Appendix A

Comments of the Steering Committee Members and Member Agencies on the April 24, 2007, Draft of the Risk Analysis Report and the Technical Memoranda (Various Dates) and the Responses of the Consulting Team

Comments on the April 24, 2007, Draft of Risk Analysis Report and Responses of the Consulting Team

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments on Risk Analysis Report

No.	Section	Comment	Response
Reviewer: Goettel, U.S. Army Corps of Engineers			
1	**General**	Seismic fragility curves and the corresponding seismic failure results need to be thoroughly and carefully re-evaluated and corrected.	The comment needs to be more specific. There are hundreds of fragility curves for tens of vulnerability classes and for tens of failure modes. All results have been peer reviewed. Reviews of the inputs and calculation results have been performed and different results are reported in the revised draft Risk Analysis Report.
2		Flood fragility curves and the flood results need to be thoroughly and carefully re-evaluated and corrected.	All results have been peer reviewed and have been revised where warranted.
3		I note that the neither of the "bottom line" expected annual failure numbers, 3.41 for seismic and 7.35 for flood, are shown in the tables nor included in the text. My inference is that that the tables were generated from intermediate assumptions deemed "reasonable" without any check on the final result.	Probability of failure numbers from each island should not be added to estimate the total number of failures in the Delta for seismic or flood. The results of the analyses have been peer reviewed.
1	**Summary Report**	The Summary Report is a generally good synopsis of the (massive) Risk Analysis report.	No response needed.

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2		A couple numbers of page "i" of the Summary appear incongruent – pending more detailed review of the main report: (a) 4th bullet says 2.6 island failures per year, which I presume is current conditions. This is much higher than the reported 166 breaks overall (not just islands) since 1900 or the about 1 break per year since 1920. See Comment 15 below. Perhaps this difference arises from probabilistic consideration of multiple failures in floods larger than observed in the historical record, which is credible, but if so, this basis should certainly be mentioned in the Summary Report. (b) 5th bullet of an average of 30 or more islands may breach (I don't understand what an average of 30 means in this context): appears very inconsistent with the above 2.6 per year???	a) No response needed. b) By average it is meant the mean estimate. The average word was used for the lay person or general public reading the report. The results are currently being reviewed and updated as part the peer review process and comments. c) Flood numbers have been revised.

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2 (cont'd)		(c)The "frequency" of the 100-year storm will increase by 50% by 2050 and 100% by 2100. Very confusing. The 100-year storm is still the 100-year storm. Does this mean that a flood with flood elevations that are now the 100-year event will become the 67 year and 50-year events by 2050 and 2100. If so, I'm baffled by why the water levels rise this much – unless this is entirely the effects of projected sea-level rises? In any case, this statement needs clarification as it will surely confuse most readers.	Yes. The frequency of the present (year 2000) 100-year storm Delta inflow may increase so the same inflow will be the 67-year storm in 2050 and the 50-year storm in year 2100. This is due only to the climate change impact of less snowfall and more intensive runoff. There is much uncertainty about this; these increases in frequency are at the lower end of those indicated by the four climate change simulations. Some indicate much more severe increases in frequency. Change of water surface elevations due to sea level rise is a separate and additional factor that will affect the number of failures during floods.
3		The Executive summary should give the annualized expected breaks for both flood and seismic – which are missing now – in addition to noting the expected increases in both with time. The current annualized breaks are first order essential information that really should be stated up front, not buried in the body. SEE major comments above about these numbers which appear to be well outside the bounds of credibility.	The annual expected breaks for flood and seismic events are contained in large tables that are in the full report. We will include a discussion of the key findings in the executive summary of the Summary Report.

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4		Page "ii" first two bullets seem inconsistent. If the P (overtopping in a 100 year flood) increases by a factor of two by 2050 and a factor of 3 by 2100 (2nd bullet), it's hard to understand how the overall probability of failure only increases by 10% and 20% respectively. That, is the probability of all types of failures should be directly related to the amount of (decreasing) freeboard above various water levels.	This discussion is separating the risk of overtopping if levees are not raised from the other failure modes. The 10% and 20% refer to the increased probability of failure due to the non-overtopping failure modes. This will be clarified in the text.
5		Page "ii" last bullet: define the important X2 line for non-technical readers.	Will define where used.
6		The volume below sea level should include the current figures as well as the increases.	The volumes are uncertain due to inadequate topographic data. The subsidence team, the hydrodynamic modelers, and the GIS data base have developed volume estimates. Prior to publication, all should be refined and made consistent based on improved topographic data.
7		Area below sea level is another very important metric which should be listed in this summary – including brief commentary on areas 0 to -5, -5 to -10,…<-20 feet as per Figure 3.	See response above regarding volumes below sea level. The same comment applies to areas.
8		Page "vi" the 6 scenarios aren't very meaningful without a little more explanation of which (presumably representative) islands were assumed to flood.	Clarification will be provided.

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9		Page 5, above photo: Delta includes large areas subject to liquefaction – I don't recall any maps of this in the seismic section and noted the apparent lack of such geotechnical information anyplace in the DRMS TM's - seems to be a major omission.	There is an entire study dedicated to the geotechnical issues in the Levee Vulnerability Technical Memorandum (TM).
10		Page 10 Table 1: the use of "infrastructure" is confusing to me. I presume that this includes buildings (contents??) and not just the normal usage of infrastructure (transportation and utility systems). This is a very important definition for understanding the numbers here and throughout DRMS – and really MUST be clarified unambiguously whenever this is used to avoid major misinterpretation of such numbers.	For the DRMS project, the term "infrastructure" is used to designate all structures and buildings, and their contents.
11		Pages 15 and 18 totals at bottom of tables are presumably for Delta and Suisun – not just Suisun? Or, the last line is indeed Suisun, but these are not totals: the totals are central results and MUST be shown.	The results are kept for each island and are not meant to be added. The probability that all islands fail (the total of all the Delta) is very small. Each number of failed islands and the combination of those islands have their own probability of occurrence. Suisun Marsh was treated as one single analysis zone.
12		Figures 8, 9 and 10 seem out of place vis-à-vis the other figures and very puzzling why they are placed between the seismic failures and flood failures sections.	The order of appearance will be changed.

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13		Page 21 – end of seismic discussion. Need explanation of WHY seismic risk increases with time – will be baffling to many readers.	The Seismology TM discussed the time-dependant seismic hazard.
14		Page 21: Flood risk is by far the greatest risk: This should surely be presented first, then sunny day failures and seismic, followed by the cumulative risk. I note also that the wind/wave section has ZERO results – I expected there to be separate tables of failures for wind/wave effects which do NOT seem to be included in the flood sections.	Wind-wave erosion is part of the emergency response. The Wind-Wave TM provides the wind model and wave emergency. The Emergency Response and Repair TM calculates the amount of erosion that takes place when an island is flooded. The wind action by itself on the outside armored slopes of levees was estimated to be of lesser concern because of the armoring, the slow rate of erosion, and human intervention.
15		Table 4 (page 22) has some extraordinarily short return periods for failures with 16 zones having return periods from 2.7 to 5.1 years. This extreme level of risk appears grossly discordant with recent historical experience. On average each of these tracts should have flooded about 5 times in the past 20 years which simply has not happened. This appears to be a major discrepancy which has profound interpretations for the overall risk	The numbers have been revised.

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15 (cont'd.)		conclusions and certainly for Phase II strategies. These extreme return periods urgently need to be re-examined and probably corrected. This bears on the previous statement predicting 2.6 breaks per year which is 2.6 times the historical number since 1920. Overall, the 2.6 breaks per year could certainly be explained by consideration of multiple breaks in long return period major floods not represented in the historical record – but this doesn't seem to be stated explicitly – if this is the case, this discussion should really be included. But, even so, the return periods of 2.7 or 5 years for many levees just seem incorrect and incompatible with decades of recent data. NOTE: see comments below on Section 13 Risks and the beginning of my comments about the apparently out of bounds bottom line results for the expected annual number of levee failures.	
16		Pages 23 and 26: totals for Flood Risk and Overall Risk need to be added to be parallel with sunny day and seismic tables. Some explanation may be needed since the "return periods" for flooding and overall risk will be well below 1 year.	Done.
1	**Executive Summary**	The comments on the Executive Summary of the "summary" apply to the Executive Summary of the main report as well, since the two executive summaries are nearly identical.	OK.

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Reviewer: Goettel, U.S. Army Corps of Engineers			
2-1	**Section 2**	Page 2-8, bullets 3 and 4. Define ROD again (not in common use) and list the four CALFED objectives – these are important and worth restating here, especially since they are short.	Record of Decision (ROD) added to text.
2-2		Page 2-8, last bullet, item "b". A 100-year and 1000-year earthquake are not meaningfully defined (on what faults or just probabilistic ground motions). Yes, such was "declared" in AB 1200, but the DRMS study report needs to be technically correct and clear.	These are the return period of the ground motions at the site contributed by all faults in the region. Uniform hazards.
2-3		Here, and elsewhere, references to previous publications need to be complete (with dates) so they can be located (presumably) in the references section. Example the reference under item "c" is bibliographically incomplete.	References have been added.
2-4		Page 2-9. Need reference to the Public Policy evaluation and also, what CALFED "program" …such cannot be tracked down without better referencing.	Proper references will be added.
3-1	**Section 3**	Section 3.5, page 3-5: Hazards. The risk summaries in the Summary included: flood failures, sunny day failures and seismic failures, but NOT wind/wave failures. This section notes wind/wave failures but NOT sunny day failures. ALL modes should be addressed in the Summary and here in the body of the report. This seems like a major inconsistency – in reporting at least – and perhaps in the risk calcs.	See response to comment 14 on the Summary Report.

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Reviewer: Goettel, U.S. Army Corps of Engineers			
3-2		Section 3.6: public buildings (damages and loss of use) are omitted. This list of "consequences" should be complete (others missing?)	Public buildings are included. A bullet will be added to the list.
4-1	**Section 4**	Page 4-3. bullets: the performance of levees characterized as mutually exclusive damage states is oversimplified, because for a given levee and a given earthquake ground motion, seismic fragility curves yield probabilities of each damage state, not an absolute determination of one damage state or the other. Plus, the P (failure) depends on the length of levee, closely analogous to pipe damage calculations. I presume that such was done for actual calcs, but it should be explained more precisely here. Levee-length factors were considered for flood failures (Section 7.6.4) but there is no evidence that length-factors were considered for seismic failures. If not, this is a significant omission/error in the seismic calcs.	For an earthquake of a given magnitude that produces a ground motion at a levee location, the seismic fragility curves do in fact define mutually exclusive states of levee performance. This must be the case, since a given levee reach cannot both fail and survive as a result of a given seismic event. The approach used for seismic was to divide the levees into smaller vulnerability classes (smaller levee reaches); each reach was analyzed for its probability of failure. So, implicitly the length effect was included because the probabilities of failure of each reach forming an island were considered.

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4.2		Page 4-3 bottom paragraph. A levee damaged from an earthquake has a higher P (failure) from floods or sunny day failures for some period of month or years until full repairs are made. I presume this was accounted for in the calcs? Probably doesn't make too much difference, since the P (failure) from seismic is a relatively small fraction of the P (failure) from floods and also because the duration of increased flood vulnerability is relatively short. Please verify that these effects were included in the analyses.	Most of the islands in the Delta are below sea level. If they fail during an earthquake, they instantaneously flood. The joint occurrence of a flood following an earthquake was not explicitly modeled. The effect of wind waves on flooded islands was considered.
4-3		Page 4-3 Emergency Response and Repair. For an event (seismic or flood) which results in many breached or damaged levees, the question of how to prioritize repairs is very important. I presume this is addressed carefully someplace in the DRMS report? OK – found this later.	No response required.

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4-4		Page 4-5, last paragraph about "Normal Events". The statement that even for 20 or 30 island failures that there is NO salinity intrusion impact that adversely affects Delta water quality and export is extremely surprising. Is this really true??? This conflicts with the statements at the top of Page 4.	The referenced statement is from the discussion about hydrologic (flood) events. Major floods that cause multiple levee failures involve a significant volume of freshwater inflow to the Delta over a period of time. As a result, hydrodynamic calculations that were performed for over hundreds of combinations of year types (wet, dry, etc. as available in the CALSIM trace) and event months (the month when the event occurred) indicate that there is no salinity impact that compromises water exports. Normal (sunny day) events occur one island at a time and, at worst, may disrupt water exports for three months. Although this is noticed by water users, it does not have large economic impacts. There are major water supply impacts under 20 or 30 breaches caused by seismic events in the Delta, especially during low flow periods.

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Reviewer: Goettel, U.S. Army Corps of Engineers			
4-5		Section 4.4.3 External hazards. The P (failure) from earthquakes depends not only on levee and site geotechnical characteristics and earthquake source/size, but also on water stage. A given amount of slumping in an earthquake may cause failure at a high water stage, but no failure at a low water stage. I presume that the seasonal variation in water stage was considered in the earthquake failure analysis. Please verify. There is no documentation of this in the seismic section.	This aspect of the comment is fully discussed in the Levee Vulnerability TM.
4-6		The discussion on page 4-15 is very important for projections for 2050 and later years. This section is very fuzzy. What are changes in frequency and severity of "normal forces" Etc? The key issues are: i. temporal changes in seismic hazard levels ii. temporal changes in sea level iii. temporal changes in elevations (subsidence), and iv. temporal changes in the seasonal patterns of rainfall/snow melt with resulting changes in the magnitude and timing of peak flows, v. possible changes in reservoir operations that may partially mitigate "iv". These issues should be explicitly stated here for clarity.	Section 14 addresses those concerns. We will add an explicit reference to Section 14 for a description of those effects.

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4-7		Several of the tables and figures in Section 4 do not appear to be referenced in the text and some may be numbered out of order vis-à-vis the reference locations in the text.	These will be corrected in this second draft.
5-1	**Section 5**	Section 5 (and throughout). In some cases various categories of assets are identified for the "Delta" which seems to be used as the Delta islands and tracts in some places and in other places to mean the statutory Delta plus the Suisun marsh area. VERY important to be consistent and clear.	The study scope is required to address the risk to the legal Delta and Suisun Marsh Area. Infrastructure includes government (public) buildings. Infrastructure asset inventory extends to the 100-year floodplain limit, which is beyond the Delta islands and tracts. Building contents are included. Outbuildings and vehicles are not included; we believe this is a relatively small cost compared with building contents costs.
5-2		Inventory here again omits public buildings and seems also to exclude inventory outside of the Delta Islands and tracts: inventory should be complete for the entire study area. Again, "buildings" doesn't seem to include contents, which is a big value for the entire study area.	Infrastructure includes government (public) buildings. Infrastructure asset inventory extends to the 100-year floodplain limit, which is beyond the Delta islands and tracts. Building contents are included. Outbuildings and vehicles are not included; we believe this is a relatively small cost compared with building contents costs.

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5-2 (cont'd)		Also, unclear whether the at-risk inventory includes outbuildings, vehicles, etc. which cumulatively are significant in total value at risk. It continues to be ambiguous/inconsistent whether term "infrastructure" includes buildings – the usage needs to be clear and consistent throughout. A clearer terminology would be infrastructure and buildings for the total inventory or total assets – since the common usage of infrastructure does not include buildings.	
5-3		Bottom of page 5-7, last bullets – no reason for not making reasonable guesses as to the value of the omitted assets – to make the results more complete. A reasonable guess is better than a zero!	Bulleted items were deleted.
5-4		Section 5.6 is repetitive of similar material presented previously.	Noted.
5-5		Table 5.1 has some odd values: 4 employees in natural gas distribution? Again, the definition of the geographic area meant by "Delta" needs to be explicit and consistent throughout.	The employment information is from data provided to URS. Many of the employees may work out of neighboring locations.
5-6		Many of the figures in Section 5 are not be referenced in the text.	See subtitle captions under linear assets (Section 5.5.1).

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Reviewer: Goettel, U.S. Army Corps of Engineers			
6-1	**Section 6**	As noted in earlier comments on the Seismic TM, the activity of the Delta faults may be overstated, with a corresponding overestimate of seismic hazard curves and an overestimate of P (failures). Not a huge effect, but not ideal.	No explicit conservatism was factored into the characterization of Delta faults. There is admittedly very little data and information, but the uncertainties in our model for the Delta faults were adequately addressed and incorporated. Our model recurrence compares reasonably well with the historical seismicity given its incompleteness.
6-2		The use of firm soil hazard curves, and presumably for the P (failure) calcs is baffling, given that the Delta does not appear to be firm soil. Softer soils will amplify OR deamplify, depending on the soils, the level of shaking, and the frequency (short period, long period) of ground motions involved.	The calculation of the site response was conducted for each site with the specific soil ground conditions, including organic deposits, soft clay, and loose sand. The Levee Vulnerability TM addresses these issues.
6-3		There is no documentation that the thick peat layers were considered in the seismic analysis (yes, they were considered in the levee failure modes). Won't these peat layers, in effect, act as isolation bearings especially for short period ground motions and really damp the ground motions? If so, then the P (failures) may be substantially and systematically overestimated.	See response above. The effects are represented in the site response analyses.

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6-4		In this context, the comment that there were zero (!!) failures in 1906 might be due to the peat layers not because "the levees were lower then" as stated in the text. Yes, the levees were lower, but surely they were non-engineered levees as well. The DRMS fragility curves imply that there should have been failures and thus the DRMS fragility curves almost certainly overestimate P (failure). This is a major inconsistency because the largest contributor to seismic levee failures is the San Andreas. This mis-match between 1906 and the current DRMS results reinforces my comments at the beginning that the seismic failure results appear to be well outside the bounds of credibility.	There is no detailed documentation of the conditions of the levees (heights, slopes, crest widths, locations, number of miles) to be able to reconstruct a model of the 1906 conditions and test it. There were also no detailed levee performance observations after the 1906 event against which we can calibrate the test runs. The 1906 information is not fully available to make a case on the expected performance of today's levees against future earthquakes.

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6-5		The fragility curves for levee failure show non-zero P (failures) at VERY low PGAs 0.05 and lower (I presume). If this is real, then the historical experience in more recent earthquakes Loma Prieta, Livermore, Santa Rosa that would have yielded small ground motions in the western Delta should have resulted in some displacements at least or failures. IF NONE, then the fragility curves are simply NOT correct at low PGAs.	The fragility curves represent the combination of the expected deformations under those ground motions and the probability of failure given those displacements. The calculation of the deformation curves have been fully checked and reviewed. The conditional probability of failure given the deformation was established by various experts who are familiar with the Delta. For levees that are susceptible to liquefaction, the calculations show that they will undergo large deformations at 0.1g. The fragility rises from 0.05g to 0.10g, and it is flat outside the range. Calculations can be found in the Levee Vulnerability TM.
6-6		The small P (failure) at very low PGAs 0.05 g or less may contribute a large fraction of the calculated P (failure) because the annual probabilities of these ground motions are orders of magnitude higher than the annual Ps for 0.3 or 0.4 or 0.5 g. Thus, the low PGA tails are very important in the overall risk assessment and should be re-assessed. See Comment #38 below.	See above response.

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6-7		More realistically, the P (failure) and the likely displacements should be set to zero below some truncation point. Fragility curves with truncation (no P (fail) below a threshold) are commonly used for utility seismic risk assessments.	The truncation point is 0.05 g.
6-8		Overall, the seismic fragilities simply appear unrealistic – with far too many breaks to be credible.	Noted. The team is currently in the process of reviewing the calculations.
6-9		Seismic Risk page 6-2. Items 3 and 4 and top of the page. I continue to be baffled why the seismic hazard results are presented for "stiff soil site conditions" when such does not appear applicable to the bulk of the Delta. This appears to be a major shortcoming of the entire seismic risk analysis, and was also noted by the CGS and USGS reviewers (as I recall from the seismic section comments).	The stiff soil is used as the reference site on top of which the site response is calculated (including peat and soft deposits). These were explained to USGS and CGS and they understand that a 2-D and 1-D (SHAKE and QUAD4 type analyses) were used to characterize the site response. The reviewer is directed to the Levee Vulnerability TM.
6-10		Section 6.1.6 page 6-5. As noted in my previous comments on the Seismic Section and in the CGS and USGS comments, the DRMS seismic team seems to have placed higher than consensus values on the frequencies of events for the poorly known near field sources (Midland north and south etc) in or near the Delta. Thus, the overall seismic hazard for the Delta may be somewhat overstated.	See response to 6-1.

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6-11		Section 6.2.1 Seismic failure modes. The failure modes for levees seem to assume slumping along the entire length of given levees. Yes...this might happen. But…it might not. Surely, heterogeneity in site and levee characteristics can result in localized slumping? Figure 6-32 shows a levee which slumps 10 feet. Surely, this is an extreme slump –and in reality levees may slump from a few inches to many feet… Were these plausible variations in levee slump failures considered probabilistically, or was only the extreme case of entire levees slumping by many feet considered. If the extreme case only was considered, then the seismic calcs almost certainly overestimate the probability and number of seismic failures.	For each island, the levees have been discretized into small reaches of similar characteristics (as far as the subsurface data can describe). These have been characterized as vulnerability classes. Each class (whether it is few feet or hundreds of feet) is expected to have similar seismic response.
6-12		Top of page 6-10. Initial free board is a key parameter for seismic levee failure calcs – free board varies with water stage, time of year. Was this considered in the analysis? If so, how? Such details need to be documented, otherwise it is impossible for anyone to follow the train of logic in the calcs. Being able to follow the calcs is a central tenet of any scientific publication.	The MHH was assumed as the water level to use in the seismic vulnerability analyses.

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6-13		The discussion on pages 6-11 and 6-12 is based on contiguous spatial zones of levees in a given levee vulnerability classification, along with the assumption that the entire zone slumps (if slumping occurs). This doesn't seem quite right because: surely there is a probability only a small section slumping, and intuitively a 1000' section of levee Class X would have a lower probability of a slump induced failure than a 50,000' section of Class X that experiences the same level of ground motion. So, to some extent seismic failures might be overestimated for small islands and underestimated for large islands. As noted above: length effects are explicitly considered for flood failures, but seem to have been ignored for seismic failures.	Most islands in the Delta are comprised of multiple vulnerability classes. For each island, all classes were modeled. To the extent that larger islands have more vulnerability classes, it is implicit that length effects are considered. In the seismic analysis, if a levee reach (vulnerability class) has failed, it is assumed that one breach occurs (therefore the island has flooded) and the remaining part of the reach was damaged (not breached) and thus also requires repairs. This model assumes a perfect correlation in the performance of all parts of a levee reach.
6-14		Table 6-3: the abbreviations should be defined in a footnote (BPT etc.)	Comment is justified and noted. Sorry that we missed making the change in this round.
6-15		Table 6-4: Models A…F should be defined. Very unclear what these are.	Comment is justified and noted. Sorry that we missed making the change in this round.
6-17		Table 6-5: Needs a footnote, explaining that the out year ground motions include time-dependent probabilities for some of the earthquake sources.	Comment is justified and noted. Sorry that we missed making the change in this round.

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6-18		Table 6-6. The Vulnerability Class Index is NOT defined and why are there 24? The parameters listed on page 6-3 should yield 20 classes not 24?? Define Ground motion (PGA?). An explicit list of the classes is required, especially since the seismic classes differ from the flood classes: both need to be explicit. It would be very useful to see the mathematical form of the vulnerability functions – otherwise no one can evaluate the meaningfulness of the results.	Vulnerability classes have been discussed in the text.

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6-19		The probabilities of failure data in Table 6-6 are confusing. (a) The magnitude dependence in Table 6-6 seems extraordinarily extreme. For Class 1, at 0.1 g (PGA?) the P failure is 0.0038 for an M 6.5 and 1 (!!!) for an M 7.5. Is this a duration effect or simply that smaller earthquakes are assumed not to result in liquefaction? I don't understand why there is this gigantic M dependence. Needs to be explained in the text. (b) The ground motion dependence for some vulnerability classes looks like a step function not a lognormal fragility curve. How can the P (fail) be 0.0038 at 0.05, 0.1 and 0.2 g then jump to 1? Is this step a no/yes on liquefaction? Doesn't seem to make any sense. Needs to be explained in the text. Why does the "jump" happen at low g for some classes, while other classes have much more gradual increases with g? (c) Normally, so to speak, fragility curves have big betas so the P (fail) varies smoothly and broadly with increasing ground motion – that's what I expected to see.	a) For M=5.5 the site liquefies at 0.3g, for M=6.5 the site liquefies at 0.2g, and for M=7.5 the site liquefies at 0.1g. The larger magnitudes have a longer duration of shaking. The liquefaction triggering is also related to magnitude. b) It looks like step function only for the site that is susceptible to liquefaction. Once it triggers, the failure becomes large. c) It is correct for non-liquefiable site; please see fragility functions for non-liquefiable sites. d) The calculation of deformation induced by liquefaction of sandy levees is presented in the Levee Vulnerability TM). Both simplified Newmark analyses and 2-D finite element time-domain analyses are presented in that document.

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Reviewer: Goettel, U.S. Army Corps of Engineers			
6-19 (cont'd)		Presumably the assumption of liquefaction is an important factor in driving the "jumps" in the P (failure) table. In reality, whether or not liquefaction occurs and if it does occur to what extent should be probabilistic and gradual rather than yes/now and if it occurs assume a tremendous amount of slumping. In my opinion, the seismic failure results are not credible without further documentation of the calculations.	
6-20		Figure 6-34 shows contours of thicknesses of organic materials near levee toes – there are substantial thicknesses over nearly all of the Delta area. It seems to me that these thick layers of peat will, in effect, act as isolation bearings to really damp ground motions, especially at shorter periods or for PGA calcs. I see no documentation that these layers were considered in the seismic fragility modeling – noting the "stiff soil" statements about seismic hazard curves which were presumably used for the risk assessment. If so, it seems possible that the P (failures) for levees may be substantially overestimated. This is an important issue to address. If the calcs were indeed done properly, taking into account the peat, then it surely needs to be documented in the report!	The answers are provided in the Levee Vulnerability TM. The reviewer is referred to it.

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No.	Section	Comment	Response
Reviewer: Goettel, U.S. Army Corps of Engineers			
6-21		Figure 6-37 – equations in the figure need to be defined in a footnote.	Comment is justified and noted. Sorry that we missed making the change in this round.
6-22		Figure 6-38, 6-39 H1 and H2 need to be defined.	H_i 's are the two horizontal components. They will be defined.
6-23		Figure 6-39. The displacements must depend on the assumed thickness of the liquefiable foundation layer as well as on thickness of the peat. What was assumed? Perhaps just the worst case scenario?	Those are the deformation functions for the none liquefiable sand in the foundation. This is just a sample of the deformations curves. All curves for all classes are presented in the Levee Vulnerability TM.
6-24		Figure 6-40. Displacement must depend on the assumed thickness of the liquefiable levee sand layer. To get 10 feet of displacement, the liquefiable sand layer must be considerably greater than 10 feet thick. Is this realistic and representative or just the worst case? If the worst case, then P (failure) is overestimated, which certainly seems to be the case.	The FLAC Model was used for the conditions at the site. The data for these cross sections can be made available. Two different companies were given the raw data and the earthquake time histories and were asked to calculate the deformations using their own version of FLAC and modeling. They came up with similar results.

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Reviewer: Goettel, U.S. Army Corps of Engineers			
6-25		Figure 6-41. The P (failure) at vertical displacements of only a small fraction 20%, 30% 50% of freeboard seems very high. What is the basis of these assumptions? Since these displacements will occur at lower PGAs with much higher probabilities, the failure Ps are likely dominated by this part of the curve. Overall, it seems as though the P (failures) are consistently estimated over-conservatively – that is, the P (failures) seem to be overestimated systematically. For example: Figure 6-40 implies that for a M7.5 event this type of levee has a 10% chance of displacing 10 feet at all PGAs >0.10. This seems REALLY EXTREME. Surely there is a gradual rise with PGA and also a wide variation from levee to levee in the thickness of liquefiable levee sand so a realistic, representative curve cannot be a step function.	Eight experts in earthquake-induced deformation modeling were asked to develop independently the conditional probability of failure for a range of percentages of free-board loss. This curve was developed solely using expert elicitation.
7-1	**Section 7**	My expertise is certainly not H & H and so I will make only a few general comments on this section.	No response required.
7-2		The analysis and data on inflows seems out of proportion – perhaps more detail than necessary.	No response required.
7-3		In contrast, the discussion of levee vulnerability, which probably has more uncertainty than the H & H, and which contributes directly to the calculated P (failure) seems thin overall with not a lot of documentation or discussion.	A large volume of work was devoted to the levees in the Levee Vulnerability TM.

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Reviewer: Goettel, U.S. Army Corps of Engineers			
7-4		Page 7-3 first paragraph. Another important factor is levee improvements over time, which presumably reduces out of channel flows upstream and thus may also contribute to the temporal pattern of high flood flows into the Delta.	Noted.
7-5		Section 7.5.1 Historical Failures. There is a nice discussion with figures illustrated average annual number of failures over various time periods. Obviously, interpretation of these data is complicated by temporal changes in levees (especially) as well as by the relatively short record, with distortions in failures from a few events. Nevertheless, it seems appropriate for the DRMS study to interpret the record and draw a conclusion with the best estimate of the likely failure rate under current conditions. The omission of a conclusion is a significant shortcoming for this section is the lack of direct comparison of the historical rates of failure with the stated dreams value of 2.60 (NOT referenced in this chapter at all) or the tabulated value of 7.35 failures in Table 13-4, which is not mentioned anyplace in this TM.	The intent of Section 7 was to provide methodology and data. The results are contained in Section 13. A map of historical failures has been added to Section 13 to provide some perspective for the modeling results.

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Reviewer: Goettel, U.S. Army Corps of Engineers			
7-6		In the above context, the failure rate of 1.32 per year from 1951 to 2006 is probably a reasonable median estimate, with the 1.62 from 1981 to 2006 being a reasonable upper bound (distorted by a few events with return periods longer than 25 years). As noted earlier, I continue to be baffled by the statement in the introductory summary of an average annual expected number of failures of 2.6, which appears substantially discordant with the historical data (not to mention recent improvements in levees). Note added: this comment applies much more for the tabulated value of 7.35 flood failures for year in Table 13-4.	The flood numbers have been revised.
7-7		Page 7-10. Through-seepage, last sentence. The numerical results need clarification. If 40% of historical failures are from through-seepage, the annual probability of through seepage failure only if none of the difference between the historical failure rates and the DRMS estimated failure rate of 2.6 per year is due to through-seepage. 40% of 2.6 is over 1.0 per year. Again, the difference between the historical rate and estimate rate of failures needs to be CLEARLY explained.	The numbers and the text have been revised.

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Reviewer: Goettel, U.S. Army Corps of Engineers			
7-8		To non-specialists, the results in Table 7-12 are confusing – several of the column headers are not defined and it is unclear what the confidence levels or the numbers under the confidence levels actually mean. Needs to be explained very clearly.	Section 7 has been revised.
7-9		Table 7-12 is for no human intervention. The text on Page 7-13 and Figure 7-26 show a substantial reduction in P (failure) with intervention. However, the final results (Figure 7-27) and Table 7-12 evidently discount human intervention. Why? A reasonable guess as to the extent and effectiveness of human intervention seems to be required to give a realistic "best guess" estimate of P (failures) from these modes. Ignoring human intervention results in levee failure Ps from this mode which are systematically too high.	The analysis has been revised to include human intervention for the smaller events. However, as the magnitude of the flood increases, human intervention becomes ineffective.
	Section 8	The last paragraph of this section notes several exclusions from the study. The omission of the joint probability of high wind/wind waves and high water levels seems like a major omission, since high water times are exactly when high wind/wind waves would be most likely to cause failures. This omission would seem to result in a systematic underestimate of failures from wind/waves.	The wind-wave information was primarily used to estimate erosion on the land side face of the levee after a levee had been breached and flooded. The erosion was applied to the case after the storm passed and the water levels were tidally driven.

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No.	Section	Comment	Response
Reviewer: Goettel, U.S. Army Corps of Engineers			
		After reading Section 8 Wind and Wave Risk Analysis, the section seems profoundly incomplete in that the methodology is discussed schematically but there are NO results presented and NO risk analysis for levee failures.	The results and risk analysis are provided in the Risk Analysis Report. The purpose of the wind wave analysis was to provide inputs to the risk analysis and Emergency Response and Repair TM.
		no wind data are presented – it would be useful to see some wind hazard curves arising from the modeling,	Wind hazard is discussed in the Risk Analysis Report or the Emergency Response and Repair TM.
		similarly, no wave height data are presented – it would be useful to see some wave height – frequency curves for various locations	Tables 2 and 3 provide lookup tables for values of wave heights and periods. Frequency curves for wave heights and periods were not generated, as they are not needed for the emergency response analysis.
		NO risk assessment is done for wind/wave failures. What is missing is the P (failure) for various levees from wind/waves. Why is this completely omitted from the discussion? If wind/wave failures are negligible for levee failures (which seems very unlikely) this should be stated, but it not, then the P (failures) need to be calculated and added to the other failure modes – flood, sunny day, and seismic.	The failure due to wind/waves was not calculated. There are no known failures due solely to wind waves.

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Reviewer: Goettel, U.S. Army Corps of Engineers			
		Presumably, there are results in the TM dealing with Wind/Waves…but it would be very helpful to have a synopsis of results here in the Risk TM and surely the P (failure) from wind/waves needs to be included in the overall P (failure) compilation in the Risk TM.	The purpose of the Wind-Wave TM was to provide inputs to the emergency response analysis; therefore, there are no results.
	Section 9	The Sunny Day analysis is admirably direct, concise, and clear.	Noted.
		One significant question: failure rates per year per levee mile were calculated separately from the 6 Delta failures and 2 Suisun failures: "These rates" will be applied to all levees for each study area. 6 and 2 failures are certainly small numbers. Are these rates being applied separately to the Delta and Suisun or combined into an overall P (failure) and applied to all levees within the MHHW boundary?	Yes.
		The levees differ between the Delta and Suisun, but 2 is an awfully small number from which to infer statistically meaningful results. So…perhaps combining the two data sets to get 8 failures in 56 years in 986 miles is the best available estimate: 1.45 $x10^{-4}$/year/levee mile, which is much closer to the Delta value than the Suisun value because there are so many more miles of levees in the Delta.	There are enough different foundation and levee conditions between Suisun Marsh and the Delta to warrant keeping them separate.

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Reviewer: Goettel, U.S. Army Corps of Engineers			
		Also, it would seem that modern engineered or semi-engineered levees would be less vulnerable than older non-engineered levees. Is this trend evident in the historical data? If so, then the sunny day failure rate should be disaggregated into levee classes. If not, then it should be stated that there simply isn't enough data to sort sunny day failures by levee class.	There are simply not enough data to develop any meaningful statistical model.
	Section 10	The narrative notes correctly that three logical factors for prioritization of response are: population, infrastructure, and effects on salinity.	Agreed.
		Another factor, which is related to, but not the same as population is life safety – which depends on population, depth of flooding and rate of inundation.	The commenter is correct. In presenting the present version of our risk modules, we were unable to include this refinement. We look forward to including it in the next version.
		The relative rankings by the three factors seem reasonable, but the section complete omits analysis/discussion/conclusions on the central issue: How are these factors combined? That is, if three islands are at imminent risk or have failed with one each having high priority for population, infrastructure, and salinity, which one is responded to first? This is really the bottom line key question which was completely sidestepped by this section.	The discussion in the final five paragraphs of Section 10.5 describe the relative priority order established. These priorities were explicitly listed in Table 10-2. To provide additional clarification, an additional table (Table 10-6) was added setting forth the resulting priority order of all the islands given the damaged but not yet flooded (Category A)versus already flooded (Category B) condition.

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Reviewer: Goettel, U.S. Army Corps of Engineers			
		A similar sidestep was done for infrastructure – with separate priority groups for four types of infrastructure and three highways – what is needed is a combined priority for infrastructure with explicit priorities not relative priorities for different classes. That is, if the Mokelumne Aqueduct is more important than Highway 4 or the railroad or gas storage, then it needs to be so stated (or visa-versa).	The infrastructure priority is explicitly established in Table 10-4.
		What is really needed here is an explicit combination of the factors into a weighted priority that literally and explicitly ranks every island/tract by importance and priority for response from 1 to the last one. Is such a ranking politically controversial?	Yes, such a ranking is politically controversial. But such a ranking was given anyway because it is essential for running a repair model. Apparently, the ranking was not stated with adequate clarity. Table 10-6 explicitly states the resulting priority for Category A (unflooded) islands and Category B (flooded) islands.

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No.	Section	Comment	Response
Reviewer: Goettel, U.S. Army Corps of Engineers			
	Section 11	Salinity Impacts. The introduction to this section provides an excellent synopsis of the complexity of salinity impact modeling. The impacts of initial flooding appear straightforward, although are a very large number of combinations of individual island floodings or near simultaneous multiple island floodings. The real complexity arises from the human-controlled responses which involve upstream releases, in Delta use for agriculture, and water export, with competing/conflicting priorities between agencies and end users.	Agreed.
		Section 11.3 notes that WAM assesses in-Delta demands based on normal irrigation net consumptive use, adjusted for flooded island. This assumption appears very restrictive, because one response alternative to a salinity event would be to restrict or eliminate in-Delta water use for agriculture. The options of changes in normal operations for reservoir operations and exports are including, so it is puzzling why changes for in-Delta use do not appear to be considered in the analysis.	Changes of in-Delta uses for irrigation are not included except when forced by island flooding or high channel salinity. This is because in-Delta water users have riparian or senior water rights and are also able to act independently. There is no current policy or plan or mechanism in place to restrict in-Delta water use and enforce the restriction. Thus, such an approach is not business-as-usual. The details behind this are presented in the WAM TM, Appendix B.

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Reviewer: Goettel, U.S. Army Corps of Engineers			
		In much the same manner as Section 8 discussed above, Section 11 presents a brief overview of the methodology, but essentially no results.	Results are presented in Section 13.
		Presumably, results are presented in another TM, but it would be very helpful for summary results to be included in the Risk TM.	Results are presented in Section 13 of the Risk Analysis Report.
	Section 12	Page 12-1 notes the importance of life safety modeling, but then declines to make estimates of life safety risk. Estimates of potential casualties can be made based on a range of warning times (or not), evacuation effectiveness (or not), and flood rise times for specific tracts. Given the importance of life safety in policy decisions, there is no reason not to make such estimates.	Population at risk will be noted. More detailed modeling of life safety risk is not feasible with the presently available schedule.
		Page 12-3 economic costs – direct damage to "infrastructure" is incomplete since such estimates should (and do) include buildings (and contents?). The definition of economic costs is more restrictive than that used in the Economics section, which included some other categories including displacement costs for temporary housing (per FEMA methodology). Several other categories of costs are also included later in Section 12 – so the initial language about "costs" based on Federal flood control project calculations is not entirely correct.	For the DRMS project, the term "infrastructure" is used to designate all structures and buildings, and their contents. The damage costs in Tables 12-1, 12-2, and 12-3 are not scenario related; clarified by footnotes.

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Reviewer: Goettel, U.S. Army Corps of Engineers			
		The environmental impacts narrative appears reasonable and balanced, but this not my area of expertise and so I do not provide detailed comments here.	No response required.
		Section 12.2.1 Repair and Recovery Costs. As I recall, the economic TM suggested some cost multipliers for multiple-breach scenarios. Such multipliers are important and, for consistency, should be referenced in this section. These multipliers are mentioned in Section 12.2.2, but not in this section.	We believe the reference in 12.2.2 is adequate.
		Section 12.2.2 direct damages: again, the “assets” include a huge amount of buildings, which only obliquely referenced in the general discussion of damage to “infrastructure”.	The analysis includes the contents of structures.

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No.	Section	Comment	Response
Reviewer: Goettel, U.S. Army Corps of Engineers			
		Page 12-17, the cost scaling factors for repairs to buildings and infrastructure appear excessive, at first glance. Most of the island have relatively modest inventory of assets, and, at first blush, multiple island breaches would seem most likely to affect repair times to a much greater extent than repair costs.	The cost for repairs due to multiple island failures is likely to be more than for a few island failures due to many complexities. If there is a sudden increase in the demand for construction, it is expected that the construction costs would increase due to the supply and demand issues for equipment, labor and, materials. With multiple levee failures, scaling factors (multipliers) have been used to increase costs of repair. The insurance industry refers to what has been termed “scaling factors” in this TM as “post-event inflation” or “demand surge.”

No.	Section	Comment	Response
Reviewer: Goettel, U.S. Army Corps of Engineers			
		Page 12-17 (cont'd)	To support the use of scaling factors, literature from post-catastrophic events was reviewed, including Hurricane Katrina (see Section 8, References, of the Infrastructure TM for websites used to estimate cost scaling factors for multiple island failures). Estimates of scaling factors were found to typically range from about 1.2 to 2.2 for extreme catastrophic events such as Hurricane Katrina. Based on the literature review, cost scaling factors estimated for multiple island failures are indicated below. For multiple island failures (up to 30), scaling factors will be applied to the estimated costs. Linear cost scaling factors (for both point and linear assets) that would be applied to more than five island failures are as follows: • 1 to 5 island failures: 1.0 • 10 island failures: 1.2 • 20 island failures: 1.6 • 30 island failures: 2.0

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Reviewer: Goettel, U.S. Army Corps of Engineers			
		Page 12-18. Residential structures. The displacement costs total across all analysis zones are NOT meaningful – what would be meaningful would be such costs tabulated for individual zones – the totals are dominated by a couple of urban zones. Or – at this ambiguity really must be removed – perhaps the quoted numbers are for the Delta islands only.	See the Economic Impacts TM; these costs are estimated by analysis zone when evaluating a particular breach event. Section 12 summarizes the methodology.
		To be meaningful, ALL of the Costs/Losses must be disaggregated on an individual analysis zone basis – totals are NOT really meaningful (without big caveats every time such are quoted) – because no plausible scenario floods all of the analysis zones in the same event. To prioritize mitigation measures, all results must be dis-aggregated by individual analysis zones.	Above response applies to this comment as well.
		This narrative repeats much of what is already in the Economics TM.	Noted.
		Tables 12-1 and 12-2 quotes "asset cost damage" island by island. It is critical to define exactly what assets are or are not include in "asset cost damage." Such definitions should be in the title or footnote on every table.	The following was added as footnotes to Tables 12-1, 12-2, and 12-3: For the DRMS project, the term "infrastructure" is used to designate all structures and buildings, and their contents. Contents, environmental cleanup costs, and debris removal are included in damage loss estimates.

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No.	Section	Comment	Response
Reviewer: Goettel, U.S. Army Corps of Engineers			
		Table 12-3 has asset damage for scour – same comment as above. From the economics TM, it appeared to me that scour damages were to be calculated assuming that the entire "potential" scour zone around the perimeter of an island is assumed to undergo scour. If this was done, then the scour damages are gross overestimated, since any given event will result in scour in only a small fraction of the total perimeter – that is, at the breach location. Please clarify and correct calculations if necessary.	The following paragraph was inserted as a footnote to Table 12-3: Scour was not assumed for the entire scour-prone zone of each analysis area. Scour holes could occur anywhere within the island perimeters.
		Table 12-4: Business sales and costs are meaningful only on an analysis zone basis –totals are not meaningful – see Comment "h" above. This is a substantial shortcoming in the presentation of results.	See the Economic Impacts TM; these costs are estimated by analysis zone when evaluating a particular breach event. Section 12 summarizes the methodology.
		The bases for some of the tables (12-6, for example) are not given in the text. References should be given to the sources and documentation (e.g., the economics TM).	Sources added.
		Overall, my previous comments on the Economics TM apply here and edits to the Economics TM should be carefully propagated here for consistency.	Noted.

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No.	Section	Comment	Response
Reviewer: Goettel, U.S. Army Corps of Engineers			
	Section 13	Sunny Day: as noted above, it seems worth considering a refinement of sunny day risk by class of levee (at least engineered and non-engineered), and I'm skeptical of the significance of the Suisun estimate which is based on only two sunny day failures – perhaps combining the Delta and Suisun data would be more meaningful.	The analysis of available data on sunny day failures was not seen as supportive of this refinement. However, such a refinement can be reconsidered in a future analysis phase.
		Seismic Risk: Table 13-2 is extremely informative – it should be also in Section 6 Seismic. At first glance, the calculated annual number of failures is, to be polite, "extraordinary." Many islands have annual probabilities of failure of 5% or more, with 55 having annual probabilities of 3% or more. There is no total expected annual number of failures shown (should be added!) but by eyeball the number appears to be about 3 (wow!!), or roughly 300 per hundred years. 41 islands have a 99% chance or greater of seismic failures in 100 years and 72 have a 75% chance or greater. Given that the observed number of seismic failures in the past 100+ years is …ZERO… these estimates seem simply extremely high.	Results are presented in Section 13 only to avoid redundancy. The seismic risk estimates were carefully reviewed, and adjustments have resulted in lower estimates.

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No.	Section	Comment	Response
Reviewer: Goettel, U.S. Army Corps of Engineers			
		Seismic Risk: Table 13-2 (cont'd) Something appears to be wrong with the seismic risk calculations and then entire seismic risk calculation, especially the fragility curves, needs to be re-examined and perhaps corrected. These results do not appear to be a realistic representation of the level of seismic risk. The seismic hazard is well understood (albeit perhaps slightly overestimated), so the likely source of the apparent errors is in the fragility curves which may overestimate the P (failure) at low PGAs.	The seismic risks estimates were carefully reviewed, and adjustments have resulted in lower estimates.
		As an example, consider an individual levee with a 5% annual chance of failure from seismic events. In 10 years, the P (failure) is about 41% and in 100 years the P (failure) is about 99.5%. So, even allowing for some time dependence in seismicity, the above calculations are simply discordant with the historical record. That is, for this not-the-most-extreme example, there is less than a 1% chance of no failures in 100 years – in conflict with the historical record.	The commenter needs to recognize several factors that are relevant: 1. Seismic failures will tend to occur in groups – many islands in the same event. 2. The last 100 years have been relatively quiet for the relevant seismic sources and magnitudes. 3. The levees have evolved to a more vulnerable state over the last 100 years. Nonetheless, the seismic risk estimates were carefully reviewed and adjustments have resulted in lower estimates.
		Table 13-3 –seismic source contribution simply doesn't need to be here – similar is in Section 6 Seismic and it serves no purpose to repeat it here.	The seismic source contributions to the risk results are supposed to be reported only in Section 13.

No.	Section	Comment	Response
Reviewer: Goettel, U.S. Army Corps of Engineers			
		Table 13-4 Flood Risk - is extremely informative – it should be also in Section 7 Flood. At first glance, the calculated annual number of failures is, to be polite, "extraordinary," albeit not as extreme as the seismic results above. There is no total expected annual number of failures (should be added! but the total is approximately 7.5 per year. Where did this number come from? The observed failure rate for 1951-2006 is 1.32 per year and the previously quoted DRMS estimate (which is NEVER discussed in the text, only in Summary of Risk and the introductory Summary the main Risk Chapter) is stated as 2.60 per year. 2.60 is within the realm of reason, if the probability weighted effects of large floods (beyond those in the short historical record) were included to supplement the historical record, but 7.5 per year seems out of bounds. The entire flood risk calc, especially the fragility curves, needs to be re-examined and perhaps corrected. My guess is that the flood hazard is presumably well modeled and well understood, so it is likely that the fragility curves overestimate P (failure).	Flood risk results are reported only in Section 13. Section 7 presents the analysis method and describes the important inputs. The flood risk estimates were carefully reviewed, and adjustments resulted in lower estimates.

