resolution and incorporates the major rivers draining the west side of the Sierra. This is what we used.*

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.

*The first question relative to wind is whether climate change can be expected to cause changes in the frequency (or intensity or duration) of Delta-region, sustained wind events. Furthermore, if a change were expected, would one expect winds (or frequencies) to increase or decrease? This is a more limited objective than implied by the comment. This limited first objective will be stated more clearly in the next revision of the Phase 1 Report and in the Climate Change TM. The TM Section3.3 is aimed at answering that question. It looks at what can be done with global climate models and nested regional scale wind models and concludes that the results are not yet adequate to support conclusions, even on these limited questions. Thus, the TM basically agrees with the comment that such models are "not ready for prime time."*

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.

*Available time was an important limitation, even for using global climate simulations that are available.*

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

*Again, the original Tech Memo clearly states that climate model wind projections for the Delta are not reliable. So there is no disagreement here.*

## 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.

*The question on significant digits is understood and will be considered in conducting the review and revision of the Levee Vulnerability TM that is presently underway.*