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No.	Section	Comment	Response
Reviewer: Goettel, U.S. Army Corps of Engineers			
		As noted above, the failures from wind/waves seem to be omitted from the calcs. What happened to these?	Wind/wave impacts were judged to be most relevant to the conditions of interior levee slopes on flooded islands. This was particularly important in multi-island flooding events with long repair periods. These impacts were calculated in the Emergency Response Module
		Table 13-4 presumably simply adds the seismic and flood P (failures) and is thus subject to the apparent overestimates of P (failure) above. As above, a total expected annual number of failures is an important number and all of these (presumably to be corrected) tables need to have totals shown for comparison and reference.	Summing the mean annual frequencies of individual island failures is not appropriate; it can be misleading.
		Table 13-7 has scenario damages for various multiple levee failures in seismic events. These scenarios really need to be defined: which islands are assumed to flood??? The results are not useful or meaningful without specifics of the assumptions.	Specific scenarios were defined and figures were provide showing which islands were flooded in each scenario.
		Tables 13-9 to 13-20 contain a vast amount of information that needs better definition. This section does not define: Maximum Adverse Risk Factor with any degree of understandability, the breach codes are not defined etc. The information is of minimal use in the present format.	Improved definitions and discussions of results have been provided.

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No.	Section	Comment	Response
Reviewer: Goettel, U.S. Army Corps of Engineers			
		Figure 13-2, in the text, this figure is stated to be the source contributions for all of the islands. But…in the figure the bottom caption states: Case 2 and 3 Scenarios Island Flooded, which is not the same thing.	The figure was mislabeled. It has been corrected.
		Figures 13-7 to 13-13 show particular islands flooded in various scenarios. The results of these scenarios are NOT very informative, because there are many different scenarios with different island(s) flooding, with substantially different economic impacts depending on which islands are selected. What are these figures and data intended to illustrate?	The scenarios are intended to illustrate how costs and impacts change as the number of islands flooded as the result of a seismic event increase.
		In evaluating Tables 13-2 (seismic) and 13-4 (flood), I don't understand why there are 86 areas considered for seismic failures, but 126 for floods. Certainly all areas have some degree of seismic failure probability??	Seismic failures were analyzed within the mean higher high water boundary. Flood failures were analyzed for all areas within the 100-year flood areas, which are more extensive.
	Section 14	The extrapolations to 2050 and 2100 appear more or less reasonable with suitable caveats on the uncertainties inherent in such extrapolations.	Caveats have been added.
		It is not clear that some of the technical information (sea level rise for example, is consistent between Chapter 14 and other places – such as the flood chapter and the climate change chapter: as individual TMs are updated, propagation of the changes through other TMs is essential and requires editorial diligence.	Agree.

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No.	Section	Comment	Response
Reviewer: Goettel, U.S. Army Corps of Engineers			
	Section 15	I have two versions and it is not entirely clear which is the current version – I assume it is the separate Chapter 15, not the one bundled with the Risk TM.	Assumption correct.
		This section is somewhat rambling and not precisely written. Needs a lot of editorial polishing, but after the technical results are firmed up. Some of the "conclusions" appear to be stated here for the first time, rather than being a synopsis of conclusions drawn in the individual chapters of the TM. All conclusions here really must be documented and stated in the corresponding sections of the TM.	The section is being extensively edited and reorganized.

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No.	Section	Comment	Response
Reviewer: Jerry Johns, Deputy Director DWR			
	Executive Summary	This is a very interesting report and has some very important conclusions. The Executive Summary could be better written to make these more evident. Right now it is just a complication of points from the chapters and could be better organized into a summary that regular people will read and take away important messages. For example, if I am reading table 5 correctly, it is a coin toss (50% probably or greater) that the major west and central delta islands will fail individually or in combination within the next 10 years. I don't see that summarized anywhere. Also, the figures use an annual frequency of failure. You need to help folks do the math to get a likely failure over a period of years. Can you just multiply the number of years by the annual failure percentage? If so does that mean that all the red area on Figure 15 have a greater than 2/3 chance of failure in 10 years and a 140% chance in 20 years. Does that make it a certainty? If so that does not seem to match with the data on table 5. Bottom line is that this could be made so much more user friendly with a little editing and some sample math calculations for people.	Replaced bullets with text, removed tables, and revised figures in the executive summary of the Summary Report. We edited the discussion on probability of failure; we hope that the revised figures are clearer.
		This whole report paints a very dire situation. Are we sure the data and analysis are correct. I would hate to have someone poke holes in this from a technical point of view and argue that we are just trying to scare people. Has this been pier reviewed?	The first draft was not fully peer reviewed. The second draft underwent internal peer review, but an external peer review was not conducted. Both seismic and flood risk results have been modified from first draft.

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No.	Section	Comment	Response
Reviewer: Jerry Johns, Deputy Director DWR			
		The first point in the executive summary the report has reference to 0.1 g and 0.2 g or higher. I have no idea what that means is terms of earthquake magnitude. Can you convert this to something the rest of us can put into context? Like Richter scale or reference to past earth quakes we have seen in California. This is a pretty technical summary and can be made more understandable with a little work.	More explanation has been provided in Section 6, Seismic Risk Analysis.
		Point number 4 mentions a frequency of flood related failures of 2.6 per year. Has not been our recent experience has it. It would be good to give right here what we have seen in then past to give the report some creditability. Many will try to poke holes in this. The more we tie this back to experience the better. The main body of the report has some of this information but it would be good to have it in the summary.	The main report explains the results in context of historical experience. Historically, there have been 166 failures that led to island flooding since 1900. The new results for the flood hazard project 209 potential island floodings in the next 100 years.
		It would be good to have someone check the ecological consequences. I don't think it will be as dire as the report states on Delta Smelt. If habitat is lost other habitat will likely open up. Certainly large open bodies of water will not be great for most of the endangered fish but don't over sale this. Talk to Ted Sommer about the smelt.	The findings have been reworded.
		The executive summary uses MHHW without defining it. It is defined in the report but also needs to be defined here.	Done.
		The economic costs need to make clear what are in-delta costs and what are impact costs or impacts out of the Delta. It looks at first blush that these are in-Delta impacts when I'll bet they are out of delta Impacts.	Done.

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No.	Section	Comment	Response
Reviewer: Jerry Johns, Deputy Director DWR			
	Page 3	The report uses the business-as-usual as the baseline for the analysis. I can see the logic of this, but what is the cost of the baseline. It would be good to point that out. In that the COE will likely not support the restoration of islands in the future does this assumption assume that the State will repair all the levee breaks or is it more a business as-usual with regard to levee maintenance. If the latter it would be good to make that specific.	More detail added.
	Page 9	Has bullet on the value of Delta Business of 35 billion annually. It would be good to break this out from Sacramento and Stockton and see what the real in delta primary zone business value is. The parts of the delta that need protecting in terms of business value are not the same across the Delta and are not evident on some of the most vulnerable islands.	Done.
	Page 9	The disruption of through delta and export supplies is discussed. It states that 27 million people water supply would be affected. If we have 36 million people in California that 3/4 of the people of the State. That's a big number and should be emphasized.	Clarified that about ¼ of California's urban water comes from the Delta.
	Page 15	This report is of course a summary report. But some information about how the calculations were made would be a help. On page 15 the individually analyses are summarized and some review of how this was done or reference to a report where more detailed analysis can be found would be a big help.	Brief discussion of methodology has been provided for seismic, flood, and sunny day failures.
	Page 21	Page 21 represents that the seismic risk in 2050 and 2100 will increase (I assume from today's levels) by 12% and 27 %. Why? I again assume that it is due to sea level rise but that is	This point has been clarified.

Delta Risk Management Strategy (DRMS) Phase 1
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No.	Section	Comment	Response
Reviewer: Jerry Johns, Deputy Director DWR			
		not clear. The same goes for a similar conclusion on page 24. The sea level impacts need to be more clearly stated.	

Delta Risk Management Strategy Phase I
Response to Comments on Risk Analysis Report

No.	Section	Comment	Response
Reviewer: Wim Kimmerer, Steering Committee Member, May 20, 2007			
1	**General**	I read or skimmed this entire document. It is reasonably clearly written although it needs the firm hand of an editor. The graphics are outstanding, but need to be integrated better with the text in some places.	No response required.
2	**Sections 6 and 7**	I know little about seismic or flood risks, but I read through those sections by way of preparation for my review of the ecological impacts. Somewhere there ought to be a discussion of the magnitude of the risks to individual islands from seismic or flooding failures. Specifically, there should be a clear explanation of why more than half of the islands have >3% annual rate of seismic failure (equivalent to a 23-year interval between individual failures) given that there has never been a seismic failure of a Delta levee. I am not arguing that these numbers are impossible, although they seem to be; assuming these numbers have been well-checked, I am merely pointing out the obvious discrepancy with ordinary experience, and suggest some explanation is in order. It should go in the executive summary too.	The seismic results have been updated in the report. The seismic risk cannot be viewed strictly in the context of the most recent seismic experience in the Delta. For example, there has been a relatively quiet period in the Bay Area for some time, but no one would argue that the seismic risk is low.
3	**Sections 6 and 7**	Similarly, 23 islands are said to have a probability of flood failure > 7%. Neglecting the islands with lower failure rates, and using only the 7% figure, this means that there are 23 islands that should have been flooding at least every 10 years! Yet the 23 most-flooded islands had between 3 and 8 breaches in 107 years. Has the risk really increased by ~3-fold? It would seem not given the time course of previous failures (most of which were due to floods). This demands a	The risk results have been updated, but the risk is still relatively high.

No.	Section	Comment	Response
Reviewer: Wim Kimmerer, Steering Committee Member, May 20, 2007			
		clear and prominent explanation.	
4	**Section 13**	*The section of the Risk Assessment on ecological consequences is inadequate as it stands.* There are several basic problems with this section (mainly 13.4 with associated tables). First, this section is weakly linked to the rest of the Risk Assessment. There is no overall statement of how the results of the ecological analysis tie in with the rest of the RA.	In the version of the report that was reviewed, specific risk results were not provided. The present version of the report has similar issues. The results of the ecosystem analysis (aquatic in particular) for the scenarios evaluated appear to be too coarsely defined to quantify the risk.
5	**Section 13**	Second, as discussed above the various calculations are not really shown. Equations are given somewhere, but it is just not clear how the numbers in the tables were determined. A reader should be able to replicate those analyses with the information provided, but I don't think that is possible.	We agree with the comment about the level of detail. However, typically a project report will not provide enough detail so one could replicate the results. In the time available to complete the next version of the report, we were not able to prepare a detailed description of the risk analysis methodology.
6	**Section 13**	Third, there is no interpretation of these results. What does it mean if delta smelt suffers xx% of the maximum adverse risk factor? The tables starting with 13-9a are incomprehensible.	The text has been expanded to offer further description and understanding of the risk index.
7	**Section 13**	Compare with Table 13-8 which gives economic losses for each scenario in different categories. Regardless of the relative degree of confidence in the economic and ecological numbers, at least this table can be read and understood, it is compact, and needs very little explanation. Not only are the	The probability of extinction could not be calculated at this time.

No.	Section	Comment	Response
Reviewer: Wim Kimmerer, Steering Committee Member, May 20, 2007			
		ecological numbers uninterpretable, they don't even address the key issue, which is probability of extinction. This is raised in numerous places, and then the only apparent result of all this is the statement on page 13-5 that delta smelt "...have a high risk of population-level impacts potentially leading to species extinction as a result of a large-scale levee failure." If this is supposed to be a risk analysis, why were the risks not analyzed? Probability of extinction is about the only ecological consequence that reaches the level of severity of some of the other impacts (e.g., economic, infrastructure, life and property). Since risk of extinction is inversely related to population size, a calculation of this risk would seem to be more important than a lengthy, pseudo-quantitative analysis such as that presented here.	
8	**Section 13**	The summary of ecological risks that appears in the Executive Summary could have been written without doing the analysis. Likewise, a lot of the discussion in Chapter 13 is not directly relevant to the RA. Some of it that may be relevant could be triaged - i.e., some things are big (e.g., initial losses), some are likely to be small (e.g., dewatering of small channels, which was not even mentioned in the Water Analysis Module), and some are going to be a big shrug (changes in habitat on the islands during the time they are open). Yet, each topic gets about the same amount of discussion in this chapter. A table or list presenting the different impact mechanisms, or maybe some sort of symbols of importance on the conceptual diagrams would help.	Good suggestions. We were not able at this time to tell the story more succinctly.

Delta Risk Management Strategy Phase I
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No.	Section	Comment	Response
Reviewer: Wim Kimmerer, Steering Committee Member, May 20, 2007			
9	**Section 12**	Part 12.1.2 on contaminants could be greatly pared down, as it is totally based on speculation and generalities. It is not as if the Delta were otherwise pristine. Is it realistic to infer significant effects from the mobilization of contaminants, at a time when the entire Delta has been turned upside down?	The point is well taken. We believe it is important to raise the issues here.

Delta Risk Management Strategy Phase I
Response to Comments on Risk Analysis Report

No.	Section	Comment	Response
Reviewer: Frances Mizuno, Steering Committee Member			
1	**Draft Summary Report**	There is no reference to the potential length of export outages for each failure scenario.	Tables and/or text have been added to the seismic, flood, and sunny day failure sections to clarify this issue.
2		There needs to be more discussion regarding impact to water operations as a result of flood events/ seismic events.	Discussed under consequences for each hazard.
3	**Section 12**- Economic Impacts from Reduced Water Supply-	Discussion of impacts to the various services needs to be more balanced. There is not enough analysis of the impacts to SOD Ag districts. The Economic Impacts from Reduced Water Supply analyzed shortages to urban agencies by calculating the level of shortages to each agency in all the counties. This analysis should be also performed for each agricultural agency impacted. There is no reference to total acres of agricultural land effected, how much is permanent crop, land fallowing That would take place, third party impacts, the total value of the agricultural production for the area, etc.	Tables and language have been added to describe analyses for agricultural uses. Detailed information can be found in the Economic Impacts TM.

Delta Risk Management Strategy Phase I Response to Comments: Risk Analysis Report			
No.	**Section**	**Comment**	**Response**
Reviewer: Victor Pacheco, DWR			
1	**General**	Of particular concern is the omission or lack of clarity in addressing Suisun Marsh. For example, although Highway 12 and Interstate 680 are identified in the report, these transportation corridors bordering the northern and western edges of Suisun Marsh are likely to be inundated due to sea level rise projections listed in the report. However, the report appears to omit addressing the vulnerability of these facilities.	Estimated sea level rise in the Delta in 2050 is 1 foot (see Infrastructure Technical Memorandum [TM], Section 5.2). I-680 and Highway 12 are at the edge of the 100-year floodplain; a 1-foot sea level rise may not inundate these highways.
2	**Page 3 in the Summary Report**	The reference to the Suisun Marsh Charter Implementation Plan on Page 3 in the Summary Report should be revised to the Habitat Management, Preservation, and Restoration Plan for Suisun Marsh (Suisun Marsh Plan) currently being prepared by the Suisun Marsh Charter agencies.	Done.
3	**Page 1-1 from the Draft Risk Analysis Report**	States there are approximately 230 miles of exterior levees in Suisun Marsh. However, Tables 13-1, 13-2 from the Draft Report and Tables 2, 3, 4 and 5 from the Summary Report identify 75 miles of levees for Suisun Marsh which is about one-third of the approximately 230 exterior levee miles in the marsh. It is unclear which levees have been addressed and why others have not been included.	The 75 miles is in reference to the levees for MHHW level.
4	**Tables 2, and 3 from the Summary Report**	It is unclear why the Suisun Marsh is lumped as a single area in Tables 2, and 3 from the Summary Report. In order to provide useful information for development of the Suisun Marsh Plan, each individual island should be evaluated similar to the Delta. Although, use of the term “and others” under URS Name categories for Suisun Marsh in Tables 4 and 5 from the Summary Report makes it difficult to understand which areas are being referred to within the marsh, at a minimum, the Suisun Marsh areas used in Tables 4 and 5 should be used to provide figures for the rates of sunny day and seismic failures. This approach would seem to be more consistent with Figure 15 Composite Annual Frequency of Failure of Individual Islands in the Summary Report.	Tables have been revised.

Delta Risk Management Strategy Phase I Response to Comments: Risk Analysis Report			
No.	**Section**	**Comment**	**Response**
Reviewer: Victor Pacheco, DWR			
5	**Page 2-8 from the Draft Risk Analysis Report**	States that Suisun Marsh levees are "much closer to several fault lines". Figures 6-1 and 6-2 from the Draft Risk Analysis Report show faults that run directly through Suisun Marsh as well as other faults that run through the Delta. The general statement regarding proximity of Suisun Marsh levees to faults should to be more specific, as Delta levees are closer to several blind thrust faults.	Sentence has been removed.
6	**Section 14.1.1 on Page 14-1 from the Draft Risk Analysis Report**	Indicates Figure 14-1 is a "Subsidence Map for 2100". However, Figure 14-1 in the draft report is showing "Oroville Changes in Monthly Runoff Patterns". This Figure should be replaced by Figure 22A from the Subsidence Technical Memorandum.	Corrected.
7	**Page 5-9 from the Draft Risk Analysis Report**	States that subsidence rates of about 0.5 inches per year were common in the marsh and some areas appear to have experienced 1.0 inch per year. However, the location of points in Suisun Marsh used to estimate subsidence rates identified by Figure 7 on Page 16 from the February 2007 Subsidence Technical Memorandum appear to be primarily on levees that are known to settle over time. In fact, the negative subsidence rates in Table 2 on Page 17 from the Subsidence Technical Memorandum may actually reflect the work done on these levees to increase their height over time or settlement rather than subsidence. There appears to be a lack of data from areas within Suisun Marsh where subsidence may potentially occur. Several areas of the marsh are managed wetlands that are flooded year-round or existing tidal marsh that are unlikely to subside and may over time actually accumulate vegetation/sediment.	Some text changes have been provided. This is addressed in the Subsidence TM.

Delta Risk Management Strategy Phase I Response to Comments: Risk Analysis Report			
No.	**Section**	**Comment**	**Response**
Reviewer: Victor Pacheco, DWR			
8		Suisun City is missing from Figure 5-4 and subsequent figures in the Draft Risk Analysis Report. In addition to the small communities of Collinsville, and Birds Landing this community of nearly 30,000 located along Suisun Slough is the most likely to be affected by levee failure and sea level rise within the Suisun Marsh region and warrants inclusion on the map. In particular, the Suisun City Marina should be included with other businesses in Suisun Marsh in Figure 5-9 in the Draft Risk Analysis Report.	Communities added.
9	**Page 12-24 of the Draft Risk Analysis Report**	Indicates that not all the locations of Kinder Morgan pipes have been identified, the Kinder-Morgan pipelines crossing Suisun Marsh (See listing on Page 12-24) were not shown in the Status and Trends Report due to security concerns in identifying their location. However, the Kinder-Morgan pipeline LS9 crossing the Delta is included in Figure 5-9 from the Draft Risk Analysis Report. There should be consistency in showing the critical infrastructure that passes through each region. At a minimum, Figure 5-9 should include a footnote on the pipelines crossing Suisun Marsh.	The Kinder Morgan pipelines that cross Suisun Marsh are not in the GIS database for DRMS and are not shown on Figure 5-5. LS9 was available in the GIS database and is shown on Figure 5-6.
10		The Morrow Island Distribution System and the Roaring River Distribution System within Suisun Marsh are missing in Figure 5-6 from the Draft Risk Analysis Report. These are joint State Water Project and Central Valley Project facilities constructed to meet the water management needs of local landowners in Suisun Marsh. In addition, the Suisun Marsh Salinity Control Gates is another joint SWP/CVP facility that could be included in Figure 5-6 or in Figure 5-10 Miscellaneous Data. A map identifying the location of these facilities is included below for your convenience.	Added.
11	**Figure 7-11**	Flow Stations Used with Flood Hazard Analysis from the Draft Risk Analysis Report does not identify all the islands within Suisun Marsh. In addition to Van Sickle and Chipps Island other islands within Suisun Marsh should be identified for the reader.	Figure 2-2 in Section 2 of the Risk Analysis Report identifies various islands in Suisun Marsh.

Delta Risk Management Strategy Phase I Response to Comments: Risk Analysis Report			
No.	**Section**	**Comment**	**Response**
Reviewer: Victor Pacheco, DWR			
12	**Figure 7-20**	Locations of Levee Failures from the Draft Risk Analysis Report is missing a portion of Suisun Marsh as well as information on breach/flooding locations during 1998 and 2006 such as Honker Bay Reclamation District. Please see the maps included below for additional locations.	Figure 2-4 of the Risk Analysis Report identifies historical flooding in the Delta and Suisun Marsh since 1900.
13	**Table 10-1**	It is not clear why Van Sickle Island in the Suisun Marsh is not considered a "significant" island for inclusion in Table 10-1. It is one of the few areas in Suisun Marsh included as part of the Levee Subventions/Special Projects Program with significant investments made to repair and improve levees. It is also not clear why Suisun City with a population of nearly 30,000 has not previously been included in the report and maps as commented earlier, yet SM-124 (Suisun Marsh, Southwest of Suisun City) is included in Table 10-1. The omission of various items throughout the Suisun Marsh as noted in these and the attached comments on the April 2007 Appendix A, "Delta and Suisun Marsh Fishery Resources" report makes it unclear if they were properly addressed in developing the priority tables.	Criteria used to include an analysis area in Table 10-1 include (a) population of more than 500 people, (b) existence of infrastructure with prospect of major disruption, and (c) importance in terms flooding volume and location for drawing salinity into the freshwater portion of the Delta or potentially disrupting freshwater if the area were to be flooded with saline water. SM-124 was an obvious inclusion based on population. None of the other Suisun Marsh areas (including Van Sickle Island) were perceived to meet the stated criteria.
14	**Page 12-20**	In-Delta recreation losses described on Page 12-20 should be revised to note that Suisun Marsh fishing and boating recreation would have similar impacts as the Delta due to levee failure.	Done.

Delta Risk Management Strategy Phase I Response to Comments: Risk Analysis Report			
No.	**Section**	**Comment**	**Response**
Reviewer: Victor Pacheco, DWR			
15	**Section 13.2.1 on Page 13-2 from the Draft Risk Analysis Report**	Indicates there are approximately 75 miles of exterior levees within the MHHW boundary for Suisun Marsh. However, Figure 5-11 indicates the MHHW and 100-year flood boundary includes the majority of Suisun Marsh and the more than 230 miles of exterior levees. Therefore, it is unclear which levees are and are not being addressed in the risk analysis. The annual frequency of failure identified may not properly represent the marsh with less than one third of the actual exterior levees being included in the analysis and failure rate tables in the report.	Those numbers will be reviewed for accuracy.
16	**Section 14.1.3 on Page 14-4 from the Draft Risk Analysis Report**	Discusses increased hydraulic head and subsidence in 2050 and 2100 which appear to refer to only the Delta subsidence rates. While the levee vulnerability analysis may not change significantly using lower subsidence rates in Suisun Marsh, this should be specifically addressed in the results being provided.	Text added.
17	**Page 15-2 from the Draft Risk Analysis Report**	States "Suisun Marsh will subside by about 1 to 2 feet by 2050, although significant areas of the Marsh where highly organic soils prevail will subside about 3 feet." While the uncertainty analysis from Section 4.2 on Page 42 in the Subsidence Technical Memorandum addresses the uncertainty of elevation data, it fails to address the lack of data from areas within Suisun Marsh where subsidence is more likely to occur as illustrated by Figure 22C on Page 29 in the Subsidence Technical Memorandum.	Addressed in response to comments on the Subsidence TM.
18	**Page 15-2**	A bullet states that "For comparable soil organic matter contents, Suisun Marsh subsidence rates are similar to the Delta rates." However, it is known (and the report previously acknowledged) that management in Suisun Marsh is significantly different than the Delta and that while the marsh may subside 1 to 2 feet by 2050 the Delta would subside by 1 to 5 feet in the same time period.	Text added.

Delta Risk Management Strategy Phase I Response to Comments: Risk Analysis Report			
No.	**Section**	**Comment**	**Response**
Reviewer: Victor Pacheco, DWR			
		Some additional context is needed to clarify the bulleted statement.	

Delta Risk Management Strategy (DRMS) Phase 1
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No.	Section	Comment	Response
Reviewer: Patricia Quickert, DWR			
General Comments			
1	**General Comments**	This draft document appears to have been put together on a very tight timeline based on the overall poor quality of the writing. Incorrect syntax and sentence structure was evident throughout the document, making it difficult to read and comprehend. The document contains a large number of grammatical and punctuation errors including: • misspellings (i.e. 'braded' for 'braided' and 'abraided' for 'abraded'); • incorrect verb tenses; • use of plural instead of singular tense and vice versa; • incorrect word placement (i.e. use of 'and' instead of 'a', 'effect' instead of 'affect' and vice versa); • lack of, extraneous, or incorrect prepositions; • missing numbers in non-bulleted lists; • misuse of commas; • improper spacing between words; • inconsistent use of numbers (whether written as numerals or words); and • lack of italics on scientific names.	Agree on the schedule constraint. The text has been edited and corrected for typographical errors. No period of time was available to perform a detailed proof reading or editing of the text though. We are currently taking advantage of this period of time (after submitted the second draft) to go over the report in detail. Additional editing plus the public comments will be used to revise the report properly.

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Response to Comments on Risk Analysis Report

No.	Section	Comment	Response
Reviewer: Patricia Quickert, DWR			
2		Final two paragraphs of Section 1.5 • Page 1-7 • Bulleted list beginning on page 2-4 (lack of prepositions, incorrect verb tense, and arbitrary definition of terms [i.e., why define 'introduced' but not 'endemic'?]) • Page 3-4 • Page 13-6, final paragraph, 2nd sentence	Rewritten.
3		The reviewer had a difficult time following the lines of logic due to the significant amount of technical information located in other Technical Memoranda (TM).	Due to the large amount of information in the TMs, the authors had to make a judgment call on how much information to bring forward into the Risk Analysis Report.
4		Insufficient coverage of important aspects of the Delta such as subsidence, effects of dams and upstream water management, other anthropogenic changes, and invasive species.	Revised some, but this was not intended to be a complete history. It was intended to be a short, two-page perspective on each topic. All 12 topics are covered in detail in their respective TMs.
5		There were an inadequate number of citations in the body of the document and a surprisingly small number of references. Since URS documents represented over 20% of all the references cited, the reviewer is left to assume that those documents contained references that 'should' have been cited in this TM. However, the reviewer found that the Ecosystem TM also	The TMs list their own specific references. Only the relevant or cited references are included in the Risk Analysis Report.

No.	Section	Comment	Response
Reviewer: Patricia Quickert, DWR			
		contained a relatively small number of references.	
6		The reviewer assumes that this document was supposed to evaluate effects of different flooding scenarios. However, the specifics of the flooding scenarios could not be located in the document, with the exception of the tables in Chapter 13. The document needs a section that describes the breach scenarios with their specific locations, flooding depth, and duration. Since all ecosystem effects will be determined by these parameters, it would be beneficial that this information be clearly stated.	Section 13 of the Risk Analysis Report has been revised to include more information on scenarios. Maps were added to illustrate the locations of the breached or damaged islands.

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No.	Section	Comment	Response
Reviewer: Patricia Quickert, DWR			
7		The reviewer was informed that the intended audience for this document was "the general public". However, the document does not appear to the reviewer to have been written in a transparent style that would make it easily understandable to an interested lay person, especially without the other TMs.	There are three reports in addition to the detailed TMs. The Phase 1 Risk Analysis Report provides the most detail of the three reports. The Summary Report is intended for the informed, but non-technical audience. The executive summary, prepared by DWR, is intended for the general public.
Primary Substantive Comments			
1	**Section 2**	Lots of important information either downplayed or overlooked in History Section. Misleading statement about reversal of Delta subsidence. Needs more coverage about critical impacts of dams and upstream water releases on Delta ecosystem.	See response under Section 2 detailed comments below.

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No.	Section	Comment	Response
Reviewer: Patricia Quickert, DWR			
2	**Section 5 Page 1-8**	It is stated that Chapter 5 describes "the current conditions used for the risk analysis", but a description of the ecosystems in the Delta and Suisun Marsh was not included, and it is needed. The reviewer found it confusing to find so much on Linear Assets and so little on ecosystems. There is no list of the species used in the analyses. There is discussion about the effects of flooding on vernal pools, but there are very few vernal pools in the Delta, so this effect is relatively unimportant. (See comment C.15.	See response under Section 5 detailed comments below.
3	**Section 12**	Depth and duration of flooding should be added to the impact analyses for plants and terrestrial wildlife. There is poor information on seed banks and plant species adaptations to salinity.	See response under Section 12 detailed comments below.
4	**Section 13**	Although nearly all the listed terrestrial species discussed in this document are from Suisun Marsh, none of the breach scenarios included breaches within Suisun Marsh, which means there would be very little direct impact to species in the marsh. Therefore, there should be a Suisun Marsh breach scenario, because there is a need to assess impacts to these listed species within the marsh.	See response under Section 13 detailed comments below.

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No.	Section	Comment	Response
Reviewer: Patricia Quickert, DWR			
5	**Section 15**	Information about an endangered plant, *Cordylanthus mollis mollis* is erroneous. References—surprisingly few. Also the text lacks citations for information taken from other sources	See response under Section 15 detailed comments below.
Specific Comments			
1	**ES-1, 1st line**	"Observations" are made, not revealed.	Text revised.
2	**Page 2-1 to 2-3**	History section: This section is much too brief with not enough emphasis on pre-CVP/SWP. Seemingly capricious choices of what to include and what to leave out—marginal information for the most part, with odd little details thrown in. For example, there is no mention of hydraulic mining sediments or associated mercury, no mention of Gold Rush era harvesting of natural resources such as fish, shellfish and waterfowl.	The two pages were not intended to be a detailed history. They were intended to be a brief perspective. We tried to list the major things that would relate directly to the risk of levee failure.
3	**Page 2-1, 2nd Paragraph, 1st line**	Suggest changing "5 thousand" to either "five thousand" or "5000". st ¶: Suggest mentioning that acreage purchase limits were repealed in 1868.	Population now shown numerically. Considered comment about acreage, but it did not seem important to the Risk Analysis.

Delta Risk Management Strategy (DRMS) Phase 1
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No.	Section	Comment	Response
Reviewer: Patricia Quickert, DWR			
4	**Page 2-2, 3rd Paragraph**	This paragraph should include the word 'subsidence', and tell the full story—subsidence in the Delta is anthropogenic and due to much more than simple oxidation and wind loss, i.e. a large volume of peat soil was used for levee construction. Also, a little more information about soil burning should be included since it was a regular agricultural practice. 4th ¶: The mention of "flooding problems" appears to be a bit understated here.	Done.
	last paragraph, continues on Page 2-3	Another understatement is the impacts of dams and water releases on Delta hydrology. These water management actions don't merely "generally lower peak flows and increase dry weather flows", they caused a significant alteration of the hydrology and ecosystem of the Delta, and therefore should get more coverage than a single sentence.	Done.
5	**Page 2-3, Section 2.3, 1st paragraph, 1st sentence**	The majority of lands in Suisun Marsh are managed primarily for waterfowl hunting, not "conservation". 3rd sentence: This is the first mention of the Primary and Secondary Zones, with no explanation. Need to at least add a reference to Figure 2.2.	Added mention of hunting. The use of conservation was intended as a land use term as opposed to urban and agricultural; clarified. Done.

Delta Risk Management Strategy (DRMS) Phase 1
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No.	Section	Comment	Response
Reviewer: Patricia Quickert, DWR			
		Last ¶: There is a need to better define the term 'services', because the features of the bulleted list don't coincide with the reviewer's idea of 'services'.	Done.
6	**Page 2-4, 1st bullet**	Flood Management: Question-How far below sea level are the islands subsided? Ecosystem, 4th bullet: Correction is needed; wetlands do not "reside"	Detailed discussion on subsidence, amount of subsidence, and rate of subsidence is provided in the Subsidence TM. Subsidence map under today's conditions is presented in Figure 3 of the Summary Report. Future subsidence is depicted in Figure 14. Done.
7	**Page 2-5, Section 2.4**	Reviewer does not concur that "future trends for the Delta and Suisun Marsh are difficult, or impossible, to predict". Services will become increasingly expensive and difficult to maintain; it's only the specific details of future scenarios that are difficult to predict.	Sentence removed.

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No.	Section	Comment	Response
Reviewer: Patricia Quickert, DWR			
8	**Page 2-6, 1st line**	Question-Aren't there more references on future trends than simply one other URS document? 3rd bullet: This bullet is misleading; it is stated that Delta elevations can be increased by changing crop types, but that is not necessarily true. Although rice may work on the scale of centuries, subsidence is more reasonably reversed by shallow flooding and growth of emergent wetland plants such as tules.	The document provides many other references. Agree that shallow flooding and tules may be more reasonable, but did not alter text because rice may help stabilize and reverse subsidence over the long term. The bullet was simply intended to show that there are alternatives to conventional agricultural practices in the Delta. Actual strategies to reduce risk are the subject of the future Phase 2 report with strong emphasis on emergent wetland plants. Steve Deverel, the lead author of the papers that the reviewer cited, as well as R. Miller of the USGS, who has conducted research on wetlands as a method to reverse subsidence, are currently working with URS on proposals to utilize changing crops or constructing wetlands as methods to reduce subsidence. The best available information indicates that both require management; the current costs and

No.	Section	Comment	Response
Reviewer: Patricia Quickert, DWR			
			benefits of each method do not result in one method being more "reasonable" than the other. Reports on these topics are currently in draft form to DWR and will be made available to the public.
9	**Page 2-7, Section 2.5**	Recent Growth of Concern: One of the most important aspects of the Delta as a "changing dynamic system" has to do with salinity fluctuations to support native species of fish and deter many non-native species, but this document has little to say about this ecosystem condition.	This topic is outside the scope of DRMS. This topic is being addressed in other ongoing studies.
10	**Page 2-9, 4th bullet**	Although the Public Policy Institute is mentioned in this bullet, none of their publications, including *Envisioning Futures for the Sacramento-San Joaquin Delta*, are in the References	Oversight; reference added.
11	**Page 3-3, 1st Paragraph, 4th line**	States that "the higher sea level requires higher levees." This sentence needs to be qualified because sea level rise "requires" nothing. It is humans that have "requirements". This sentence pre-supposes that levee maintenance will be the 'vision'—even before any alternatives are presented. Regional Climate Change bullet: Further	This wasn't intended to be a vision. Reworded. Sentence on climate changed. It is discussed in later sections in more detail.

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		discussion is needed on this issue; runoff does much more than just "Delta salinity control and flushing".	
12	**Page 3-5**	Habitat Restoration bullet: All other factors in this section are expected to remain as they are today, but it is assumed that there will be no 4 habitat restoration, despite the fact that there are current projects. This exception requires further explanation.	The business-as-usual was used so the analysis could have a common basis for comparison. At the level of detail that the analysis uses, it was felt that the current projects would make little difference in the conclusions.
13	**Page 4-1**	Section 4.1.1: Only POD is listed as a risk specific to the ecosystem. Invasive non-native species should be added, at least.	Done.
14	**Page 5-1**	Section 5.1, 1st ¶, 2nd sentence: Change "26 thousand" to either "26,000" or "twenty-six thousand". 4th ¶, 1st sentence: "Many" is not an estimate. Suggest deleting this sentence.	Done. Deleted text in parentheses.
15	**Page 5-2, 1st Paragraph, 2nd sentence**	Replace the term 'spiders' with 'wildlife'. Question-Why were "spiders" part of this sentence in the first place? In the reviewer's experience, effects on spiders are not currently considered to be of much	Detailed analyses of beta diversity were conducted on spiders at the interface (Traut 2005), and this analysis was used to support the description of the habitat as an ecotone; ecotones are

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		importance by any of the regulatory agencies. 3rd sentence: Question-Why consider vernal pools? There aren't many vernal pools in the Delta or the primary zone of Suisun Marsh. Diana Hickson, a botanist at DFG, whom the reviewer contacted for information, lists only the following vernal pools in the legal Delta, and all are located on the periphery of the Delta and unlikely to be impacted by Delta flooding: There are "vernal pools within the Legal Delta boundary, including the ones at Tule Ranch at Yolo Bypass, between Calhoun Cut and Barker Slough near Dozier (Jepson Prairie area), on the military land south of Dixon near Bunker, and the alkaline pools near the Byron Airport." Section 5.3: Need a table of listed and sensitive species. Although there are a lot of nice maps in this section, a map of the CNDDB occurrences of listed species should be added. 2nd ¶: There is a need for discussion of the effects of levee failure on plants and	considered biodiversity hot-spots. Again, the project objective was to assess the risk, not assign subjective assessments of the importance of habitat or species. Vernal pool habitat is not found in areas flooded due to Delta levee breach; however, they are inundated in future scenarios (see Section 14) of sea level rise. The text was revised. The objective is to describe the risk, not assign subjective assessments of the importance of a habitat. In particular, the risk assessment methodology did not entail excluding the discussion of risk to a habitat based on the area the habitat occupies in the Delta or Suisun Marsh. Vernal pools are arguably quite important for the following reasons: Vernal pools in California are known to support many species that are listed as endangered species under the Federal Endangered Species Act, and the California Endangered Species Act,

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		animals, because levee failures can result in temporary loss or extirpation.	including plants (e.g., grasses, flowers), branchiopods, and the tiger salamander. Vernal pools in the Delta also support plant species endemic to the Delta (e.g., Delta button celery). Migratory birds also use vernal pools as stopovers for forage during migrations. In addition, vernal pools perform a critical ecosystem service of recharging drawn-down aquifers. At least three areas of vernal pools are inundated by means of higher high water for predicted sea levels in year 2100 (140 cm higher than mean sea level), according to the Holland Vernal Pool Dataset (see attached map). These areas occur near Cache Slough along Cache Creek, in Egbert Track, and in Cache Haas. The inundation of even a small area of vernal pool habitat may have a very large impact on the survival of species endemic to the Delta that are supported by vernal pool habitat in the Delta. The current extent of vernal pool habitat is less than 10 percent of its original

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			extent, and therefore further loss of habitat may have a large increase in the probability of species extinction. A table of listed and sensitive species is included in the Ecosystem Consequences TM.
16	**Page 12-1, 1st Paragraph, 1st line**	States that "The consequences of Delta and Suisun Marsh levee failures are far reaching…", but none of the breach scenarios include breaches in the Suisun Marsh. These scenarios should be included. 3rd sentence: There is a need to list in this section the species and groups that were used to assess ecosystem impacts. 2nd ¶, last two sentences: Timing, depth, and duration of flooding should be included in the models of effects of levee breaches on plants and wildlife, because these parameters are critical to the survival of these ecosystem components.	The analysis is not intended to be location specific, but on a more regional scale. Suisun Marsh was considered in developing estimates of consequences. Correction: the consequences are not far reaching for failure of Suisun March levees compared to the Delta. Noted. The impacts of depth and duration of flooding are presented in the Ecosystem Consequences TM.

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17	**Page 12.5, 1st and 2nd bullet:**	Both the vegetation and wildlife risk models should include timing, depth and duration of flooding. 2nd bullet: There should be an explanation about why home range of terrestrial species was considered important, because the reviewer can't see how it matters. If an island floods, chances are the entire island will flood, so it won't matter if a species' normal home range is 1500 square meters or 5 square miles—each affected individual will lose its entire 'home range'.	See above response. The comment is unclear. The home range of an individual is not included in the assessment. The analysis is conducted as follows: The spatial overlap between an organism's home range and the spatial distribution of the vegetation type in which the organism occurs was used in the model to spatially define the habitat of the organism. The risk to a species was calculated as the percentage of that area that was lost. Further qualitative assessment of the risk was made to take into account whether that area within the Delta encompassed the species entire home range or only a fraction.

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18	**Page 12-12**	Section 12.1.3, Flooding bullet: This bullet should state that plants not adapted to soil saturation will die. Even those wetland plants adapted to saturated condition will die if inundation is too long or too deep. Salinity bullet: As written, " Plants adapt to high salinity levels…" an individual response is implied. The plant *species* is either adapted or it is not—adaptation does not occur in individuals in immediate response to a flood or introduction of salinity. The first sentence should be changed to read: "Plant species which are adapted to high salinity levels do so by physiologically tolerating the salt or by avoiding salt." The last sentence should be changed to "Plant species that are adapted to high salinity conditions…"	The comment is unclear. The flooding bullet describes in detail the precise mechanisms through which plants not adapted to flood conditions will die. The reviewer is not correct. Within the same individual, different life history stages of plants have different tolerances for salinity. In particular, high marsh plants, which experience extremely high soil salinity in the summer, only germinate during winter rains, when salinity is very close to freshwater conditions. In particular, describing a plant species as "adapted" to high salinity is controversial, as exemplified by common crop plants that are not described as being adapted to high salinity, but that are currently being bred to thrive in irrigation water with higher and higher salinity (e.g., >10 ppt). For clarification, the text has been modified.

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19	**Page 12-12 and 12-13**	bullets on Flowering Time and Lifespan: The issues raised all assume the plant itself survives, which is highly unlikely in any long term or deep flooding event.	The issues raised do not all assume that the plant itself survives. For example, the size of the extant seed bank is related to the frequency of reproduction whether or not the plant survives. The risk assessment included flood events of short duration.
20	**Page 12-13**	bullet on Seed Banks: States "little is known about the impact of flooding on seed viability for the range of communities found in the Delta and Suisun Marsh". Although this is *strictly* true, there is a large body of literature about the effects of flooding on seed banks, which could be applied here. Seed banks in general are commonly known to be long-lived. In addition, the land managers in Suisun Marsh (DFG and SRCD) are very knowledgeable on the effects of flooding there.	The comment is unclear. Adaptations found in plants to high levels of salinity are described in the "salinity" section. Seed bank tolerance to salinity is described in the section entitled "Seed Banks." There are few data for focal species examined on plant or seed tolerance of salinity. Due to the great differences among even closely related species in tolerance to abiotic factors, we restricted the data use to only data published on focal species. A description of the methodology is provided in the Ecosystem Consequences TM. Steve Chappell, Executive Director of Suisun Resource Conservation District,

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			was consulted about seed banks and re-growth of wetland vegetation from seed banks. Steve was the source of the series of images in the Ecosystem Consequences TM showing currently available quantitative information in Suisun Marsh on the impacts of duration of flooding on the re-establishment of vegetation from seed banks or vegetative propagules. Also, Suisun Marsh managers have the most knowledge on the re-vegetation of marsh plants from seed banks, which comprise half the vegetation types examined, and are less familiar with the five upland types and riparian trees and riparian shrubs.
21	**Page 12-14**	Bullet on Disturbance: "*Sedimental*" is not a word. "*Abrade*" is misspelled.	Sedimental means sedimentary according to Webster's dictionary; "sedimental burial" is a technical term used in peer-reviewed scientific papers for burial under sediment (e.g.. as used in Tanaka, Y. and M. Nakaoka, Morphological variation in the tropical seagrasses, *Cymodocea serrulata* and *C. rotundata*, in response to sediment conditions and light attenuation.

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			Botanica Marina v.49 (2006) (5/6) 365–371.)
		Section 12.1.4, Terrestrial Wildlife Risk Assessment: This section is too Brief, and it conveys very little information.	"Abrade" has been corrected. The Terrestrial Wildlife Risk Assessment Section describes the method used to assess risk to wildlife in the Risk Assessment model.
22	**Figure 12-4**	Conceptual model on impacts to vegetation:	
		• Shows that duration and depth of flooding are tied to "saline water inundation". However, duration and depth will have critical effects on plants, regardless of salinity.	"Saline water inundation" has been altered to "water inundation of varying salinity levels."
		• A separate arrow with "duration and depth of flooding" should point to something like "possible mortality of all standing vegetation."	Depth and duration has been added to the arrow pointing to "Plant mortality."
		• Question-Why is "change in hydrologic regime of vernal pool habitat" included? Is this about possible increases in ground water elevations in areas bordering the Delta and Suisun Marsh? Because according to DFG, there aren't any vernal	Reference to vernal pools was removed, because even though vernal pool habitat is impacted due to sea level rise in future conditions (Section 14), vernal pools are not impacted as a consequence of levee breaches.

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		pools behind the levees. (See comment #15, above.)	
23	**Page 13-1**	1st line: States that "…risk analysis results considering levee failures in the Delta and Suisun Marsh." Reviewer suggests removing 'Suisun Marsh' from the sentence because none of the breach scenarios include breaches in Suisun Marsh.	An analysis of a hypothesized levee breach in Suisun Marsh was conducted for terrestrial vegetation and wildlife to address specific concerns of Suisun Marsh management. This analysis was included in the Ecosystem Consequences TM.
24	**Pages 13-6 and 13-7**	Section 13.4.2, Vegetation: Spelling errors need to be corrected.	Done.

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25	**Page 13-7**	1st sentence: States that "Breaching of these levees resulted in no impacts to…. salt marsh harvest mouse, saltmarsh common yellowthroat, California clapper rail, and Suisun ornate shrew, whose habitats are restricted to Suisun Marsh." This statement is misleading since none of the scenarios had breaches in Suisun Marsh which would result in direct impacts to these species. In all scenarios, breaches are located only in the Delta, and these will have very little direct impact on species in Suisun. Since most of the sensitive (listed) terrestrial species in the region are found in Suisun Marsh, it is important to include scenarios with Suisun breaches.	The sentence has been changed.
26	**Page 15.5, last bullet, 2nd sentence**	Sentence states that "…Delta levee walls include all known occurrences of a federally-threatened species, *Cordylanthus mollis mollis*, in a twelve county area." This is wholly incorrect information on *Cordylanthus mollis mollis.* This plant does NOT occur on levee banks, it occurs only in brackish tidal marshes. Furthermore, its status is Endangered, not Threatened and it currently occurs in only three counties, not twelve, as stated.	The occurrence of *Cordylanthus mollis mollis* is correct according to the following analysis: A search was conducted in the CNDDB December 2006 database for the 12 counties including and surrounding the Delta. Within this area, there was one occurrence of *Cordylanthus mollis mollis*. The occurrence was on the channel side of a Delta levee. The text has been altered to clarify this.

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		Question-What is a levee wall? The reviewer is not familiar with this term.	The status has been changed to Endangered. "Levee wall," as defined in Table 4-3 of the Ecosystem Consequences TM, represents the side slopes or water side berms of Delta levees within 200 ft of the centerline of the levee. This definition has been added to the text on page 15.5.

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Reviewer: Michael Ramsbotham, U.S. Army Corps of Engineers			
1	**General**	How does the Emergency Response and Repair priority list of islands repair correlate / relate to the calculated risk?	There is a strong relationship between the sequence of repair for multiple islands failures and water export interruption. The approach used in the repair was based on expected response (islands with high population, salinity impact, etc.); the decision on the sequence was not based on the outcome of trying various sequences and selecting the best. This information will be refined in Phase 2.
2		What is the minimum peak ground acceleration expected to cause serious damage to levees in the Delta?	The minimum peak ground acceleration that causes failure is 0.05g.
3		The estimated 30 or more island breaches in the next 25 years due to flood events seem too high / pessimistic.	The draft results are being reviewed and checked for accuracy. If any different, the results will be updated. Historical failures have an average annual occurrence of 1.6 (160 failures have occurred in the last 100 years).
4	**Executive Summary**	The executive summary should be divided into subsections which address the following aspects of the study, the stressing events, system response, ecological impacts, economic impacts, and risk. Risk is not adequately addressed.	A discussion of risk will be added to the executive summary of the Summary Report.

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5		An executive summary should include a not only a summary of findings, but also conclusions, and recommendations	Any conclusions from the results of the analysis will be added to the executive summary of the Summary Report. The recommendations will be included in the Phase 2 report.
6		Early in the document include a definition of risk and failure and make sure the document uses the terms correctly throughout. (Risk = probability of a stressing event x the effectiveness of the defense/ probability of failure x the consequences.) (Failure = levee breach and flooding.)	A definition of risk will be added.
7		The executive summary should include a discussion of risk with respect to potential loss of life.	A discussion of risk to population will be added.
8		The executive summary should include a discussion of risk with respect to the impact of loss of water supply.	A discussion of risk to water supply interruption will be added.
9		My impression is that without adequate backup information, discussion of large catastrophic seismic events and their consequences still dominate. A large seismic event may pose the largest risk, it is just not readily apparent in the Summary Report how risk is being evaluated / compared.	Evaluation of seismic risk will be further discussed.

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10		Subsidence is still being popularized as a major threat to the Delta without sufficient supporting documentation. Consequences and risk do increase with increased by interior island subsidence, but there is little substantiating data to support the position that interior island subsidence significantly impacts seepage, stability or seismic vulnerability. The old cartoon showing significant ground surface elevation loss at the toe of the levee because of subsidence is no longer true for much of the Delta	Addressed in the Subsidence Technical Memorandum (TM).
11		Section 15.6 has some important information that should be included in the executive summary. Recommend including observations / statements on the Complexity of reservoir management, Restrictions on Delta water operations, Water rights, water quality standards and contracts, Order of repair, Various year types, Future water supply and demands.	Key points on the subject of water management will be added to the executive summary of the Summary Report. However, we are trying to be brief and stick to the key findings in the executive summary.
12		Flood Risk of Levee Failures. The evaluation appears to focus only on overtopping failures. It is not clear what failure modes are included in the calculation of the probability of failure for each island. Information presented in the executive summary is not provided in Section 7 Flood Risk Analysis.	Flood risk was calculated by considering three modes of failure: overtopping, under-seepage, and through-seepage (including land side sloughing). Section 7 has been revised to include discussion of modes of failures.

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13		Flood Risk of Levee Failures. Include information on the different modes of failure contributing to the overall probability of failure. What is the contribution to the probability of failure of overtopping, through seepage, underseepage, erosion, overtopping by waves, and slope instability.	The contribution from each mode of failure has been included in Section 7.
14		I like Figure 15, Composite Annual Frequency of Failure of Individual Islands. This figure indicates the Delta is a risky place to live and do business, and that certainly appears to be true.	No response needed.
15		The Draft Summary should include information contained in 7.6 Flood System Model pertaining to the spatial modeling of physical responses, island failure probability under multiple failure modes, probability of damaged levees, and length effects.	The authors of the Summary Report wanted to limit discussion to background and findings. They recommend that the procedures, methods, and details of the approach be referred to in the main Risk Analysis Report.
16		Recommend that Figures 12 through 14 be crafted in terms of an annual frequency of flooding.	This comment has been incorporated.
17	**Introduction**	No comment.	No response needed.
18	**Section 2**	Page 2-2 fifth paragraph incorrectly suggests that the US Army Corps of Engineers is responsible for maintenance of the SRFCP levees. These levees are also maintained by local entities.	Noted and corrected.

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19		Status of the Delta and Suisun Marsh. What about Isleton? Coordinate with 5.5.2 Point Assets, Residences, pp5-6.	Done.
20		Since the Delta is very different from the historical ecosystem in which the native organism evolved and is already undergoing rapid change, what chance do we have of preserving or predicting what is going to happen in the Delta?	The natural ongoing changes in the Delta and Suisun Marsh that were out of the scope of DRMS were not addressed. Other ongoing processes address these.
21	**Section 3**	Please review this section to make sure terminology (particularly the word risk) is being used correctly throughout.	Noted and corrected.
22		Page 3-4 Flood Protection Urban areas, specifically SAFCA are targeting 200 yr level of protection for Sacramento. How does future 200 yr level of protection affect the risk / calculated consequences?	Future improvement to Delta islands was not included in the analysis. The example given on page 3-4 is for the business-as-usual (BAU) condition. Will be addressed in Phase 2 risk reduction.
23		Page 3-5 Habitat Restoration. It seems inappropriate to assume ecosystem restoration will be static under the BAU baseline.	We agree, but not knowing which direction the changes will take place, we assumed BAU.

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24	**Section 4**	4.4.6 Combination of Events. Is the impact a flooded island has on adjacent islands due to increased seepage a part of the evaluation. It seems that island flooding would increase the hazard and or the vulnerability of and adjacent island.	The authors recognize this effect and have studied the phenomenon in the past. Increased underseepage will be observed, especially across narrow sloughs. However, the effect by itself was not estimated to cause failure, as documented in previous island flooding, including the 2004 failure of Jones Tract. Text has been added in the report.
25		4.4.7. Risk Metrics and Table 4-3. It is not apparent what the environmental metrics are. How is the environmental "cost" measured, # of an individual species lost or gained or habitat (acres) lost or gained.	A relative scoring system to estimate impacts to various species in the ecosystem was developed by the Eco-team. Discussion of these values is included in the Ecosystem TM. The same discussion has been added in the risk report and the metrics defined in Table 4-3.
26	**Section 5**	Why aren't the levees considered "infrastructure assets"?	The levees were considered specifically as a unique part of the infrastructure and were evaluated in detail in the Levee Vulnerability, Emergency Repair & Response, and Economic Impacts TMs in term of failure and the cost and duration of repair. The other infrastructure elements were included in the Infrastructure TM because they

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			were evaluated for cost of repair and recovery, and economic impacts of each element.
27	**Section 6**	Table 6.1. Add Notes.	Comment is justified and noted. Sorry we missed making the change in this round.
28		Table 6.2. Complete and add notes.	Comment is justified and noted. Sorry we missed making the change in this round.
29		6.2.3 Vulnerability Classes. List those material properties that were treated as random variables and which were considered deterministic.	A listing was made in the text.
30	**Section 7**	This section does not appear to be adequately represented in the executive / Draft Summary Report. There are a few figures and or tables that should be consider for inclusion in the Draft Summary.	A discussion of the key findings will be added. Again, we note that the Summary Report presents only the main findings.

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31		7.1 Delta Inflow. It seem that to determine the influence dams have on flood flow it would be necessary to plot the peak flow per frequency for the before and after dam construction cases.	This was considered in the Flood Hazard TM. It was found that, in the 50-year record available for analysis, larger Delta inflow occurred after the dams were constructed than before because the larger storms occurred in the latter half of the 50-year period. Flow routing to determine the effect of dams on particular storms is outside the scope and would require a large effort.
32		Table 7-10 Prospect island is now flooded. Levees breached and the island flooded in 2005/2006.	Agree and considered as such in the recent runs; table has been updated.
33		7.5.2 Flood Failure Modes. Probability of failure due to slope instability and erosion should be evaluated and addressed in more detail.	The erosion and potential slope failures were not explicitly represented. They were implicitly included as a fraction of the total failure modes. The Levee Vulnerability Team also believes that through-seepage would lead to slope instability (land side sloughing). The slope stability analyses indicate factors of safety above 1.0. See response to comment 34 below.

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34		7.5.2 Flood Failure Modes. Provide supporting documentation on the past failures that led the Levee Vulnerability Team to "believe" that 80% of the failures are due to seepage and 20% are due to other modes of failure.	The allocation of number of failures to different modes of failure was based on engineering judgment and the experience of the Levee Vulnerability Team members. There are no supporting documents available to verify this. Our review of past failures included review of reports and interviewing local and state employees. For most of the past failures, information regarding mode of failure, time and date of failure, and water level in the slough are either not available at all or available only in limited form. Therefore, it was necessary to make a judgment call in regard to mode of failure.
35		7.5.2 Flood Failure Modes. This section should include a summary table or figures that demonstrate the relative contribution of each mode of failure on the total probability of failure calculated for the islands. If 80% of the failures can be attributed to seepage then why is the Draft Summary Report so focused on overtopping failures?	Concur. This section has been revised to incorporate the comment regarding the need to highlight other modes of failure.

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36	**Section 9**	The probability of occurrence should be adjusted for island specific factors such as level of maintenance, number of existing problems, performance history, staffing, etc.	Concur. We recognized that collecting island-specific information (such as level of maintenance, number of existing problems, performance history, staffing, etc.) for all the analysis zones would improve the prediction of the probability of failure. However, given the schedule and the budget of this project, the project team collectively agreed that it is not practical to pursue this information at this time. The data set is too small to allow sorting of sunny day failures by levee type. Also, DWR personnel have stated that the 2004 Jones Tract failure occurred on a levee that appeared in very good condition.
37		Consider also looking at failures that occurred between October and June that are not flood related.	Failures that occurred between October and June were considered to be flood related.
38		Figure 9-1. Plot the crest elevation at the time of failure, adjusted for the new datum.	Crest elevations are already adjusted for NAVD-88.
39		Confirm that the failure rates presented have been adjusted to a per year basis.	Failure rates are adjusted to a per year basis.
40		Does the time a levee is exposed to the MHHW have any influence on the failure rate?	Time-dependent behavior was not considered.

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41	**Section 10**	10.2 Emergency Response. Having only one source of rock may not provide the "economy of scale" suggested. The SRQ rock comes at a premium. In a multiple island failure scenario rock could be obtained from other sources. Recommend other sources be investigated.	This will receive attention during Phase 2.
42		10.2 Emergency Response. The following assumptions appear to be unrealistic in the large earthquake /multiple island failure scenario, a) Sufficient transportation equipment can be made available immediately, and b) Resources are available and are not constrained by demand outside the Delta. Consequences / time to recover for this scenario should be re-evaluated.	A careful attempt was made to realistically estimate what equipment and material constraints might apply following an earthquake affecting all of the Bay Area. We will revisit these numbers again in Phase 2.
43		10.2 Emergency Response. The main constraint may not be equipment availability but availability of trained manpower capable of keeping the tugs, crane and dredge equipment operating 24/7.	Skilled/specially trained manpower is generally mobile and could be imported from other parts of the country.
44		10.2 Emergency Response. Dutra's marine fleet should be inventoried and compared to the projected need.	Not only was Dutra's marine fleet considered, but other resources along the West Coast were also considered.

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45		Has the expected interior island erosion rates and damages used in this module compare or correlated with the results of the methodology presented in the Wind and Wave Risk Analysis?	One builds on the other. The wind-wave model was used to predict wind patterns, direction, magnitude, and wave energy. ER takes those values and estimates rates of erosion and amount of erosion during the period of exposure.
46		Define island sector. If a sector is that portion of the island / atoll subject to wave attack from the prevailing winds then the assumption that, "erosion occurs at an equal rate along the entire levee perimeter of each island sector", may be realistic other wise it may not be a realistic assumption since erosion rates will be a function of vegetation, slope, and material types.	Done.
47		10.5 Prioritizing Repairs. It should be stated that the island prioritization is for modeling purposes only and that it does not represent actual priorities.	Done.
48		10.5 Prioritizing Repairs, Salinity. Up to this point the reader is lead to believe islands with population at risk, and important infrastructure have a high priority. The statement that, "If intrusion occurs, affected islands must be repaired as a top priority", seems to indicate otherwise. It appears the prioritization scheme is better explained later in the section.	This portion of the text was clarified.

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49		10.5 Prioritizing Repairs, Salinity. Please comment on how this effort was coordinated with the work presented in Section 11. Salinity Impacts.	The salinity priorities established reflect the hydrodynamic modeler's intuitive, generalized assessment of the most important salinity considerations, based on work until the date at which the priorities had to be established.
50		10.5 Prioritizing Repairs, Salinity. Based on this discussion it now appears that the Southern Islands around Old River, Middle River and the San Joaquin River are more important than the "8 Western Islands." Is this one of the new findings / paradigm shifts revealed by this study?	Actually, this change in perceived importance of south Delta islands relative to salinity was revealed by the earlier project reported in JBA, 2005. It is reflected in the priority list.
51		Recommend including the information in 10.6 Scheduling/Limits to Progress up front rather than at the end.	This recommendation was considered, but the change was not accomplished due to schedule.
52		If we can use history as an example, normal the train traffic should not be allowed through a flooded island. Traffic should be diverted or slowed until rail embankment and foundation are sufficiently dewatered.	The experience from the recent Jones Tract failure was used as a model. The railroad performed repairs and operated at reduced speeds.
53		This effort to "prioritize" emergency repairs is a bold step. Although it is for modeling only, it is informative and surprising is some ways. It seems odd that it has not caused more discussion.	Concur.

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments on Risk Analysis Report

No.	Section	Comment	Response
Reviewer: Michael Ramsbotham, U.S. Army Corps of Engineers			
54	**Section 11**	The WAM plays a significant role in determining consequences. This module should receive a high level of review from experts in this field. Include in this chapter in a separate subsection the Quality control and Quality Assurance steps / efforts taken to validate and calibrate the WAM.	Calibration and limited verification have been performed as permitted by schedule. The topics are addressed in the Water Analysis Module (WAM) TM, Appendices D and E.
55		Identify the other water quality measures / metrics important to the stakeholders. Describe what actions are planned to incorporate these measures / predictors in the WAM in Phase 1 and Phase 2.	Stakeholders identified organic carbon as being a major concern because of disinfection byproducts. Potential increases in organic carbon were analyzed in a preliminary fashion in Phase 1; the results are reported in the WAM TM, Appendix I. There is no budget or schedule in Phase 2 for the more intensive analysis that is needed.

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments on Risk Analysis Report

No.	Section	Comment	Response
Reviewer: Michael Ramsbotham, U.S. Army Corps of Engineers			
56		Comment on salinity as a predictor of other contaminant mobility or water quality measure.	The critical first step in modeling Delta water quality is to adequately represent changes in constituent properties or concentrations due to movement of the water throughout the Delta (advection) and the mixing of the water as it moves (dispersion). Salinity is the convenient and sensible marker for use in calibrating these hydrodynamic phenomena. If the model for movement and mixing of the water adequately represents salinity, this is the essential first step toward adequately characterizing the temporal and spatial changes in other contaminants.
57		Concerning Reservoir Operation, Exports and Deliveries, it appears the reservoir management sub-module drives determination of consequences. A more detailed discussion of this sub-model, particularly the incorporated / imbedded decision rules on reservoir operation.	A more extensive discussion is provided in the WAM TM and detailed in WAM TM, Appendix C.

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments on Risk Analysis Report

No.	Section	Comment	Response
Reviewer: Michael Ramsbotham, U.S. Army Corps of Engineers			
58		The BAU assumptions should also be reviewed in light of the influence reservoir operations, exports and deliveries have on the outcomes.	The BAU assumptions were derived from present laws, standards, policies, water rights, contracts, plans, and operating procedures in place. They do not consider outcomes that state and federal water project managers do not have the tools to foresee or the discretion to prevent.
59	**Section 12**	Trains should not run through flooded islands. Risk of stability / liquefaction failures of the embankments are high when saturated railroad embankments and foundations are subjected to train induced vibratory loading.	Concur. But the railroad will make its own decisions. Their response to the Jones Tract incident was used as a model.
60	**Section 13**	13.2.1 Sunny Day Risk to Individual Islands - It does not seem appropriate to extrapolate the historical rate of sunny day levee failure based on length of levee. It seems more appropriate to assign a higher rate to those islands with a documented higher frequency of problems and emergency repair and inadequate maintenance and inspection. Lower probability of sunny day failure should be assigned to those islands that have under gone significant upgrade in the last 20 years, have few documented recent problems, have good maintenance records, and an adequate inspection staff.	Concur. The data are not complete and exhaustive enough to allow such a study. The reliability of the data is not adequate at this time. Jones Tract failure was a surprise to everyone since those levees were considered the “best.”

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments on Risk Analysis Report

No.	Section	Comment	Response
Reviewer: Michael Ramsbotham, U.S. Army Corps of Engineers			
61		13.2.2 Seismic Risk to Individual Islands - Since the seismic risk can not be calibrated to historical events these results should receive additional scrutiny, possibly by an independent group of Earthquake Engineers familiar with the Delta. Overall it seems to me that the seismic risk may be overstated.	An independent review group is being convened.
62		13.2.4 Flood Risk of Individual Islands - The modeling indicates the flood risk generates from flow in the main tributaries to the Delta. It seems that the flood hazard associated with flow down the San Joaquin and the Sacramento including their distributaries through the Delta is under estimated. There is a significant history of flood fights along the Sacramento, San Joaquin, Steamboat Slough, Georgianna Slough, Sutter Slough, Elk Slough, and many other waterways in the Delta. Overall it seems to me that the flood risk is understated.	No response needed.
63		For each of the six selected failure scenarios what is the probability each of these will be caused by a flood or a seismic event? The assumption that scenarios 4 through 6, (10, 20, and 30 island flooded), apply only to seismic events is reasonable.	The probability of failure under flood or seismic event will be presented separately.

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments on Risk Analysis Report

No.	Section	Comment	Response
Reviewer: Michael Ramsbotham, U.S. Army Corps of Engineers			
64	**Section 14**	Increases in Seismic fragility, Flood Fragility and Sunny Day MHHW Fragility are overstated because of; the assumptions made with respect to the influence of subsidence, the assumption that the levees will not be raised in response to decreasing freeboard, and there is apparently no benefit recognized for continuing maintenance, repair and upgrades to Delta levees.	The assumption of the BAU was discussed earlier. It recognizes continuing levee maintenance at the 2005 funding levels (pre-bonds 84 and 1E). This assumption is likely to be a more realistic view of funding for the next 50 to 100 years than the present increases due to the bonds. With regard to levee crest raises, the assumption of no crest raises has been relaxed. But the stated assumption is now that (given the BAU funding levels) some islands will be able to keep up with sea level rise for a while but others will not. Most will fall behind due to funding problems at some point prior to year 2100.
65		The conditions and assumptions of Business as usual (BUS) should be reviewed and confirmed appropriate and realistic.	This requires a discussion with the Steering Committee (SC). The study has already been conducted under that assumption.

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments on Risk Analysis Report

No.	Section	Comment	Response
Reviewer: Michael Ramsbotham, U.S. Army Corps of Engineers			
66		The BAU assumption that levee crest elevations will not be raised in response to increased tidal and flood elevations in not realistic. Some levee crest raise should be incorporated into the evaluation. A 1 ft raise seems easily doable but a 3 ft raise seems less likely, but maybe not for 100 years of effort.	See note above.
67		Flood fragility evaluation should include estimated increases in flood levels due to the increased magnitude of the 100-yr event.	Concur.

No.	Section	Comment	Response
Reviewer: Michael Ramsbotham, U.S. Army Corps of Engineers			
68		The following statements found in this section should be considered for inclusion in the Executive Summary. • 14.3 Summary Perspectives on Future Risk • All significant risk factors are increasing as one looks forward to 2050 and 2100. The overall likelihood of a major event in increasing and the magnitude of consequences from a given event are also rising. • The climate change shift to more frequent major floods will be a major factor in increased future flood risk. • The impact and consequences aspects (water quality, in Delta population and assets, Delta ecosystems, and water exports) will be the primary factors that increase future risks from seismic levee breaches. • The economic concern with the most widespread consequences is the disruption of the state's water supply system. There may be a 300 to 400% increase in the number of people living on Delta/Suisun Islands by 2050, with a total exposed population approaching 100,000. These residents would be exposed to substantial risks from levee failures.	Good suggestions will be added or combined with the key findings already in the report.

No.	Section	Comment	Response
Reviewer: Michael Ramsbotham, U.S. Army Corps of Engineers			
68 (cont'd)		• Management and recovery from levee breach events that are now calculated to require several years may simply have to wait for one or more wet years to renew fresh water conditions in the Delta. • Major changes in Delta levee damage response and repair technology are not expected, however, availability of marine resources for levee repair in unpredictable, but is assumed not to change markedly. • Information in not available to make a comprehensive assessment of future risk. Information is available to estimate the relative increase in risks with respect to the 2005 base case.	
69	**Section 15**	Clarify. Is annual mean number of breaches from flood events 2.2 or 2.6 per year? Executive summary says 2.6.	Text has been revised to incorporate the comment.
70		Recommend the first bullet on topographic and bathymetric data gap be repeated in the Executive summary.	Done.
71		Complete 15.2. Observations, insights, findings for Seismic Hazard.	Done.
72		Complete 15.3. Observations, insights, findings for Flood Hazard.	Done.

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments on Risk Analysis Report

No.	Section	Comment	Response
Reviewer: Michael Ramsbotham, U.S. Army Corps of Engineers			
73		15.4 Observations, insights, findings for subsidence. a) Suggest that interior island subsidence be differentiated from levee subsidence. b) It seems that the single biggest influence on interior island subsidence is land use practice.	Notes will be made in the text to differentiate the two.
74		Complete 15.5.1 Key Findings and Study Limitations.	Done.
75		Complete 15.5.2 Water Analysis Module - Hydrodynamics and Water Quality	Done.
76		Recommend including observations / statements in the executive summary on the Complexity of reservoir management, Restrictions on Delta water operations, Water rights, water quality standards and contracts, Order of repair, various year types, Future water supply and demands.	Done.
77		Complete 15.7 Findings from Ecological Impact Analyses.	Done.

Delta Risk Management Strategy Phase I
Response to Comments on Risk Analysis Report

No.	Section	Comment	Response
Reviewers: Tim Smith and Bill Harrell, DWR			
Draft Summary Report			
1	**Draft Summary Report, Page iii, paragraph 2:**	Makes statement about the extinction risk to delta smelt if the *large scale breach event would occur in March or April.* This statement seems to ignore recent work by Bill Bennett studying "Big Mama" smelt that spawn in January and February.	Update that considers Bill Bennett's work is made in the text.
Draft Risk Analysis Report			
1	**Risk Analysis Report: Page 12-5, Section 12.1.1, first paragraph, sentence 2:**	Statement is inaccurate. Delta smelt are also found in the Suisun Marsh and Bay.	Comment is correct. Change made to text.
2	**Page 12-5, section 12.1.1, first paragraph, last sentence:**	Sentence is unclear and would benefit from rewriting.	Sentence rewritten for clarity.
3	**Page 12-5, section 12.1.1, third paragraph, first sentence:**	Statement is too general and therefore unable to convey meaning. The statement needs to contain some reference to baseline temperature in order to arrive at accurate conclusions. For example, if temperature is suboptimal, increased temperature would be beneficial via increased metabolic rate and increased growth translating into improved survival.	The statement has been rewritten. The statement is intended to convey the following fact: initial analyses indicated that water temperatures in the Delta would not change as a result of levee failures. The authors are sensitive to temperature issues in the Delta; however, because there was no indication that levee failures would result in temperature changes, temperature changes were not modeled in the risk assessment exercise.

Delta Risk Management Strategy Phase I
Response to Comments on Risk Analysis Report

No.	Section	Comment	Response
Reviewers: Tim Smith and Bill Harrell, DWR			
4	**Page 12-5, section 12.1.1, last paragraph, first sentence:**	Statement is unclear and would benefit from rewriting.	The sentence has been reworded for clarity.
5	**Page 12-6, section 12.1.1, paragraph 1, sentence 1:**	The definition of X2 is misleading. The water on either side of X2, 2 ppt isohaline, would best be described as brackish.	The sentence has been rewritten to increase clarity. The definition used is that found in Jassby et al. 1995.
6	**Page 12-6, section 12.1.1, paragraph 1, sentence 2:**	6. Sentence states that encroachment of X2 reduces available habitat for fish. It would be a more effective statement to include what fish species would be affected.	The statement has been reworded to be more specific. A sentence has been added to be more comprehensive. Examples of salinity-intolerant fish species/lifestages have been included. Examples of freshwater-intolerant fish and invertebrate species have been added as well. Enumeration of all the fish species that would be affected by salinity changes would be inappropriate in this paragraph.
7	**Page 12-6, section 12.1.1, paragraph 3, sentence 3:**	Sentence is difficult to read and would benefit from rewriting.	The sentence has been rewritten to improve clarity.

Delta Risk Management Strategy Phase I
Response to Comments on Risk Analysis Report

No.	Section	Comment	Response
Reviewers: Tim Smith and Bill Harrell, DWR			
8	**Page 12-6, section 12.1.1, paragraph 3, last sentence:**	*Sizeable benefit* is an ambiguous and somewhat inaccurate generalization of the non-natural mortality on fish that occurs as a result of SWP/CVP pumping operations. While delta smelt would likely derive a high benefit from a 2-year pumping curtailment, many of the other species included in the risk analysis study would probably realize only a low benefit.	This comment demonstrates the use of categorical generalizations regarding reductions in entrainment-related mortality. For example, it is unclear how the commenter concludes that "many" species … would probably realize only "low" benefit. Delta smelt and the four species of Chinook salmon would benefit from pumping curtailment in most years; indeed, mandatory reductions in pumping are part of the efforts to protect these threatened and endangered species. Other species (e.g., longfin smelt) would benefit from pumping curtailment in certain years (i.e., those with low freshwater outflows). The sentence has been altered for clarity, but we stand by the larger point (that reductions in SWP/CVP exports can benefit species/life stages that use the Delta).
9	**Page 12-6, section 12.1.1, paragraph 4, sentence 4:**	Sentence states *reductions in flow would impair downstream fish habitat.* This generalized statement would benefit from the inclusion of a reference to timing and of what species would be affected as it would not necessarily be true for all fish species	This sentence has been modified to reflect these comments.

No.	Section	Comment	Response
Reviewers: Tim Smith and Bill Harrell, DWR			
		included in this risk analysis study. The second half of this same sentence could use some clarification to more effectively convey the intended information.	
10	**Page 12-7, section 12.1.1, paragraph 1:**	10. These overly broad statements are not necessarily true, i.e. flushing flows in upstream salmonid habitat could result in channel scour disrupting redds and killing eggs.	The paragraph has been modified for clarity. Obviously, the number of potential management responses is large and each response would have different effects on different species, and these effects would be contingent on a number of variables (e.g., time of year, duration). Describing all the potential results of all potential water management changes is beyond the scope of this paragraph. The point of the paragraph is to demonstrate that different management responses have potentially positive and potentially negative implications for aquatic habitats and species in the Delta.
11	**Page 12-7, section 12.1.1, paragraph 3, and sentence 4:**	Statement is inaccurate. Juvenile longfin smelt and striped bass are not restricted to shallow waters. In addition, the second statement regarding vegetation providing refuge from predators is inaccurate and conflicts with a statement in the last paragraph of page 12-8, which cites *Egeria*	The comment is correct; juvenile longfin smelt and striped bass are not restricted to shallow waters. Also, the statement regarding vegetated refuge from predators is confusing, as the comment indicates. These sentences have been removed.

No.	Section	Comment	Response
Reviewers: Tim Smith and Bill Harrell, DWR			
		densa results in creation of habitat for invasive predatory fish…	
12	**Page 12-8, section 12.1.1, paragraph 3, sentence 1:**	Sentence should include statement acknowledging the light limiting impacts of suspended sediment which would have a negative influence on phytoplankton production until the sediments settle out.	The sentence and the paragraph have been modified to reflect the potential impact of suspended sediment on phytoplankton production and the effect of changes in phytoplankton production on Delta fish populations.
13	**Page 12-8, section 12.1.1, paragraph 3, sentence 2:**	Sentence that begins *invasive bivalve* is unclear and needs revising.	Sentence and paragraph revised.
14	**Page 12-8, section 12.1.1, paragraph 3, last sentence:**	This sentence could be improved by the addition of the word short-term just before the phrase *phytoplankton production in the Delta.*	Sentence and paragraph revised.
15	**Page 13-6, section 13.4.1, paragraph 1, sentence 1:**	Sentence could be improved by the inclusion of some examples of aquatic plants that would benefit from the newly created habitat.	Section 13 of the Risk Analysis Report has been substantially revised, and this comment does not apply to the revised section.

Delta Risk Management Strategy Phase I
Response to Comments on Risk Analysis Report

No.	Section	Comment	Response
Reviewers: Tim Smith and Bill Harrell, DWR			
16	**Page 13-6, section 13.4.1, paragraph 1, sentence 2:**	*Striped bass* **may** (strike would) *benefit from flooding because they* **can** *hunt….*	Section 13 of the Risk Analysis Report has been substantially revised, and this comment does not apply to the revised section.
General Comments on Draft Risk Analysis Report			
17	**General Comments**	There are many grammatical and spelling errors included in this report, some of which are described above. In addition, we encountered some factual inaccuracies and have provided editorial comments on each of these we found. After reading portions of the Draft Risk Analysis, the Summary Report, and the supporting materials contained in the Ecosystem Impacts Technical Memorandum it is readily apparent that substantial effort was put into creating these reports. Unfortunately, the Draft Risk Analysis and the Summary Report are not as effective as they could be at summarizing the work of DRMS and conveying it to the reader. Many statements contained within the reports are sweeping generalizations that fail to lead the reader to the desired conclusion. Also, some portions of the report would benefit from the inclusion of additional supporting information to	Comment noted. The aquatics section has been substantially revised to remove sweeping generalizations and to add supporting information on calculation of fish risk score.

No.	Section	Comment	Response
Reviewers: Tim Smith and Bill Harrell, DWR			
		improve clarity and comprehension for the reader.	

Delta Risk Management Strategy Phase I Response to Comments: Risk Analysis Report			
No.	**Section**	**Comment**	**Response**
Reviewer: Ralph Svetich, Department of Water Resources			
Comments on Draft Phase 1 Summary Report			
1	**Summary Report**	General Comment – A lot of the bullets need editing, as it is not always easy to understand what is trying to be said by the bullet.	The Summary Report has been reformatted and rewritten. Any remaining bullets are reworded.
2	**Page I, Executive Summary, 1st bullet**	I don't think the term PGA would mean much to a layperson. Can a PGA of 0.1g and 0.2g be related to probable island breaches? I think it would mean more to the public if we put this probability in terms of probable island breaches or failures.	Discussion of PGA in the Summary Report is now limited to the seismic section.
3	**Page i, Executive Summary, fourth bullet**	Is this island failure frequency based on today's (current) status, or does this mean in the next 100 years we can expect to have 2.6 failures per year?	It is based on today's conditions. The text has been clarified.
4	**Page I, Executive Summary, last bullet on page**	Commas are needed after, seismicity, and sea level rise. Change second sentence to read something like, "This results in an increased probability of levee failure of 12% in the year 2050, and an increased probability of levee failure 27% in the year 2100."	Text has revised and the report reformatted and rewritten.
5	**Page ii, Executive Summary, first bullet on page**	Start sentence with "As a result of sea level rise …"	Text has revised and the report reformatted and rewritten.
6	**Page ii, Executive Summary, second bullet**	It seems as though a word is missing after 100-year flood? Could it be flood level, event? I would also put " as a result of sea level rise" just before 64%.	Text has revised and the report reformatted and rewritten.

Delta Risk Management Strategy Phase I Response to Comments: Risk Analysis Report			
No.	**Section**	**Comment**	**Response**
Reviewer: Ralph Svetich, Department of Water Resources			
7	**Page ii, Executive Summary, third bullet**	Same question as above, it seems as though a word is missing after 100-year flood.	Text has revised and the report reformatted and rewritten.
8	**Page iv, Executive Summary, Economic Impacts, fourth bullet**	I would either delete the last sentence, or re-word it to get rid of the words, " it appeared to us".	Text has revised and the report reformatted and rewritten.
9	**Page 3**	Either delete the chart "Program Functional Organization", or explain what it means, or at least put DWR on the top of the chart!!!	Text has revised and the report reformatted and rewritten.
10	**Page 15**	Are there probabilities associated with each of these scenarios? If not, it is difficult to understand the meaning of the consequences. i.e., if the 30 island failure is so small to be inconsequential, why even worry about it.	Yes, probability are associated with the scenarios. They have been added.
Comments on Draft Phase 1 Risk Analysis Report			
1	**Page 6-10, First paragraph, top of page**	"Expert elicitation was sought." Who were these experts? I would think the IRP members would like to know their names. Please provide, if you know who they are.	Clarified.
2	**Table 6-2**	None of the superscripts are listed on any of the pages of this table. Either delete the superscript, or provide a listing of what they mean.	Description of the subscripts was added.

Delta Risk Management Strategy Phase I Response to Comments: Risk Analysis Report			
No.	**Section**	**Comment**	**Response**
Reviewer: Ralph Svetich, Department of Water Resources			
3	**Page 10-2, Section 10.4.2, second sentence**	What is meant by the user inputs the mean growth rate? Who is the user? The person who runs the model, if so, why do we care that they input the information. Either delete or clarify who the user is.	Sentence removed as unnecessary.
4	**Page 12-4, last paragraph on page**	This paragraph mentions a detailed description of toxins potentially released during a levee breach, which was not available for the Eco TM is provided. Where is this discussed in this section? I probably missed it, but it may help clarify this paragraph, if there was a reference listed that shows where the effects of the toxins are discussed.	Section 12.1.2.
5	**Page 13-4, third paragraph, last sentence**	I believe this sentence is so important to the understanding of the Economics discussion, that it needs to be shown in Section 12 also. I would recommend this sentence is also placed on page 12-3, at the end of the first paragraph on the page.	Done.
6	**Table 13-6**	Shrink this table to fit on one page.	Done.
7	**Figure 13-7 through 13-11**	Add the number of islands flooded to each title block. i.e., Case 1 Scenario **1** Island Flooded, etc.	Tables revised.
8	**Page 14-12, Section 14-3, last sentence**	I think this sentence needs to be part of the Executive Summary of the Summary Report. Where I don't know, please consider placing it in the ES.	Done.
9	**Page 15-8, fourth bullet**	Again, I think this bullet about the CCWD should be included in the Executive Summary of the Summary Report	Done.

Delta Risk Management Strategy Phase I Response to Comments: Risk Analysis Report			
No.	**Section**	**Comment**	**Response**
Reviewer: Ralph Svetich, Department of Water Resources			
10	**Page 15-8, middle of the page**	I would suggest that this section that starts with "The risk analysis was carried out,…" should be Section 15.7 Data Gaps.	Section 15 was reformatted and rewritten.
11	**Page 15-9, next to last bullet on the page**	I'm not certain I understand what this data gap is trying to say. The relative importance compared to what? If this is clear to the author of this section, then disregard this comment.	Section 15 was reformatted and rewritten.

Delta Risk Management Strategy Phase I
Response to Comments on Risk Analysis Report

No.	Section	Comment	Response
Reviewer: Robert Twiss, Steering Committee Member			
1	These brief comments are simply to record and summarize points I made verbally at the DRMS Steering Committee meeting of May 22, 2007.	Based upon review of the TM documents, review of comments by Wim Kimmerer, and verbal communication with other reviewers, I recommend the following: Given the constraints, further time and resources should not be put into the TM per se; beyond correcting any errors or unsubstantiated conclusions identified by reviewers. Efforts to re-write, re-organize, and clarify the existing documents should be re-directed to preparation of a summary, bridging document.	Agree.
2		This short summary document should help bridge the gap between the TM materials and the Risk Analysis. It is understood that the TM is not intended to identify probabilities, but the TM should explain the implications of hazards and events, so that when the Risk Analysis is performed, the implications for the ecosystem can be accounted for. Such a clear assessment and enumeration is thus far lacking in the current TM. I agree with other reviewers that this bridge document should focus on topics on which quantitative results were possible, and list and describe others on	Agree with the concept of preparing a Summary Report. Explanations have been provided in the TMs and Risk Analysis Report to clarify what conditional probabilities are provided and how they relate to the integrated risk in the Risk Analysis Report. It is true that the level of detail and model sophistication used to present risk and uncertainties vary from one topic to the other.

No.	Section	Comment	Response
Reviewer: Robert Twiss, Steering Committee Member			
		which qualitative analysis was all that could be accomplished.	
3		The TM itself should explain the nature of this shift in report organization. It should state that the TM documents per se have not been revised in response to peer review. It might point out that the new addendum serves the purpose of response to peer review, and that the two, taken together, are intended to meet the terms of the contract for this work. But of course I would defer to the contracting staff on this.	Many comments have been received on both the TMs and the Risk Analysis Report. Responses to comments and revisions to the TMs have been provided.
4		The Risk Analysis might be clarified to explain that many items (especially in the Ecosystem TM) could not be subject to formal, quantitative risk-analysis methodology, but are included in the report because valuable results were nonetheless obtained.	Agree with this comment, and we will add clarification in the Ecosystem TM.

Comments on Seismology Technical Memorandum and Responses of Consulting Team

and

Comments of Probabilistic Seismic Hazard Review Panel (PSHRP) and Responses of Consulting Team

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments: Seismology Technical Memorandum

Comments	Responses
Reviewer: Goettel, U.S. Army Corps of Engineers	
General Comments	
The probabilistic seismic hazard analysis (PSHA) follows the current state of the art "usual" practice; the approach is very well vetted. For the most part, as expected, the PSHA follows and draws heavily on the Working Group consensus PSHA models for the greater SF Bay area. However, as usual, however, the devil is in the details, and in some cases the DRMS team diverged from the Working Group consensus. In part these differences reflect newer data, but in part they also simply represent differing professional opinions among seismologists. Both CGS and USGS provided detailed technical comments on a previous draft of this TM, and URS provided detailed responses to both the CGS and USGS comments.	For the time-dependent hazard calculations, we adopted the characterization of the San Francisco Bay Area faults from WGCEP (2003) without exception. Note that the WGCEP (2003) did not characterize the Delta faults. In the time-independent hazard calculations, we did differ from WGCEP (2003) in two instances. However, the time-independent hazard was not used in the risk analysis.
The main CGS points were: 1. A number of faults in the Delta that make significant contribution to the Delta's seismic hazard are not documented.	Delta faults are now documented in the revised (draft 2) Seismology Technical Memorandum (TM).
2. The value assigned to the probability of activity for Delta faults seems arbitrary and high,	Bases for probability of active Delta faults have been documented in the revised (draft 2) Seismology TM.
3. The fault model appears to imply a relatively high occurrence rate for earthquakes in some cases. Specifically, the rate for the Greenville fault is higher than the known historical seismicity,	The higher rate for the Greenville fault is only in the time-independent hazard calculations, which was <u>not</u> used in the risk analysis. The model recurrence compares favorably with the historical seismicity.
4. It appears that URS has made a number of conservative assumptions regarding activity rates on structures that are not well understood. The sum of these conservative assumptions is a seismic hazard model that appears to be quite conservative, and may be inconsistent with the overall rate of fault movement across the region and with historic rates of seismicity.	There were no explicit conservative judgments made in the DRMS PSHA. The objective of this PSHA was to develop the best possible seismic source model based on the available information and data and to include the associated uncertainties. Comparison of our model recurrence is in reasonable agreement with the historical record, given its incompleteness.

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments: Seismology Technical Memorandum

Comments	Responses
The main USGS points were: 1. There is considerable uncertainty in, insufficient documentation of, and an apparent lack of a community consensus for, the characterization of the Delta area faults used in the seismic TM.	Characterization of Delta faults has been documented in the revised (draft 2) Seismology TM.
2. A significant part of the seismic hazard will be the response of the deltas to long-period (T >1.0 second) motions from large earthquakes (M > 7.5) on one or more elements of the San Andreas fault system.	Levees may not be sensitive to long-period ground motions with periods greater than 1 sec. Discussion of this issue was added in the revised (draft 2) Seismology TM.
3. The important issue of time-dependent seismic hazard was not addressed in the draft version of the TM.	See above comments.
4. The URS PSHA assumed soil site Class C throughout the Delta, whereas the USGS noted that "it is likely that much of the Delta is underlain by lower velocity site Class D deposits.	URS used a reference site (100 feet deep) shear-wave velocity of 1,100 feet/sec. The site response analysis considered all shear-wave velocity data that characterize the near-surface soil column. The site response analysis is documented in the Levee Vulnerability TM.
URS provided detailed responses to the CGS and USGS comments, and generally agreed to provide more documentation and to adjust some of the input assumptions for the PSHA to reflect the CGS and USGS concerns. Overall, I concur with ALL of the CGS and USGS comments on the previous draft. URS has incorporated most of these comments into the current draft, to varying extents. An important refinement was to include time-dependent hazard estimates, along with the time-independent results.	No response required.

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments: Seismology Technical Memorandum

Comments	Responses
Goettel, Other General Comments	
1. Overall, the PHSA results appear conservative (by design) in that identification of faults as active, slip rates, etc. may overestimate the actual level of seismic hazard. My preference would be for a straight down the middle best estimate, but conservative is OK, as long as it is explicitly acknowledged as such. Therefore, I suggest adding such an acknowledgment.	See previous response regarding conservatism on page 1.
2. The remaining differences in professional opinion between CGS, USGS and URS regarding earthquake source are not negligible, but they do not appear to be large enough to substantially affect the results – that is, tweaking the input data sets a little this way or that way would not likely change the main conclusions drawn from the seismic hazard results.	No response required.
3. USGS comment 4 (above) addressed the site Class C vs. D. issue, which is very important for modeling seismic failures of levees. As per the IBC soil/rock factors, seismic ground motions at any give site depend directly on the amplification or deamplification of soils (which varies with the intensity of shaking). The URS response (public comments on the DRMS website), page 14 states: "Our models do not rely on the use of the site classes (C or D)." I'm completely baffled by this statement. Yes, the source characterizations are independent of site class, but the hazard curves at any location are NOT independent of site classes. Page 2 of the TM (Item 3) notes that seismic hazard results are defined for a stiff soil condition (Class C) which appears incorrect per the USGS comments. **This issue is important and warrants further consideration/response/ correction by URS.**	URS used a reference site (100 feet deep) shear-wave velocity of 1,100 feet/sec. The site response analysis considered all shear-wave velocity data that characterize the near-surface soil column. The site response analysis is documented in the Levee Vulnerability TM. If it is of any interest, the NEHRP site class is more like F.

Comments	Responses
4. The issue of whether time-independent or time-dependent seismic hazard results should be used for Delta levee risk studies has a significant impact on the inferred level of seismic risk for the levees. The TM presents both results, but with little or no commentary or recommendations for use. I believe that the TM seismology team has an obligation to make an explicit recommendation in this regard. My gut feeling, without reviewing the seismology in detail, is that the evidence for time dependence for the major SF Bay area faults is pretty strong and thus I suggest that the time-dependent results are more meaningful for Delta risk studies.	It was stated in the TM (page 1, fourth paragraph) that the time-dependent hazard was to be used in the risk analysis.
5. Major omission. The seismology TM focuses, as perhaps appropriately, entirely on seismology – sources, probabilities, hazard curves, etc. However, for the actual seismic risk evaluation of Delta levees there are two other critical issues: a. Site Class (which governs amplification/deamplification for various scenario earthquake events) and thus profoundly affects levee failure calculations. This issue was addressed briefly above and surely warrants more detailed consideration.	These "omissions" were evaluated in the Levee Vulnerability TM.
b. Geotechnical issues for the Delta. Seismic risk for the levees depends profoundly on geotechnical issues for the soils underlying and within levees. Seismic risk for the levees cannot be meaningful evaluated without consideration of the potential for liquefaction, settlement, and lateral spreading. Thus, a geotechnical review of soils data/modeling for the Delta appears central to the risk evaluation for levees. **However, NONE of the TM appear to address the geotechnical issues. Perhaps, I've missed something, but if**	Site response analysis was performed in the Levee Vulnerability TM. Please review that TM.

Comments	Responses
not, this appears to be a major omission in the DRMS study. I expected a full TM on geotechnical issues.	
6. Major omission: hazard maps. The numerous figures include are all dis-aggregated results for specific faults. For an overview, it would be VERY helpful to see seismic hazard contour maps with, for example 2%, 5%, and 10% in 50 year ground motions (PGA, 0.3 sec, 1.0 second) for the whole study area. Such maps would allow readers to see immediately where seismic hazard levels are higher or lower within the Delta. **Including such maps would greatly improve the usefulness of the TM.**	Hazard maps have been completed and are now included in the revised (draft 2) Seismology TM. However, only maps for PGA and 1.0 sec SA and for return periods of 100 and 500 years, the most significant return periods to the Delta, have been produced.
Specific Comments	
1. Page 1 notes that the products of the PSHA will include site-specific acceleration response spectra at selected levee sites throughout the Delta. **Where are these? None are included in the TM.** Hazard maps of the Delta are also being developed by are not described in this TM? **Where are these? When will they be available?**	Response spectra were not calculated, so page 1 has been revised. Hazard maps are now included in the TM.
2. Page 7 paragraph 2 notes that time-dependent rates for the major faults are "forthcoming." Aren't these in Table 2, already included?	Page corrected.

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments: Seismology Technical Memorandum

Comments	Responses
Reviewer: Benvenuto, U.S. Army Corps of Engineers	
Introduction. Comment: Although the Final Draft memo lists the Graymer map in the list of references, I could find no reference to the map in the text. URS' response appears inadequate because the Final Draft doesn't discuss why their fault model is not based on the most recent data (by Graymer, et. al.) or how their model is "consistent" with the Graymer map.	The Graymer et al. map and documentation is a compilation from other sources. The DRMS team used the original sources to develop the seismic source model.
Introduction. The paper by Argus and Gordon (2001) appears to be important and relevant to the DRMS model. The Final Draft doesn't reference or discuss the paper, or why data from it wasn't used. Furthermore, I don't understand the statement "uncertainties in the reduced GPS data …… are permissive of the low slip rates we have adopted."	The Argus and Gordon paper discusses GPS results for a large region. The more relevant data from d'Alessio et al. and Prescott et al. are now discussed in the revised (draft 2) TM.
i. Documentation. Comment: I was unable to find a clear discussion of the sources of uncertainty that affect the model results.	The revised (draft 2) TM has been changed to include additional discussion of uncertainties and their impact on hazard.
B) I was unable to find any discussion of dip weighting, down-dip fault extents, and fault widths. Many of the faults described do not include a discussion of fault dip.	The revised (draft 2) TM has been updated to include discussion of dips for Delta faults.
C) No response in the Final Draft DRMS to this comment was found.	The revised (draft 2) TM has been updated to include hazard sensitivity to Sierra Nevada faults.
D) No response in the Final Draft DRMS to this comment was found.	Cascadia megathrust and 1857 have now been included in the PSHA.
i. No response in the Final Draft DRMS to this comment was found.	Sensitivity analysis was performed and documented in the revised (draft 2) TM.
ii. No response in the Final Draft DRMS to this comment was found.	Comparison was not possible because the historical seismicity in the Delta is <u>not</u> adequate.
iii. No response in the Final Draft DRMS to this comment was found.	Comparison was not possible because the historical seismicity in the Delta is <u>not</u> adequate.
Implications i. No discussion of the displacement discrepancy was added to the Final Draft	Discussion of rate discrepancy has been added to the revised (draft 2) TM.

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments: Seismology Technical Memorandum

Comments	Responses
DRMS. However, URS' response doesn't indicate they would add a discussion, even though it probably warrants it.	
Ground Motion iv. Comment: The results of the finite element modeling do not appear to have been incorporated into the PSHA study, as the USGS suggested in their comment.	PSHA was done for 1,100 ft/sec site condition. Site response analysis was performed in the Levee Vulnerability TM.
PSHA Approach i. No response in the Final Draft DRMS to this comment was found.	Discussion of uncertainties and impact on hazard has been expanded in the revised (draft 2) TM. Comparison between pre-NGA and NGA models was not performed because it is not considered to be relevant.
ii. No response in the Final Draft DRMS to this comment was found.	Sensitivity analysis was performed and discussed in the revised (draft 2) TM.
iii. No response in the Final Draft DRMS to this comment was found.	Comparison was made and discussed in the revised (draft 2) TM.

COMMENT	RESPONSE
Introduction	
The Scope of Work for our review of the Seismic Hazard Technical Memorandum (SHTM), dated Oct. 27, 2006, posed the following questions: 1) Have the best and/or most appropriate scientific information been used in developing the technical data? 2) Are the methods of collecting information (existing or new) understandable, scientifically defensible, fully documented and the best available? 3) What information (e.g., data, conceptual models, etc.) is missing that should have been presented or addressed? 4) Were the processes and methodologies (e.g. analyses of data) that were used understandable, scientifically defensible, fully documented, and the best and/or most appropriate? 5) What results are missing that could reasonably be obtained? 6) Are the key findings and conclusions supported by the stated data, methodologies or conceptual models, and analyses results?	NRR
The time-independent Probabilistic Seismic Hazard Analysis (PSHA) in the SHTM uses accepted practice, and we consider it appropriate for a conventional time-independent PSHA analysis. However, as described in the following sections in some detail, the lack of sufficient documentation throughout the SHTM makes it very difficult to answer the questions posed in the Scope of Work, including the last, and perhaps most important question, 6) whether the key findings and conclusions are supported by the stated data.	Seismic sources characterized as part of this study or in previous studies in which documentation is not readily available will be described in the Final DRMS Seismic Hazards Technical Memorandum.
Because no new data were collected for the SHTM, we did not evaluate the methods of collecting information used for the SHTM.	NRR
Data and results are missing from the SHTM that could easily be obtained. A new map and database for all Quaternary active faults in the San Francisco Bay Area has been recently published (Graymer et al., 2006). The subsurface geometry of the Pittsburg-Kirby Hills fault can be re-evaluated using new high-resolution seismic reflection images across it (Parsons et al., 2002). New Global Positioning System (GPS) based geodetic models of the regional strain field are available in Argus and Gordon (2001), d'Alessio et al. (2005), and Coyle et al. (2006). The latter presents new results from continuous GPS stations installed by the Plate Boundary Observatory in the Great Valley and East Bay Hills; these data are available online at pboweb.unavco.org.	We have examined the Quaternary fault map and our seismic source model is consistent with the map. This comment is not specific about what data are missing. Based on discussions with R. Graymer at the 21 November 2006 meeting, we assume that this comment refers to Quaternary faults in Livermore Valley. We have included these structures as sources in the revised DRMS model. The high-resolution reflection data from the Sacramento River channel presented in the Parsons et al. (2002) paper were acquired during the BASIX program and

NRR = No Response Required

COMMENT	RESPONSE
	originally presented in an AGU poster by McCarthy et al. (1994). The Thrust Fault Subgroup of Working Group 1999 cited the original McCarthy et al. (1994) data and abstract to model the Pittsburg-Kirby Hills fault as a steeply east-dipping to sub-vertical fault accommodating strike-slip displacement (Thrust Fault Subgroup, 1999). The correlation of the deep seismicity in the western Delta region with the trace of the fault in the Sacramento River channel in Figure 10 of Parsons et al. (2002) suggests that the fault may dip about 80° east instead of being vertical as modeled in the DRMS source characterization. We will incorporate uncertainty in the dip of the Pittsburg-Kirby Hills fault in the revised seismic source model. This comment is not specific as to how the GPS data should be used, and does not state whether the reviewers feel the DRMS model violates these data. As a general reply, we are familiar with the recent work by Argus and Gordon (2001), Prescott et al. (2001) and d'Alessio et al. (2005). In our opinion, the uncertainties in the reduced GPS data and velocity field at the latitude of the Delta are permissive of the low slip rates we have adopted for the Delta faults. It is beyond the scope of our work to evaluate raw PBO data.
A more fully documented and comprehensive seismic hazard assessment will make the SHTM study more definitive, and lead to its greater acceptance by the scientists, land owners and users, and policy makers that are its intended audience. Our specific comments on the Seismic Hazard Technical Memorandum (SHTM) are divided into three main topics: (1) earthquake source characterization, (2) ground motion and attenuation, and (3) PSHA methodology and approach. In the following sections we amplify our comments on these three main topics.	The Final Technical Memorandum will provide improved documentation of the PSHA and the bases for the seismic source model.

NRR = No Response Required

COMMENT	RESPONSE
Earthquake Source Characterization	
Data Documentation	
The earthquake sources used for the computation of strong ground shaking at sites in the Delta, which are collectively referred to as the seismic source model, are listed in Table 1 of the SHTM. The parameters listed for each fault include the probability of activity, rupture scenario, segment name, rupture length, width, dip, direction of dip, sense of slip, magnitude, slip rate, and notes such as references. These are the basic parameters needed to characterize seismic sources, particularly with regard to the magnitude and average recurrence intervals of earthquakes. In this regard Table 1 is the core of the PSHA.	NRR
Inspection of Table 1 shows that there are four groups of faults: 1) Major regional faults in the San Francisco Bay Area whose characterization is taken directly from large review group efforts such as the Working Group on California Earthquake Probabilities (WGCEP, 2003) or the California Seismic Hazard Map (Cao et al., 2003). This fault category is well-documented and straightforward.	NRR
2) Faults characterized by WGCEP (2003) and Cao et al. (2003) whose characterizations in the SHTM have been modified. Examples of this group are the Greenville and Concord-Green Valley faults. The SHTM neither describes how the original characterizations of these faults were modified nor describes what data/observations were used to make these changes. Inspection of the references provided in the notes section of Table 1 for these sources show they are either unpublished and non-peer reviewed Technical Reports to the USGS External Grants Program or are personal communications.	We have revised Table 1 and added a discussion of the Delta faults to improve the documentation of our seismic source model.
3) Earthquake sources that have not been characterized in previous consensus estimates (e.g. WGNCEP, 1996; WGCEP, 1999, 2003) of regional ground motion. Three examples are the Cull Canyon-Reliz Valley-Lafayette fault, the Las Trampas fault, and the Briones seismic zone in the East Bay Hills. These structures, which are assigned slip rates of up to 3 mm/yr, are based on unpublished and non-peer reviewed technical reports to the USGS. The SHTM does not present any of the primary data used to estimate the parameters for these faults.	The majority of the new, original work on the northern East Bay Hills sources was a NEHRP study completed in July 2002 (Unruh and Kelson, 2002), and thus post-dated the recent Working Group efforts. Although northern East Bay hills sources have not yet been incorporated in a community consensus model like Working Group 2003, they have been included in recent PSHA studies for a LNG facility at Mare Island, and for deterministic ground motions at Chabot Dam. The Final Technical Memorandum will include a summary of primary data used to develop the source parameters.

NRR = No Response Required

COMMENT	RESPONSE
4) A large number of faults, including those we refer to as the Delta faults (see below), whose characterization is documented only with short notes in Table 1, sometimes no references (e.g., Montezuma Hills zone), sometimes with just a single reference (the one reference for the Northern Midland fault and the Thornton Arch zone is the California Division of Oil and Gas, 1982), and personal communications.	The Final Technical Memorandum will include a more complete discussion of the Delta faults, including citation of all sources of data used to develop the source parameters.
Our principal concerns, and fundamental weaknesses of the SHTM, are: 1) the lack of documentation of the primary data used to characterize earthquake sources; 2) a reliance on unpublished and non-peer reviewed data and personal communication to characterize sources; 3) the absence of discussion of the basis for decisions on the quantification of source parameters (for example, what data allow a range of rupture lengths or dips to be selected for a source); 4) the lack of description of the data and the basis for weighting choices of parameters such as probability of activity and fault slip rate for use in the PSHA logic tree; and 5) an absence of discussion of the uncertainties in earthquake source parameters.	Most of the seismic source model is based on WGCEP (2003) for the major faults and the CGS (Cao *et al.*, 2003) for the remaining faults. Seismic sources characterized as part of this study or in studies where the documentation are not readily available will be documented in the Final Technical Memorandum.
Delta Faults	
For discussion, we use the term Delta Faults to refer to the group of faults that have been identified within the Delta proper or along its western margin. These are the Southern and Northern Midland faults, Vernalis fault, West Tracy fault, Montezuma Hills zone, Thornton Arch zone, Midway fault, Black Butte fault, Pittsburg-Kirby Hills fault, Portrero Hills fault, and the Coast Range-Sierra Block boundary faults north of the Delta. Of these the Southern and Northern Midland faults, Montezuma Hills zone, West Tracy fault, Pittsburgh–Kirby Hills fault, and northern Coast Range-Sierra Block faults are listed in Table 3 as the controlling seismic sources for PGA for the six sites specified in the SHTM.	NRR

NRR = No Response Required

COMMENT	RESPONSE
i. Documentation. The information on which the characterization of the Delta faults is based is presented only as short notes, some of which contain no references (see the Montezuma Hills zone, Table 1), or only one reference (see, the Northern Midland zone and Thornton Arch, Table 1). From these it is not possible to reconstruct the decision making process for development of the specific input parameters for these sources. Discussions at the DRMS meeting on November 21st, 2006 also indicated that there are alternative views on issues such as fault activity, segmentation models, and potential rupture lengths for the Delta faults, but these uncertainties are neither acknowledged nor incorporated in the present analysis. In our view both complete documentation and the development of a community consensus source characterization of the Delta faults are crucial for the acceptance of the results of the SHTM.	The Final Technical Memorandum will include documentation and discussion of the Delta faults. The scope of the DRMS evaluation of the Delta faults is approximately a SSHAC Level 1 analysis, with some additional querying and elicitation of unpublished information from experts in the Delta geology (i.e.., Dr. Janine Band and Mr. Scott Hector). Development of a community consensus source characterization of the Delta faults is not part of the scope of the DRMS source characterization. The Final Technical Memorandum will include discussion of the sources of uncertainty in Delta fault parameters.
ii. Probability of Activity. A review of Table 1 raises issues about the consistency with which the weighting of the probability of activity has been applied to the Delta faults. A short guideline for weights assigned to probability of activity is listed at the end of Table 1. A weight of 1.0 implies certainty about the activity of a fault. Either it has produced a historical earthquake or has geological evidence of Holocene movement. In the SHTM the Southern and Northern Midland fault, the West Tracy fault, and the Vernalis fault are all assigned a probability of activity weight of 1.0, implying that all have Holocene activity. None of these faults has ruptured historically and, except for a note that refers to “apparent” displacement of a Holocene peat on the Southern Midland fault, we did not find documentation for Holocene rupture in the SHTM. These faults are not shown as Holocene active faults in a recent USGS compilation of active faults in the Bay Area (Graymer et al., 2006). In contrast, the Sargent fault, which has historically produced small magnitude earthquakes, is creeping, and has evidence for repeated Holocene slip (Graymer et al., 2006), is given a weight of 0.8 for probability of activity.	All P(a)s have been reviewed and in some cases revised based on revised criteria (see footnote #1 in Table 1).

NRR = No Response Required

COMMENT	RESPONSE
Faults that are either "Late Pleistocene or [have an] inferred association with historical seismicity" are assigned a probability of activity weight of 0.7 in Table 1. The phrase "inferred association with historical seismicity" requires clarification. How close does an historical earthquake have to occur for a fault to be associated with it? Bakun (1999) has demonstrated that the location uncertainty of pre-instrumental historical earthquakes can be large, especially for events near the Delta. Because of this uncertainty, we suggest the addition of a discussion of the rationale for assigning an earthquake uniquely to a particular earthquake source. Faults whose "activity [is] inferred from [its] fault geometry [which is] considered likely to move under current tectonic regime" are assigned a probability of activity weight of 0.5. Is this a standard definition? It should be quantified in terms of the maximum deviations in strike and sense of fault motion relative to the regional stress field permitted by this definition. This definition does not require any independent evidence for recency of faulting. Moreover, there are mapped faults regionally having a favorable geometry with respect to the major strike-slip faults that are not active. Table 1 lists at least one fault, the Thornton Arch zone, with a probability of activity weighting of 0.3. This weighting value is not defined in the SHTM. What is the physical basis for it? Does a weighting of 0.0 correspond to pre-Quaternary faulting or an unlikely fault geometry in the current tectonic regime?	The criteria of association with historical seismicity is generic in nature and includes small magnitude seismicity. This criteria has only been used in a collaborative sense for the Pittsburg-Kirby Hills fault zone in this study.
iii. Earthquake magnitude. The magnitude of a future earthquake is a critical input to estimating ground motions. Table 1 lists expected magnitudes and their weights for the Delta faults. Estimates of magnitude are generally calculated from fault surface area, which is the product of fault rupture length and fault width (down dip extent and angle). A number of steps are commonly undertaken to estimate magnitude. These include:	NRR

NRR = No Response Required

COMMENT	RESPONSE
A) Estimating rupture length. The expected length of rupture is based on a rupture model or rupture scenario for a fault. The scenario defines whether a fault is segmented or unsegmented; and if it is segmented, whether it ruptures as single segments or as a combined multisegment rupture. Rupture scenarios are developed from available geological, seismological, geophysical, and geodetic data. Interestingly, the rupture scenario for the majority of the Delta faults is listed either completely or in part as "floating earthquake". The terms segment, multisegment, and unsegmented are clear, but what is a "floating earthquake"? There is no definition of this in the text of the SHTM. How is a magnitude, or a range of magnitudes, developed for the "floating earthquake" scenario, particularly if there is no information on rupture length? For some of the Delta faults a rupture length is given (Midway, Black Butte, Vernalis, and West Tracy faults) but the rupture scenario is a "floating earthquake" with a weight of 1.0. Why aren't these also treated as unsegmented faults? In addition we note that where a length is given for a Delta fault no uncertainty is assigned to it. These lengths seem too precise (tenths of a kilometer), given that they are estimated from subsurface data.	The floating earthquake and its maximum magnitude will be described in the Final Technical Memorandum. A floating earthquake is an event of some specified maximum magnitude distribution whose rupture length is less than the total length of the fault. The event is not associated with a specific segment and is thus allowed to "float" along the length of the fault. The Mmax is based on the observations of the rupture behavior of other faults. No rupture length is given in tenths of a kilometer. Uncertainty in rupture lengths is considered to be sufficiently small that they have very little to no impact on hazard. Uncertainties in Mmax from uncertainty in rupture length is included in the ± 0.3 used in the analysis.
B) Fault dip and width. It is important to provide the geological and geophysical documentation in the SHTM describing how dips for the Delta fault zones were estimated. Where weights are assigned to different dips, the basis for the weighting should be presented. Additionally, the basis for defining the down dip extent of faults (base of the seismogenic zone) in the Delta needs to be presented to support the estimate of fault width (dip and extent) required for calculating magnitude.	The final DRMS report will include a complete discussion of geological and geophysical data used to evaluate fault dip and width.
C) Magnitude calculations. We assume that the magnitudes listed in Table 1 for the Delta faults are based on magnitude/area or magnitude/length relations developed in Wells and Coppersmith (1994). Would using more recent relations developed by the Working Group on California Earthquake Probabilities (2003), i.e., Ellsworth (in WGCEP, 2003) or Hanks and Bakun (2002), yield appreciably different results? Whichever relations and equations are used should be referenced. From the information available in the SHTM it is extremely difficult to reproduce the magnitude estimates of future earthquakes proposed for the Delta faults.	Magnitudes not adopted from WGCEP (2003) or Cao *et al*. (2003) were computed using the rupture area relationships of Wells and Coppersmith (1994) and Hanks and Bakun (2002) and the rupture length relationship of Wells and Coppersmith (1994). The relationships are equally weighted. The latter was only used for **M** ≥ 7.0.

NRR = No Response Required

COMMENT	RESPONSE
iv. Slip Rates. The SHTM should provide comprehensive documentation describing how the slip rates listed in Table 1 for each of the Delta fault zones have been estimated, and the uncertainty in these slip rate estimates. Some of the slip rates are on faults with reverse-oblique slip. In the presentation to us on November 21st, 2006, the evidence for vertical separation was discussed but not shown, and the conversion from vertical separation rate to net slip rate was only briefly discussed.	The Final Technical Memorandum will include discussion of geological and geophysical data used to evaluate slip rate. Constraints on both horizontal and vertical separation will be discussed. We will clearly state where we make arbitrary assumptions about the ratio of vertical to horizontal slip.
v. Weights. The weights for different input parameters to the PSHA logic tree represent the opinion of the DRMS technical group as to which values are more likely to be correct based on the available information, and they strongly affect the final hazard calculations. As we have repeatedly noted, there is no description in the SHTM of the basis for the weights presented. The SHTM should provide comprehensive documentation describing how the weights were assigned in Table 1 for all of the Delta fault zones.	The basis for the weights will be described in the Final Technical Memorandum for the Delta faults.
Additional Earthquake Sources	
i. The earthquake sources characterized for use in the PSHA are listed in Table 1 and shown on Figures 2 and 3 of the SHTM. However, there are other sources, both local and regional, that have the potential to produce strong ground motions at levee sites in the Delta that have not been considered. Some of these faults are likely to be more significant in the PSHA analysis for the Delta than other faults presently included (such as the Monterey Bay-Tularcitos fault or the Zayante-Vergeles fault). We suggest inclusion of the following additional sources:	NRR
A) Faults west of the Delta. These include the Verona, Las Positas, and Williams Faults south of Livermore Valley that are near Clifton Court (our attached Figure 1). The Holocene activity of these faults is suggested by the presence of multiple Holocene and latest Pleistocene fluvial terraces along the stream valleys south of Livermore Valley, including Arroyo Mocho and Arroyo Valle (Herd and Brabb, 1980; Unruh and Sawyer, 1997; Knudsen and others, 2000; Witter and others, 2006).	The faults have been included in the PSHA and are now added to Table 1.

NRR = No Response Required

COMMENT	RESPONSE
B) Faults north and south of the Delta. Levee cracking and subsidence were reported at several locations, including on Webb Tract, after the M6.7 1983 Coalinga earthquake, located 240 km south of the Delta (Finch, 1985, 1987, and 1992). Although whether this cracking occurred as a result of the earthquake may be controversial, these observations and numerical modeling described below suggest that Coast Range-Sierra Block boundary sources should both be extended well to the north and the south beyond those used in the SHTM.	Sensitivity analyses indicate that other segments of the CRSB boundary zone do not effect the hazard in the Delta.
C) Faults east~~west~~ of the Delta. We think that it would be useful to include faults in the western Sierra Nevada foothills as well as the strike-slip and normal faults along the eastern margin of the Sierra Nevada in the PSHA calculations. Even if these faults are found to contribute little to the PSHA hazard, as some have suggested, the SHTM will have demonstrated this result to the wider DRMS community.	Sensitivity analysis indicate that the faults in the Sierra Nevada do not contribute to the probabilistic hazard in the Delta. We will document this analysis in the Final Technical Memorandum.
D) Regional Sources. There are two regional sources of potentially significant ground motions at long periods that should be considered in the SHTM. The first is the Cascadia subduction megathrust in northern California and extending into Oregon and Washington. The last known occurrence of this M9+ source was in 1700, and on average it occurs every 500 to 600 years, but repeat times between 270 and 1500 years have been determined (e.g., Atwater and Hemphill-Haley, 1997; Goldfinger et al., 2003). In the context of 0.005 annual probabilities of exceedance, the possible recurrence of a Cascadia megathrust should be evaluated. The second is the possible impact of a repeat of the 1857 Fort Tejon earthquake that ruptured the southern San Andreas fault. Preliminary numerical simulations using 3D seismic velocity models for California suggest that large-amplitude long-period waves may travel efficiently along the Great Valley (Kimura et al., 2006).	The Cascadia subduction zone megathrust and the 1857 rupture have been included in the PSHA.

NRR = No Response Required

COMMENT	RESPONSE
Background Earthquakes	
The SHTM states that the hazard from background earthquakes—those not associated with known or mapped faults—is accounted for by dividing the region into two regional seismic source zones: the Coast Ranges and Central zones. Recurrence for the Coast Ranges zone is based on previously published work (Youngs et al., 1992). The Central zone is attributed to URS Corporation, presumably for this PSHA. The maximum earthquake for the Central zone is stated as M 6.5 ± 0.3. No other information is given, including delineation of the actual physical limits of the zone or the supporting historical and instrumental seismicity used to define the range of earthquake magnitudes, including the maximum, and their recurrence. There is no discussion of how the background earthquake contributes to seismic hazard in the Delta, and the background earthquake is not identified in any of the eleven source-specific hazard curves for the six Delta sites. The justification for the magnitudes and recurrence of earthquakes in the background source zones should be stated in the SHTM. This documentation is important because in regions where slip rates on individual faults are low (presumably the case for the Delta faults) background earthquakes can represent a significant contribution to hazard. The background earthquake was discussed at the November 21, 2006 meeting. Three of the major points, with the goals of increasing understanding of Delta seismicity and its relation to both Delta faults and overall hazard, are restated here:	A discussion of background earthquakes and their hazard contribution will be included in the Final Technical Memorandum.
i. Conduct a sensitivity study in which the Delta faults are replaced by a single areal source (the Central seismic zone) characterized by Gutenberg-Richter statistics (with parameters *a*, *b*, and **M**max), as best these parameters can be defined by the instrumental record of earthquakes in the Delta region. The predictions from the latter investigation could be tested against the historical earthquake catalog.	The recommended sensitivity analysis will be performed and described in the Final Technical Memorandum.
ii. We suggest that the SHTM compare its predicted seismicity rate across these Delta faults (for events greater than M5.5) with the rate obtained from Toppozada et al.'s (2002) analysis of historical seismicity. As noted in Figure 1 of the SHTM, but not emphasized in the text, there is a relatively rich M>5.5 seismicity in the eastern Coast Range in the fifty years prior to the 1906 earthquake. This seismicity includes six events. Their dates and estimated magnitudes are:	The recommended comparison will be performed and described in the Final Technical Memorandum.

NRR = No Response Required

COMMENT	RESPONSE
7/15/1866 M6.0 West San Joaquin 4/10/1881 M6.3 Western San Joaquin 5/19/1889 M6.0 Montezuma Hills 4/19/1892 M6.6 Vacaville 4/21/1892 M6.4 Winters 4/30/1892 M5.8 Vacaville. As shown in attached Figure 2, the 1881 earthquake lies to the south of the Delta area, but the other earthquakes are all near the Delta. The events in 1892 appear to represent a seismic swarm rather than a simple main shock/aftershock sequence.	
iii. Related to comment i, we suggest that the SHTM discuss the seismicity rates in the vicinity of the Delta area as revealed by the Northern California Seismic Network (NCSN) catalog from 1976 to the present. Does this seismicity cluster along any of the proposed Delta faults? Although Figure 1 in the SHTM provides a map of magnitude 3 and larger earthquakes for the entire Bay Area, it would be instructive to provide a seismicity map in the SHTM that is focused on the Delta area and that includes all M2 and larger earthquakes since 1976.	The seismicity rates in the Delta will be evaluated and described in the Final Technical Memorandum. The recommended figure will be produced.
Implications of Regional Constraints on Slip Rates	
i. GPS measurements can provide a framework for strain accumulation rates on a regional basis. The WGCEP (2003) compared geologic slip rates from three transects across the Bay Area and showed that, within the uncertainties, the centuries to millenial geologic rates (36-43 mm/yr) compared well to the decadal GPS rates (39.8± 1.2 mm/yr, Prescott and others, 2001). In reviewing the SHTM and the slip rates shown in Table 1, we constructed several transects across the seismic sources listed in the table from east of the Hayward-Rodgers Creek fault to the Delta-Central Valley. South of the Sacramento River the slip rates are about 10.5 mm/yr (primarily the Calaveras and Greenville faults). North of the river rates averaged about 7.5 mm/yr (primarily the West Napa, Green Valley, Pittsburgh-Kirby Hills). Does this difference simply represent uncertainty in the slip estimates or does it indicate that some slip is not presently accounted for by the identified faults? Even a couple of mm/yr of unrecognized additional slip in the Delta region could affect hazard estimates, especially for background sources.	This comment addresses important, unresolved problems about the distribution of slip on major strands of the San Andreas system east of the Hayward fault, and the discrepancy between geologic and geodetic estimates of slip rate. As the comment correctly observes, the integrated slip rate in the DRMS model among major strike-slip faults east of the Hayward fault is lower north of the Sacramento River (about 7.5 mm/yr) than south of the Sacramento River (about 10.5 mm/yr). Although the 3.5mm/yr discrepancy falls within the uncertainty in the geologic slip rate estimates, we acknowledge that it could be due to higher rates adopted for eastern strands of the San Andreas system south of the Sacramento River in the DRMS model (i.e., Greenville fault and Delta faults). The higher slip rate on the Greenville fault in the DRMS model relative to the Working Group characterization (4 mm/yr vs 2 mm/yr) could account for about 2 mm/yr of the discrepancy. The Delta faults in the DRMS model have low slip rates (less than or equal to about 1 mm/yr) but generally lie south of the Sacramento River and thus potentially contribute to the apparent 3.5mm/yr discrepancy.

NRR = No Response Required

COMMENT	RESPONSE
	We also suggest the alternative hypothesis that the discrepancy reflects a systematic underestimate of slip rates north of the Sacramento River in the DRMS model. Models of GPS data in Prescott et al. (2001) and d'Alessio et al. (2005) consistently place higher slip rates on the eastern strands of the San Andreas system than those adopted by the Working Group. For example, kinematic models of GPS data find preferred slip rates on the Green Valley fault of about 7 mm/yr or more (Prescott et al., 2001; d'Alessio et al., 2005), compared to the mean centered average about 6 mm/yr in the Working Group model. The GPS model of d'Alessio et al. (2005) has a preferred slip rate of 4 mm/yr on the West Napa fault, which is higher than the upper bound of 3 mm/yr in the DRMS model. Increasing the slip rates in the DRMS model for the West Napa and Green Valley faults slightly would be permissible within the constraints of the GPS data, and would reconcile the apparent dextral slip deficit north of the Sacramento River noted in the comment above. Note that by increasing the slip rate on the Greenville fault to about 4 $\pm$ 2 mm/yr, the DRMS model is more consistent with modeled slip rates of about 5 mm/yr on this structure from analysis of GPS data (d'Allesio et al., 2005). To summarize, we acknowledge the discrepancy noted in the comment above, and believe it reflects a deficiency in the existing data and models accounting for the distribution of slip among faults of the eastern San Andreas system. Although the discrepancy is present in the mean-centered slip rate estimates in the DRMS model, the discrepancy is encompassed within the uncertainty expressed by the modeled ranges in slip rates.
Ground Motion and Attenuation	
i. The weighting of the attenuation relations is not clearly stated. It is not clear whether only NGA relations were used and whether equal weighting was applied to all the NGA relations.	The use of the NGA attenuation relationships and their weights (all equal) will be documented in the Final Technical Memorandum.

NRR = No Response Required

COMMENT	RESPONSE
ii. Although the National Earthquake Hazard Reduction Program (NEHRP) site classification everywhere along each levee is important for both the DRMS PSHA and levee failure studies, we gained the impression that these studies may not be as well coordinated as they need to be. We suggest that both Technical Memoranda include a discussion of the interdependency between the PSHA and the levee failure analysis. Generally speaking, the PSHA is used to calculate the ground motion at the base of the Levee model, which then evaluates the potential failure of the levee.	The ground motions from the PSHA are calculated for a reference site with V_S30 of about 1100 ft/sec shear wave velocity. To maintain consistency and correlation between event/source/site from the PSHA and the dynamic response at each levee site, those motions are consistently deconvolved and applied at the base of the FEM model for each site analyzed.
iii. In the DRMS study, the finite element model for the levees comprises the upper 25 m of the near surface structure. From top to bottom the model includes: (1) the built up levees themselves, (2) an underlying peat layer, (3) Holocene sediments of variable thickness, and (4) an underlying aeolian sand. The *in situ* shear-wave velocity for the buried aeolian sand in the Delta is 1200 ft/s, or 365 m/s, only slightly higher than the velocity of the Merritt Sand (325±66 m/s) (Holzer et al., 2005), which is its Bay Area, and perhaps, better investigated equivalent.	We agree with the statement. The FEM model extends down to a depth of about 85 feet below the ground surface or about 115 to 120 feet below the crest of the levee. Five downhole P- and S-wave profiles were conducted in the Delta (See Levee Vulnerability Technical Memorandum). The downhole geophysical surveys show that the lower dense aeolian sand and stiff clays (80 to 100 feet below the ground surface) have on average a V_S30 of about 1100 ft/sec.
A) NEHRP-C or NEHRP-D? NEHRP classifies deposits in terms of their average shear-wave seismic velocity in the upper 30-m (VS30). The Merritt Sand and the Delta aeolian sand are classic NEHRP CD deposits, that is, their VS30 velocities lie near the boundary between the NEHRP-C (360-760 m/s) and NEHRP-D (180-360 m/s) deposits. The DRMS SHTM has chosen to evaluate the PSHA analysis for a NEHRP-C site condition in the Delta. Although this choice may be appropriate *senso stricto* (the 365 m/s for the buried Aeolian sand is slightly higher than the 360 m/s NEHRP-CD limit), we think that it is likely that much of the Delta is underlain by lower velocity NEHRP-D class deposits. Unpublished DWR suspension log measurements of shear wave velocities to a depth of over 75 meters in the Delta suggest that average velocities at 30 m depth are less than 350 m/s (i.e., they should be classified as NEHRP-D sites).	For the reference site (the lower dense aeolian sand and stiff clays) a V_S30 of 1100 ft/sec was used in the analysis. See response above.

NRR = No Response Required

COMMENT	RESPONSE
B) New CPT Soundings and Vs measurements. Tom Holzer's group at the USGS Menlo Park recently obtained 3 seismic Cone Penetration Tests in natural deposits in the Delta to depths as much as 100 feet. Two of the soundings penetrated peat and all three penetrated about 10 m of Merritt Sand equivalent. In the two deepest soundings, this sand was underlain by interbedded sands and clay. All three sites, all on and between Bethel and Holland Islands in Contra Costa County, fall into NEHRP-D class deposits. Some particulars of the new USGS CPT and Vs measurements: Location: CCA001 (X 624,662m, Y 4,203,791m) CCA002 (X 620,747m, Y 4,206,291m) CCA003 (X 620,795m, Y 4,207,743m). Preliminary interpretation of the generator source downhole Vs travel time data: CCA001 - VS30 = 171 m/s (penetrated 30.7 m) CCA002 - VS30 = 254 m/s (projected; penetrated 14 m) CCA003 - VS30 = 258 m/s (projected; penetrated 24.7 m) Shear Wave Velocity of deepest unit sampled: CCA001 (Vs=293 m/s, 21.3-30.5 m) CCA002 (Vs=301 m/s, 3.8-13.8 m) CCA003 (Vs=443 m/s, 19.8-24.4 m) These data will made available online at http://quake.usgs.gov/prepare/cpt/ in the next few months.	URS would like to obtain the recently acquired V_S data from the USGS. Since our analyses use time-domain non-linear analysis, we used the shear wave profiles in our FEM model as they were measured for each site. Our models do not rely on the use of the site classes (C or D).

NRR = No Response Required

COMMENT	RESPONSE
C) Deep Basin sedimentary sites. All Delta levee sites are probably "deep sediment" sites. Throughout California, similar deep sediment sites are almost always NEHRP-D sites. The Delta lies along the western margin of the Great Valley, where the sedimentary basin is thickest and is thought to exceed a thickness of 12 km (Wentworth et al., 1995). More recent seismic tomography models for the greater Bay Area show the lowest compressional wave anomalies extending to depths of 14 km are centered on the Delta region (Hole et al., 2000). Numerous oil industry sonic logs from the Delta area extending to several kilometers depth and empirical relations between compressional and shear wave velocities (Brocher, 2005a, b, c) suggest that the depth to the VS = 1000 m/s velocity likely exceeds several hundred meters throughout the Delta. For example, the Mantelli #1 borehole in Brentwood penetrated non-marine sediments having a Vp less than 2500 m/s (equivalent to a Vs less than 1000 m/s) to a depth of 1.25 km (Figure 4 in Brocher, 2005a). We think that these deep sediment sites should be modeled as NEHRPD, not as NEHRP-C sites.	The NGA attenuation relationships attempt to address basin effects through the use of the depth to basin parameter. Basin effects are contained in the strong motion database used to develop the relationships (e.g., Northridge). The response of the levees appears to be sensitive to ground motions with periods out to about 1 sec. At the annual exceedance frequencies being considered for this study (e.g., $4x10^{-4}$), the controlling seismic sources are the Delta faults and faults adjacent to the Delta. In a probabilistic and risk context, the San Andreas fault and more distant regional faults, which can generate long-period ground motions are not significant contributors to the hazard. In a deterministic sense, these faults and 3D basin effects beneath the Delta may be important but they are not relevant in this study. Also see response to comment iii on page 13.
iv. We think that the DRMS study of earthquake hazard should be more complete in recognizing the high likelihood of strong long-period shaking in the Delta. The DRMS study lies in the period range for the PSHA, which now covers PGA, PGV, SA0.3, and SA1.0. The motivation for this restriction comes from the current finite element model for the levee: DRMS study director Said Salah-Mars stated at our meeting on November 21st, 2006, that the cross-section levee model was primarily sensitive to 1 Hz waves. However, this model does not incorporate the much longer longitudinal aspect of the levees along channels (consistent with longer periods), or the seiches induced in the water filling the channels between levees. A broader view of the levee geometry may be necessary. In any case, we suggest that the results of finite element modeling be incorporated into this PSHA study to insure that the proper periods are addressed in the SHMT. Then it would be possible to judge whether the PSHA should evaluate the hazard at longer periods, possibly 2 seconds or more, and to reconsider the contribution to the hazard from the largest events: a recurrence of the 1906 earthquake and a multi-segment failure of the Hayward fault.	See above response.

NRR = No Response Required

COMMENT	RESPONSE
v. Reports of levee cracking and subsidence at several locations in the Delta were made after the M6.7 1983 Coalinga earthquake, located 240 km south of the Delta (Finch, 1985, 1987, and 1992). If these represent valid observations of coseismic damage, they would have been caused by a moderate earthquake at a distance of 240 km and would suggest that long-period motions were responsible, because only weak high frequency energy would be expected at these distances. Numerical calculations of strong ground motions from the 1906 earthquake predict efficient propagation of surface wave energy within the Great Valley (Kimura et al., 2006; Rodgers et al., 2006). We think that the possibility that strong shallow earthquakes near the margins of the Great Valley can create damaging long-period surface waves within the Valley should be investigated.	See response on page 15.
PSHA Approach and Methodology	
i. The discussion of the uncertainties in the PSHA calculations should be expanded. These uncertainties include, but not limited to, the uncertainties in the probability of activity of faults, fault rupture scenarios, scenario earthquake magnitudes, and slip rates. We suggest including a discussion of how much the ground motions has been lowered as a result of using new NGA attenuation relations instead of older attenuation relations.	The recommended discussion will be included in the Final Technical Memorandum.
ii. We suggest a sensitivity study that would more readily quantify the impact of the Delta faults to the PSHA than the figures presented in the SHTM (Figures 16 to 39). We suggest eliminating the Delta faults altogether from the analysis, either by assigning a zero-probability of activity to them or by setting their slip rates to zero. This would be a simple way of deaggregating the contributions of the Delta faults to the seismic hazard.	The recommended sensitivity analysis will be performed and described in the Final Technical Memorandum.
iii. We recommend that the SHTM results be compared to the latest USGS National and CGS California Earthquake Hazards Maps. The reasons for any significant differences from these Earthquake Hazards Maps should be discussed.	The comparison will be described in the Final Technical Memorandum.
iv. This SHTM report presented a standard PSHA time-independent hazard characterization that ignores the importance of 3D geologic structure in the Delta. We think that a significant part of the seismic hazard will be the response of the Delta levees to the long-period (T>1 s) from large earthquakes (M greater than or equal to 7.5) occurring on one or more elements of the San Andreas fault in the Bay Area.	The NGA attenuation relationships address basin effects through the use of the depth to basin parameter. Basin effects are contained in the strong motion database used to develop the relationships. A deterministic approach to basin effects for the Delta is beyond the scope of this study. See response on page 15.

NRR = No Response Required

COMMENT	RESPONSE
v. We suggest that the DRMS study include a more comprehensive approach to ground motion analysis than the conventional time-independent PSHA available for this review. For example, it is well known that the Delta overlies one of the largest and thickest basins in California (Brocher, 2005a, and references therein). It has the largest low gravity anomaly in California. Numerical models, using current generation 3D crustal models for Northern California suggest that 3D wave propagation and basin effects will be very large in the Delta area (e.g, Larsen et al., 2006; Rodgers et al., 2006). Coupled with strong source directivity associated with the direction of rupture, this 3D crustal structure may dramatically increase the amplitude and duration of seismic waves in the Delta over standard 1D analyses. None of these factors are accounted for directly in the present standard PSHA time-independent hazard characterization. Perhaps these scenarios can be addressed in the Delta in the same way that the Southern California Earthquake Center has approached similar problems in southern California using the CyberShake approach: calculating the site response for literally hundreds of thousands of earthquake rupture scenarios for hundreds if not thousands of fault ruptures.	See response to comment iv.
Figures:	
Figure 1. Portion of recently released compilation of Quaternary active faults in the San Francisco Bay Area (Graymer et al., 2006) showing location of the Las Positas and Verona faults. Red lines show historic fault ruptures; orange lines show Holocene active faults.	NRR
Figure 2. Historical seismicity in the Bay Area and Coast Range, replotted from Toppozada et al. (2002). The locations and magnitudes for the 1889 Montezuma Hills earthquake, the 1892 Vacaville-Winters earthquakes, and the 1898 Mare Island earthquake are taken from Bakun's (1999) isoseismal inversions. The red lines indicate the rupture extent of the 1868 Hayward earthquake and the rupture extent within the Bay Area of the 1906 San Francisco earthquake.	NRR

NRR = No Response Required

COMMENT	RESPONSE
Summary	
CGS has the following general comments:	
1. A number of faults in the Delta, that make significant contribution to the Delta's seismic hazard, are not documented.	The characterization of the faults in the Delta will be documented in the Final Technical Memorandum.
2. The value assigned to the probability of activity for Delta faults seems arbitrary and high.	Some of the values have been reduced. All P(a)s have been reviewed and in some cases revised using a revised criteria (see footnote #1 in Table 1). The basis for the P(a) of the Delta faults will be described in the Final Technical Memorandum.
3. The fault model appears to imply a relatively high occurrence rate of earthquakes in some cases. Specifically, the rate for the Greenville fault is higher than the known historical seismicity.	The seismic source model recurrence will be compared against the historical record to insure that there is not a significant difference. However, differences between the recurrence for a specific fault from the geologic record and the historical record is not unexpected. The recurrence of the Greenville fault will be documented in the Final Technical Memorandum.
4. It appears that URS has made a number of conservative assumptions regarding activity rates on structures that are not well understood. The sum of these conservative assumptions is a seismic hazard model that appears to be quite conservative, and may be inconsistent with the overall rate of fault movement across the region and with historic rates of seismicity.	Judgments on new seismic sources described in this project have been made based on limited data but the intent has been to estimate the range of parametric uncertainties and to encompass the range of possible interpretations. No conservative assumptions have been intentionally made. See updated Table 1.
URS Fault Model	
The URS fault model uses the fault parameters published in the WG03 report for the San Andreas, Calaveras, Hayward, San Gregorio, Rodgers Creek, and Mt. Diablo faults, although specific rupture scenarios have been modified. Changes in fault length and slip rate based on recently completed investigations by Sawyer and Unruh (2002) were made for the Greenville fault and unspecified changes were indicated for the Concord-Green Valley fault zone. Some of the more significant sources affecting the delta area have not been previously characterized and are not adequately documented (refer to table below for specific comments).	See response to # 1. Revisions to the Greenville and Concord-Green Valley faults and the Delta faults will be documented in the Final Technical Memorandum.

NRR = No Response Required

COMMENT		RESPONSE
Probability of Activity (POA) uncertainties are not adequately quantified and in some cases seem arbitrarily high. For example, several blind thrust sources are characterized with a probability of activity of 1.0, but the cited references do not contain documentation of Holocene or historic deformation. Some of the documentation infers Quaternary deformation, suggesting the POA would be 0.7, or a possible association with other active structures, suggesting the POA would be 0.5. In contrast, some faults that have documented Holocene or historic activity, such as the Sargent fault, are assigned a POA of 0.8.		All P(a)s have been reviewed and in some cases revised based on a revised criteria (see footnote #1 in Table 1). The bases for P(a)s less than 1.0 are now discussed in Table 1.
Slip rate estimates for poorly documented sources should be better discussed in the Notes field. It is important to discuss the displacement parameters, ages of displacement, and quantify the uncertainties of the slip rate calculations.		Seismic sources characterized by the Project Team as part of this study or in previous studies in which documentation is not readily available will be described in the Final Technical Memorandum.
Several faults west of the delta have not been considered. These faults include the Verona, Williams, and Los Positas faults. Traces of the Verona and Los Positas faults are encompassed by AP Earthquake Fault Zones. The Los Positas fault had minor surface fault rupture associated with the 1980 Livermore earthquake.		These faults have been included in the analysis. Fault parameters are provided in Table 1.
Seismic sources east of the delta, such as the Foothills Fault System along the western Sierra Nevada, and more active sources along the eastern front of the Sierra Nevada, such as the Carson Valley fault zone and faults along the west and north side of Lake Tahoe, may contribute ground shaking hazard to the delta region. These sources should be considered in the model.		Sensitivity analyses have been performed for these seismic sources and they do not contribute to the hazard in the Delta. See Final Technical Memorandum.
How would large displacements along the Cascadia subduction zone affect the delta? This source should be considered in the model, as well as additional segments of the Coast Range-Great Valley Margin structures north and south of those included in the current seismic source model.		The Cascadia subduction zone megathrust has been added to the PSHA. The hazard contribution of the CRSB boundary zone segments north and south of the current model will be evaluated and documented in the Final Technical Memorandum.
Comments on Table 1. Bay Area Seismic Source Parameters		
Fault Name	**Comments**	
Greenville	Minor fault length changes from WG03. Slip rate is doubled, based on results of dedicated slip rate study by Sawyer and Unruh (2002). Need to better document what the slip rate was based on in the Notes field.	The Working Group trace of the Greenville fault includes parts of the Marsh Creek-Clayton fault system east of Mt. Diablo. The Marsh Creek-Clayton faults have been extensively trenched for housing developments in the Clayton area during the past decade, and to date no positive evidence for late Pleistocene or Holocene displacement has been observed on these structures (Jim Joyce, personal communication, 2006). These recent observations are consistent with the conclusion of Wright et al. (1982) that trench exposures of the Marsh Creek-

NRR = No Response Required

COMMENT		RESPONSE
		Clayton fault system "do not impress one as features characteristic of a major active fault." We modified the WG trace to reflect detailed mapping of recently active strands of the Greenville fault by Herd (1977), Hart (1981; 1982) and Unruh and Sawyer (1998), which do not include parts of the Marsh Creek-Clayton faults. The Working Group assigned a slip rate of 3 ± 2 mm/yr to the Greenville fault based primarily on work by Sawyer and Unruh that was current as of about 2000. Additional work by Sawyer and Unruh subsequent to 2000 included paleoseismic investigations of the Greenville fault, and AMS dating of bulk soil carbon from offset marker units observed in trenches. This work was presented at the 2002 AGU meeting (Sawyer and Unruh, 2002a) and summarized in a NEHRP final technical report (Sawyer and Unruh, 2002b). Both of these references cite a preliminary slip rate estimate of 4.1 ± 1.8 mm/yr for one of two known traces of the northern Greenville fault based on the new data. Sawyer and Unruh (2002b) note that this is a minimum rate for the entire fault zone because at least one additional splay of the fault was located several tens of meters east of the trench site. PSL dating by Glenn Berger in 2004-2005 of right-laterally offset channel gravel deposits in the trenches provided independent support of the 4.1 ± 1.8 mm/yr age estimate of Sawyer and Unruh (2002a). For the DRMS source characterization, we revised the slip rate estimate for the Greenville fault to reflect this new information.
Concord-Green Valley	Need to better document what changes to WG03 parameter were made in the Notes field. Is fault creep considered?	WGCEP (2003) characterization of the major faults has been used without change in the quasi time-dependent hazard calculations. For the time-independent hazard calculations, some revisions have been made to the WGCEP (2003) parameters. They will be described in the Final Technical Memorandum.
Ortigalita	Should further discuss how slip rate was derived. Also, extent of this fault is different that WG03 and extends 10-20% farther than AP surface traces.	See added commentary in Table 1.

NRR = No Response Required

COMMENT		RESPONSE
Mt. Oso	How was slip rate estimated? Any independent evidence of activity other inferred than slip transfer?	The estimate of slip rate is based entirely on the assumption that dextral slip on the Ortigalita fault steps west (left) across Mt. Oso anticline to the southern Greenville fault, i.e., the slip rate on the inferred Mt. Oso thrust fault is a function of the slip rate on the Ortigalita fault. To date, the only observations that support Quaternary activity of the Mt. Oso fault are geomorphic relations documented by Hart (1981) and Unruh and Sawyer (1998) that tectonic-geomorphic expression of the Greenville fault increases significantly north of the western end of the Mt. Oso anticline.
West Napa	Citation for Wesling and Hanson is cited in Notes field as 2006, but shown as 2005 in references. What are slip rate estimates based on?	See added commentary in Table 1.
Los Medanos Fold and Thrust Belt	Probability of activity is shown as 1.0. The references cited do not state that these structures exhibit evidence of Holocene displacement. Based on documentation cited, lt. Qt uplift is inferred by assuming link with uplifted Montezuma Hills, and assumption that there is a structural relationship with Mt. Diablo Fold and Thrust Belt. Based on the descriptions of probability ratings, 0.5 or 0.7 seems more appropriate.	We have prepared summary discussions of the data used to characterize all of these structures for the Final Technical Memorandum, including evidence for late Quaternary activity. We are not using a Holocene criteria for activity or non-activity of potential sources.
Potrero Hills	Probability of activity 1.0. What is evidence of Holocene displacement?	We have prepared summary discussions of the data used to characterize all of these structures for the Final Technical Memorandum, including evidence for late Quaternary activity. We are not using a Holocene criteria for activity or non-activity of potential sources.
Thornton Arch	Probability of activity of 0.3 is not defined.	We have prepared summary discussions of the data used to characterize all of these structures for the Final Technical Memorandum, including evidence for late Quaternary activity. We are not using a Holocene criteria for activity or non-activity of potential sources.

NRR = No Response Required

COMMENT		RESPONSE
Wragg Canyon	What is evidence of recency? O'Connell and Unruh (2000) build a case for a potentially active fault, based on strike of fault being similar to Green Valley fault, and surface expression characterized by a linear valley. Probability of activity 0.5 according to criteria. How was slip rate derived?	We adopted a P(a) = 1 based on data in O'Connell et al. (2001) showing a spatial association of seismicity with the Wragg Canyon fault. The slip rate was derived from a kinematic model whereby at least some slip on the Pittsburg-Kirby Hills fault is transferred to the Wragg Canyon fault through a restraining stepover across the Potrero Hills. In this model, the slip rate on Wragg Canyon fault (0.3 ± 0.2 mm/yr) is assumed to be somewhat less than the Pittsburg-Kirby Hills fault (0.5 ± 0.2 mm/yr).
Midway/Black Butte	What is evidence of Holocene activity? Notes field indicates fault has late Quaternary activity, which would suggest probability of activity of 0.7. Sowers, et al 1994 not listed in references.	We have prepared summary discussions of the data used to characterize all of these structures for the Final Technical Memorandum, including evidence for late Quaternary activity. We are not using a Holocene criteria for activity or non-activity of potential sources.
West Tracy	What is evidence of Holocene activity? Notes field indicates Quaternary activity. Probability of activity of 0.7 according to criteria.	We have prepared summary discussions of the data used to characterize all of these structures for the Final Technical Memorandum, including evidence for late Quaternary activity. We are not using a Holocene criteria for activity or non-activity of potential sources.
Vernalis	What is evidence of Holocene activity? Notes field indicates Quaternary activity. Probability of activity of 0.7 according to criteria.	We have prepared summary discussions of the data used to characterize all of these structures for the Final Technical Memorandum, including evidence for late Quaternary activity. We are not using a Holocene criteria for activity or non-activity of potential sources.
Sargent	Probability of activity of 0.8 not defined. Should be 1.0. Strands of the southern Sargent fault zone are encompassed by AP Earthquake Fault Zone and exhibit creep evidence of about 3-4 mm/y and are characterized by well-defined microseismicity.	See added commentary in Table 1.

NRR = No Response Required

COMMENT		RESPONSE
Briones source zone	Need better documentation in the Notes field. What is evidence of Holocene deformation? How was slip rate derived?	A geomorphically well-defined fault in the Briones source zone is parallel to and spatially associated with the NNW-trending alignment of epicenters in the Briones earthquake swarm (Unruh and Kelson, 2002). We interpret both the seismicity and the association of a mapped fault with seismicity as evidence for Holocene activity of the Briones zone. Unruh and Kelson (2002) describe tectonic-geomorphic evidence for late Quaternary of other faults within the Briones source zone. The slip rate was estimated from dextral offset of late Cenozoic structures and stratigraphic units across faults in the northern East Bay hills. Based on offset stratigraphic contacts in the northern East Bay Hills, Unruh and Kelson (2002) estimated about 5 km of post-late Neogene dextral slip on the Lafayette-Reliez Valley fault zone, which implies a long-term average slip rate of $\geq$ 1 mm/yr over the past 5 Ma. The upper end in the range in slip rates adopted for DRMS (3.0 mm/yr) encompasses the possibility that the Briones zone comprises a system of faults that transfers slip to the West Napa fault (maximum slip rate = 3.0 mm/yr).
Foothill Thrust system	Probability of activity of 0.6 not defined. Most faults have geomorphic evidence of late Quaternary displacement, but most lack evidence of Holocene displacement.	See added commentary in Table 1.
East Valley Thrust	Probability of activity of 0.6, which is not defined. Strands of some of these faults are encompassed by AP Earthquake Fault Zones, indicating probability of activity should be 1.0.	See added commentary in Table 1.

NRR = No Response Required

COMMENT	RESPONSE
Implications of Fault Model for Earthquake Occurrence	
Time constraints have precluded a detailed analysis of the seismic hazards implications of the URS fault model. As a spot check, we have examined the rate of earthquake occurrence implied by the model for the Greenville, South Midland, Midway/Black and West Tracy faults. These are the faults with the greatest contribution to the seismic hazard for higher levels of ground motion at the Clifton Court site. URS provided CGS with the input file to the PSHA program. This file and Table 1 from the SHTM provided our understanding of the earthquake occurrence model. With this information, we calculated the incremental and cumulative earthquake occurrence function for the above four faults.	NRR
Figure 1 shows the incremental earthquake frequency as a function of magnitude for the Greenville fault. The figure compares the implications of the URS fault model with the fault model of the Working Group on Bay Area Earthquake Probabilities (WG02). We note that the URS model implies a significantly higher seismicity rate than the WG02 model. This difference is primarily the result of URS' use of 4mm/yr slip rate, as opposed to WG02's 2 mm/yr (see discussion above?).	We have adopted the WGCEP (2003) characterization of the Greenville fault for the quasi time-dependent hazard calculations. For the time-independent calculations, we have characterized the Greenville fault based on more recent data. See response on page 2.
By comparison, the faults within the Delta generally have considerably smaller incremental earthquake frequency (Figure 2) than the Greenville fault. The one exception to this is the Southern Midland fault which, in the low-magnitude 6 range has a similar or higher occurrence rate than the Greenville fault. It is surprising that two faults with nearly an order of magnitude difference in slip rate would have similar seismicity rates in any magnitude range. This magnitude range is about that appropriate for the regional historical seismicity. The 1980 Livermore earthquake, M5.8, occurred on the Greenville Fault. There are no known historical earthquakes associated with the Southern Midland Fault.	This issue will be investigated and documented in the Final Technical Memorandum.
A different view of the model earthquake occurrence rates is shown in Figure 3, cumulative magnitude-frequency plots. These show the rates of earthquakes with magnitudes greater than or equal to a given magnitude. For example, on the Greenville Fault, magnitude 5.0 and greater earthquakes are modeled to occur every 42 years. The recurrence time for magnitude greater than or equal 6.0 is slightly less than 100 years. These rates are considerably higher than the one M5.8 earthquake (1980) known to have occurred along the Greenville fault in historical times.	See response on Greenville fault on page 2.

NRR = No Response Required

COMMENT	RESPONSE
The cumulative occurrence rates for the Southern Midland Fault are everywhere lower than the Greenville Fault. In the model, magnitude 5 or greater events occur every 200 years (5.7 or greater every 300 years). The historical record is only about 200 years long, and events of magnitude 5 to 6 or so could easily have been missed. Nevertheless, the seismicity rates for the Southern Midland Fault and the Greenville Faults seem to be high, at first glance.	The recurrence rates for each significant fault are being checked against the historical rate to insure consistency. In some cases, there may be a discrepancy between the historical and geological rates. More weight is given to the longer paleoseismic record. The bases for recurrence rates for faults characterized as part of this project will be documented in the Final Technical Memorandum.
We recommend that URS consider the seismicity implications of its fault model for each fault in the model.	See above.
Probabilistic Seismic Hazards Analysis	
Time constraints and an incompatibility between the URS fault model components and the PSHA software used in the National Seismic Hazard Maps have precluded a direct comparison of hazard curve calculations. We have compared hazard curves for the Greenville Fault for the URS model with the WG02 model and a floating earthquake model. The results compare favorable.	NRR
URS uses the "Next Generation Attenuation" relations (NGA) in their PSHA. The available versions of these relations are all in draft form and still subject to revision. At several recent workshops held by the USGS' National Seismic Hazard Map project, participants recommended that NGA, and not previous relations, be used in the National Map. For this reason, we don not recommend against using NGA, but recommend caution when using draft relations.	NRR
We note (SHTM Figures 16-27) that at several sites studied in the SHTM the seismic hazard at the 2500 year level (that of the new 2008 building code) is dominated by the local buried thrust faults, faults whose levels of activity are not well understood.	NRR

NRR = No Response Required

COMMENT	RESPONSE
Recommendations	
The California Geological Survey recommends the following:	
1. URS should document the parameters for the Delta faults where those parameters are not based on already-published information.	The characterization of the Delta faults will be documented in the Final Technical Memorandum.
2. URS develop and apply an objective and consistent approach for the probability of activity of faults included in the model.	All P(a)s have been reviewed and in some cases revised based on revised criteria (see footnote #1 in Table 1).
3. URS examine the earthquake occurrence implications of each fault in its model, comparing predicted seismicity rates with the historic and paleoseismic record.	We will make these comparisons for selected faults and evaluate potential differences particularly those where we have developed the characterization.

NRR = No Response Required

Comments on Geomorphology Technical Memorandum and Responses of Consulting Team

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments: Delta Geomorphology Technical Memorandum

Comments	Responses
Reviewer: Lewis Hunter, U.S. Army Corps of Engineers	
1. This TM is an interesting read and provides useful background on the dynamics of the Sacramento-San Joaquin Delta, impacts to the delta system placed upon it through anthropogenic modification of the sediment and water distribution system and impacts related to projected forcing due to global climate change. Comments that follow are intended as suggestions that might improve overall clarity and justification to the conclusions drawn.	Noted.
2. Discussion found in section 2 (Findings) appears out of place to the reviewer. The discussions are in the form of a summary that are appropriate for an Executive Summary or conclusion; however, as presented they appear unsubstantiated with no reference to the data or studies where they came from. Either refers to where in the TM the supporting data are provided or move section towards the end of the report where it can summarize the results provided previously in the document.	Section 2 has been removed and the text placed into the conclusions (Section 5 of the revised Delta Geomorphology Technical Memorandum [TM]). The conclusions have been re-arranged to make sense and avoid duplication.
3. Reviewer performed a quick scan between this TM and those on Subsidence and Climate Change and noted that there were some discrepancies among the numbers presented, e.g., accommodation space. Some of the discrepancy may be in how the numbers are being applied but reviewer request that authors back check their numbers and verify that they are consistent with other TMs developed with the DRMS. It is also noted that the TM vary in terms of units, some English, some mixed English and metric. This may be beyond scope of this TM, but reviewer feels the TMs should be consistent in the units used in these documents.	The Climate Change TM discusses future sea-level rise and provides a range of recommended values in 2100 relative to 1990 of 0.7, 1.6, 3.0, and 4.6 ft. These values are biased towards higher estimates by inclusion of a paper by Rahmstorf (Dec 2006), which we were unaware of. These values are higher than our estimated value of 1 foot based on mid-range estimates of IPCC 2007 and Cayan et al. 2006. We have added text and a sensitivity analysis (Section 4.4.4 of the revised TM) to assess the impact of a higher sea-level rise (see also response to Scott Stonestreet's comment #2). The Subsidence TM provides information

Comments	Responses
	on total subsidence to 2100 and provides an estimate of the amount of accommodation space created by this subsidence. The estimated values are lower than our estimate based on Mount and Twiss (2005), and we have reconciled this by introducing a lower estimate of subsidence into a sensitivity analysis (see response to Scott Stonestreet's comment #2). Text has been added to page11 of the revised TM to highlight these different estimates.
4. Not all readers will be familiar with the geography of the Delta area. As such, all important location names (e.g., Carquinez Strait, Suisun Bay, Brown Island, Sherman Island, etc.) should be labeled on Figures 2 or 5.	New captions have been added to Figures 2 and 5.
5. Use of the term "tule" marsh is more of an ecological descriptor to the reviewer and not necessarily a geomorphic one. Reviewer suggests that a "tule marsh" be defined where the term if first introduced then used specifically where the "tule" vegetation has a specific importance to the section, otherwise describe as a freshwater tidal marsh.	Tule has been replaced by freshwater tidal where appropriate throughout text and an ecological descriptor added where tule is first mentioned (page 2 of the revised TM).
6. Were the hypsometric data used in this study generated for this study? If so, text should describe how these data was generated, what assumptions are attributed to them, how were areas and volumes calculated from then and what are their accuracies? If not, text needs to cite report(s) where more detailed discussions are located.	The hypsometry was generated by combining two main data sets. The Delta islands hypsometry was constructed from the U.S. Army Corps' Interferometric Synthetic Aperture Radar (IfSAR) mapping undertaken in 1997 and 1998. The data are in the form of a DEM that covers the topography of the legal Delta and has a grid cell size of 10 meters. This is described on page10 of the revised TM. The Delta channel hypsometry was constructed using USGS bathymetric data consisting of an integer grid of water depth developed from a database of point soundings from multiple existing data sources post-1980. The grid has a cell size

Comments	Responses
	of 10 meters. This is described on page 9 of the revised TM.
7. Discussions regarding the sediment budget. a. In discussions regarding a project that I have recently become involved in the engineers described a knick point that was migrating up the American River that they associated with adjustments related to the passing of a sediment wave associated with the hydraulic mining around the turn of the century. I would have assumed that there would have been some rise in the elevation of the channel bed associated with the extreme sediment load during those times. Subsequently, as the fitness of the channels has adjusted to upstream damming of the rivers, loss of hydraulically mined sediment, and urbanization effects that the channels profiles have been adjusting accordingly. b. In addition to the hypsometric curve, it would seem useful to compare elevation profiles of select streams (or particular sections) through time to see how they have adjusted and to predict what adjustments might occur in the future. c. Additionally, how much sediment accumulates in the shipping channels that subsequently have to be dredged out – would this volume be significant in terms of the general sediment budget presented?	a. Comment noted. b. We agree that to look at the impacts of tidal prism increase on tidal channel scour and stream elevation profiles is a useful exercise. Note that in Figure 8 we show a hydraulic geometry relationship for the Delta between tidal prism and channel cross-section area. Other relationships are available such as TP v channel depth, and TP v top channel width. However, it was beyond the scope and budget of this study to analyze these relationships in more detail. c. Approximately 900,000 tons is dredged annually from the Delta (Shvidchenko et al. 2004). The sediment budget outlined in the study accounts for loss of sediment due to dredging.
8. With regards to sea level rise, suggest adding citations to the 2007 IPCC Summary for Policymakers. Values are in general agreement with Cayan et al. (2006) and would provide added weight to the discussions.	Reference to IPCC (2007) has been added to page10 of the revised TM and the document has been added to the References (Section 6).
9. Comments regarding Figures: a. Fig 2. Colors (Yellow shades) used to differentiate the “delta boundary” from the “Intertidal Freshwater Marsh” are difficult to differentiate. Additional contrast in	Figure 2-6 has been updated.

Comments	Responses
colors would be beneficial. b. Figs 3 & 6. i. Although close, for comparison purposes make the x-axes on both plots the same. ii. Figures should note that the x-axis represent the cumulative area. iii. Fig 3. Behind the labels for "Tidal Prism" and "Tidal Volume" add the volumes described in text in brackets, e.g., 100,000 acre-ft and 700,000 acre-ft, respectively. iv. Fig 6. Define the "red" line and I do not see the dashed line of the "Hypsometry of historic Delta" – clarify. c. Fig 4. Simplified geologic cross section implies no channel migration. Is this supported by geologic evidence? Reviewer would assume periodic breaching of historic natural levees leading to channel migration and subsequent burial of coarse-grained facies. With subsidence and differential compaction of fine-grained sediment there may then be some surface expression of these buried features that may have historically impacted flow and may cause differential subsidence within an "island".	

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments: Delta Geomorphology Technical Memorandum

Comments	Responses
Reviewer: Scott Stonestreet, P.E.: U.S. Army Corps of Engineers, Sacramento District	
1. Overall, the documentation does a pretty good job of taking a very complicated subject and getting a concise overview of the relevant items of significance.	Noted.
2. Given the uncertainty in estimating potential outcomes of various assumptions, it would be good to conduct sensitivity analyses for the critical assumptions made in the assessment instead of going with just the "most likely" or "middle of the road" values. For example, what if global sea level rise over the next 100 year is 2.4 feet and not the best estimate of 1 foot? What if sediment inflow to the Delta stays at 3 million tons per year instead of dropping to one million tons per year? My point is that it is great to have a "best estimate" set of values, but it is also good to define the edges of the envelope and in doing so may show a scenario (within the envelop of possibility) whereby a critical threshold is passed and the whole thing goes south very rapidly.	We have included a sensitivity analysis as Section 4.4.4 (page 14 of the revised TM). This looks at the impacts of tripling global sea-level rise, halving soil subsidence rates, and tripling sediment deposition.

Comments on Subsidence Technical Memorandum and Responses of Consulting Team

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments: Subsidence

Comments	Responses
Reviewer: Victor Pacheco, DWR	
1. Page 5-9 from the Draft Risk Analysis Report states that subsidence rates of about 0.5 inches per year were common in the marsh and some areas appear to have experienced 1.0 inch per year. However, the location of points in Suisun Marsh used to estimate subsidence rates identified by Figure 7 on Page 16 from the February 2007 Subsidence Technical Memorandum appear to be primarily on levees that are known to settle over time. In fact, the negative subsidence rates in Table 2 on Page 17 from the Subsidence Technical Memorandum may actually reflect the work done on these levees to increase their height over time or settlement rather than subsidence. There appears to be a lack of data from areas within Suisun Marsh where subsidence may potentially occur. Several areas of the marsh are managed wetlands that are flooded year-round or existing tidal marsh that are unlikely to subside and may over time actually accumulate vegetation/sediment.	We recognize the large uncertainty in the Suisun Marsh subsidence estimates based on the only available data. However, the correlation of subsidence rates in highly organic soils with subsidence rates indicates similar processes to the Sacramento-San Joaquin Delta. Also, subsidence during the large period of time (over 50 years) between surveys probably reflects longer-term soil processes and not shorter-term changes in levees. We described a large uncertainty associated with our subsidence estimates (over 100%). We added additional text to this section of the report to better address this comment.

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments: Subsidence

Comments	Responses
2. While the uncertainty analysis from Section 4.2 on Page 42 in the Subsidence Technical Memorandum addresses the uncertainty of elevation data, it fails to address the lack of data from areas within Suisun Marsh where subsidence is more likely to occur as illustrated by Figure 22C on Page 29 in the Subsidence Technical Memorandum.	We attempted to address the uncertainty in Suisun Marsh subsidence estimates based on the uncertainty in the past elevation measurements. Also, we attempted to account for the spatial variability in subsidence rates using soil maps and the correlation of subsidence rates with soil organic matter content. Therefore, for those areas most likely to subside, we developed high and low estimates based on the error in elevation data. The estimates of about 2.9 to 3.6 feet during the next 50 years for the areas of highly organic soils are consistent with what we know about organic soil subsidence from the Delta and estimated subsidence of about 5 feet for soils with similar organic matter content there. Lower subsidence rates are expected due to wetter conditions.
3. Similarly, on Page 15-2 a bullet states that "For comparable soil organic matter contents, Suisun Marsh subsidence rates are similar to the Delta rates." However, it is known (and the report previously acknowledged) that management in Suisun Marsh is significantly different than the Delta and that while the marsh may subside 1 to 2 feet by 2050 the Delta would subside by 1 to 5 feet in the same time period. Some additional context is needed to clarify the bulleted statement.	Thank you for pointing this out. The text has been changed to read as follows. "Rates were generally lower than historic rates for Sacramento-San Joaquin Delta soils with similar organic matter content. This is probably the result or wetter conditions under different management practices."

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments: Subsidence

Comments	Responses
Reviewer: Patricia Quickert, Environmental Scientist Environmental Compliance and Evaluation Branch Division of Environmental Services	
4. Risk Analysis Report Page 2-2, 3rd paragraph. This paragraph should include the word 'subsidence', and tell the full story-subsidence in the Delta is anthropogenic and due to much more than simple oxidation and wind loss, i.e. a large volume of peat soil was used for levee construction. Also, a little more information about soil burning should be included since it was a regular agricultural practice.	While it is true that significant volumes of organic soil were used to build levees in the late 1800s and early 1900s, it was small relative to the volume lost since reclamation to oxidation, consolidation, wind erosion and burning. Moreover, this is not a relevant process in the Delta today. The Subsidence Technical Memorandum provides a summary of burning practices that were prevalent in the Delta before 1960.

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments: Subsidence

Comments	Responses
Reviewer: Michael Ramsbotham, COE	
5. Subsidence is still being popularized as a major threat to the Delta without sufficient supporting documentation. Consequences and risk do increase with increased by interior island subsidence, but there is little substantiating data to support the position that interior island subsidence significantly impacts seepage, stability, or seismic vulnerability. The old cartoon showing significant ground surface elevation loss at the toe of the levee because of subsidence is no longer true for much of the Delta.	Subsidence will increase volumes below sea level on the order of one-half. million acre feet during the next 50 years. Interior island subsidence will affect seepage through levees due to increased hydraulic gradients across levees. This has been demonstrated through groundwater flow modeling on Twitchell Island. Subsidence is still occurring near levees as demonstrated by leveling surveys and extensometer data. For example, the extensometer on Twtichell Island within 500 feet of the levee currently records about 0.5 inch per year. Elevation determinations near levees on Bacon Island indicated 1 to 2.5 feet of subsidence from 1978 to 2006.

Comments on Water Analysis Module Technical Memorandum and Responses of Consulting Team

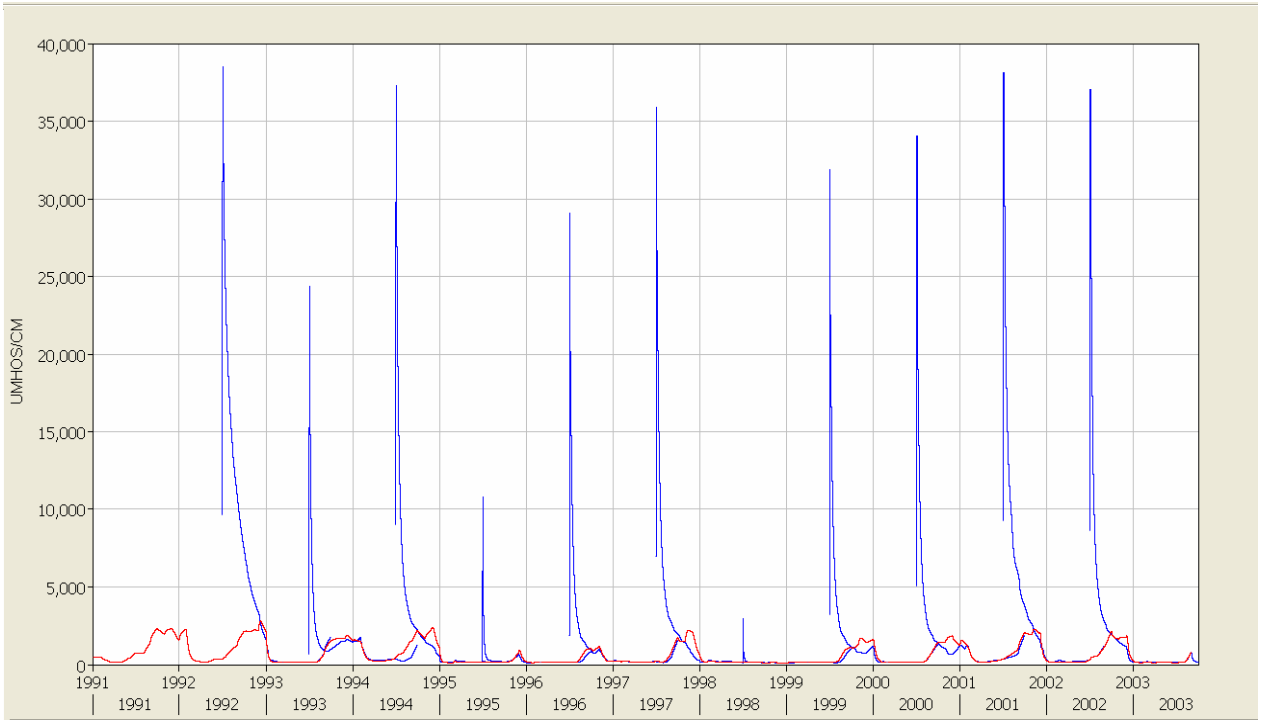
Delta Risk Management Strategy (DRMS) Phase 1
Response To Comments: Water Analysis Module

Comments	Responses
Reviewer: Roger Fujii, USGS, Steering Committee	
1. The WAM TM treats water quality as only salinity. This is a serious error. There are other water quality parameters that need thorough consideration ….	WAM addresses salinity first in developing a model of water quality impacts in the Delta from levee breaches and island flooding – especially from flooding of multiple islands. These impacts can be as severe as tens of thousands mg/l of dissolved solids making Delta water unfit for use either for municipal or agricultural uses. The unfit for use condition can last for months or years. The salinity impacts are relevant to the ecosystem as well as to water supply. The first question of importance is "How severe is the salinity impact?" and the second is "How long does unfit for use last?" WAM was specifically developed to answer those questions and does so successfully and efficiently for the many thousands of levee breach scenarios that are considered in the risk analysis and for approximately 1,000 different event start times based on historic hydrology. Development of this model was an essential first step. Because of limited schedule, it has not been possible yet to include other water quality parameters (such as organic carbon) into WAM. However, the project did perform preliminary analyses on organic carbon that have been added to the WAM TM as Appendix I. Organic carbon can be added to the WAM model when DWR can provide schedule and budget to do it. Other parameters can also be considered for addition.
2. Dissolved and particulate organic carbon form disinfection byproducts during water treatment with chlorine, some of which can have serious health effects. Use the data on organic carbon collected on Jones Tract.	The WAM modelers are aware of organic carbon contaminants interacting with chlorine and forming the mentioned disinfection byproducts. The preliminary analysis of organic carbon now provided in the recently added WAM TM Appendix I used the data collected on Jones Tract and extrapolated for several examples of multi-island breach scenarios. The scenarios did not include any

Comments	Responses
	particle tracking hydrodynamics or any effort to manage organic carbon discharges in island dewatering. Minor problems were found in the cases where ten islands or less were flooded. More substantial problems were found in the 20 and 30 island cases. With no specific hydrodynamic tracking of organic carbon loadings and no effort to manage organic carbon in dewatering, large multi-month spikes with more than 3 mg/l increases were estimated that result in total organic carbon concentrations assumed to be in excess of 6 mg/l. It may be possible, through more detailed modeling, better understanding of background organic carbon concentrations and management of dewatering locations and rates, to lessen the predicted magnitude and duration of spikes. Additional treatment costs for the preliminary calculation results presented were estimated at as much as $68 million. Although this is substantial, it is less than 0.15% of the overall disruption-of-water-supply impacts for the cases addressed. Of more importance is the question whether organic carbon concentrations over 6 mg/l are treatable to achieve potability. If not, municipal contractors may have to refuse delivery after salinity improves to the point that pumping can resume. Thus, they may experience an even longer disruption period. This highlights the importance of extending WAM when DWR's schedule allows.
3. Other water quality parameters of concern include nutrients, mercury, and selenium.	Other parameters can also be considered for addition to the WAM model, but it is less clear how they can be meaningfully addressed in a model that is used to calculate varied water quality impacts from tens of thousands of different levee breach scenarios. If they must be addressed in an overall, applicable-to-all-breach-scenarios way, or based on the gross number or area of islands flooded, familiarity with what WAM calculates for salinity and organic carbon will be an

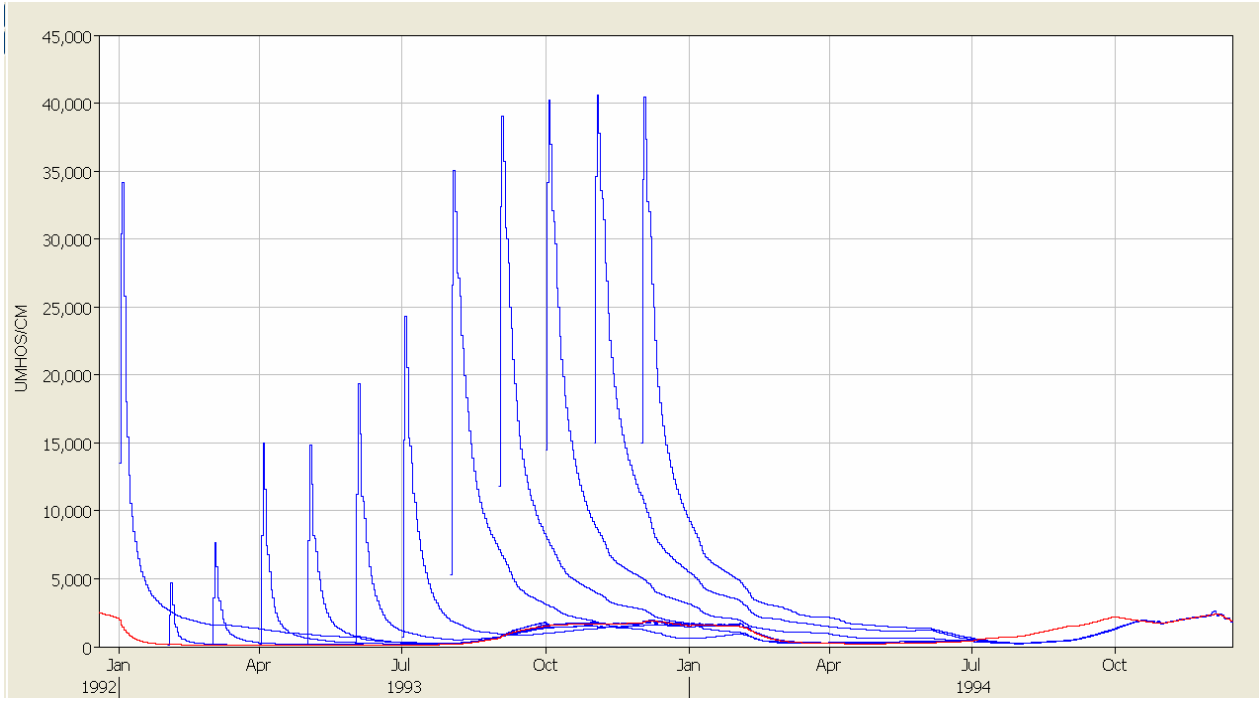
Comments	Responses
	important part of the input that is used to draw these generalized conclusions. A discussion of methylmercury is presented in Appendix E of the ecosystem impacts TM.
4. Conceptual models for processes affecting water quality during flooding.	As WAM is refined and extended to characterize a wider variety of water quality characteristics, Dr. Fujii's ideas on conceptual models for the water quality processes involved in island flooding will be an invaluable and welcome input.
5. Dr. Bill Glaze memo.	Dr. Glaze presents an admirable summary of numerous complex and important water quality concepts. To make discussions of these very sensitive, low concentration phenomena meaningful, one first needs to have a relatively stable, gross characterization of water quality. Normal operations of state and federal project exports from the Delta provide a somewhat stable reference. For Delta levee breach events, water quality in the Delta may be dramatically unstable. We have chosen to present a gross characterization of the Delta's unstable water quality by modeling salinity. The attached figures illustrate that, in a levee breach event, water quality, by any measure (even salinity), is dynamically variable. The very important fine points of water quality that Dr. Glaze summarizes must be discussed with specific reference to some portion of this progressive change in gross water quality throughout the levee breach event and prolonged recovery. Now that WAM characterizes this temporal progression of gross water quality variation, we look forward to future efforts to relate the temporal progression of event salinity to more subtle water quality phenomena.

Comments	Responses



WAM Hydrodynamic Calculation of the Jersey Point Salinity Response to a Multi-Island Levee Breach Event Occurring on July 1 in Various Years (red is base case with no levee breaches)

Comments	Responses



WAM Hydrodynamic Calculation of the Jersey Point Salinity Response to a Multi-Island Levee Breach Event Occurring (Alternatively) on the First of Each Month During 1993 (red is base case with no levee breaches)

Delta Risk Management Strategy (DRMS) Phase 1
Response To Comments: Water Analysis Module

Comments	Responses
Reviewer: Ralph Finch, DWR	
1. WAM Ability to Model Historic Conditions – "results at the interior and export locations do not support the above statements (reasonable results; fairly accurate). A Delta model that cannot explain half or more of the variability of salinity is neither reasonable nor fairly accurate."	Additional calibration has been performed, shared with the commenter, and incorporated in final calculations. Match between computed and observed daily averaged EC at the SWP and CVP export locations was improved by three adjustments to the model. First, the observed daily EC at Vernalis was used for the San Joaquin EC boundary condition in place of the CALSIM monthly-averaged Vernalis EC record. Second, the impact of the temporary barrier on Grant Line Canal was more accurately represented in establishing the net flow through Old River at Head. And third, the multiple linear regression relationship for estimating flow through Turner Cut was improved. These revisions will be discussed in the TM.
2. Ability of WAM to Reproduce Numerical Model Results – "and it is necessary to validate WAM output with the numerical model results using data not used for calibration. But this comparison is not provided in the Memorandum."	The relationship for net salt flux between channels and flooded islands used in the WAM was calibrated by comparing results to the RMA Bay Delta model for a 20 island levee failure scenario. The reviewer's comment is well taken and if additional time becomes available, follow-up comparisons to other breach cases would be valuable. There is a comparison to the historic Jones Tract breach case as a validation (which is updated with calibration adjustments noted for Comment 1 above). However, the Jones Tract event is only a single island failure with limited salinity impact. A modeling exercise is currently underway to compare results from DSM2 and the WAM HD for a CALSIM base run with no breaches. This comparison will be made available as soon as it is complete.

Delta Risk Management Strategy (DRMS) Phase 1
Response To Comments: Water Analysis Module

Comments	Responses
3. Ability of Numerical Models to Model Historic Conditions – "A validation of the numerical models with observed data not used in their calibration is essential to evaluate the WAM process. But this comparison is not provided in the Memorandum."	The WAM HD was calibrated for salinity (EC) by manual adjustment considering the historic period of 1992 through 2003. 2004 has been simulated without adjustment of parameters and includes the Jones Tract breach event. With additional time, a further simulation of 2005 and 2006 could be made as validation tests. Ultimately, if the WAM HD is to receive wider use, implementation of an automated calibration process would be valuable. At that time, the historic period could be formally separated into calibration and validation sets.
4. Other Observations – "The fundamental approach of developing an experimental modeling system is not questioned nor systematically compared to other potentially valid and more traditional approaches."	A discussion of the rationale for developing WAM relative to using existing models was presented in the original Technical Framework document. Portions of that discussion will be brought forward into the TM. The fundamental reason for developing a new model is that there was no existing model that coupled coordinated system operation and Delta hydrodynamic/salinity response under levee breach scenarios in a manner <u>that meet the requirements of the Risk Analysis Framework</u>. Comments on the design of the Risk Analysis Framework are addressed elsewhere; but a fundamental requirement is for calculation of the water quality impacts of a large variety of levee breach events. This requires WAM calculations to be performed for a single event within a few minutes.

Delta Risk Management Strategy (DRMS) Phase 1
Response To Comments: Water Analysis Module

Comments	Responses
Reviewer: Koger, Corps of Engineers (General)	
Note that comments 1 through 5, if they exist, were not submitted for response.	
6. The model is stated to be one dimensional, and does not consider a vertical profile or currents. The model will indicate when export pumping should resume. From a risk perspective, some considerations are whether the export pipes are below water surface, and if so is the model valid for that depth. If the model is for surface water, how robust is the assumption that the export sites are adequately represented by the model?	As explained in Section 2.5, the one dimensional, tidally averaged model considers the impacts of tidal and other currents and vertical stratification by relying on dispersion coefficients to calculate the mixing effects and then provides a cross-section average value of salinity. Thus, the model provides a depth-averaged, across-the-channel-averaged, and tidal-cycle-averaged flow, water surface elevation, and salinity concentration. Very low water surface elevations may occur during initial flooding of islands during the first hours or days following a breach event. The model assumes that pumps are not operated during the initial flooding. After the islands have filled, water surface elevations will fluctuate about mean sea level. The addition of newly flooded islands will generally decrease the tidal range and lower low water will tend to be higher than in the un-breached condition. So after the initial period of island flooding, water surface elevations are not expected to constrain pumping operation. Clarifying language has been added to Section 2.5 of the WAM TM.
7. Continuing with the model assumptions, it would be prudent to validate the model with localized field measurements prior to resuming pumping in areas that would be difficult to flush after salt water intrusion. An ad hoc network could be the municipalities or entities pumping the water	We presume such real time monitoring will occur in an actual levee breach event. It did not need explicit recognition in the WAM model.

Delta Risk Management Strategy (DRMS) Phase 1
Response To Comments: Water Analysis Module

Comments	Responses
8. In the WAM, a basic assumption is water quality conditions are represented by salinity. Are there other potential contaminants of concern after levee breach, or is salinity the driving contaminant? Suggest providing justification for only evaluating salinity during a breach response.	Salinity is the driving contaminant and the one that is the first hurdle in developing a useful model for the risk analysis. Other contaminants, such as organic carbon are also important, but must be addressed later because of limited time for model development. Additional discussion is provided in the WAM TM.
9. Pumping may be allowed if the salinity in the south Delta channels is above standards, but allowable based on other criteria. What other criteria are being evaluated? Is the evaluation the responsibility of the entity pumping the water?	The entities pumping the water, their customers, and regulatory agencies would undoubtedly discuss whether pumping should begin when salinity was somewhat above the 500 mg/l normal standard for salinity. After a prolonged pumping shutdown, users may not want to wait several more months for salinity concentrations to decrease from 600 mg/l (or some similar number) down to 500 mg/l. WAM cannot model such a consultative process. Using a number, like 600 mg/l may be more sensible than using 500 as an absolute requirement. Additional language is provided in the TM.

Delta Risk Management Strategy (DRMS) Phase 1
Response To Comments: Water Analysis Module

Comments	Responses
Reviewer: Koger, Corps of Engineers (Specific)	
1. Page 3, Limited Pumping bullet, lines 5-6 – "fresh water because of the extra volume of tidal flow under a breach conditions and the resultant mixing." Please revise for clarity.	When islands are flooded, the volume of water flowing in and out of the Delta on each tidal cycle is increased because part of the flooded island becomes an active part of the tidal exchange. This causes extra tidal mixing. So a larger inflow of fresh water is needed to repulse salinity. Additional language is provided in the TM.
2. Page 9, Section 2.2, paragraph 4, lines 2-3 – "If an island is flooded, irrigation demand ceases, as does seepage, and return flow." Does return flow cease? Please revise for clarity.	When an island is flooded, many of that island's drainage/return flow pumps are under water and inoperative. Even those pumps above water will not operate, because any water discharged will just be replaced by water flowing into the island through the breach – a waste of pumping energy. More explanation is provided in the TM.

Delta Risk Management Strategy (DRMS) Phase 1
Response To Comments: Water Analysis Module

Comments	Responses
Reviewer: Keer, Corps of Engineers (General)	
1. Document doesn't read smoothly.	We have edited for smoothness as permitted by schedule.
2. Define the term "hydrodynamics" early in the text.	Done.
3. The topical areas covered in the Phase I Risk Analysis listed in the preamble needs to be consistent with the listing of documents provided in the website. There's no Global Warming ITF or TM, should be Climate Change; there doesn't seem to be a water quality topical area presented; there's no Water Management and Operation ITF of TM, it should be Water Analysis. Consistency.	Noted.
4. Many of the documents read that "this study relies solely on available data". There needs to be mention of the research that was done to discover "available data," an explanation of what was found, and a discussion of how it was utilized in this analysis or why it wasn't utilized.	In assembling our team, we selected modelers who were fully aware of available data. They have likely skipped the requested step, since (to them) it was common knowledge. We have inserted references where we identified omissions.
5. Knowing that the authors have an understanding of Corps processes, it would be a great benefit to have a chapter or possibly a separate document presenting an open discussion of how the authors intend this analysis to fit within the framework of the Corps process and possibly present their view of the analysis' shortcomings.	Though obviously useful, this is beyond the scope, schedule, and budget established for DRMS by DWR. We would be pleased to prepare such a document under Corps sponsorship.
6. Starting conditions and assumptions need to be outlined in a table, bulleted, brought out of the text so the reader doesn't have to fish for them.	WAM model inputs and outputs are carefully enumerated. Starting conditions vary. The model is designed to start for any combination of breaches/flooded islands with the initiating event occurring on the first day of any month in the CalSim historic record.
7. Are there any triggers within any of the hydrodynamic/water quality elements looking at whether or not engaging reservoirs to flush the system will directly or indirectly further exacerbate levee failure?	Reservoir flushing is limited by reservoir low-level outlet capacity and available water. Flushing flows implemented are generally less than 10,000 cfs total Delta inflow from the Sacramento River. It is assumed that these flows do not exacerbate levee failures.

Delta Risk Management Strategy (DRMS) Phase 1
Response To Comments: Water Analysis Module

Comments	Responses
Reviewer: Keer, Corps of Engineers (Specific)	
1. Page ii; Definitions and Assumptions, first paragraph; the phrase "some confidence" needs to be explained.	This phrase was inserted to avoid the interpretation that future conditions could be estimated with a "high degree" of confidence.
2. Page ii, Definitions and Assumptions, third paragraph; is this stating that there will be no without-project future conditions? If so, can we get through the Corps process without it?	No. This is stating that all of Phase 1 assumes "Business as Usual," even for future analysis dates. Thus, Phase 1 (BAU) is the "without project condition" or it is the foundation from which the Corps may derive a "without project condition" reflecting any without project change that the Corps wishes to forecast or assume.
3. Page 1, Section 1.1, last paragraph; the statement that the WAM tool needed to be "accurate enough" should be clarified.	Done.
4. Page 3, Section 1.2, first paragraph; what is the delay time between levee breaches and when upstream reservoirs adjust their outflows?	For Business as Usual, a delay of three days is assumed to allow for consultations with management and with regulatory and fish agencies. A shorter delay may be possible with development of specific emergency operating procedures (addressed in Phase 2).
5. Page 3, Section 1.2, first paragraph; does the statement that the *initial* versions of the models reflect input from operators and policy makers suggest that it's intended that this input be removed or that future models will exclude them?	The opposite. The DRMS schedule has not allowed for as much interaction with operators and policy makers as we would have liked and would have been appropriate. Thus, additional input and revisions are anticipated for future versions of the model. TM text has been revised to clarify this.
6. Page 3, Section 1.2, Island Flooding; what are the initial flow and salinity conditions?	Initial flow and salinity conditions are those provided in the CalSim base case for the date upon which the breach event occurs. The model is capable of starting on the first day of any month in the CalSim historic record, i.e., 12 months times 82 years = 984 start times.

Delta Risk Management Strategy (DRMS) Phase 1
Response To Comments: Water Analysis Module

Comments	Responses
7. Page 3, Section 1.2, Flushing; what is the "normal" downstream location for the fresh water/saline water gradient? Is this computed on a daily time step? How does the flushing component dry the islands; in other words, does the model account for the out-pumping of saline water once the island's levees are repaired? If the computational interval is done on a daily time-step, how does the model account for the diurnal tidal fluctuations? If it ignores any fluctuations, does it use the daily max tide? Section 1.2 on page 4 mentions "tidal averaging simplifications…to achieve computational efficiency." This is unclear.	The normal downstream location of the salinity interface is as calculated by CalSim in response to Delta inflows and the requirements of SWRCB D-1641. In WAM it is computed daily and reported monthly with the target of achieving compliance as quickly as possible. When levee breaches are repaired, island pump out is included (water discharge rate and salinity). The model is a tidally averaged model; it works with average concentrations for the tidal cycle on a daily time step and includes the mixing effect of the tides by using dispersion coefficients. We have included language to provide additional explanation.
8. Page 4, Section 1.2, last paragraph; what is too much computer time for WAM? This analysis is of national significance…how much time is too much time, and why can't efforts be made to utilize more efficient processors that the UC systems might have?	Because of the vast number of scenarios to be calculated (tens of thousands) and the large number of other computer calculations in the risk analysis, a goal of having WAM run in about 10 seconds for a single start time was established. We achieved a run time for complex breach cases of about one minute. This compares with several days for a two dimensional model and a few weeks for a three dimensional model. If super computer access were available, it would be used to address other computational problems in the risk analysis; WAM would still be used as a simplified model of reservoir management and hydrodynamics, but might be enhanced in ways that doubled or tripled its run time.
9. Page 4, Section 1.3; what is the timing between request and observation of benefit?	The timing of flushing benefit depends on the flow time from the reservoir making the release, which is variable but generally between one-half and five days.

Delta Risk Management Strategy (DRMS) Phase 1
Response To Comments: Water Analysis Module

Comments	Responses
10. Page 5, Figure 1-2; wouldn't the scheduling of any potential south of delta deliveries be computed *after* the module has determined that the system has recovered? Shouldn't some of the communication paths be bi-directional (i.e., communication between M3 and H5, M3 ↔ H5).	One criterion for making the model "simple" was to avoid feedback loops. There is some compromise of this rule by virtue of the daily time step used for calculation. South of Delta calculations for one day produce results that can be used to influence upstream submodels during the next time step (i.e., what that upstream submodel does on the next day). Since results are only reported monthly, this provides a manageable form of feedback that can be dampened.
11. Page 6, Section 1.3, first bullet; what hydrology is being used to describe the initial state of the Delta?	The CalSim historic record (82 years) and what it says the Delta conditions are at the specified breach event start time.
12. Page 6, Section 1.3, third bullet; shouldn't there be a delay in the model's response to new islands flooding? Might it be possible that as more levees are breached, flood-fighting response is quick, because resources are already staged locally and that as the number of breaches increases response timing decreases because it gets more difficult to disperse and allocate emergency response resources?	WAM responds as soon as a new breach occurs because, at that time, whatever effort has been made to prevent the breach has failed and the island is being flooded in an uncontrollable and accelerating way. WAM receives the time of the new breach from the Emergency Response and Repair Module, which considers flood fighting or other mitigating measures as well as allocation of emergency resources.
13. Page 6, Section 1.3, second to last paragraph; what marks a "recovered" system?	The system has recovered when the Delta is back to normal (as indicated by the CalSim base case) and when upstream and South of Delta storage is also back to normal as indicated by the CalSim base case.

Delta Risk Management Strategy (DRMS) Phase 1
Response To Comments: Water Analysis Module

Comments	Responses
14. Page 7, Section 1.3, H2; the term "tidally averaged dispersive flux" needs to be explained. If the term is referring to the diurnal ebb and flow tides in and out of the Delta, wouldn't the fact that the model is computing on a daily time-step temper that tidally averaged dispersive flux?	Tidally averaged dispersive flux is a quantitative measure of how much salinity a tidal cycle carries upstream and leaves farther upstream than it was when the cycle started. WAM uses dispersion coefficients to calculate this quantity and thus avoid tempering the tidally averaged dispersive flux. In using a daily (tidally averaged) time step, the model must include the water movement due to a day of average flow to downstream plus the mixing that would be caused by the ebb and flow of tides. The mixing part is addressed by the dispersion coefficients that calculate the amount of dispersive salinity flux. The salinity movement caused by net downstream flow and the tidally averaged dispersive flux of salinity to the upstream tend to cancel each other out so that the location of the salinity interface moves only a little, if at all.
A15. Page 8, Section 2.1, third bullet; where and how is CalSim playing a role in this analysis all of a sudden? I believe the purpose of the CalSim model is to evaluate the water supply capability of the CVP and SWP…. In this application, CalSim has developed its own input hydrology; how is this hydrology different from that developed within the DRMS framework and how is this inconsistency documented and the uncertainty associated with using different sets of hydrology, within the same analysis, documented?	DRMS uses the CalSim hydrology (82 years of simulation based on historical hydrology and 2005 conditions – land development, water demand, and water system infrastructure). The main difference with DRMS is that CalSim assumes no levee breaches. DRMS therefore uses CalSim as the no-breach base case and models the breach scenario with the water that CalSim says is available during the incident. WAM is able to start the incident on the first day of any month in the CalSim hydrologic sequence.
16. Page 9, Section 2.1; how are the levee breach sequences determined? Is each breach determined by current conditions or are the breach scenarios dictated by the user, determining what levees breach, when they breach, and how the repair process is executed?	The breach sequence or scenario is determined by other modules based on earthquakes, floods, and levee fragility. These are inputs to the Emergency Response and Repair Module, which evaluates secondary breaching and establishes repair and pump out schedules and then forwards all that data to WAM. WAM could be used independently to evaluate any combination of breaching and schedule of repairs that one cared to define.

Delta Risk Management Strategy (DRMS) Phase 1
Response To Comments: Water Analysis Module

Comments	Responses
17. Page 9, Section 2.2, second paragraph; what are the major Delta flow paths as defined by the hydrodynamics submodel and how are these flow paths different from those assumed in the Flood Hazard analysis?	The flood hazard analysis is oriented to conveyance of floods from tributaries (Yolo Bypass, Sacramento River, Mokelumne River, San Joaquin River, etc. to the mouth of the Delta. WAM is concerned with flow paths for Sacramento fresh water flow across the Delta to the various south Delta pumps. Figure 2-1 illustrates the flow paths incorporated into WAM. During floods, flow paths are not a big concern because the whole Delta is fresh.
18. Page 9, Section 2.2, fourth paragraph; "If an island is flooded, irrigation demand ceases…" is the assumption the cessation of processes is for the flooded island only?	Yes, for the flooded island only and only until the island is repaired and pumped out.
19. Page 10, Section 2.3, third and fourth paragraphs; the assumption here is that releases will be made according to D-1641 requirements regardless of the mechanism degrading water quality. Is it truly safe to assume that conditions resulting in increased salinity in the south Delta, such as a levee breach, are (or will be) treated as "standard project operating procedures?" Is the assumption that upstream CVP and SWP operators will <u>always</u> release water for flushing to sustain exports – regardless of the mechanism initiating the increase in salinity? "Upstream reservoirs" needs to be defined.	The water quality standards in D-1641 do not include exceptions for unusual circumstances such as multi-island flooding due to an earthquake. What the SWRCB staff said in our meeting with them is that the state and federal projects are on the hook to meet the water quality standards and would be expected to do everything reasonably possible to meet them. So the projects have two reasons to do what they can to help the Delta recover – water quality standards and resumption of pumping. The projects are assumed to release water as much as they can (responsibly) to accomplish this recovery. The upstream reservoirs are the project reservoirs – Shasta, Oroville, and Folsom. New Melones was not addressed because the CVP does not have control of a significant amount of stored water in New Melones; senior water rights have control.
20. Page 10, Section 2.4; emergency reservoir operating decisions are made to balance the amount of water released while emergency repairs progress…. What is the scope of the "emergency decisions" that are being made? Are they limited to non-flood scenarios?	Yes, they are limited to non-flood scenarios or time periods and are constrained by reservoir flood operation requirements. The balancing is to send as much flushing water as is really useful without depleting storage in an irresponsible way, given that the wetness or dryness of the next winter is not known.

Delta Risk Management Strategy (DRMS) Phase 1
Response To Comments: Water Analysis Module

Comments	Responses
21. Page 14, Section 3; the input, output, and submodel modifications section is difficult to read. It's unclear what the inputs are, it's not explained how and why CalSim is used. This section really should be rewritten.	Revised.
22. Page 14, Section 3.1, second bullet; there's no explanation of how the breach locations and sizes are determined for each scenario.	This is an input from the earlier modules in the computational process; it is a consolidated input to WAM received from the Emergency Response and Repair Module.
23. Page 15, Section 3.1, first complete paragraph; how many different sets of hydrology are being used in the DRMS analysis – a set for CalSim, a set for the climate change analysis, a set for the flood hazard analysis…?	The 82-year, 2005 CalSim hydrology is used for the DRMS Phase 1, 2005 base case. For 2030, CalSim has a similar 82-year hydrology for their 2030 base case; we would use that for 2030 without climate change. We were unable (because of schedule and budget) to apply WAM for climate change. We would use a modified CalSim hydrology for future years reflecting forecast changes in annual runoff and the changed portions of runoff in the winter versus the spring. A special CalSim modification would be developed for each future year studied. We have (from global climate models) four different future scenarios, so we would ideally develop four different CalSim modifications for each future year to be studied.
24. Page 15, Section 3.1, Levee Breach Event Sequence Data; language states that the WAM module will not need to make specific allowances for the greater combined effects that sea level rise, seismic and flood hazards may have. The reasoning that the WAM has found it appropriate not to address this is unclear.	The important point is that these factors (though they are important) are reflected in WAM's inputs. They do not have to be accommodated through changes in WAM's structure or modeling relationships. Since changed hydrology will be accounted for by a modified CalSim run (a WAM input), the only future changes that WAM has to make internally are revised island volumes below sea level (flooding volume change due to subsidence and sea level rise) and revised dispersion coefficients to account for greater tidal mixing with sea level rise. The revised dispersion coefficients are addressed in WAM TM Appendix H3.

Delta Risk Management Strategy (DRMS) Phase 1
Response To Comments: Water Analysis Module

Comments	Responses
25. Page 16, Section 3.3, Delta Water Operations; it is unclear why this section is included. Does the inclusion of the proposed south delta barriers incur a different operation in a levee breach emergency or not? Is it safe to include and assume the benefits in operation of a structure that's not in place yet?	The section is included because WAM must make specific assumptions on how Delta pumps and gates will be operated when there are levee breaches. Since the broad topic here is future conditions, the south Delta operable barriers are recognized as a real possibility. Since operation of these barriers has not yet been defined, they will be assumed (for now) to operate like the existing barriers if they are installed.
26. Page 16, Section 4.3; the intent of the paragraph is unclear. With the prospect of global warming and climate change altering the hydrometeorological conditions in the Central Valley, how is it that refinements to operating upstream reservoirs can be avoided? With less precipitation falling as snow and more of it falling as rain – these reservoirs will operate differently. Can it be expected that future conditions will really be a "business as usual" operation?	Business as Usual is not advocated as a reasonable picture of the future, but simply as a place to start in thinking about the future. Reservoir operation is a good example. Will the Corps demand and achieve larger flood control reservations to deal with more winter precipitation as rain rather than snow and the resultant increasing flood frequencies? Or will water supply interests demand and obtain more reservoir space for winter water storage to capture winter flows? We don't know. So assuming present operating rules is a reasonable place to start.
27. Appendix B, Page B-6, Table B-1; Table indicates that there are no (zero) releases from New Melones in support of sustaining or reestablishing water quality in the Delta; if true, why is it that this assumption seems to violate the criteria put-forth by D-1641? Question marks need to be replaced with descriptive text. How the "upstream reservoirs" are utilized in the analysis should get better explanation. Is the table indicating that the only upstream reservoirs assisting in the flushing efforts are Folsom, Oroville, and Shasta? Their limitations need to be presented in a clearer fashion.	Revised. New Melones is not used because senior water rights holders (not the CVP) control essentially all of the stored water.

Delta Risk Management Strategy (DRMS) Phase 1
Response To Comments: Water Analysis Module

Comments	Responses
28. Appendix C, Page C-7, Figure C-2; the figure does not indicate boundary condition flows as stated within Section C4, Page C-6.	The figure indicates the locations of the boundary condition flows that are extracted from CalSim output. Since initiation of the levee breach event can be chosen to be any one of 984 start times, the boundary conditions themselves can take any of 984 values. The text has been clarified.
29. Appendix C, Page C-6, Section C4; how exactly is the Sacramento River index used in the Reservoir Management submodel? How does the "hydrologic year" dictate boundary conditions?	CalSim simulates management of the state and federal water projects. One of the indicators used within CalSim to make management decisions is the Sacramento River Index. Thus the index influences the reservoir management submodel in the fist instance though the CalSim base case. When an event start time is specified, it is specified in terms of a CalSim year and month. Thus, for example, it may be specified as June, 1977. The reservoir management model then extracts the CalSim output data for June 1, 1977 and those data become the boundary conditions with which WAM starts. The fact that 1977 was a Critically Dry year is used in managing the upstream and South of Delta reservoirs.
30. Appendix C, Page C-10, Section C6.2; second bullet; does statement indicate that the maximum upstream releases could be zero?	In the unusual situation when the factors indicated in the second bullet indicate that available water is less than would be needed for minimum releases, the minimum release is assumed. In the very rare instance when dead pool is reached, a deficit account is maintained. Allocation of deficits to downstream beneficial uses (north of Delta contractors and fish flows) has not yet been accomplished in the model. This is recognized as a loose end that needs to be addressed as soon as DWR authorizes further work.
31. Appendix E, All Figures; there's no map to reference any of the station locations.	Added.

Comments	Responses
32. Appendix F, Page F-4, Section F1.4; paragraph is a bit unclear. Is the word "adjusting" indicating that the base condition CalSim inflows are adjusted to represent inflow hydrology representative of climate change, or are the inflows substituted? It's still unclear how the location and extent of the levee breaches are dictated.	Levee breach locations and extent are established outside of WAM and are received by WAM as an input. They come principally from the Seismic Hazard Module and the Flood Hazard Module, via the Levee Fragility Module (which estimates which levees fail) and via the Emergency Response and Repair Module (which estimates secondary failures and establishes a repair schedule). WAM receives all this information as an input and the input may include a specification or restriction of breach event start times. For example, flood events can only occur in winter months. To model water quality for this event at a future time (say 2050) as influenced by climate change, WAM needs a 2050 CalSim run reflecting climate change and no levee breaches as a base case. This CalSim run must be created by adjusting each of the major CalSim monthly inflow streams (approximately 15) for all 82 years of record. This is done using the trend analyses described in Appendix F and includes indicated percentage increase or decrease in total annual inflow for each stream and changed proportion of total annual flow in each month. Then WAM uses the adjusted CalSim input to run CalSim and create a new base case as the source of input hydrologic data for modeling the breach event and recovery.
33. Appendix F, Page F-6, Section F2.4, first complete paragraph; shouldn't the description of CALVIN as a management model be that it shifts scarce supplies to higher value uses?	Clarified. The text was written from the opposite point of view, indicating that cutbacks are allocated to lower valued uses.
34. Appendix F, Page F-6, Sections F2.4 and F2.5; the a2 climate change scenario should be qualified with an explanation of the "mega drought" earlier…and in other TMs where the climate change analysis is	Noted. The information has been forwarded to the Climate Change Team.

Comments	Responses
influencing the analysis results.	
35. Appendix F, Page F-9, Section F3, Figures F-1a through F-4b; what are the trend lines used for? Some are trending upwards…some are trending downwards. How good a fit are these trends to the data?	The trend is used to adjust the 82-year monthly hydrologic record used as CalSim inflows. The adjustment is based on the trend value at the year of interest (e.g., 2050).

Comments on Impact to Infrastructure Technical Memorandum and Responses of Consulting Team

Delta Risk Management Strategy Phase I
Technical Memorandum: Draft Impact to Infrastructure

Comment	Response
Reviewer: Goettel – U.S. Army Corps of Engineers	
General Comment: The use of "infrastructure" seems incomplete, since much of the discussion and much of the damages deal with buildings (residential, commercial, and public), as well as agricultural and marine facilities that are not commonly designated as "infrastructure". A more generic title such as Infrastructure and Building Stock would be more descriptive.	The term "infrastructure" has been used to designate all buildings and structures. Clarification is added to Section 2.2
1. Section 2 has a nice summary of inventory included. It could be improved by adding approximate quantities: miles of roads, miles of transmission/pipe lines, number of residential structures, number of commercial/public structures etc. Such numerical totals would give a better "picture" of the magnitude of at risk inventory. Similarly, an approximate replacement value for each category would be informative and emphasize the magnitude of at risk assets in the Delta.	Approximate quantities are included in Table 2-1 at the end of Section 2.2.1.
2. The Figures showing the various categories of inventory are VERY informative. Since there are only a few figures (10 or 12), the usefulness of the report would be enhanced by moving these to Section 2 so they are more visible to the reader.	To avoid splitting main sections of the TM, the figures need to be at the end of the TM.
3. Similarly, the Maps of the analysis zones are inexplicably buried in Appendix C. At a minimum, these should be with the other figures, but preferable these should be in the body of the report, for easy reference.	See response to Comment 2 regarding location of the figures in the TM. These figures are renumbered as Figures 3-2, 3-3, and 3-4, and are with the other figures.
4. Section 3.2. Means 2005 is two+ (since Means 2005 is generated in 2004) years out of date. Why not update to 2007 values? Should be simple tweaks to spreadsheet calcs.	Costs are kept at the 2005 values because the economic losses and evaluations are based on 2005; both costs need to be at the same base date.

Comment	Response
5. Section 3.2. Note that Google Earth had 30% more structures than HAZUS for 20 islands. 30% is a big difference. Which numbers were used? Note, however, that many of these structures may be storage etc, not residential.	The HAZUS numbers were used. "Google Earth" only applied to 20 islands, and as noted in comment, not all structures are residential. This check indicates a limitation of the data. See clarification in Section 3.2.
6. Section 4.1.1. MHHW should be clearly defined for non-technical readers.	There are two high tides and two low tides each lunar day (24 hours and 50 minutes) in the San Francisco Bay and Delta. The two tides are unequal, one of the high tides is higher than the other, and one of the low tides is lower than the other. The higher of the high tides each day is known as higher high water (HHW) and the lower of the low tides is known as lower low water (LLW). Mean Higher High Water (MHHW) is the mean value of all the higher high tides for each day over a 19-year period. (Mean Lower Low Water (MLLW) is the average of all the lower low waters during the same 19-year period.) Footnote added for clarification in Section 4.1.
7. Section 4.1.2. Using the 100-year flood is logical and representative of flood damages, but: a. The footnote states that FIRMs are under revision. Are drafts available? In any case, some qualitative estimate of the likely change would be very helpful, such as flood elevations may be one to two feet higher? b. For below-sea-level islands, there probably aren't big differences in damages for other floods smaller or larger than the 100-year flood. However, for urban areas, there will likely be much larger differences – some commentary on this should be added to the existing text.	a. FEMA is currently in the process of updating some of the 100-year flood maps in California including the Bay Area. We are not aware of drafts that are available. At this time we do not know how the new floodplains will differ from the existing floodplains. b. The largest differences between damages for the 100-year event and other events would be for infrastructure located near the edge of the floodplain or areas with topographic relief. Added clarification to Section 7.2

Comment	Response
8. Page 16. The discussion (just above Section 4.2.2 should be amplified for clarity. Damage percentages were obtained from HAZUS depth-damage relationships for various types of infrastructure and buildings, for the average depths (if this was done) in each analysis zone.	This paragraph was deleted. See also response to Comment 10.
9. Scour damages. The draft has NO information about how scour damages were calculated. Per the modeling, scour is assumed to affect only 2000' x 2000' sections of a breached analysis area, but the location of the breach is random. Thus, the location and extent of scour damage will vary markedly from next to none (if there is nothing much in the scour zone) to a lot, if something big and vulnerable is in the scour zone. For critical infrastructure (Mokelumne Aqueduct, for example), scour damage may or may not occur. Thus, how scour is handled probabilistically is very important for interpreting the vulnerability of infrastructure to scour damages. Essential to include more details in this calculation. If (hopefully not), scour was assumed for the entire scour-prone zone of each analysis area, then scour damages in any flood would have been grossly overestimated. In any case, estimating scour damage is tricky and needs to be carefully explained.	Probability analysis is the subject of the Phase 1 Risk Analysis Report, not the technical memoranda; see Section 1.2. [The scour holes (2000' x 500') were handled probabilistically in the Phase 1 Risk Analysis Report.] Scour was not assumed for the entire scour-prone zone of each analysis area. Scour holes could occur anywhere within the island perimeters. Clarification was added in Section 4.2.2.

Comment	Response
10. Tables 4-1a and b. Urgently need explanation. These are stated as being for the MHHW or 100-year "event". "Event" for what analysis areas? One, several, all? Since which analysis areas flood in a given event is largely probabilistic, there is no such thing as a MHHW or 100-year event, rather there are MHHW or 100-year flood events which flood analysis area A, B, or C…. a. Also, the totals at the bottom of each table have almost no meaning – because they sum apples and oranges (gas wells, schools, cell towers etc.). I suggest removing the "grand" totals at the bottom of the tables.	Tables 4-1a and 4-1b were deleted.
11. Page 20. Cleanup costs for petroleum pipe breaks are estimated to be $10 million. Since such breaks are likely largely from scour, how is the probability of such breaks calculated?	Probability analysis is the subject of the Phase 1 Risk Analysis Report, not the technical memoranda.
12. Page 20. Omission of debris costs is puzzling. FEMA covers debris removal costs for all Federal disasters, so surely estimates of debris removal costs are available. Debris removal estimates should be added to other loss estimates.	Debris removal costs are included; the tables (in Section 7.1) were updated to include damage cost to contents. See Section 4.3.1.

Comment	Response
13. Section 4.3.2. The purported cost multipliers for repairs (and presumably replacement) are simply stated with little justification. For flooding "islands" which have relatively little inventory (compared to say, Sacramento), there is little or no justification for such cost multipliers. Flooding of 30 islands would surely increase the repair times, but it's hard to believe that total repair/replacement costs would be 2.0x those for flooding 5 islands. This is an important issue, since it affects the magnitude of losses and thus bears on public policy decisions. At a minimum, I'd like to see two sets of numbers: with and without the purported cost multipliers.	If there is a sudden increase in the demand for construction, it is expected that the construction costs would increase. With multiple levee failures scaling factors (multipliers) have been used to increase costs of repair due to the supply and demand for equipment, labor and, materials. In investigating the use of scaling factors, we have reviewed literature from post catastrophic events. The insurance industry refers to what we have termed "scaling factors" as "post event inflation" or "demand surge". The findings of this review support the scaling factors assumed in the draft TM. These findings are documented and assumptions clarified in Section 4.3.2. Tables referenced in Section 7.1 do not include the cost multipliers. Cost multipliers are used in the risk analysis for multiple island failures.
14. Page 22: Repair times vs. damage are an important element in the calculations. I'd like to see the algorithms be explicit rather than mentioned without any documentation of what values were used. The replacement times should be qualified as "typical" or "average". One difficult question, which should be addressed, is: if an island floods will homes be rebuilt at all? Or, if so, will it in fact take many years/decades before people choose to rebuild in an area that just had very deep, very long duration flooding? Important policy questions which should at least be acknowledged. As well documented by New Orleans, a really big flood changes the "picture" of rebuilding and future development in profound ways that will likely endure for decades	Algorithms are included in Section 4.4.1. The policy questions have been acknowledged in Section 4.4.2, first paragraph.

Comment	Response
15. Longer repair times for multiple island floodings appear more credible than the higher repair costs discussed above.	Noted.
16. Section 4.4.3. The technical terms used are not entirely self-explanatory and should be explained clearly. Otherwise, this section is less useful than it could be.	The technical terms are defined in Section 4.4.3
17. Page 20, fourth bullet: What scour zones were assumed for which analysis zones? As noted above, whether or a not a given breach results on scour damage to infrastructure depends enormously on where the breach is vis-à-vis at risk inventory. How was this modeled for the calcs?	See response to Comment 9. It is believed that this comment pertains to bullet 9 in Section 4.5, which pertains to scour.
18. Sections 5.1, 5.2 and 5.3. It would be informative to provide summary results for the ranges of changes in damages expected from 2007 to 2050, from the effects noted here. Is this a 1% change, a 10% change, a 100% change???	Such an overall summary is included in Section 7.2. (Section 5 discusses methodology.)
19 Section 6.2 Epistemic Uncertainty. Very unclear how the estimates in this section were applied to the calculations. Were the probability weighted damage functions used for the DRMS loss estimates? a. The stated weighting of expected values 50%, plus 30% values at 25%, and minus 15% values at 25% yields an adjusted value for repair costs which is slightly larger than the stated "expected value" (3.75% higher, from the asymmetry of the uncertainty). b. Why DRMS used an asymmetric distribution rather than the symmetric distribution used by the USACE is unclear. c. Also, there is no discussion of the magnitude of uncertainties assumed for DRMS vs. USACE (essential to provide this for reference).	This section describes the approach for addressing the epistemic uncertainty in cost estimation. The DRMS loss estimates use the expected repair costs based on the median depth-damage relationship. a. The approach defines three discrete depth-damage relationships with confidence values of 50%, 25% and 25%. The expected value from these three levels is not calculated. We agree with the comment that if the expected value were to be calculated, it would be slightly larger than the one associated with the 50% confidence level. b. An asymmetric distribution is used for cost because it is consistent with our experience with the accuracy of estimated repair costs and with typical engineering cost estimation procedures. It is based on the observation that we are more likely to underestimate than

Comment	Response
	overestimate the cost. c. In the USACE HEC-FDA, the percent standard deviation in the depth-damage function is a user input. No specific value for that measure of uncertainty is recommended. Furthermore, USACE HEC-FDA combines epistemic and aleatory uncertainties, while the DRMS approach treats them separately. We believe no direct comparison of the magnitude of the uncertainty between the two approaches is feasible.
20. Despite the above discussion of epistemic and aleatory uncertainties, the results Tables 7.1, 7.2 etc. contain only single values. Are these expected values using the HAZUS relationships or the "adjusted" expected values using the asymmetric DRMS epistemic uncertainty estimates? Where are the upper and lower bound results? (Certainly don't need full tables, but should at least have a discussion indicating the combination of epistemic and aleatory uncertainties vis-à-vis the expected values: that is, lower bound is X% below the shown values and upper bound is Y% above. Or, whatever is the appropriate statistical measure (if there is one) rather than "upper" and "lower" bounds.	The values shown are the expected values assuming the median depth-damage relationship. As noted in the TM, for a given depth-damage relationship, the repair cost is assumed to follow a lognormal distribution with the shown expected values and a coefficient of variation of 15%. Using the lognormal distribution, one can calculate different percentiles of the repair cost. For example, one standard-deviation upper- and lower-bound factors on the median cost would be 1.16 and 0.86, respectively. These are not shown to keep the tables relatively simple.
21. MHHW vs. 100-year flood. There are results for about 50% more study areas for the 100-year flood compared to the MHHW flood results. Presumably this is explained someplace in the DRMS report, but this is an IMPORTANT difference and really must be explained at the beginning of this TM, when the MHHW and 100-year flood are first introduced. Otherwise, just very confusing to the non-technical reader.	Clarification has been added to Sections 4.1 and 4.1.2.

Comment	Response
22. Page 30 bottom paragraph. **"Assets within the scour zones are assumed to be completely destroyed."** If this was really done (hopefully not), the scour damages are grossly overestimated by factors of 10x or 100x because for any given study area, only a small fraction of the potential scour zone will actually be scoured in any given breach. That is, for any flood event, the most likely number of breaches per study area is probably 1.0 (or very slightly more than 1.0 – the entire perimeter of a study area cannot be breached (with scour) in a single flood event. **Clarification and revision (if this was really assumed) is CRITICAL. This appears to be a major computation blunder.**	See response to Comment 9. Concur: the entire perimeter of a study area cannot be breached (with scour) in a single flood event. Clarification was added in Section 7.1.
23. Section 7.3. The omission of values for the listed facilities is baffling. Surely reasonable estimates can be made for the value of such facilities on a unit cost per SF or unit cost per capacity. Such numbers are readily available for the listed "excluded" types of assets. Not a big impact on the overall results, but an unnecessary limitation for a study of this level of effort.	Section 7.3 was clarified. We consider damage to infrastructure assets that could result from levee breaching and island flooding (refer to Section 1.2). Infrastructure assets that would not be damaged by levee failure are beyond the scope of the TM.
24. Interpretation of results. **Given the massive amount of results data, it would be highly beneficial to provide a significant amount of commentary, analysis and summary tables.** For example, commentary on the ranges of damages for small and big islands, summary tables showing the 10 study areas/island with the largest damages, etc. etc. etc.	Commentary is in Section 7.2. Damage costs were handled probabilistically in the Phase 1 Risk Analysis Report.

Comment	Response
25. 2050 results area about 20% more damage. Is this from more inventory or from higher water depths (or, actually, how much from each?). I presume that all of the quoted damage values are in 2007 dollars? This is an important distinction and must be clearly stated in the text and in a footnote to the 2050 tables.	Clarification added in Section 7.2. Increased damage would result from more inventory and from greater water depths. 2005 dollars are used; see footnotes added to Table 7-8. See also response to Comment 4.
26. Are "building" damages really only for buildings or do they include contents? This is a very important difference and the basis for the building calcs needs to be explicitly stated in the text and footnotes. If indeed these are for buildings only, then the omission of contents is an inexplicable and major omission which should be corrected throughout by adding columns for contents damages. To omit such a major category of damages as "contents" is simply unacceptable.	Building damages have been revised to include both the structural damage to the building and damages to contents. The approach and percent values used to estimate these damages are based on FEMA's HAZUS-MH software. Costs are estimated as a percent of the structure replacement. With this revised data, the tables (in Section 7.1) were updated to include damage cost to contents. The draft TM was revised to include damage to contents and the assumptions used for estimating damages; see Section 4.3.1.

Delta Risk Management Strategy Phase I
Technical Memorandum: Draft Impact to Infrastructure

Comment	Response
Reviewer: Bedker – U.S. Army Corps of Engineers	
1. Page 13, last sentence on page: Discussion assumes that a breach occurred during the MHHW. How often does a MHHW occur? Daily? Weekly? Monthly?	As described in the response to Comment 6, above, MHHW is the average of the higher high waters that occur each lunar day. This means that for roughly half of the days the highest tide of the day would exceed MHHW and about half the days would have a highest tide that was less than MHHW. Most of the days with a highest tide that exceeded MHHW would be clustered together during a period known as a spring tide and most of the days with the highest tide less than MHHW would be clustered together during period known as a Neap Tide. There are usually two spring tide periods per month. See footnote to Section 4.1.
2. Page 14, first paragraph: In the middle of the paragraph there is a sentence that states "Where both exist, the value in the floodplain was used." What is the "value" that is used? What is the derivation of the value and what does it refer to?	On the FEMA FIRM maps for the Delta, elevations of the 100-year water surface (100-year flood elevation) provided for an island may differ from the value of the flood elevation given for the adjacent river. In these cases, the flood elevation provided for the island was used to generate the 100-year flood map since the flood map was meant to represent the case with flooded islands. See footnote to Section 4.1.2.
3. Page 14, paragraph referring to "Sour Zones": The scour zone is assumed to be 2,000 feet long. Please source the assumption. The cost to repair these sites is significant and there should be more information supporting this assumption.	See Section 4.2.2: The dimensions of scour holes were based on historical data from Delta levee failures as discussed in the Levee Vulnerability TM.
4. Page 15, first paragraph under "Point Assets": The last sentence in the paragraph refers to the total loss of the entire structure if 50 percent of the structure is damaged. Within Corps guidelines, this assumption is open to debate as well as the use of replacement cost assumptions.	Noted.

Delta Risk Management Strategy Phase I
Technical Memorandum: Draft Impact to Infrastructure

Comment	Response
5. Page 15, third bullet: The narrative indicates that a levee failure would affect water quality and thus the pump station's ability to deliver water. Are there restrictions on pumping through the pumps based on water quality? If so, please indicate the assumed restrictions and indicate the regulations impacting these deliveries.	As noted in Section 4.2.1, a levee failure would affect water quality and thus the pump station's ability to deliver water. The pumps would not be operated if salinity were excessive. This portion of comment is beyond the TM's scope.
6. Page 15: Are there any damage estimates noted for gas or water wells? Ports? Bridges?	There are damage estimates for bridges (both rail and road) and ports. Gas/water wells are not considered in the repair cost estimates.
7. Page 15, first paragraph under "Linear Assets": The first paragraph indicates that repair costs were either obtained from historical records by asset owners or by appropriate cost estimates. Can you elaborate on the "appropriate cost estimate" base used to determine the cost estimates?	The wording used is *approximate* cost estimation, not *appropriate* cost estimation. Nonetheless, clarification has been added in Section 4.2.1; used the term conceptual level cost estimation.
8. Page 20, last paragraph: Can you give an example of the type and quantity of cleanup that has historically taken place. Please provide examples.	Suisun Marsh is cited in Section 4.3.1. The type and quantity of clean up is cited. Cost of clean up is not available.
9. Page 20, last paragraph: Is there a reason that disposal of debris is not included in the cost estimates?	See response to Comment 12. above.
10. Page 21, Multiple Island Failure: What is the basis for these scaling factors? Have they been used in other studies in the past? Please identify these other studies and the methodology around the scales.	See response to Comment 13, above.
11. Page 21, first paragraph under General Methodology: The last sentence of the paragraph indicates that "judgment was used to estimate repair times." Please indicate whose judgment was used and the experience or thought process in arriving at the repair times.	Clarification was added to Sections 4.4.1 and 4.4.2.

Comment	Response
12. Page 21, under General Methodology: The infrastructure is lost from the time the levee breaks until the island is pumped out and dried and fixtures are fixed. How much time is estimated for this and what is the approach you use to compute this?	Pump out time is the subject of the Emergency Response and Repair Tech Memo. Repair times in the Infrastructure Tech Memo are from the time that island pump out has been completed. See Section 4.4.1.
13. Page 22, fifth bullet point: Assuming that it takes less than a month to fix the highways and roads, how long does it take to pump-out the islands? Assume breach of levee until the water pumping is completed. Wouldn't the repair time be based somewhat on the use of the road or highway and the economic cost incurred due to its' downtime? The assumption conflicts with the repair times noted at the bottom of page 23 for Interstate 5 and 205, state highways and other roads.	See response to Comment 12, below. Concur. The repair times would be based on the use of the road or highway, as noted in Section 4.4.2 (see bullets under Linear Assets). Clarified in Section 4.4.1, last paragraph.
14. Page 31: Is there an accepted published approach using the scaling factors cited?	See responses to Comments 13 (previous set of responses) and 11 (this set of responses). See also Section 4.4.3 for comparison with reconstruction activities following Hurricane Katrina.
15. General comment: It is hoped that the data indicated in the tables are available for use electronically. The citation by Kates was extremely helpful in the documentation of infrastructural damages.	The data in the tables presented in Section 7 are available electronically.

Comments on Flood Hazard Technical Memorandum and Responses of Consulting Team

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments: Flood Hazard Technical Memorandum

Comments	Responses
Reviewer: Jensen and Burnham, U.S. Army Corps of Engineers, General Comments	
1. Climate change assumptions and procedures used are not clearly stated.	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 method was not intended for more detailed studies but was designed for use in the Risk Analysis Report, where thousands of different simulations were conducted. Thus, the method needed to be simple and easily implementable.
3. The daily time interval used is too long to capture the peak flows, tidal effects, timing effects, outflows from the Delta, etc.	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 Report. Hourly stage and tidal data were used in the analysis.
4. The presented procedures do not take into account reservoir operations, by-passes/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 indicated that the stage could be estimated with reasonable accuracy for purposes of the Risk Analysis Report. The analysis incorporate Yolo Bypass diversions. Operation of Delta Cross Channel is, in general, constant during the wet season.
5. 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.
6. The results presented are not accurate enough for sizing and design 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 <u>specific location</u>, not a specific inflow into the Delta.
7. While the procedures applied for estimating flow-frequency curves	The Flood Hazard TM has been updated to provide a more accurate description of the

Comments	Responses
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.	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.
Jensen and Burnham, Specific Comments	
1. Section 1.1, page 5. More discussion is needed here on the specific results of the technical memorandum, i.e., what information is being produced as an input for the Risk Analysis model.	Section 1.2 was expanded to better explain model output.
2. Section 2. This section states that a daily time interval is adopted for the analyses. The Corps presently uses a one hour time interval to model its reservoir systems for floods. The technical memorandum report must demonstrate that a daily time interval is appropriate for flood estimates throughout the Delta. One way would be to show the differences between daily and shorter time intervals in peak stages at various gages throughout the Delta. The comparisons should be made for the range of annual peak events of record for all pertinent recording stage gages in the Delta.	The hydrologic analysis does not have a time component. The hydrologic analysis develops hydrologic events and probabilities to use in the Risk Analysis. In developing the events and their associated probabilities, daily flow rates were used but hourly measured stage data were used to develop inflow-stage relationships. The methodology provides accurate estimates of water surface elevations considering the wide range of variables that must be considered in the Risk Analysis.
3. Section 2.1, page 6. The text states that Figure 2-1 shows where water surface elevations were measured. The only gage locations shown on Figure 2-1 are the flow measurement locations.	A new Section 2.1 has been added. Figure 2-1 is presented in Section 2.2 and references only the flow measuring stations; Figure 5-2 shows the stage-measuring stations. The text was corrected.

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments: Flood Hazard Technical Memorandum

Comments	Responses
4. Section 2.2, page 6, also last paragraph page 10. The use of the PMF obtained from USBR would seem appropriate. Where are their values? The use of the data set in Table 2.1 with mostly very small drainage areas (DAs) and the application of a simple regression equation to estimate PMF peak flows for the significantly larger basins with totally different characteristics flowing into the Delta are inappropriate. The arbitrary assignment of a 1 in a million probability to the PMF is also inappropriate. Therefore, it is not appropriate to use the PMF as an upper bound on the frequency curve. The Corps does not assign or estimate probabilities associated with the PMF. One could determine where the PMF falls on an adopted frequency curve, but its frequency will vary with location.	The PMF data used in the analysis are provided in Table 2-1. The PMF estimates are for watersheds located throughout the U.S., including three in California. It is true that the PMF does not have a probability associated with it. However, the purpose of using the PMF was not to associate a probability with the PMF but to bound the flow associated with extremely rare inflow events, should that be needed for the Risk Analysis. Generating extremely rare flows from a frequency distribution can result in flows that are impossible; it is therefore necessary to bound the upper limit on flows from the probability distribution. We recognize that this approach is simplistic, but the results are not sensitive to the probability assigned to the PMF. It should be noted that the USBR PMF estimates include 61 floods located throughout the United States, including three floods located in Northern and Southern California.
5. Section 2.3, page 7. The discussion on selection of the period of record used is good. The 13 years of pre-Oroville Dam record is probably meaningless. Regardless, the process used to select the record for analysis seems reasonable.	Noted.
6. Last paragraph, page 7, and second paragraph, page 8. It is likely that the adoption of a daily time step significantly dampens the true impact of reservoir operations on peak flood flows and stages.	Several inflow hydrographs were reviewed and showed that the inflow peaks were very long (several days) and flat (little variation in total inflow). Also, the reservoirs are located far from the Delta, so interday or even daily changes in reservoir operations are unlikely to affect the flows into the Delta, as the changes will be smoothed out during the travel time to the Delta.
7. Second paragraph, page 8. The technical memorandum states that reservoir and diversion operations are not considered in the analyses. How then will base and future conditions alternative reservoir systems operation strategies, required for analyses	It is not clear that current reservoir operations have a significant impact on flood inflows into the Delta. Therefore, it is unclear that future operation of the reservoirs will have a significant impact on reducing flood damages. Also, the intention of the Flood Hazard TM

Comments	Responses
of climate change (significant snow-pack reduction and more rainfall) and Delta levee modifications, be consistently evaluated against the presented existing conditions?	was not to analyze reservoir operations. See the Water Analysis Module (WAM) TM for analysis of reservoir operations.
8. Last paragraph, page 8. If only 18 events are higher than 200,000 cfs during the entire 50-year period of record, are only 18 data points used in the frequency analysis? It appears in Section 3 that 50 points were used: Why then are we discussing only looking at flows over 200,000 cfs in this section? Needs clarification.	Fifty (50) annual peaks were used in the frequency analysis. The 200,000 cfs lower limit has been removed, and the analysis now includes all annual flows.
9. Section 3.1, page 9. There are inherent errors when using curve fitting techniques for flow frequency analysis. There should be some discussion on the estimates of this error and what it might mean in the analysis. Gordan et al. (1992) shows a 25% error if only 48 years of record are used to estimate a 100-year event.	We are not sure what the commenter means by "error," but we agree that there is a large uncertainty in estimating rare events from small data sets (such as the 100-year event with 48 years of record). Also, uncertainty exists in the choice of frequency distribution to use in estimating the frequency of a particular flood event. Section 3.3 discusses the uncertainty analysis used in the flood frequency analysis. For each flood event (e.g., 100-year event) the 5%, 20%, 50%, 80%, and 95% confidence bounds were also calculated. The Risk Analysis Report uses all the confidence bounds in its calculations instead of a single estimate for each flood event.
10. Last sentence, page 10. While the Delta inflow may not be sensitive to the PMF frequency, the integration of the damage-frequency curves for without and with project conditions analyses can be very sensitive to the frequency assignments to all flow values including those that are extreme.	It was anticipated that the Risk Analysis Report would not need to simulate results for extremely large events (e.g., > a 500-year event), as most levees would fail before such a large event. Once a large number of levees have failed, simulating larger events does not significantly increase the level of damage. We were not sure where this level would occur, but it was assumed to be much smaller than the PMF or 1 in a million event.

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments: Flood Hazard Technical Memorandum

Comments	Responses
11. Section 3.3, page 11. More discussion is needed to explain the arbitrary cutoff of the PMF at 3,000,000 cfs.	The number of flow bins was expanded to cover a wider range of probabilities. However, as listed in Table 3-5, the upper limit is still at 3,000,000 cfs. It was assumed that it is highly unlikely that a greater flow than this would ever occur in the Delta and furthermore that even if such as flow were to occur, all the levees would have failed long before 3,000,000 cfs reached the Delta. So, it was decided not to provide probabilities for larger inflows.
12. Section 3.4, page 11. Need more discussion about how the results are used for input into the Risk Analysis.	Text added to Section 3.4 to clarify.
13. Section 4. The procedures presented are interesting and logical for a basic level of analysis developed with limited resources for a very complex study.	Noted.
14. Section 4.2, page 13, Eqn 4-3. Would like to see the reference cited for this equation.	Neter, John, and William Wasserman, *Applied Linear Statistical Models* (Richard D. Irwin, Inc., 1974).
15. Last paragraph of Section 4.2, page 14. Need more discussion and backup calculations to describe why the regressions are adversely affected by the San Joaquin and Consumnes rivers.	The text was changed to better reflect why the flows were analyzed in the prescribed order.
16. Section 4.3, second paragraph, page 14. Describe the curve on Figure 4-1 that represents the fraction of inflow. How is this curve used in the calculations?	The curve for the fraction of inflow was not used in the calculations; rather, it was provided because it is an intermediate result used to calculate flow in the tributary.
17. Sections 5.1 through 5.4. Procedures and assumptions made for data adjustments are valid.	Noted.
18. Section 5.5.1, page 19. Second paragraph makes an excellent point about bias and removal of the low flow data sets from the analysis methods. Why is 57,000 cfs used as the cutoff for high flow here, whereas in Section 2, the cutoff for high flows was 200,000 cfs?	The limited stage data required lowering the minimum value of "high inflows." If 200,000 cfs were to be used, we would not have enough data points for a regression analysis at some measuring stations. We used 57,000 cfs to include the highest flows in the available data for the San Joaquin River.

Comments	Responses
19. Section 5.5.2, page 19. Equations 5-1 and 5-2 do not consider outflows to the SF Bay, pumping, diversions, or non-federal levee failures (storage) that will occur during modest flooding. Why are these factors not considered? How significant are they to estimating an accurate water surface elevation?	It is true that factors other than inflows could affect the water surface elevations at a given station in the Delta. This was noticed to be especially true for stations near the Federal and State Project pumps. Appendix A provides a comparison between measured and predicted stages, and the accuracy was considered adequate for the Risk Analysis. The stage elevation at the Golden Gate was considered to define the tail water condition. Outflows were considered a function of the inflows and stage elevation at the Golden Gate. Pumping is assumed to be steady state and not variable and certainly makes a difference. This difference is already accounted for in the historical stage elevations. We looked at levee failures and found them to be fairly frequent. We considered eliminating data from events that had levee failure. The records we have on the exact timing of levee failures are vague, and the timing of their eventual repair was not readily available. As nearly every major storm broke a levee some where, if we were to eliminate these storms there would be little data left to examine. Also, for major events the inflow data indicate that the volume of inflow is significantly greater than island storage and once the island is filled, it would have less impact on water levels in the Delta, though more inflow may be directed to the flooded island due to the added tidal prism.
20. Section 5.5.2, page 19, first paragraph. The following relationship is used: the hydraulic head is proportional to discharge to the 0.67 power. The use of this relationship as the exponents in the equations seems arbitrary. However, given the type of analysis, it is a very good	See first paragraph after Equation 5-2.

Comments	Responses
assumption. A reference should be cited.	
21. Section 5.6, page 20. Much of the validity of the procedures hinges on the results described in this section. The comparison of stages at the gages presented in Figure 5-4 and Appendix A is limited to a selected number of events. Review of comparisons shows that some predictions are very good and others are off as much as 1-2 feet. Tables for each stage gage, non-recording and recording, need to be developed to show all the annual peak events, the predicted peak mean daily stage, and the recorded peak mean daily stage. For recording stage gages, all other available shorter time period data (such as 12-hr, 6-hr, 1-hr) should be shown as well. Differences in feet between the recorded and predicted stage values should also be provided in the tables.	A table was added to Appendix A comparing the observed and predicted peak stage at each station with at least 4 years of data. The observed stages are from hourly data. Results are not shown for different time periods, as information on different time periods is not needed for the Risk Analysis Report. The Risk Analysis only needs a single value for each station for each scenario.
22. Section 5.7, page 20. Given the high levels of flow during floods, failures of numerous non-federal levees, complex and dynamic flow patterns, potential of high tides, etc., how accurate is the interpolation? Can an assessment of the accuracy be made? An example location and calculation would be helpful. An HEC-RAS model of the North Delta was completed by UC Davis within the last couple of years. A comparison of stages developed from this Flood Hazard technical memorandum's regression model and from the HEC-RAS model at several locations within the North Delta (using same inflow values) would give more confidence in the accuracy of the regression model.	The commenter provides an idea worth pursuing. If more time were to become available, then an analysis such as the one the commenter has proposed could be explored.
23. Section 5.8, page 20. The discussion of assumptions and limitations is good, although more could be added.	Noted.

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments: Flood Hazard Technical Memorandum

Comments	Responses
24. Section 6.1, page 21. The technical memorandum on Climate Change does not go into sufficient detail about the modeling process of the four scenarios or the assumptions made to feel comfortable with the results. Significantly more background information is needed on assumptions, analytical procedures, and results. What are the 23 streams, how were the synthetic records generated, etc. Finally, as precipitation is critical to the results, and climate change models are known to have problems in analyzing it, how is precipitation addressed in the climate models?	Addressed in the Climate Change TM.
25. Section 6.1, page 21, third paragraph. The assumption of the 100-year runoff event for each of the 23 watersheds above the reservoirs being added together to produce a total 100-year inflow frequency in itself would essentially discredit the climate change analysis. The results for these very large watersheds would produce a far rarer frequency event for the Delta inflow. As presented, the use of the 7-day mean daily flows added together would negate some of this problem but not address the reservoir system operation factor. How could this be improved?	The text was misleading in how the future conditions were calculated. The text has been updated to better describe how the future conditions were estimated. The analysis determined the 1% chance of the sum of the 23 stream flows, not the 1% chance of each of the 23 stream flows summed.
26. Sections 6.2 through 6.5. These sections describing the general procedures used to depict the frequency curves for the four climate change scenarios seem logical and valid. The assumptions associated with the 23 watersheds' synthetic record generation and assumed Delta total inflow frequency still leave major questions about the validity of the climate change analyses results.	Addressed by the climate change group.

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments: Flood Hazard Technical Memorandum

Comments	Responses
27. Section 6.2.1, page 22. Briefly include a discussion of the four climate change scenarios and include a summary in the table instead of a meaningless number	An unnumbered table was added to the text to provide an explanation of the numbering system used for climate change.
28. Section 6.2.4, page 23, first sentence. This assumption is simply not valid and discredits the entire results of the global warming analyses for the four alternatives. The frequency analysis plotting positions would be much different. How to resolve this issue is paramount to the climate change part of the technical memorandum. Also, comparing the synthetically generated flow data to the observed flow data records is not credible without a detailed explanation of how the records are generated.	The method used in the analysis was not clear in the text; the text has been corrected.
29. Section 6.5, page 25, Equation 6-1. Using Manning's equation to approximate the stages due to rises in the ocean seems very simplistic, given the many factors involved and complexities of the hydrodynamics of flows in the Delta.	The method is simple but provides a measure of the how far sea level rise may extend inland during a storm event. Although simple, the method was considered adequate for the level of detail needed by the Risk Analysis Report.
30. Page 26. The technical memorandum text ends abruptly. Add a conclusion or summary section.	A summary section was added (Section 7).

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments: Flood Hazard Technical Memorandum

Comments	Responses
Reviewer: Keer, U.S. Army Corps of Engineers	
1. Analysis needs to research approaches taken by other studies; listing what the modeling needs are and explaining how what's already been done fits within the confines of the existing study. The documentation provides no indication that any research of existing studies was done and provides explanation of what was found and how that information was incorporated into the DRMS analysis or why it was excluded.	The purpose of the Flood Hazard TM was to develop inputs for the Risk Analysis Report. The inputs required were specific and needed to be stated in a probabilistic framework. Other studies were reviewed, such as the USACE Sacramento-San Joaquin Delta Special Study (Hydrology) 1992 and the sections of the Sacramento and San Joaquin River Basins Comprehensive Study (2002) and FEMA FIS. However, these studies were conducted for different purposes, and the results were not found to be relevant to the Risk Analysis.
2. Language states that "other team members will use the results of these studies to evaluate risks of potential damages in the Delta". The document needs further develop the application of this process within the text; there's no mention of how it's intended to be applied beyond the Phase I and Phase II stages of the DRMS analysis. Is it expected to support any type of alternatives analysis? Are there specific agencies that the current DRMS analytical team expects will be utilizing the information they've developed? If so, what are those agencies and how does the current Phase I and II analysis fit into the scope of their analysis? How does this analysis fit within their minimum acceptable criteria?	The purpose of the Flood Hazard analysis was to develop inputs for use in the Risk Analysis Report for Phase I of DRMS. If similar information is needed for Phase II, the Phase I information will be modified appropriately. The information developed as inputs to the Risk Analysis Report was not intended for or designed for use for other purposes or by other agencies outside of DWR.
3. Might the general conversion of all station datum to NAVD 88 be eliminating subtle differences in stages between gages – the stages at each of the gages are averaged and that average compared to another average.	To use the stage data for a Delta-wide analysis, it was necessary that all the data be on the same datum. It is possible that the errors in converting stages from one datum to another could be on the order of "subtle" differences between datums, but without converting the stages to the same datum the "subtle" differences cannot be identified.

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments: Flood Hazard Technical Memorandum

Comments	Responses
4. Only two flow seasons are being considered; high - 16 December through 15 April, and low - 16 April through 15 December. This delineation simplifies the analysis, but might provide misleading results if this is the same delineation used in the frequency analysis (i.e., separating dependent and independent data). Annual peaks represented for water years 1971, ‘76, ‘85, and 2005 are classified within the “Low Runoff Season” the rest are in the “High Runoff Season”. Frequency analysis doesn’t separate flow components based on their source. What are the implications of relying on statistics developed from mixed populations when the recommended application of the Log Pearson III distribution by the IACWD is applied independent data?	It is unclear what the commenter is asking. Each of the three frequency distributions was analyzed independently. The annual frequency distribution was based on the annual peak for each year, the high season was based on the annual peak during the high season, and the low season was based on the annual peak during the low season.
5. USBR PMF data is used to determine the upper limit of inflow into the Delta. The PMF data cited was developed for other states across the continent – applying inferences from this data specifically to the Delta is inappropriate. The estimate is based on an equation defining the trend of PMF cfs/mi2 versus watershed area mi2; based on PMF CSM values developed by the USBR for different drainage basins throughout the country. CSM values can vary widely-just in comparing basin productivity between the Sacramento Basin and the San Joaquin Basin - one can be twice that of the other. Plotted against the CSM values, the CSM value represented herein lay on the extreme lower end of the trend…possibly as an outlier. PMF discharge values are typically used for spillway design…what is their application in this analysis; economics…they’re not needed-all this study should be worried about is maybe a 1/500 stage.	The PMF analyses were used to give an approximate upper limit on the flow that could be generated from the flood frequency distribution. It is not anticipated that any flows larger than about a 500- to 1,000-year event would be needed.

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments: Flood Hazard Technical Memorandum

Comments	Responses
6. The document doesn't support it's assumption that "hydrologic risk of damages in the Delta is not expected during inflow events less than 200,000 cfs" (page 8, paragraph 3). The Middle River reach of the Jones Tract levee failure occurred 3 June; according to Figure 2-3, at a peak Delta inflow less than 150,000 cfs.	The lower limit of 200,000 cfs was removed. The analysis now includes flows from 0 to 3,000,000 cfs.
7. 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 the 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.
8. The results of the inflow patterns methodology (Sacramento River vs. Yolo Bypass) Section 4.3 assume that the fractional contribution of the Sacramento River to total delta inflow (TDI) is never less or more than between 85% and 92%. This is clear within Figure 4-2 (Flow in Sacramento River Plus Yolo Bypass versus Total Delta Inflow); is it realistic that this relationship always fit within these bounds?	It was not assumed a priori that the fractional contribution of the Sacramento River to TDI was between 85% and 92%. The only assumption was that the contribution was restricted to between 0 and 100% (due to the use of the logistic regression). The actual limits that are produced are a function of the regression coefficients and the standard error of the regression. The median estimate of the fraction may be between about 85% and 92% for flows from about 200,000 to 2,000,000, but including the variability about the median results in estimates that could vary from 60% to 99%.

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Comments	Responses
9. "Water levels, or stages, at the selected gauging stations were then used to interpolate stages at intermediate locations in the Delta" (Section 5.1, page 15). The statement assumes that there are no structures in-between the two points that would create any hydraulic inconsistencies, or levee bank elevations lower than the proposed water surface that might create an out-of-bank flow…again providing a different water surface elevation. This assumption appears to limit the ability of the DRMS analysis to be directly applied in any type of alternatives analysis.	Stages were estimated as inputs to the Risk Analysis Report. Simple methods were needed to allow for the large number of scenarios anticipated to be calculated in the Risk Analysis Report. The estimates predict the maximum river stage assuming infinitely high levees. These stages are then compared to actual levee crest elevations to evaluate the likelihood of levee failure. In Phase II the method will be modified if necessary depending on the alternatives that need to be analyzed.
10. The analysis states "That failures in the levee system for any given flow conditions are minimal and will not significantly reduce the stage elevations along the channels" (Section 5.8, page 21, 1st paragraph) This is a great assumption …how is it expected that we're to utilize this study to formulate hydraulic alternatives when this analysis itself isn't able to provide hydraulic insight. What is the elevation at which overtopping of the levees occurs?	The method was designed to provide inputs to the Risk Analysis Report. It was not designed for an alternatives study. It will be modified as necessary if alternatives are identified for the Risk Analysis that need stage data. See the Water Analysis Module (WAM) TM for a description of the detailed hydraulic analysis.
11. The assumption that "a runoff event of a given return frequency that occurs in the watershed will produce a Delta inflow of the same return frequency" implies that regulation has no effect on Delta inflows. Data development states the conclusion "…that construction of reservoirs and other developments in the watersheds tributary to the Delta [does not appear to] have a significant impact on annual peak daily Delta flood inflow characteristics…"…only applies to flood events and not non-flood flows". In conflict, with the above statement, Page 4, paragraph 4 states that "…hydrologic characteristics in the Delta during different inflow seasons were considered in the	The text was modified to read, *The future change in frequency of a watershed event with a given current return frequency will produce the same future change in frequency for the Delta inflow of the same current return frequency.* It is not clear from the data that regulation has had an impact on flood flows into the Delta. This was the basis for using 50 years of data rather than only the data since construction of the last reservoir.

Comments	Responses
studies."	
12. The last paragraph of Section 3.1 provides a justification for the DRMS flood frequency analysis by comparison with the Comprehensive Study results. This comparison is misleading unless the reader understands the assumptions of the Comprehensive Studies Rain flood Frequency Analysis. The unimpaired mainstem discharges (259,295 cfs) were, at latitude, mass routings of discharge coming off the tributaries; the HEC-5 routings of those unimpaired discharges (98,863 cfs) assumed an infinite channel (i.e., zero losses); the hydraulic routings of regulated discharge (77,300 cfs combined) through the lower basin floodplain to Vernalis made assumptions regarding upstream levee breaches. This information is quite different from any gauged data used in the DRMS analysis, which may or may not reflect over/out of bank flow – how can a comparison be made with such widely different development approaches. The last paragraph of Section 3.1 states the 1-day, 0.01 probability discharge at Verona developed by the Comprehensive Study as 60,000 cfs…it's uncertain where this value was obtained.	The comparison to the Comprehensive Study was included in response to a comment received on an earlier draft. The comparison has been removed.
13. Section 2.3 paragraph 3; how were impacts of watershed changes on Delta inflows considered?	Watershed changes are known to have occurred during the period of Delta inflow record. Analyses were made to determine if these changes resulted in any significant and identifiable changes in Delta inflows.
14. 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

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	data record is reasonable for the purpose of the Risk Analysis.
15. 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 ungaged contributions.	Disagree. 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.
16. Section 3.1 paragraph 1; is the reference to table 3-1 summarizing "annual peak" total delta inflows or the annual "daily maximum"? This ambiguity also needs to be clarified in Table 3-1.	The table summarizes the annual maximum daily average flow.
17. Section 3.1 paragraph 3; figures 3-1, -2, -3, and -4 need to display the Annual Probability of Exceedence on a probability axis; if they are, then the axis need better labeling and identification. These plots need to present for clarity the moments used in plotting these points.	The probabilities are plotted on a log axis and are labeled correctly. It is unclear what the commenter is requesting.
18. Section 3.1 paragraph 4; the first sentence states that the flood frequency analysis developed as part of the Flood Hazard Analysis has a slightly different definition than the definition typically used…how is this?	The flood frequency developed in Section 3 is for total Delta inflow, which comes from several sources. The frequency does not apply to any particular source or a discharge at a specific location. Therefore, it is possible that an estimate of the 100-year event from the frequency distribution will not result in a 100-year event on any tributary or at any location in the Delta.
19. Section 3.2 paragraph 4; there's a questionable difference between the computed and weighted skews values presented in table 3-3; maybe a typo?	Table 3-3 has been corrected.
20. Section 3.3; need further clarification on this section. It's not clear.	Some clarifications have been added to this section.
21. Section 4.2; difficult understanding the methodology; an example should be worked out.	Scheduling does not allow inclusion of an example for the Phase 1 submittal. An example will be provided during Phase 2.

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Comments	Responses
22. Section 4.3 Figure 4-2; the graphical analysis terminates at a total delta inflow of 800,000 cfs.; what about a TDI of 3,000,000 cfs? The regression oversimplifies the hydrologic contributions of the Sacramento River/Yolo Bypass; there appears to be almost 30% variability in the Sacramento River/Yolo bypass contributions to total delta inflows. There is no fit here…is the assumption that whenever the total delta inflow is 200,000 cfs that the Sacramento River is contributing 85% of that flow…always?	An inflow of 800,000 cfs covers the range of measured data. It is unclear how the regression in Figure 4-2 oversimplifies the hydrologic contributions of the Sacramento River/Yolo Bypass. It just compares the total flow in the Sacramento River/Yolo Bypass to the flow in the Yolo Bypass. The 30% variability in the Sac/Yolo contribution is what the data show. We disagree that there is no fit here. The fit is adequate for the Risk Analysis. The fit provides an estimate of the mean fraction of the flow. The uncertainty or variability analysis is used to capture the variability (e.g., the 30% variability identified by the commenter).
23. Section 4.3, page 12, paragraph 1; paragraph states that a relationship is presented in Figure 4-3, but doesn't clarify what conclusions are drawn or utilized from that relationship.	The relationship was developed as input to the Risk Analysis Report; no specific conclusions were developed.
24. Section 4.3 paragraph 2; disagree that the regression provides a visual good fit; it under predicts the main body of data because of all the low data values in that data set.	The correlation coefficients have been added to the text to better define the fit.
25. Section 4.3; references to the regressions "visually…appearing to fit the data well" are not reliable. Why are the only coefficients of correlation, provided are in Figure 4-3?	The correlation coefficients have been added to the text to better define the fit.
26. Section 5.2.1, page 16; language in the text states that the "Tide levels at the Golden Gate station are relatively independent of flows into the Delta…" at what event will Golden Gate tidal stages be dependent on Delta inflows?	For extremely large Delta outflows, there could be some effect on tides. This is an event that we have not yet seen.

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Comments	Responses
27. Section 5.3; it would be beneficial to see complete data sets highlighting adjusted data points; coded with why they were adjusted. Were there any critical data points missing from the data sets (i.e., '97 peak at Vernalis…account only for in-channel flows, there were out-of-bank contributions; daily maxima omitted because of invalid recording intervals or incomplete daily records)? How many incomplete daily records were omitted? What were the criteria for deleting records that presented constant values of stage for extended periods of time?	With additional time and effort, a list of data point adjustments and the reason for adjustment could be provided. Some data were not included, as there is not a consistent range of data available across all stations. In some cases high stages knocked out station reading equipment so that there are gaps in the data.
28. Section 5.4, paragraph 2 and Table 5-1; what water years were chosen to compute the 28-day august average stage and what were the decisions made about the number of tide cycles used in the calculation?	Generally the most recent available years were used (2005–2002). Some earlier years were also used as a check on data shifts.
29. Section 5.4, paragraph 4 and Table 5-1; table 5-1 presents the "delta stations used to develop the approximate datum adjustments for inflow…the assumption here is that there were a slew of other gages (mentioned in paragraph 1) that don't have a know datum; where are these gages and their associated data	There are over 50 stations in the Delta but most have a very limited period of record (~1 year). The records were further limited to flows greater than the minimum flow considered, thereby reducing the number of data points to less than needed for the statistical analyses. We approached the datum at each station skeptically. The datum adjustments shown on the CDEC station metadata web pages are sometimes contradictory. Also, the data shifts at BEN, BDL, ROR and others kept us guessing what the correct datum was.
30. Section 5.4, paragraph 5 and Tables 5-1 and 5-2; what relevance is the hydraulic gradient between each stations and Mallard Island (MAL)?	The hydraulic gradient was used as a reasonableness check on the datum for some stations. The gradient was not used as a method to determine the datums. The hydraulic gradient during the minimum August flows should be very, very mild.
31. Section 5.4, paragraph 6 and Table 5-2; there's no explanation why the adjustments for SSS, FPT, and MAL are not the same as those provided in Table 5-1.	The second column from the right for each table (Table 5-2 and 5-3 in the revised TM) matches. This column represents the head at each station.

Comments	Responses
	Each of the stations mentioned was adjusted from NGVD to NAVD, whereas the other stations listed were not.
32. Section 5.5.1, paragraph 2; the inflow data sets were reduced to only include high inflow events, but only TDI magnitudes > 57,000 cfs are included in the analysis? What happened to the minimum TDI of 200,000 cfs?	There were not enough data points with flow greater than 200,000 cfs for some stations, so the cutoff was reduced to increase the number of data points; 57,000 cfs also includes the highest flows on the San Joaquin, as 200,000 cfs would not.
33. Section 5.5.2, last paragraph, Table 5-3; if I'm understanding the analysis and interpreting the table correctly, values of 0.00000 are indicating that particular tributary does not have a stage?	A coefficient of 0.0000 indicates that that tributary did not contribute to the stage for that particular station.
34. Section 5.6, last paragraph and Figure 5-5; not quite certain how Figure 5-5 verifies the equations. Is the idea that stages should generally decrease towards the Mallard Island gage (MAL)? Does only three points verify this assumption? Has there been any mention or analysis of having a variable downstream boundary condition…higher peak tidal stages with higher event inflows?	Yes, generally stage decreases toward Mallard for higher flows. Three to five points are shown, as that was what was available. The downstream boundary conditions are considered to be the maximum daily tide at the Golden Gate. Alone it does not verify the equation; it only shows that the equation meets an expected behavior. The figure was removed.
35. Section 5.7; not quite certain how Figure 5-5 verifies the equations. Is the idea that stages should generally decrease towards the Mallard Island gage (MAL)? Does only three points verify this assumption? Has there been any mention or analysis of having a variable downstream boundary condition…higher peak tidal stages with higher event inflows?	See response to comment 34.
36. Section 5.7; I believe this assumes there are no structures between the gages that are being interpolated in-between.	Yes. The Delta Cross Channel and Sacramento Weir can provide inconsistent results if they are not operated consistently. Additional refinement may be possible if the operational records for events are available. Larger events probably exceed the operational range of these two structures. The Delta Cross Channel is generally closed during the wet season.

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Comments	Responses
37. Section 6.1, first paragraph; aside from incorporating the effects of climate change into the hydrology for this analysis…why did the DRMS climate change tasks group develop their own synthetic estimates of runoff? What was the point of the first four section of this report?	The first four sections of the TM were completed to provide estimates of the probabilities of inflow amounts, patterns, and tides for existing conditions. These, in turn, were used to calculate the probabilities of water surface elevations and various locations in the Delta. The probability of a set of concurrent water surface elevations throughout the Delta cannot be calculated directly from the measured water surface elevations. The synthetic estimates of runoff were only used to estimate how the flood frequency curve would change due to future climate change.
38. Section 6.1, fourth paragraph; I'm not aware that unimpaired flows are being utilized in this analysis; the influence of regulation is already reflected in the data sets and now a seven-day average is being used to further attenuate the Delta inflow…I believe this to be an underestimation of either the peak or the volume, whichever is being used (it's unclear).	Unimpaired flows were calculated for the watersheds. Changes in the frequency of these flows due to climate change were used to adjust the change in frequency in Delta inflow as determined from actual measurement of the current Delta inflow. The analysis in Section 6 is only to estimate the change in flood frequency due to climate change. The actual data used in the analysis are always based on the measured Delta inflow data.
39. Section 6.1, fifth paragraph; a seven day sum…wouldn't the annual maximum 7-day total over estimate the inflow? Was this to be a 7-day average, if so this would under estimate discharge?	The analysis in Section 6 is only to estimate the change in flood frequency due to climate change. Actual Delta inflow data were used in the analysis.
40. Section 6.2.1; the statement that "No hydrologic condition could be identified that would cause the skew coefficient to change with time" needs to be verified against the data sets that were developed within in the Climate Change TM. Skew will change as the basin response changes; as climate changes; LPIII methodology requires that the data be stationary; what were the assumptions that were put into developing the different data sets reflecting the four climate change conditions?	The climate change results did not indicate a rapid change in basin runoff characteristics, so stationarity could be assumed over a limited time span, 50 years in our analysis. Each dataset in the climate change result had a different skew. However, since results are sensitive to the skew it was decided to not change the skew between the 50-year data sets extracted from each of the four future climate change models.

Comments	Responses
41. Section 6.2.1; so the skew coefficients were developed from the 150 year records developed within the Climate Change TM…were the skews developed from the annual peaks as stated, or the annual daily maxima or 7-day average? This section need	The annual peaks of the 7-day 23-stream totals.
42. Section 6.2.2; so the skew coefficients were developed from the 150 year records developed within the Climate Change TM…and then applied to the 50-year subsets?	Yes.

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Comments	Responses
Reviewer: Scott Stonestreet, U.S. Army Corps of Engineers	
1. The following comments apply primarily to the hydraulic aspects of the assessment, not so much to the hydrologic aspects.	Noted.
2. Given that this effort was limited to using data and engineering and scientific tools readily available, the general approach of using the vast amount of historic data from the various tide and/or stream gaging location makes a lot of sense. However, while developing regression equations for existing conditions may be OK for describing existing conditions, the approach may be weak when it comes to using the equations to predict future conditions for the with-project condition, especially when the with-project condition invalidates the assumptions required to make the regression analysis meaningful. Additionally, the resolution of the gaging data is thin (e.g., only a handful of stations are available compared to 1100 miles of delta levees) and may not lend itself to producing information detailed enough to differentiate one alternative from another.	Depending on the alternatives that are analyzed in Phase II, the method developed for Phase I would likely be modified.
3. Given that this effort was limited to using data and engineering and scientific tools readily available, the general approach of using the vast amount of historic data from the various tide and/or stream gaging location makes a lot of sense. However, while developing regression equations for existing conditions may be OK for describing existing conditions, the approach may be weak when it comes to using the equations to predict future conditions for the with-project condition, especially when the with-project condition invalidates the	See the Water Analysis Module (WAM) TM for a description of the detailed modeling that was conducted. The purpose of the Flood Hazard TM was to develop inputs to the Risk Analysis Report.

Comments	Responses
assumptions required to make the regression analysis meaningful. Additionally, the resolution of the gaging data is thin (e.g., only a handful of stations are available compared to 1100 miles of delta levees) and may not lend itself to producing information detailed enough to differentiate one alternative from another.	
4. Future efforts in the Delta study will inevitably require the construct and use of a sophisticated delta-wide hydraulic model to answer the what-if's associated with flood events in the Delta (i.e.,. given the limitations of the DSM2 model with regards to flood events a model such as HEC-RAS is required). The regression analysis would not produce sufficiently detailed or high enough resolution results on which important decisions will need to be based. Thus, the documentation should discuss the need for a delta-wide modeling effort, its usefulness and limitations and show the benefits of such modeling over the regression analysis.	See the Water Analysis Module (WAM) TM for description of the detailed modeling that was conducted. The purpose of the Flood Hazard TM was to develop inputs to the Risk Analysis Report.

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Comments	Responses
Reviewer: Mike Anderson, DWR	
1. This was a challenging document to read. It lacks a clear focus and order which makes extraction of useful information difficult. As such, here is my understanding of what was conveyed in the document. The authors use the parameter Total Delta Inflow (TDI) to assess flood risk/vulnerability in the Delta. According to the authors, a maximum daily TDI of 200,000 cfs is the threshold for considering risk of flood damage in the Delta. Using this threshold, the authors use the Log Pearson III distribution to examine flood frequency of TDI. Regression equations were developed to generate river flows associated with a given TDI. The authors note that different patterns can produce the 200,000 cfs threshold which impacts the regression equations.	The threshold on considering risk (200,000 cfs) was removed from the analysis. The rest of the paragraph is accurate.
2. A second analysis was performed looking at water surface elevations. Regression equations were developed to predict stages at gaging sites given the maximum daily tide and mean daily inflow. Once stages at the gaging sites were determined, channel water surface elevations were spatially interpolated. This information was used to determine a Delta water surface elevation corresponding to a 100-year event.	The method described in the paragraph is correct. The expected 100-year event water surface elevation was not calculated; nor was any other return period event for water surface elevation calculated. A relationship was developed that could be used in the Risk Analysis Report to calculate stage for any given inflow condition.
3. The chapter then changes gears and discusses levee failures. Historical events are analyzed to determine if the frequency of levee failures has increased. Levee failure modes during floods were then reviewed. Analyses with a computer model were then carried out to determine potential levee failure sites and conditions. The results of these simulations were used to extrapolate the probability of island levee	Noted. See revised Section 7 of Risk Analysis Report.

Comments	Responses
failure.	
4. The document would benefit from an introduction that states the goal or message of the document followed by a clear description of what material will be presented to achieve this goal or message. I am not sure the discussion of dam construction is worth including in the document. It is poorly written and it is not clear if the analysis is sound.	Noted. See revised Summary Report and revised Section 7 of Risk Analysis Report.

Comments on Climate Change Technical Memorandum and Responses of Consulting Team

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Comments	Responses
Reviewer: Goettel, Army Corps	
1. Section 3.1: Consequences of sea level rise for the Delta should be discussed more prominently.	Impacts specifically relevant to the Delta are now summarized at the end of the discussion of general impacts of sea level rise (i.e., at the end of Section 3.1.1).
2. Section 3.1: Replace Figure 4 with a more up to date version.	A more recent version does not exist; no action taken.
3. Section 3.1: This statement is self-contradictory: "The state of the science does not allow quantitative estimates of the probabilities of these different projections. Even subjective, semi-quantitative probabilities cannot be reliably assigned. Although values lower than the lowest projections seem very unlikely, it seems possible to exceed the highest projections, given the rapidly-evolving state of the science."	I have deleted "Even subjective, semi-quantitative probabilities cannot be reliably assigned."
4. Section 3.1: Suggests that we consider additional, higher values for sea level rise.	The highest value for seal-level rise that I have suggested considering (140 cm by 2100) already greatly exceeds the maximum value in the IPCC FAR (43 +/- 17 cm). Thus, while I agree that we should consider scenarios that go beyond the IPCC estimates, we are already doing this, and by a large margin.
5. Section 3.1 "As noted by Rahmstorf, 2006 and IPCC FAR, most of the models used to predict future sea level rise under predict the historical sea level rise and thus likely under predict the future rise as well."	This is already noted (p. 8, fourth paragraph). No additional action taken.
6. Section 3.1: "To make rational policy decisions, I believe that the climate change team has a responsibility to make an explicit professional opinion of the most likely range of sea level rise, while, of course, correctly acknowledging the uncertainty".	The broader scientific community is unwilling at this time to make such projections. We have no superior knowledge that would allow us to make better projections than others are capable of. No action taken in response to this comment.

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Comments	Responses
7. Section 3.2: Suggests adding illustration of up-to-date projections of changes in monthly-mean river flows. Also suggests that Figure 9 (showing simulated daily mean flows) "doesn't show much meaningful information."	Replaced Figure 9 with illustration of trends in monthly mean flows.
8. Section 3.3: Highlight finding that in-Delta wind speeds will likely increase as climate change proceeds	Section 3.3 has been modified to give this finding more prominence. In addition, I added a figure (Figure 12) illustrating the small predicted response of Delta wind speeds and directions to increased greenhouse gases.
9. Section 3.4: suggests adding discussion of consequences of changes in temperature and precipitation for river flows, etc.	These hydrological consequences are not directly relevant here, since the projections presented are to be used to estimate future water demand. Nonetheless, a brief discussion along the lines suggested has been added.

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Comments	Responses
Reviewer: Burnham, Army Corps	
1. TM "fails to provide the reader with sufficient information to understand the basic assumptions, analysis procedures applied or adopted, and the results."	Substantial methodological discussion has been added (e.g., in Section 3.2 on river flows). The TM now stands on its own, in the sense that material from the ITF that was referred to in previous versions of the TM has been incorporated into the TM.
2. TM "doesn't present a comprehensive and coherent document."	This is a result of the fundamental philosophy of using pre-existing ("off the shelf") results; this means that projections of different climate quantities in some cases are based on differing models and assumptions. This in turn is a result of schedule and budget constraints. I have added a discussion of these issues to Section 2 (Technical Approach).
3. "Section 1.1, paragraph 1, page 1. First few sentences are inconsistent, stating temperatures changing too rapidly to be explained by natural internal (what is internal?) climate variability alone to …the in principle be of natural origin…"	There is actually no inconsistency. Work cited in the TM shows that warming in CA has been too rapid to be caused by sources of variability internal to the climate system – meaning the natural, unforced oscillations of the nonlinear system. Natural external forcings (specifically solar variations and volcanic eruptions) in principle could be the cause of recent warming, but this is highly unlikely. Evidently this discussion as originally presented in the TM was not clear to non-expert readers; it has been expanded and, I hope, made clearer.
4. "Section 1.1, paragraph 2, page 1. See General Comment 2 above: "The document presents global warming scenarios and not a comprehensive depiction of climate change involving both potential warming and cooling period impacts."	This comment is in response to a paragraph that lists expected climate trends in California. I have added a comment to the effect that these trends will be superposed upon variability on all time-scales, resulting in relatively cool periods. I also mention that the characteristics of climate variability may change in the future as a result of increased greenhouse gases.

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Comments	Responses
5. Section 1.1 paragraph 3, page 1. Presents a list of climate chance conditions California will experience including: reductions in snowfall: uncertain changes in monthly, seasonal, and annual precipitation; etc. Yet it also states a likely increase in daily precipitation values, which seems inconsistent with rest of list. The statements do not necessarily project more frequent and greater floods.	An increase in daily precipitation values is a robust prediction, and stems from a sound fundamental result: the increase moisture-holding capacity of warmer air. This is now stated in this section of the TM. Increased flood potential is a likely result of an increase in extreme daily precipitation events, together with the general increase in winter-season river flows.
6. Section 1.2 paragraph 4, page 1. Again, states climate chance will affect California's levees through…altered river flows on daily and seasonal timescales….Inconsistent with Comment 3 above that says seasonal impacts are uncertain. Also the report fails to define or how to analyze and evaluate the interaction impact of these assumed phenomena on levees.	Seasonal impacts on precipitation are uncertain; however, altered river flows result primarily from warming, which is not uncertain (at least qualitatively). The TM now states: "Although projected changes in precipitation are highly uncertain, the impacts mentioned above are to a large degree independent of small changes in seasonal-mean precipitation."
7. Figure 1, page 2. The figure needs to be better depicted. It can not be read or properly interpreted. Also, the notes associated with the figure needs to be significantly expanded in the text to provide the reader with an understanding of the models, assumptions, analysis procedures, results, calibration methodologies applied to develop the information show in the figure.	I have simplified the figure by eliminating one panel, and I made it more readable by enlarging the remaining two panels. The caption has been expanded and clarified. Documentation of the "models, assumptions, analysis procedures, results, calibration methodologies" is beyond the scope of the figure caption and indeed of the TM; however, a reference to such documentation has been added.

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Comments	Responses
8. Notes, last two sentences, page 2. These statements indicate that the methods adopted are not adequate to analyze snow pack accumulation, and rapid snowmelt from series of warm front (often term the pineapple express) rainfall on the snow pack that produce the largest flood events. The offending sentences are: "For precipitation, the sign of future changes is unknown. The lower right panel shows that no clear relationship is present among models between projected changes in temperature and projected changes in precipitation."	It is acknowledged (e.g., Section 2, 2nd paragraph; Section 3.4.2, last paragraph; Section 4, second paragraph) that adequate quantitative projections of future flood risk do not exist. I have added one more repetition of this *caveat*, in Section 2, 2nd paragraph.
9. Figure 2 caption. See comment 5 above. Also, is the area more arid? Does the lack of snow pack reduce flooding? The notes say the results shown are from one model and that other models would give qualitatively similar but quantitatively different results. What are the different assumptions, procedures, calibration results, and analysis results of the different models? How much do the results vary between models? Which results do you use or assume and why?	More arid than what? As noted in the TM, lack of snow may tend to reduce late-season flooding. However, increased monthly-mean flow rates and more intense daily precipitation events will tend to increased risk in wintertime. As noted above, climate model documentation is beyond the scope of this TM; I have added a reference to documentation on the model used to produce the results shown in Figure 2. Intermodel differences in projected future temperature and precipitation are shown in Figure 1; simulations of snow-water equivalent (as in Figure 2) are performed using a separate surface hydrology model driven by meteorological results from a climate model; these simulations have not been performed based on multiple climate models. Hence it is not specifically known how much results for water-equivalent snow depth would vary between climate models. It is safe to assume, however, that snow loss will increase with predicted wintertime temperature increase. We show results based on the PCM model in order to illustrate a typical possible outcome.

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Comments	Responses
10. Section 2, paragraph 1, last sentence. How accurate and useful are climate model projections of sea level rises on an hourly basis?	Hourly variations in coastal sea level depend on tidal forces and weather variations. Inland variations depend additionally on influences of friction and estuary bathymetry. Our ability to model the latter factor is evaluated in Figure 8. Weather variations in climate simulations should be correct statistically, but the timing of specific events will not be correct. This is noted in the revised TM (Section 3.1.5, 1st paragraph).
Comments below are on the ITF, which was referred to in the original TM.	
11. The material presented in the ITF should be incorporated into the main document.	Relevant technical material has been inserted into the TM. Material on effort levels and schedules has not.
12. Paragraph 2, page 1. Makes good points.	These points are made in the TM.
13. Paragraph 4, last sentence, page 1. …need to account for correlations in order to accurately project future flood risk and levee vulnerability. Excellent point.	This point is already made in the TM.
14. Paragraph 5, page 1. The two largest flood events in the Sacramento River basin are from increase warm rainfall and corresponding rapid snowmelt. How is this phenomenon affected? Also, the last sentence states the prediction is robust even though the models do not agree on the magnitude or sign of predicted changes in precipitation. If this is the case, which model results are used and why?	The effects of climate change on "rain on snow" events are unknown, and probably beyond the state of the science to simulate accurately.

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments: Climate Change

Comments	Responses
15. Paragraph 6, third sentence, page 1. ….river flow timing have not yet exceeded those possible from natural climate variability. This seems to contradict first sentence of main document and provides additional inconsistencies as stated in Specific Comment 1 above.	There is no inconsistency. The first paragraph of the main document states that "Recent *temperature* trends in California have been shown to be changing too rapidly to be explained by natural internal climate variability alone." The ITF states that trends *in river flow timing* are consistent with either natural variability or human influences. Since different quantities are involved, there is no contradiction. It may nonetheless seem surprising that trends in temperature are outside the bounds of natural variability, while trends in river flow timing, which are caused by trends in temperature, are not. The reason is that river flow timing, like other hydrological quantities, is subject to very large year to year variations. This is now noted in the TM, Section 1.1
16. Figure 2, page 3. How are the present flow values in cubic meters/second depicted in the figure for four different streams so consistent? How do they compare with observed recorded values?	Simulated flows for the present climate are consistent across multiple climate models, and agree well with observed flows, because meteorological data is subject to a bias correction before being supplied to the surface hydrology model that calculated flows. This is now noted in the TM, note to Figure 10.
17. Second paragraph, last sentence, page 4. Should 2200 be 2100? Same with first sentence page 5. If not please explain. " It is not clear how much effort will be needed to adapt the results of Cayan et al. for use by the DRMS project. At a minimum, we will need to make projections for the 2200 time frame."	This is correct as it stands. The comment refers to the fact that Cayan's projections go only as far as 2100. This, results for 2200 will have to be obtained from some other source.

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments: Climate Change

Comments	Responses
18. Second paragraph, last sentence, page 5. Good point.	I am not sure what point the reviewer is referring to here. The last sentence of the 2nd paragraph on p. 5 (" It is driven by meteorological input (precipitation, near-surface temperatures, and downwelling solar radiation), obtained in this case from simulations of the 21st century performed with global climate models (GCMs), and using scenarios for future greenhouse gas emissions.") is simply a methodological description.
19. Third paragraph, page 5. CALSIMII is basically a monthly flow water allocation model. How are these flows used to flood models and analyses?	The ITF mentions CALSIM in the context that we have obtained river flows needed as inputs to CALSIM. We have not actually used CALSIM, for any purpose. As the reviewer notes, it is a monthly-timescale model and is not suited for analyzing flood risk.
20. Third paragraph, last sentence, page 5. What assumptions and procedures, etc. are used for each of the 22 models? Also, stated in various places as 20, 22, 23 streams. How are reservoir system operations going to be performed?	This comment was addressed in responding to Comment 5, above.
21. A daily time step is not adequate for peak flood flow analysis. The Corps models use one-hour time steps to properly generate peak flood flows.	Yes, this is a limitation. This is now pointed out in the revised TM, introduction to Section 2. In general, issues involving flood risk are documented in the Flood Hazard TM.

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments: Climate Change

Comments	Responses
22. Last paragraph, page 5. The assumptions described in this paragraph are not creditable resulting in little confidence in the results. Please comment as to why you believe they are valid and will provide adequate results for subsequent Flood analyses. (" The models used in the work described here are state-of-the-art and are thoroughly documented. Another asset is that the use meteorology from multiple climate models gives an indication of climate uncertainty, i.e., uncertainty resulting from imperfect understanding of the climate response to increased greenhouse gases and other perturbing factors. Thus, the work described above gets us most of the way to what we need; its principal limitation is that the end-product is unimpaired river flows, whereas what is needed for this project is after-reservoir flow rates.")	As noted above, issues involving flood risk are documented in the Flood Hazard TM.
Comments below again refer to the TM, not the ITF.	
23. Last paragraph, page 4. Concur. Good statement. (" … models and assumptions used to produce projections of one climate quantity may be somewhat inconsistent with those used to project other climate quantities.")	This is an inherent limitation in the DRMD approach, necessitated by schedule and budget constraints.
24. Figure 3, page 5. Not sure a one-foot sea level rise will result in a constant one foot rise in the Antioch frequency curve throughout its entire range. Please comment as to why it does, considering inflows, outflows, possible levee failures etc.	The caption is simply stating that if the mean (long-term time average) sea level rises, then after short term excursions are added the net sea level will also rise by the same amount. I have re-written the caption to make this clearer.

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments: Climate Change

Comments	Responses
25. First paragraph (partial), page 6. Concur with the statement made, but it is inconsistent with other statements made about global warming throughout the text. Referring to a recent measured increase in the rate of sea level rise, the IFT says: "it is not clear to what extent this reflects a response to anthropogenic forcing, as opposed to decadal-timescale climate variability."	There is no inconsistency. Apparently it was not clear that this statement refers to an apparent acceleration of sea level rise seen in recent measurements. Whether or not this recent acceleration is anthropogenic, recent temperature increases are, at least primarily.
26. First paragraph (full), last sentence, page 6. Interesting point about the Krakatoa volcanic eruption and its impact long-term on climate change.	Yes, and possibly important, too.
27. Third paragraph (full), page 6. Good description and in paragraph on page 7.	No response needed.
28. Figure 4, page 4. The results depicted in the Figure needs to be explained in detail beyond what is presented in the notes. The legend on the figure needs to be changed to be readily understandable to the reader.	The figure caption was taken from the IPCC TAR; even after significant editing it was still pretty difficult to understand. It should be much more clear now.
29. Table 1, page 11. This table needs to be redone so that it is readily understandable to the reader. How the table is developed, including assumptions, procedures, calibration methods, and results need to be described in the text.	I have expanded the caption considerably to provide a more detailed and more clear explanation of the table.
30. Figure 5, page 12. Why is 20 centimeters used instead of another value?	Because it roughly matches the historical observed trend.
31. Section 3.2. This section needs to be expanded to include sufficient referenced material so the reader can understand the assumptions, analysis procedures, calibration (to observed data) methods and results, and overall results.	This section has been greatly expanded, by incorporating material previously in the ITF.

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments: Climate Change

Comments	Responses
32. Paragraph 2, last sentence, page 14. If other models might produce significantly different results why should one have faith in the results generated and why are they adopted instead of other methods and model results? How are the models calibrated (show results)? Why are daily flow values adequate for flood analysis?	The statement has to do with methods for estimating daily timescale results from monthly-mean meteorological quantities, as part of the process of simulating daily timescale river flows. The reviewer asks why we selected the method used by Maurer. In fact, we know of only one comprehensive set of daily-timescale river flow simulations, so no choice was actually made. As noted above, a detailed description of the river flow simulations has been added to the TM. Also as noted above, daily flow values are not optimum for flood analysis, but they are the best we have.
33. Section 3.3, page 15. The general description seems appropriate although not complete as stated.	This section has been considerably expanded, including the addition of a description of the response of simulated winds to climate change (it is very small).
34. Figure 7, page 16. The figure is difficult to read and to interpret its meaning. A better text explanation is needed.	I have expanded the figure caption to make it clearer.
35. Figure 8, page 18. Good figures. Needs more discussion in the text.	The figure caption has been expanded; the results are already discussed in the text.
36. First paragraph, page 18. Explain this independency in regards to large snow pack and rapid melt, warm and cold series of fronts (pineapple express) as typical in Northern California flooding.	As stated in Section 4, paragraph 4, simulating river flows and sea levels independently will tend to result in underestimates of flood risk.
37. Section 4, first paragraph, page 18. Important statement of limitations of approach that needs to be stated in first paragraph of the document.	This limitation is now stated in the introduction to the methods section (Section 2).

Delta Risk Management Strategy (DRMS) Phase 1
Response to Comments: Climate Change

Comments	Responses
Reviewer: Schlunegger, U.S. Army Corps of Engineers	
1. This is a good collection of efforts throughout academia of what is being done on Global Climate change and its impacts here in the delta.	Noted.
2. Have El Nino effects been studied for increased rainfall and its impacts on storage capacity? Would this be a reason to look at surface or ground storage?	Good question. Recent published results (Maurer et al. 2006) show that a strong El Nino in a warmer climate would produce an extreme version of streamflow changes expected from greenhouse warming: higher winter flows and reduced summer flows. In addition, uncertainty in annual-mean flows during El Nino were shown to be higher than in today's climate. Thus stronger-than-expected El Nino events in a warmer climate would exacerbate water-supply and flood risk problems. This is now discussed in the TM, Section 1.1.
3. Has there been a model set up to characterize salt water intrusion? I don't know if you could use a contaminate transport model or not.	This is outside of the scope of the Climate Change topical area.
4. There didn't seem to be a good distinction between global and local sea level rise. I imagine local sea level rise could differ by almost a foot, which would make a difference.	This question raises an interesting and subtle issue: namely, long-term changes in atmosphere and ocean circulation patterns consequent to climate change could result in changes in long-term mean regional sea level that differ from changes in long-term mean global sea level. (This is distinct from any effect of climate change on short timescale variations in sea level.) We know of no estimates of the potential magnitude or sign of this effect. This issue is now discussed in the TM, Section 3.1.3.
5. I would recommend reporting the information in the same unit system. Some of the historical research is in customary (English or US), and some is in metric.	Except for Figure 3, which we have no ability to alter, all sea level values appear to be in metric units. No changes made.

Comments on Levee Vulnerability Technical Memorandum and Responses of Consulting Team*

(*See also Attachment 2 of Levee Vulnerability Technical Memorandum for comments of Levee Seismic Vulnerability Review Panel and responses of consulting team.)

The consulting team did not receive any comments specifically on the Levee Vulnerability Technical Memorandum. However, many of the comments on the Risk Analysis Report (e.g., those of Ramsbotham) concern levee vulnerability issues. Please see those comments as well as those of the Levee Seismic Vulnerability Review Panel in Attachment 2 of the Levee Vulnerability Technical Memorandum.

Comments on Emergency Response and Repair Technical Memorandum and Responses of Consulting Team

Delta Risk Management Strategy (DRMS) Phase 1
Response To Comments: Emergency Response and Repair

Comments	Responses
Reviewer: Bergmooser	
1. The analysis and assumptions for this topical area are reasonable though an effort needs to be made to more clearly explain that the section is focused on post event actions (as discussed in para 2, pg 1) resulting in levee failure. A majority of those involved in emergency response/repair are focused on flood fight actions (not addressed by this strategy); as such clarity will ensure they focus their analysis.	The following text has been added to Section 2, page 1: "The ER&R model focuses on post-event actions only. Seasonal flood-fighting activities are not explicitly modeled, though the model allows for a reduction in emergency response capacity during the time of the year when one would expect non-event-related flood-fighting actions on non-flooded islands to be ongoing. As such, these activities would detract resources from the emergency operations." Further modifications made to Section 4.4 to stress that the response and repair actions in the model only represent event-related activities.
2. Page 7/Para 4.4: Repair type RT2: As further discussed in para 4.9 (Repair Rates), structural protection of an interior levee can, in most foreseen situations, be accomplished only by truck; provisions may well need to be included to strengthen/construct access roads to allow transit. Further, as demonstrated during the Jones Tract failure, interior protection may include import of borrow material to raise the levee elevation in addition to slope protection either by temporary measures or placement of rock.	Added to Section 4.4: During the dry season, erosion protection and/or repair of damage resulting from erosion (repair type RT2) can be carried out from land, with rock produced at local quarries transported and dumped onto the interior slope by truck. This repair action is limited to those levees that have road access. While the model differentiates between those islands that have land access and those that do not, it does not differentiate between roads that are in good condition and those that require strengthening to allow the land-based work to proceed. Resource and time requirements to strengthen or construct access roads to allow the truck transit associated with land-based repair are not included in the material, cost and time estimates produced by the ER&R model. Furthermore, the model does not include any temporary or permanent raising of the levee elevation in the case of erosion protection or repair.

Delta Risk Management Strategy (DRMS) Phase 1
Response To Comments: Emergency Response and Repair

Comments	Responses
3. Page 12/Para 4.9: Based on experience and industry input, from either a cost or productivity basis, material for a post-event repair would not be limited to marine delivery/placement. The exception to that statement may be repair types RT5 (breach closure) and RT4 (cap breach opening) though even there a cost analysis of options would be warranted w/the limiting factor being site access.	From an emergency standpoint, marine delivery and placement is more efficient than land-based. Even if rock were sourced from quarries with no marine access, it is likely that a facility to allow the transfer of material from truck to barge would be established to allow marine placement.
4. I am unable to assess/comment on para 5 due to a general lack of background information on the model. I would welcome an opportunity to discuss the model assumptions and results and perhaps clarify the Districts' approach to emergency response/repair, in particular as applies to breach closure.	We are willing to meet and discuss this subject.

Comments on Impact to Ecosystem Technical Memorandum and Responses of Consulting Team

Delta Risk Management Strategy (DRMS) Phase 1
Response To Comments – Impact to Ecosystem TM

Comments	Responses
Wim Kimmerer, Ph.D., Romberg Tiburon Center for Environmental Studies	
The diagram for vegetation shows "salinity exposure of marshes" as an end result of "salinity regime" which is somehow distinct from "saline water inundation." The big risk for plants is presumably mortality due to inundation by salty water (or for interior levees just by water). But then what happens?	The diagram has been modified such that arrows from the box are as follows: "change in salinity regime in delta channels" -> "change in salinity exposure of tidal marshes" -> "change in area of marsh vegetation types"
6.1.3.2 "the scenario of Suisun marsh flooding..." What scenario is this? Section 6.0 describes 3 scenarios, none including Suisun Marsh.	An analysis of a hypothesized levee breach in Suisun Marsh was conducted for terrestrial vegetation and wildlife to address specific concerns of Suisun Marsh management. This analysis was included in the Ecosystem Consequences Technical Memorandum, but was in addition to the larger risk analysis project.
6.2.1 "For the actual risk assessment modeling, the islands will be made..." I thought this *was* the actual risk assessment modeling! What is different? If the modeling in the RA document is different from this, how else does it differ?	Modeling used in the Risk Assessment Analysis differs substantially than the modeling presented in the TM, in order to streamline and simplify the model. The methodology for calculating risk to ecosystems in the Risk Assessment model and differences with the model presented in the Ecosystem Consequences Technical Memo is described in Section 12 of the Risk Assessment Report.
Koenigs,	
1. <u>Section 3.2, page 3-4.</u> The section title includes "Wildlife Communities", yet the section contains only one brief mention of wildlife.	Corrected.
4. <u>Section 3.2, 2nd para., 1st sentence</u>. Change "emergent and wetlands" to "emergent wetlands."	Corrected.

Comments	Responses
5. Section 3.2, page 3-5. Text says that vegetation was classified into 13 vegetation types, yet 14 types are listed below. These vegetation types seem most appropriate for the Suisun Marsh and, perhaps, at the furthest downstream edge of the Delta, where salinities are higher. In most of the Delta today, the tidal wetlands are freshwater wetlands, which, for the vegetation types proposed, would be included in the riparian zone type, "herbaceous wetland, perennially inundated." The zone above the tidal wetlands is riparian scrub-shrub or riparian forest, corresponding to the riparian zone's "shrub wetland" and "tree wetland," respectively. Uplands generally occur above the riparian vegetation zone or in the interior of the Delta islands. Seasonal wetlands would probably be managed wetlands on the interior of the islands.	Number of vegetation types (14) is corrected. Vegetation types were evaluated objectively to describe the habitats within the Delta and Suisun Marsh as a whole (see methodology). Specifically, detailed vegetation surveys of dominant plant species were converted to vegetation types. These vegetation types were evaluated by three senior vegetation ecologists. While there is a more even distribution of area of each vegetation type in Suisun Marsh, remnant populations of all vegetation types also occur in the Delta. Our analysis shows that the majority of area is the Delta is represented by few vegetation types.
6. Section 3.2, Figure 3-2. What is the source of this figure? As described above it does not fit most of the Delta.	While this is true that the figure doesn't represent land area in the majority of the Delta, it does provide a demonstration of the distribution of the distribution of plant communities in Suisun Marsh of which there are remnant populations in the Delta. To clarify, the title of the figure was changed to "Idealized distribution of vegetation types relative to abiotic factors such as elevation and water." See previous comment.
7. Section 3.2.1, page 3-7. . 1st para. 2nd sentence: Seeds for freshwater emergent wetland species are typically rapidly dispersed. 3rd and 4th sentences: Suggested change: "The channel sides of Delta levees are	Changes were made, with slight change to make vegetation type names consistent with rest of the document.

Comments	Responses
generally steep and heavily reinforced with riprap (rock) in many areas. They may support no vegetation (Figure 3-4), ruderal upland vegetation (Figure 3-5), or riparian forest and scrub-shrub with fringing tidal wetlands in narrow bands at the exterior (channel side) base of levees (Figure 3-6). The majority of sensitive species in the Delta are found in the fringing tidal wetlands, the interstitial islands within the channels, and in the riparian vegetation along the channels." I believe that Figure 3-7 is actually Suisun Marsh.	
8. Figures 3-4, 3-7 and 3-8. Get rid of description at top of figure and just use the one at bottom.	Stylistic change was made.
14. Section 4.2.1, page 4-7. Again, there are 14, not 13, vegetation types listed, and Figure 3-2, which is referenced here, does not fit for the Delta. I suspect that the aquatic vegetation type was not counted.	Number of vegetation types was changed to 14. See response to reviewer's comment # 5.
15. Section 4.2.2, page 4-8. How were the focal species selected? They are often not even mentioned in the description of the vegetation type.	Selection of focal species is described in the subsequent section. The section has been moved as per reviewer's comment #19 for clarity.
16. Section 4.2.2.2, page 4-8. Alkali Marsh Vegetation is a group of vegetation types and should not have its own heading.	Heading and text have been altered to clarify.
17. Section 4.2.2, page 4-10, 1st para. This paragraph describes the Herbaceous Upland Native type. The heading is missing.	Corrected.
18. Section 4.2.2.7, page 4-10. You should mention here that this	Done.

Comments	Responses
vegetation type includes the tidal freshwater wetlands that are found in the Delta.	
19. Section 4.2.3, page 4-12. This section should precede the section that includes the vegetation focal species. The last bullet includes two statements that are redundant. Also, there are at least two species, *Salicornia virginica* and *Scirpus americanus*, that occur in more than one vegetation type. If the categories that the criterium is referring to are marsh, upland, and riparian, then only *Scirpus americanus* is found in more than one, the marsh and the riparian.	The section was moved as suggested. Redundancy was removed.
21. Section 5.3.1.1, Page 5-36. 2nd para.: Suggested change: "Destruction of the levee would destroy habitat on both sides of levees in the Delta, including habitat for sensitive species. Habitat on interstitial islands may be indirectly affected by levee breaches through short duration changes in water levels and increases in erosion rates due to increased channel velocities as water is sucked into breached islands." Continuing to page 5-37: Not all breach scenarios would result in islands being flooded with high salinity waters.	Text has been altered to read" Destruction of the levee would destroy habitat on both sides of levees in the Delta, including habitat for sensitive species located on the channel-side of levees. Habitat on interstitial islands, which includes habitat for many sensitive species, may be indirectly affected by levee breaches through short duration changes in water levels and increases in erosion rates due to increased channel velocities as water is sucked into breached islands." On p. 5-37, the text has been altered to reflect the comment: "Vegetation in the interior of Delta levees would be inundated by floodwaters with varying salinity levels, including high salinity floodwaters to which many of the vegetation types in the interior of the levee are not adapted."
22. Section 5.3.1.2, page 5-39. This section might be more clearly titled, "Effects on Vegetation Due to Flooding." Effects would be variable based on the tolerance of species to	The title has been altered to read "Immediate Impacts of Flooding on Vegetation" to reflect the distinction made in the analysis between immediate and long-term impacts.

Comments	Responses
inundation and salinity, the depth and duration of flooding, the salinity of flood waters, etc. I find it awkward to refer to "adults" when discussing plants. The use of "mature vegetation" is better.	The comment is unclear: the heart of the analysis is that the effects of flooding depend on tolerance of species as the reviewer describes as well as other parameters, and is extensively described in Appendix D. The document has been altered to use the term "mature vegetation".
23. A separate discussion of salinity effects would be helpful. Levee failures can result in increased salinities in Delta channels. The effects of increased salinity on non-tolerant vegetation along the channels could be a very important effect.	Salinity was not described separately due to 1) the non-linear impacts on plants of flood waters and salinity level of flood water 2) the paucity of comparable data on salinity tolerance of focal species to a range of salinities and inundation depths and durations. It is true that the effects of increased salinity on non-tolerant vegetation along the channels could be very important. However, due to the reasons given above and the limitations of the analysis, it was not included.
24. Section 5.3.1.3, page 5-41. You define mature plants as those that are reproducing, which I assume means producing viable seeds. Does it really take 30 years for *Fraxinus* to produce viable seeds?	Data on time until maturity for plant species were obtained directly from the USDA PLANTS database, and were also reviewed by a senior botanist with extensive expertise in fauna in the Delta.
25. Section 6.1.3, page 6-23. This section falls under the three breach scenario, which does not include any Suisun Marsh breaches, yet there is discussion about Suisun Marsh levee breaches.	Analysis of a hypothesized levee breach in Suisun Marsh was conducted to address ecosystems in Suisun Marsh to address specific concerns of Suisun Marsh management. This analysis was in addition to the larger project
Patricia Quickert, DWR	
B.2. Chapter 5 There is discussion about the effects of flooding on vernal pools, but there are very few vernal pools in the Delta, so this effect is relatively unimportant. (See comment C.15.	Vernal pool habitat is not found in areas flooded to due Delta levee breach, however, they are inundated in future conditions scenarios (see Ch. 14) of sea level rise.

Comments	Responses
	The following text has been added (bold) for clarification "Several unique habitats supporting unique biodiversity exist in the Delta and Suisun Marsh including vernal pool habitat **(which is impacted in future conditions but not levee breach scenarios)** …" The objective is to describe the risk; not assign subjective assessments of the importance of a habitat. In particular, the risk assessment methodology did not entail excluding the discussion of risk to a habitat based on the area the habitat occupies in the Delta or Suisun Marsh. Vernal pools are arguably quite important for the following reasons: Vernal pools in California are known to support many species which are listed as endangered species under the Federal Endangered Species Act, and the California Endangered Species Act, including plants (e.g., grasses, flowers), branchiopods, and the tiger salamander. Vernal pools in the Delta also support plant species endemic to the Delta (e.g., Delta button celery). Migratory birds also use vernal pools as stopovers for forage during migrations. Vernal pools also perform a critical ecosystem service of recharging drawn-down aquifers. At least three areas of vernal pools are inundated by mean higher high water for predicted sea levels in year 2100 (140 cm higher than mean sea level), according to the Holland Vernal Pool Dataset (see attached map). These areas occur near Cache Slough along Cache Creek, in Egbert Track and in Cache Haas.

Comments	Responses
	The inundation of even a small area of vernal pool habitat may have a very large impact on the survival of species endemic to the Delta which are supported by vernal pool habitat in the Delta. The current extent of vernal pool habitat is less than 10 % of its original extent, and therefore, further loss of habitat may have a large increase in the probability of species extinction.
Chapter 12 Depth and duration of flooding should be added to the impact analyses for plants and terrestrial wildlife. There is poor information on seed banks and plant species adaptations to salinity.	The impacts of depth and duration of flooding are presented in the Ecosystem Consequences Technical Memo, however, in order to simplify and streamline the model, they were not included in the Risk Assessment. The comment is unclear. Adaptations found in plants to high levels of salinity are described in the 'salinity' section. Seed bank tolerance to salinity is described in the section entitled 'Seedbanks'. There is little data for focal species examined on plant or seed tolerance of salinity. Due to great differences among even closely related species in tolerance to abiotic factors, throughout the analysis we restricted data use to only that published on focal species.
4. **Chapter 13** Although nearly all the listed terrestrial species discussed in this document are from Suisun Marsh, none of the breach scenarios included breaches within Suisun Marsh, which means there would be very little direct impact to species in the marsh. Therefore, there should be a Suisun Marsh breach scenario, because	An analysis of a hypothesized levee breach in Suisun Marsh was conducted for terrestrial vegetation and wildlife to address specific concerns of Suisun Marsh management. This analysis was included in the Ecosystem Consequences Technical

Comments	Responses
there is a need to assess impacts to these listed species within the marsh.	Memorandum, but was in addition to the larger risk analysis project.
5. **Chapter 15** Information about an endangered plant, *Cordylanthus mollis mollis* is erroneous. References—surprisingly few. Also the text lacks citations for information taken from other sources	The occurrence of *Cordylanthus mollis mollis* is correct according to the following analysis: A search was conducted of the CNDDB December 2006 database for the 12 counties including and surrounding the Delta; Alameda, Contra Costa, Marin, Napa, Sacramento, San Francisco, San Joaquin, San Mateo, Santa Clara, Solano, Sonoma, and Yolo Counties. Within this area, there was one occurrence of *Cordylanthus mollis mollis*. The occurrence was on the channel side of a Delta levee. The text has been altered to clarify this. The status has been changed to Endangered. All citations are included in the Ecosystem Consequences Technical Memo.
8. **Page 2-6**, 3rd bullet: This bullet is misleading; it is stated that Delta elevations can be increased by changing crop types, but that is not necessarily true. Although rice may work on the scale of centuries, subsidence is more reasonably reversed by shallow flooding and growth of emergent wetland plants such as tules.	The reviewer is not correct. Steve Deverel, the lead authors of the papers which the reviewer cited, as well as R. Miller of the USGS who has conducted research on wetlands as a method to reverse subsidence, are currently working with URS on proposals to utilize changing crops or constructing wetlands as methods to reduce subsidence. The best available information indicates that both require management; the current costs and benefits of each method do not result in one method that is more 'reasonable' than another. Reports on these topics are currently in draft form to DWR and will be made available to the public.

Comments	Responses
	It is unclear how either of the papers for which URLs are provided by the reviewer support the reviewer's point. The first mention paper describes in detail the benefit of rice farming as a method to halt subsidence (changing crop types), and the second describes costs to farmers of continued agriculture in the Delta. Shallow flooding of areas is not possible without management for heavily subsided land in the Delta.
15. **Page 5-2,** 1st ¶, 2nd sentence: Replace the term 'spiders' with 'wildlife'. Question-Why were "spiders" part of this sentence in the first place? In the reviewer's experience, effects on spiders are not currently considered to be of much importance by any of the regulatory agencies.	Detailed analyses of beta diversity were conducted on spiders at the interface Traut 2005, and this analysis was used to support the description of the habitat as an ecotone; ecotones are considered biodiversity hot-spots. Again, the project objective was to assess the risk, not assign subjective assessments of the importance of habitat or species.
C. 15. 3rd sentence: Question-Why consider vernal pools? There aren't many vernal pools in the Delta or the primary zone of Suisun Marsh. Diana Hickson, a botanist at DFG, whom the reviewer contacted for information, lists only the following vernal pools in the legal Delta, and all are located on the periphery of the Delta and unlikely to be impacted by Delta flooding: There are "vernal pools within the Legal Delta boundary, including the ones at Tule Ranch at Yolo Bypass, between Calhoun Cut and Barker Slough near Dozier (Jepson Prairie area), on the military land south of Dixon near Bunker, and the alkaline pools near the Byron Airport."	See response to reviewer's comment "**B.3. Chapter 12. Primary Substantive Comments**."
Section 5.3: Need a table of listed and sensitive species. Although there are a lot of nice maps in this section, a map of the	A table of listed and sensitive species is in the Technical Memorandum.

Comments	Responses
CNDDB occurrences of listed species should be added.	
2nd ¶: There is a need for discussion of the effects of levee failure on plants and animals, because levee failures can result in temporary loss or extirpation.	Concur.
16. **Page 12-1**, 1st ¶, 1st line: States that "The consequences of Delta and Suisun Marsh levee failures are far reaching…", but none of the breach scenarios include breaches in the Suisun Marsh. These scenarios should be included. 3rd sentence: There is a need to list in this section the species and groups that were used to assess ecosystem impacts.	An analysis of a hypothesized levee breach in Suisun Marsh was conducted for terrestrial vegetation and wildlife to address specific concerns of Suisun Marsh management. This analysis was included in the Ecosystem Consequences Technical Memorandum, but was in addition to the larger risk analysis project. A list of species and groups analyzed was added.
16. **Page 12-1**, 1st ¶, 1st line: 2nd ¶, last two sentences: Timing, depth, and duration of flooding should be included in the models of effects of levee breaches on plants and wildlife, because these parameters are critical to the survival of these ecosystem components.	The impacts of depth and duration of flooding on vegetation and wildlife are presented in the Ecosystem Consequences Technical Memo, however, in order to simplify and streamline the model, they were not included in the Risk Assessment.
17. **Page 12.5**, 1st and 2nd bullet: Both the vegetation and wildlife risk models should include timing, depth and duration of flooding. 2nd bullet: There should be an explanation about why home range of terrestrial species was considered important, because the reviewer can't see how it matters. If an island floods, chances are the entire island will flood, so it won't matter if a species' normal home range is 1500 square meters or 5 square miles—each affected individual	The impacts of depth and duration of flooding on vegetation and wildlife are presented in the Ecosystem Consequences Technical Memorandum, however, in order to simplify and streamline the model, they were not included in the Risk Assessment. The comment is unclear. The home range of an individual is not included in the assessment. The analysis is conducted as follows: The spatial

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will lose its entire 'home range'.	overlap between an organism's home range and the spatial distribution of the vegetation type in which the organism occurs was used in the model to spatially define the habitat of the organism. The risk to a species was calculated as the percentage of that area that was lost. Further qualitative assessment of the risk was made to take into account whether that area within the Delta encompassed the species entire home range or only a fraction.
18. **Page 12-12**, Section 12.1.3, Flooding bullet: This bullet should state that plants not adapted to soil saturation will die. Even those wetland plants adapted to saturated condition will die if inundation is too long or too deep. Salinity bullet: As written, " Plants adapt to high salinity levels…" an individual response is implied. The plant *species* is either adapted or it is not—adaptation does not occur in individuals in immediate response to a flood or introduction of salinity. The first sentence should be changed to read: "Plant species which are adapted to high salinity levels do so by physiologically tolerating the salt or by avoiding salt." The last sentence should be changed to "Plant species that are adapted to high salinity conditions…"	The comment is unclear. The flooding bullet describes in detail the precise mechanisms through which plants not adapted to flood conditions will die. The reviewer is not correct. Within the same individual, different life history stages of plants have different tolerances to salinity. In particular, high marsh plants, which experience extremely high soil salinity in the summer, only germinate during winter rains when salinity is very close to freshwater conditions. In particular, describing a plant species as 'adapted' to high salinity is very controversial, as exemplified by common crop plants, that are not described as adapted to high salinity, but which are currently being bred to thrive in irrigation water with higher and higher salinity (e.g., >10 ppt). For clarification, the text has been modified as follows: "Tolerance to salinity levels vary among life stages of an individual, populations, sub-species, and species. Plant adaptations to high salinity levels include physiologically tolerating high salt concentrations (e.g., through osmotic adjustment) or avoiding salt (salt

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	extrusion, salt exclusion or dilution) (Kozlowski 1997). Some plant species inhabiting high salinity environments have specialized tissues or organs are involved with avoiding salt, such as the inner cells of the cortex of roots of vascular plants and the passage cells of the steele, which are barriers to transport of salt into the plant. Another adaptation to high salinity conditions is removing salt by leaking salts through secretory organs, such as salt glands, in which energy is used to selectively move ions from vascular tissue in the leaves (Mitsch and Gosselink 1993). The precise mechanisms through which salinity inhibits growth are complex (Kozlowski 1997). Plant species which have adaptations to tolerate high salinity conditions, such as described above, can often survive in low salinity environments, but due to the energy expended on adaptations for high salinity, are typically out-competed by non-salt-tolerant plants."
19. **Page 12-12** and **12-13**, bullets on Flowering Time and Lifespan: The issues raised all assume the plant itself survives, which is highly unlikely in any long term or deep flooding event.	The reviewer is incorrect. The issues raised to not all assume that the plant itself survives. For example, the size of the extant seed bank is related to the frequency of reproduction whether or not the plant survives. The risk assessment included flood events of short duration.
20. **Page 12-13**, bullet on Seed Banks: States "little is known about the impact of flooding on seed viability for the range of communities found in the Delta and Suisun Marsh". Although this is *strictly* true, there is a large body of literature about the effects of flooding on seed banks, which could be applied here. Seed banks in general are commonly known to be long-lived. In addition, the land managers in	There is little data for focal species examined on plant or seed tolerance of salinity. Due to great differences among even closely related species in tolerance to abiotic factors, throughout the analysis we restricted data use to only that published on focal species, as described in the methodology in the Ecosystem Consequences Technical Memorandum.

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Suisun Marsh (DFG and SRCD) are very knowledgeable on the effects of flooding there.	Steve Chappell, Executive director of Suisun Resource Conservation District, was consulted about seed banks and re-growth of wetland vegetation from seed banks. Steve was the source of the series of images in the Ecosystem Consequences Technical Memorandum, exemplifying currently available quantitative information in Suisun Marsh on the impacts of duration of flooding on the re-establishment of vegetation from seed banks or vegetative propagules. Also, Suisun Marsh managers have greatest familiarity with re-vegetation of marsh plants from seed bank, which comprises half the vegetation types examined, and less familiarity with the 5 upland types and riparian trees and riparian shrubs.
21. **Page 12-14**, Bullet on Disturbance: "*Sedimental*" is not a word. "*Abrade*" is misspelled. Section 12.1.4, Terrestrial Wildlife Risk Assessment: This section is too Brief, and it conveys very little information.	The reviewer is not correct. Sedimental means sedimentary according to Webster's dictionary; 'sedimental burial' is a technical term used in peer-reviewed scientific papers for burial under sediment (e.g. as used in: Tanaka, Y. and M. Nakaoka (2006) Morphological variation in the tropical seagrasses, *Cymodocea serrulata* and *C. rotundata*, in response to sediment conditions and light attenuation. Botanica Marina v.49 (5/6) 365-371.) 'Abrade' has been corrected. The Terrestrial Wildlife Risk Assessment Section describes the method used to assess risk to wildlife in the Risk Assessment model.

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22. **Figure 12-4**, Conceptual model on impacts to vegetation: Shows that duration and depth of flooding are tied to "saline water inundation". However, duration and depth will have critical effects on plants, regardless of salinity. • A separate arrow with "duration and depth of flooding" should point to something like "possible mortality of all standing vegetation." • Question-Why is "change in hydrologic regime of vernal pool habitat" included? Is this about possible increases in ground water elevations in areas bordering the Delta and Suisun Marsh? Because according to DFG, there aren't any vernal pools behind the levees. (See comment #15,	"Saline water inundation" has been altered to "water inundation of varying salinity levels". Depth and duration has been added to the arrow pointing to 'Plant mortality' Reference to vernal pools was removed, because although vernal pool habitat is impacted due to sea level rise in future conditions (Ch. 14), they are not impacted as a consequence of levee breaches.
23. **Page 13-1**, 1st line: States that "…risk analysis results considering levee failures in the Delta and Suisun Marsh." Reviewer suggests removing 'Suisun Marsh' from the sentence because none of the breach scenarios include breaches in Suisun Marsh.	Sections 6 and 7 of the Risk Analysis Report address levees in Suisun Marsh as well as those in the Delta.
24. **Pages 13-6** and **13-7**, Section 13.4.2, Vegetation: Spelling errors need to be corrected.	The single typo 'Suisn' has been changed to 'Suisun'.
25. **Page 13-7**, 1st sentence: States that "Breaching of these levees resulted in no impacts to…. salt marsh harvest mouse, saltmarsh common yellowthroat, California clapper rail, and Suisun ornate shrew, whose habitats are restricted to Suisun Marsh." This statement is misleading since none of the scenarios had breaches in Suisun Marsh which would result in direct impacts to these species. In all scenarios, breaches are located only in the Delta, and these will have very little direct impact on	The sentence has been changed to say "Breaching of Delta levees resulted in no impacts on several terrestrial wildlife species of concern including the federally endangered Saltmarsh harvest mouse, Saltmarsh common yellowthroat, California clapper rail, and Suisun ornate shrew whose habitats are restricted to Suisun Marsh."

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species in Suisun. Since most of the sensitive (listed) terrestrial species in the region are found in Suisun Marsh, it is important to include scenarios with Suisun breaches.	
26. **Page 15.5**, last bullet, 2nd sentence: Sentence states that "…Delta levee walls include all known occurrences of a federally-threatened species, *Cordylanthus mollis mollis*, in a twelve county area." This is wholly incorrect information on *Cordylanthus mollis mollis.* This plant does NOT occur on levee banks, it occurs only in brackish tidal marshes. Furthermore, its status is Endangered, not Threatened and it currently occurs in only three counties, not twelve, as stated. Question-What is a levee wall? The reviewer is not familiar with this term.	This text has been significantly changed based on the reviewers comments. The occurrence of *Cordylanthus mollis mollis* is correct according to the following analysis: A search was conducted in the CNDDB December 2006 database for the 12 counties including and surrounding the Delta (Alameda, Contra Costa, Marin, Napa, Sacramento, San Francisco, San Joaquin, San Mateo, Santa Clara, Solano, Sonoma, and Yolo Counties). Within this area, there was one occurrence of *Cordylanthus mollis mollis*. The occurrence was on the channel side of a Delta levee. The text has been altered to clarify this. The status has been changed to Endangered. 'Levee wall', as defined in Table 4-3 of the Ecosystem Consequences Technical Memorandum, refers to an area calculated as 'on channel-side of Delta and within 200 ft of centroid of levee'. This definition has been added to the text on page 15.5.
Carl Wilcox	
Sec. 5.3.1.1, par 3, line 1: "Due to high levels of subsidence in the Delta Islands, vegetation would be flooded in deep water during the time it takes to close the breach, and would be submerged under different depths of water depending on the elevation of the vegetation until all the water is pumped out. Depth and duration of	The simplification misses the point that there is complex topography on the islands, and that different locations on the islands, particularly the perimeters of the levees would be submerged for shorter periods of time, and at shallower depths. Depth and duration both have impacts on growth

Comments	Responses
flooding are positively correlated because the duration of pump-out is related to the total volume of water in the breached island." This statement could be simplified: When an island is breached, plants are submerged until the levee can be repaired and the water is pumped out. The more subsided the island the deeper it will be flooded and the longer it will take to pump out.	of vegetation and seedling emergence.
Chapter 3, Background: The number of special status plant species is called out (on page 3-4), but no mention of how many special status terrestrial species that live in the habitats that could be affected by levee failures and sea level rise. At a minimum need to talk about species mentioned in Tables 6-32, 6-56, 6-79.	Table 6-32 is on terrestrial species. There are no tables with labels 6-56, 6-79; perhaps these refer to page numbers which have tables of terrestrial wildlife species; there is no table on page 6-32.
Chapter 4, Species Selected for Analysis: Suggest specifically discussing the species evaluated (again at least the species in Tables 6-32, 6-56, 6-79) and it would probably be worth discussing why certain species were not evaluated (SMHM and Clapper rail for example).	A description of the terrestrial wildlife species selection criteria has been added. SMHM and the Clapper rail were evaluated.
Chapter 5, Methods: Need a discussion of how analysis was done for terrestrial wildlife – is it only habitat based? Were existing known occurrences evaluated (i.e. nest sites for Swainson's hawks, potential replacement habitat for riparian dependent species, etc.)? Was the result of temporal loss of riparian habitat evaluated?	Actual occurrences were not evaluated because they may move around. Instead foraging habitat was analyzed. Temporal loss of riparian habitat for wildlife was not specifically considered. Instead the worse case scenario that it was lost completely was used, although it could come back in future.

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Page 6-56, Third paragraph, last sentence: The loss of 1200 acres of habitat (32%) sounds quite significant. If this loss has a minimal impact on the population, the argument should be strengthened. What is being considered as the population? What is the distribution of Black Rails throughout the range of this population?	My understanding is that the Delta is not a center of the population for Black rails and as such, the loss may be a large hit on those that reside in the delta, but not a large hit on the population as a whole.
Page 6-79, first line: The fifty breach scenario is always described as having THE SAME impacts as the thirty breach scenario. It is fine to say that the impacts will be similar, or just slightly greater, but if the impacts are the same, then why separate the scenarios? I'd suggest a change to something like: "Potential effects on evaluated wildlife from the fifty-breach case are similar, though slightly greater, than described for the thirty-breach condition." Next paragraph, last sentence: Suggest change to: "Effects of these habitat losses would be slightly greater than, though similar to, those described for the thirty-breach condition."	The effects on the terrestrial wildlife would not be greater for the 50- than the 30- breach scenario. Results for the 30- and 50- breach case were separated to maintain consistent formatting with aquatic and terrestrial vegetation sections.
Chapter 7, Future Environmental Conditions: Page 7-18, 7.2 Terrestrial and Wildlife Communities, second to last line: Suggest change for clarity to "…, it could result in shifting waterfowl and greater Sandhill Cranes to wintering habitats in other locations." Page 7-36, first paragraph, second-from-last sentence: Suggest one change for clarity to: "…, is expected to increase in the future as the effects of changes in sea level, climate, land-use, and species' dynamics interact and accumulate." *This whole paragraph is rather hard to read…*	The suggested changes were made and the paragraph was changed: "Overall, results of these analyses demonstrate that a large-scale levee failure event under both current and future conditions has the potential to significantly impact the abundance and distribution of aquatic and terrestrial species inhabiting the Delta and Suisun Marsh. The analyses indicate that a large-scale levee failure, occurring in current or anticipated future environmental conditions, would reduce the abundance of sensitive species, and increase their risk of extinction. The risk of significant adverse environmental impacts to aquatic and

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	terrestrial species 1) increases with the magnitude of the levee failure and associated inundation of Delta islands 2) is expected to increase in the future, particularly for native species, as the cumulative effects of changes in sea level, climate, land-use, and species' dynamics interact and accumulate."
Appendix B: The terrestrial section does not adequately address the issue of long term loss of habitat. For slow growing habitat like riparian forest, the issue of what species do while waiting for the habitat to reestablish should be specifically addressed, not just part of a brief line about post-inundation habitat suitability.	The purpose of DRMS Phase I is to describe the risk to the Delta. The objective of DRMS Phase II is to describe and evaluate ideas to mitigate the risk. Phase I identified the risk the long period of time re-establishment of slow-growing habitat; what to do while waiting for slow growing habitat to re-establish would need to be addressed in Phase II.

Comments on Economic Consequences Technical Memorandum and Responses of Consulting Team*

(*See also Appendix H of Economic Consequences Technical Memorandum for comments of Professor David Sunding, University of California, Berkeley, and responses of consulting team as well as comments of Ignatius Anyanwu, Regional Economist, U.S. Army Corps of Engineers.)

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Comments	Responses
Reviewer: Gary Bedker, U.S. Army Corps of Engineers	
1. Page 3, 2nd full paragraph: It is recognized from studies concerning Hurricane Katrina that disaster payments and other emergency social assistance outlays comprise a significant cost to the American public. An effort to capture this would be highly recommended.	The cost of emergency/temporary housing is included in the analysis (e.g., pages 8 and 9). Other emergency costs are outside the scope of this technical memorandum (TM).
2. Page 4, paragraph 2: DWR and HAZUS both have estimates concerning residential structures. It is difficult to follow the differences in these two estimates and the rationale for choosing the one that was chosen.	DWR did not provide data on residential structures mapped to the analysis zones. DWR did provide the number of households. HAZUS data on residential structures was mapped to analysis zones over the entire study area. The ratio of households to structures for the area covered by both (11,000/18,900, see page 7 of text) was used to estimate households based on the HAZUS number of structures.
3. Page 4, last paragraph: It is understood that the $747 per household is the monthly rental cost. What is the assumed duration of displacement used in the analysis? Additionally, what are the federal and state administrative costs associated with the lost resident use?	FEMA does not account for program administration as a separate item. In addition to the $747, there is a $500 one-time cost plus $500 per month for "other monthly costs." This cost could include the federal and state administrative costs. Documentation was not available from FEMA.
4. Page 5, first paragraph: The differences between DWR and HAZUS seem to be quite large. Is there a reason to explain the differences?	DWR counts households, HAZUS counts structures. Many structures in some areas of the Delta are seasonal or vacation homes.
5 Page 5, second paragraph: The last sentence in the paragraph indicates that $85 per person per day is the cost of housing people in emergency shelters. Is there additional information about the "FEMA" method approach? Are these numbers national averages? Can they be regionalized to reflect the California region? How was the $85 computed?	The FEMA BC Helpline experts responded as follows: "These values were developed along with the original modules (software) over 10 years ago. The values were based on NFIP claims data and FEMA Individual Assistance displacement cost data. The BC Helpline is not aware of any reports that document the development of original source of the values."

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6. Page 5, last line: Business sales and employment: What year are these estimates based on?	The PBS&J data are based on infoUSA data from January 2006. Citation included in Table 1.
7. Page 6, Table 1: Please indicate in the title of footnote the estimated year that these data reflect. This is a comment throughout the document.	See response to comment 6.
8. Page 9, first paragraph under "Public Services": It is expected that the value of public services should be at least the cost of providing it; otherwise it should not be provided. Is there another way to value public service that would provide a more robust approach to the valuation of public services?	No other approach was discovered.
9. Page 9, second paragraph under "Public Services." The continuity premium for police and fire is 10. How is this "10" derived? Is there reason to believe that this factor should be "10" over the nation or is it possible to adjust this for varying regions throughout the nation?	FEMA provides no documentation, so the answer is not known.
10. Page 9, second paragraph: Why is there no continuity premium for medical services?	According to FEMA's BC Overview Training Materials DATA DERIVATION DRAFT, "Floods do not generally result in a significant increase in demand for medical services beyond normal levels" (Page 9).
11. Page 10, first full paragraph: The cost of lost government service per day is provided in the paragraph. Wouldn't the services be picked up by another office and not necessarily "lost" to the public for the entire inundation period?	Only if it is assumed that there are underemployed resources available in other offices. If there are no slack resources, then the transfer of services must result in a loss of other services elsewhere.
12. Page 12, last paragraph: The third sentence discusses the loss to the grower being valued at the un-depreciated cost of stand establishment plus the loss of net	Reviewer misread discussion. Language has been clarified.

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income until the stand is bearing. Doesn't this overestimate the loss, since the computation assumes the re-establishment of a new orchard that has not been depreciated? It is expected by this reviewer that some depreciation be applied so that the value of the "new orchard" be exactly the same as the stand that was subject to flooding.	
13. Page 15, paragraph "a": If the crop is lost due to flooding, what is left to experience the effects of the water degradation? Is there an assumption that the water degradation will cause long term effects on the cropland? If so, how long are these effects expected to be felt?	Paragraph a (page 16) refers to land that is "scoured." Historically, land that is scoured is removed permanently from farming. Language changed to make this clearer. Alternatively, paragraph a on page 20 refers to salinity effects on crops in non-flooded zones.
14. Page 16, first full paragraph: Delta Recreation Data dated 1997 was used for the analysis. Is there anything more current?	Most recent analysis was used. Where analysis existed, updating was outside the scope of project.
15. Page 22: The analysis is done with no assumption for comparative recreational opportunities which will overstate the value of recreation lost.	The analysis explicitly reduces recreational losses to account for substitute recreational opportunities. As discussed on page 28, it is assumed that lost recreation from the Delta has substitutions elsewhere. The lost recreation is described as having a symmetric triangular distribution between 0 and 50 percent range, implying a mean value of 25 percent of lost recreation having substitutions elsewhere.
16. Page 42, end of first paragraph: Table 18 is mentioned as being provided but not used as it is outdated and does not include congestion costs. Why include it in the document if it is not used in the analysis?	Discussion removed.
17. Page 42, paragraph beginning with "FEMA provides a cost of $32.23 per hour …": What is the derivation of the number?	The $32.23 is believed to be representative of the early to mid-2000s condition and is within rounding error. Updating for a small number

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Is it national average, regional average, season average? Please explain.	of years' inflation implies accuracy that is not appropriate.
18. There is a wealth of information that is contained in the report that illustrates the number of issues that need to be adequately addressed to cover the requirements of AB 1200. Much of the data and assumptions are good for use concerning reconnaissance level analysis. It is this reviewer's opinion that much is yet to be done to provide a concentrated feasibility report concerning specific issues that have yet to be articulated. The use of the replacement costs verses the depreciated replacement cost is an issue that the Corps of Engineers has. Use of Corps methodology requires risk analysis and depth damage relationships. These data are not apparent in this report and would require extensive work to evaluate the geographic area regarding flood damage protection using Corps guidelines.	Depth damage and repair estimates are not part of the Economic Consequences TM, but are discussed in other TMs.

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Comments	Responses
Reviewer: Goettel, U.S. Army Corps of Engineers, General Comments	
1. The review of economic costs and economic impacts that may arise from levee failures is generally balanced and objective. The methods used generally follow well accepted economic methods and/or appear to be reasonable approximations for cases where well-defined methods do not exist. Common pitfalls in modeling economic losses and impacts are: double counting and counting transfers as losses. The authors have been careful to minimize such effects. For example, recognizing that some (many?) lost business sales will be made up by increased sales by other California businesses and correcting for such effects using the regional purchase coefficients, as done by the authors, is the correct computational approach for such issues. However, in contrast to the computational methods, the explanations of, interpretations of and commentary on the results are relatively weak. The report suffers significantly from the absence of quantitative comparisons of the relative importance of the various categories of losses and impacts evaluated, from the absence of summary tabulations, and the absence of much commentary on the geographic distribution of the losses and impacts, and on a lack of comparison with the corresponding categories of damages and repair costs (for reference and context).	Response to general comments on lack of results, lack of results by location, and lack of comparisons to give policy guidance as to the relative importance of segments: The goal of this document was to provide the source of values used in the modeling, and not to develop results related to particular scenarios. All estimates are in the model by location, and can be summed once a scenario has been specified. However, without a specific scenario, no results, general or location-specific, can be developed. Development of such results was outside the scope of this document.
2. As outlined in Section 1.2 (Purpose and Scope), the intent of the study is to calculate the economic losses to the California economy, along with broader economic impacts such as employment,	Language added to Section 1.2.

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value of output, wage and salary income, other income such as proprietor's income, and value added. The economic analysis excludes estimates of damages, repairs and restoration, which are covered in other reports. The difference between economic losses and economic impacts is significant and needs to be explained very clearly in language understandable to non-economists. In the main sections of the reports, the various categories of losses and impacts tabulated need to be clearly classified as to which they are (losses or impacts), and the reader reminded, perhaps, briefly what the difference is.	
3. An important point for these calculations is that the focus is California, not the United States as a whole. However, as stated in Section 1.3, the economic loss calculations do not include certain types of payments, actions and economic effects which often (always!) occur after major floods, including: 1) the compensation by private insurance companies, disaster payments or other social assistance, 2) the positive effects of reconstruction on businesses and local or state economies, and 3) the effects of flooding and reconstruction on prices of goods and services required for reconstruction, which may be positive for some persons. The above exclusions are surprising, given	Effects of insurance, other compensation: These effects depend on the source of the payments and the size of the payments. The Paterno decision also suggests that the state would be liable for a portion, possibly a significant share, of these payments. To the extent the state is liable, or payments are made by others within the state, these are transfer payments and have no benefits. To the extent money is brought in from outside the state, this will be a benefit. We do not know what portion of these costs will be borne by the state, and what by others outside the state. The proportion of households with flood insurance is not known (note: many of the households assumed subject to flooding are in a FEMA-designated flood zone). Because of these large unknowns, the subject was not investigated. Language has been added to explain this.

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the broad content of the loss and impact calculations. Omission of these important effects will systematically bias the economic results: economic losses and impacts to California will be overestimated by this approach, especially from the first two categories listed above, both of which can be large effects that are well known (1) or have been studied in many previous disasters (2). A much preferable approach would be to at least estimate the above effects approximately, which would give a more complete, more accurate result. If not, then, at the very minimum, the resulting overestimation of losses and impacts should be explicitly stated in the report.	
4. The main sections of the report: residential, business, agriculture, water etc. are completely separate (with different flavors/styles obviously arising from different authors). That's more or less OK, but there are two significant omissions, arising perhaps from this joining of separate pieces into this report: a) The methods are outlined, but the presentation of results is very incomplete, with many sections have no numerical results for the dollar amounts of economic losses or impacts. b) There are virtually no cross comparisons between these major categories to highlight which ones are likely big and which ones are not, as well as perhaps the spatial variation of where these losses/impacts happen (between the three major "regions" [my term: meaning Delta Island, Suisun and Other (what is other?)]) and the Urban areas. Such summaries and cross	Lack of totals and the "temptation of big numbers": As the reviewer states, totals are not meaningful because no reasonable scenario is likely to result in ALL losses being realized. This is why totals were not provided. However, totals were requested on some tables. Where totals were inserted in response to comments, footnotes were added to point out that the totals did not reflect the likely effects of an event.

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comparisons would be VERY helpful and meaningful. Here the totals for all regions would be useful measures (the caveats below about the meaning of such totals notwithstanding). c) There is also no cross comparison between the economic losses and impacts calculated in this report and the damages and repairs costs calculated elsewhere. When tabulating loss of function costs for residential (or business or public, etc.) it would be useful and informative to compare the losses/impacts to those for damages to buildings, contents, etc...again to give context and perspective. Without such context and perspective the chapter is largely just numbers without much interpretation and context for the reader to understand the relative importance of the various results calculated. d) For inputs into eventual policy decisions, the relative importance of these various damages, losses and impacts (and the geographic variation in these) are absolutely critical inputs to rational policy making. Economic calculations are done by zones/areas, reflecting the reality that various flood scenarios may flood one zone or many, but not all of the zones in a single event. However, in the text in many places, the temptation of big numbers has resulted in showing many totals across all zones/areas. Such numbers should be given very carefully, with acknowledgement that not all zones will flood in any given event. As noted above, such numbers are useful largely for making cross-comparison, and should NEVER be used in a sense where the reader might interpret such as likely	

Comments	Responses
numbers from a given flood event. Suggestion to Consider: I think the entire report would be easier to read if each major section were more clearly distinct, with large font titles and headers to distinguish sections. I also suggest considering moving the appendix tables to each section, since each table is strongly tied to the specific section and the tables are pretty much lost, buried in an appendix.	
Reviewer: Goettel, U.S. Army Corps of Engineers, Specific Comments	
1. The Appendices contain numerous tables with tabulated results that arise from each section of the analysis. However, few, if any, of these tables are referenced in the text. The usefulness of these tables would be much improved if the text systematically contained references to the corresponding Appendix Tables. For example, Page 4 deals with residential structures, and there are many tables bearing on such in Appendix A.	This issue has been addressed by referencing the appendices in the revised text.
2. Some the Appendix Tables appear substantially incomplete or just plain missing: Tables A-1 and A-2 contain residential inventory and lost use data for Delta Islands. Table A-3 has lost use data for the Suisun Marsh and other Areas, but there is no table of inventory analogous to Table A-1. More importantly, there seem to be no such tables at all for Sacramento and the other urban areas which constitute the great percentage of residential inventory in the study area. Since other data includes these urban areas, the omission here is major and should be corrected.	Appropriate data are not available.

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3. Tables A-1 to A-6, etc. do not have totals. Totals would be informative, to show the relative total exposure in the Delta, Suisun, and urban areas, as long as footnoted with an appropriate caveats about not all areas flooding in a given flood event.	Included. Most tables did have totals, but at the end of multiple pages.
4. Table 2 has no total. A total and comparison to the grand total in Table 1 would be useful information, showing that the vast majority of sales are in only a few areas with the remaining areas constituting only X% (TBD) of total business sales. Again, to give measures of the relative total exposures in various areas.	Included totals in Table 2. Discussion of this issue is added to page 9 of revised TM.
5. Table 3: add per day in Title; add caveat about not all zones flooding in any one event (here and for every such "total" table throughout).	Table 3 includes both per day and one time costs.
6. Page 4: FEMA (2005) method for residential lost use calculations. An essential parameter in the FEMA-based calculation is the displacement time to temporary quarters, which in the FEMA model is proportional to the percentage of building damage, which in turn is proportional to water depth. What was assumed for water depths, building damages, and displacement times for these calculations? Without such information, the data and logic for the calculated residential (and business etc.) loss of use costs cannot be meaningfully evaluated. Was the FEMA (soft) cap of 12 months of displacement used? Was a longer value or shorter value used as "typical"? How was the variation in water depth within a study area considered for such calculations?	There is no fixed time. The displacement time varies from analysis zone to analysis zone and from scenario to scenario, and is the time from the levee failure incident until the infrastructure has been rebuilt. The FEMA "soft cap" was not used. Katrina has shown that displacement times can be longer than a year. Whether or not FEMA is paying for the displacement, the displacement occurs until the zone is restored, and someone, either a government agency or the individual, is paying for that.

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Comments	Responses
7. On page 5, the number of residential units is estimated. The typical number of units in 50+ unit multi-dwellings cannot be 50, since all are 50+. A reasonable estimate should be used instead of 50; a trivial error in the big picture, but obviously incorrect and such minor errors diminish credibility overall.	No justification for any number other than 50. Could use, e.g., 51, but no justification for it. Effect on results is small.
8. On page 6, above Table 1: "the economic cost analysis for lost sales assumes that sales stop for the duration of lost use and that businesses do not pay rental costs (for temporary quarters, I assume). This paragraph acknowledges the FEMA method to consider resumption of sales in temporary quarters but then does not use the method. This limitation will result in overestimation of lost sales.	The economic costs of lost businesses were discounted to reflect the potential for transfer of activity to other locations and to other service providers using the RPCs from IMPLAN. This approach was felt to deal with the issue sufficiently.
9. Table A-8 has estimates of operating costs and costs/person for various agencies. Some of these costs/person are >$1M and must include pass-through funds which should not be counted in the value of services, per the FEMA method used.	Most public offices in the PBS&J database did not include any actual operating expenses. The DRMS analysis assumed $100,000 in operating costs per employee; employment is reported in the PBS&J data. This assumption was used rather than the operating expenses in the previously-reported Table A-8, precisely because of the pass-through funds. Table A-8 has been modified.
10. Page 10: Agriculture. Agriculture is obviously significant in the delta, but Table 1 shows that agriculture is only about 1% of the study area economy. This is an example of important information about the relative importance of various categories of losses and impacts which really should be included as context for this section, along with a measure of the relative importance of agriculture in the Delta Islands and Suisun (which is presumably much higher than 1%). Similarly for other sections.	This is a methodology document, not a document for input to policy. The appropriate policy input discussion is found in the Delta Vision context memos on economics.

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11. Similarly for in-delta recreation losses. The report needs context by documenting the scale of the recreation-based economy relative to the total economy (perhaps even smaller than agriculture). To omit such key reference data may result in inappropriate balancing of competing interests.	See above.
12. Similarly, page 17 delta recreation data: let's see the boating/fishing visitor day data. Also, the Zone A… Zone D paragraphs list numbers of small, medium and large marinas, but do not define "large". Show how many berths total, again to give a quantitative basis for the relative importance of various sectors. Oops, I found these in Appendix C. Throughout; the Appendix Tables should be referenced in the corresponding text section.	Tables added to appendix and referenced in text.
13. Page 18: Show number of Visitor Days as well as percentages. Same logic as above.	See above.
14. Page 19. Table 9, Mean CS and SE are not defined in the table – write out for clarity. The coefficient of variation seems like overkill, since previous numbers throughout the report were all median estimates, I presume.	Mean CS (Consumer Surplus) and SE (Standard Error) defined in Table 9 headings.
15. Table 12: SE not defined. Table 13 and Figures 3 and 4. Again, I'm baffled by the detailed statistical analyses for a very small part of the picture, with only a few million dollars per year, when there was none for residential/business losses which are MUCH larger than these by factors of several orders of magnitude. Seems completely imbalanced compared to the other major sections.	Defined SE (Standard Error) in Table 12 heading.

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Comments	Responses
16. Pages 20 to 25. The detailed equations seem like overkill. A brief narrative about the calculation methods and reference to the original source of the method would suffice here. This level of technical detail seems unnecessary and impedes the reader in understanding the overall chapter.	Edited; discussion moved to Appendix C.
17. Disruption of Water Supplies (Agriculture): First order questions are: what volumes and percentages of CVP and SWP delta pumping goes to agriculture? Such background information is essential for understanding the relative importance of various uses of water and understanding the impacts of outages and well as constraining strategies for dealing with outages. I'd like to see summary tables for a number of years.	Included in Table 15 (page 33 of revised TM).
18. Page 27. Some key terms are undefined: TAF and MAF. Define these.	Modified (see page 30 of revised TM).
19. Page 27. Missing data for the South Coast regions appears to be a major hole in the agricultural water calculations. Absent any data (which hopefully can be obtained), a rough estimate could be made by scaling in proportion to the amount of water distributed (assuming equal or adjusted relative values of crops vis-à-vis water usage).	Our inquiries indicated that groundwater could be substituted for disrupted South Coast SWP agricultural delivery in most, though not all, examples of a fixed period of interruption of water supplies. MWDSC delivers little ag water, and much of this is based on interruptible contracts, and it is likely that farmers using this water have backup groundwater supply. As a result, the losses from an emergency interruption would be small. The SWP water is used to correct historical groundwater overdraft, so the results would be different for a permanent reduction in SWP supplies.
20. Table 14. To gauge the relative and absolute importance of the various regions, it would be desirable to see water supply data for these regions (perhaps averaged over something like 5 years or such to give	Provided in Table 15.

Comments	Responses
meaningful relative amounts), along with percentages of surface and ground water used by each region. Same logic as my discussion at the beginning of my review about comparing relative impacts of various categories of damages, losses and impacts.	
21. Appendix E has the agriculture calculations, this should be Appendix D, because agriculture comes before urban water in the main body (Urban water appendix is D). Appendix E tables are not numbered, and these results should be referenced in the main body. A major omission in this Appendix is the absence of economic loss totals for any scenarios – the tables all appear to be intermediate calculation tables with ground water pumping amounts and costs, but nothing showing expected agriculture losses. This omission is critical, because we cannot compare the agriculture losses to other losses and impacts calculated elsewhere. Page 33 says the SOD Farm Income Loss model was run, but I don't see ANY results of this and there is certainly no discussion of the results in the main body of the report.	Corrected.
22. Water Supplies to Urban Users: An important assertion is that outages for customers of the North Bay Aqueduct would be minimal is important. Please verify the correctness.	This assumption was made after discussions with hydrodynamicists, whose models show rapid clearing of North Bay Aqueduct diversion region.
23. As with the other sections, the 5 pages in the main body for Urban Water have NO results or tabulations of losses; all of which are buried in Appendix D. At a minimum, the main results should be presented in the body with summary tables.	This is a methodological document, and results are outside the scope of this document.

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Comments	Responses
24. This section has major omissions: a) The discussion/tables in Appendix D has discussion of water supplies for six agencies, but does not have any discussion for MWD which supplies water to about 70% of the population served. b) There is no discussion about the economic impacts for any duration of water supply outage: such discussion/calculations should be included for a range of outage durations, covering the plausible range for various flood events affecting the water supply from the delta. See Comment "c". c) Page 37 presents a method for estimating water shortage costs, with the equations and a statement that "The shortage costs estimated by agency and customer group were multiplied by the appropriate number of acre-feet and summed to get the total shortage cost for the agencies analyzed." NO SUCH RESULTS ARE SHOWN IN THE BODY OR IN APPENDIX D. d) Given "c" it appears that the draft is simply incomplete since the most important results are mentioned but not included???	Expanded to include discussion of other agencies and information. However, this is a methodological document, and results are outside the scope of this document.
25. Infrastructure of Statewide Importance: Mokelumne Aqueduct. Page 38 states that the economic consequences (fuzzy language vis-à-vis "losses" and "impacts") is considered as part of the analysis of water supplies to urban users. NO SUCH RESULTS SHOWN in the urban water section.	Added to Table 18 (page 40), and discussion added on page 42.

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Comments	Responses
26. Good discussion of the 500 kV lines, with reference to a "to be added table" which is described as the extreme worst case event, occurring in less than 1% of the time that such a failure occurs. OK to show worst case, but should also show "typical" events.	Discussion expanded on pages 46 and 47 of revised TM.
27. The analysis of lines below 500 kV serving the Delta quotes PGE values, which seem extraordinarily high: Delta Population, 24,825, Table A-1; Value of Service Lost, $337,000,000, 8 hour outage (summer?); Value lost per person, $13,575 calculated, FEMA Value, $188 Economic impact per person per day(includes national average impact on economy and impacts to residential customers, including value of people's time) A $300+ million value of service lost for an 8 hour (short!) outage seems high by a factor of at least a 100x. The FEMA (2005) value (What is a Benefit? Guidance on the BCA Toolkit CD) of $188 is a generous (let's support mitigation projects) number (in 2001 dollars). The impact for the Delta will be lower because there is relatively little economic activity compared to the average for the US as a whole, the basis of much of the FEMA estimate, and because flooding in the delta will clearly significantly reduce demand for electricity in the delta. The $337 million estimate needs to be revisited and revised (or justified, which seems outside the bounds of credibility).	Estimate removed from revised TM.
28. Highways. Table 17 (Highways that could be closed in 100-year event). Need an important caveat that all of these routes	Note added to Table 20 and text (page 49 of revised TM).

Comments	Responses
are highly unlikely to be affected in the same flood event.	
29. The FEMA value of $32.23 is per vehicle hour of delay or detour (and should be defined as such). This is a 2001 value which should be updated.	A total of four years of inflation is well within the uncertainty of these estimates. Adding an inflation adjustment would imply more accuracy than is reasonable.
30. The analysis, including congestion costs (obviously an important factor) appears to use consensus state of the art modeling. Results in Table 19 are informative, but I-5 and some other routes are omitted from the Table. Why?	I-5 is included in the revised TM in Table 23. Initial assumption was that I-5 would not be disrupted. Discussion in text says that I-5 results of the QP model were felt to be unreliable and so were not included in the previous Table 19 (now Table 21).
31. Figure 7, page 45, societal cost estimates for outage of gas Line 57B: a) Are these values per day? Define! b) The most meaningful number would be the temperature-probability weighted average cost , which can presumably be extracted from the temperature-probability data (which are a bit confusing in how they are labeled in the figure). Since the higher temperatures have higher probabilities, the typical value will be very much toward the low end shown in Figure 7.	Revised information on page 56 of revised TM.

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Comments	Responses
32. The petroleum products pipeline section (acknowledged as placeholder) seems to discount the possibility of ruptures during flooding, which could occur from scour and or uplift forces. Repair times of "a few days" including some times underwater may not apply for Delta Island flooding with large water depths and pump-out times of weeks or months. Rupture also poses a risk of substantial environmental impacts and cleanup costs. Such impacts/costs should be discussed and estimated, if even semi-quantitatively (order of magnitude).	Rupture during flooding is considered unlikely, as pipelines are constructed to be operated and repaired underwater. However, rupture is expected through scour.
33. Wastewater facilities. The FEMA values quoted are 2001 values and should be updated.	A total of four years of inflation is well within the uncertainty of these estimates. Adding inflation adjustment would imply more accuracy than is reasonable.
34. Changed reservoir operations. The detailed mathematics of the model seems perhaps unnecessary here. I suggest just describing the method and giving the references. As with the other categories of economic losses and impacts, it is highly desirable to see the results, not just the method: what are the economic losses from changed reservoir operations from various flood scenarios?	Document is methodological. Results are beyond the scope of the TM.
35. Economic Impacts: IMPLAN results. IMPLAN is, obviously, the standard way of doing I/O modeling for changes in economic outputs, etc. Disasters, such as major floods, are fundamentally different from normal internal changes to economic activity and I'm not completely convinced that "normal" I/O modeling is accurate for disasters where: a) The effects of the disaster are temporary,	Discussion in Section 2.2 of TM expanded.

Comments	Responses
b) For floods, people are displaced within California and so many "losses" and "impacts" may simply be transfers, c) There are tremendous inflows of Federal and insurance dollars from outside California, as well as positive multiplicative impacts from rebuilding (which are inexplicably excluded from the DRMS study). Given the above, it appears to me that IMPLAN I/O modeling, under the above conditions, will systematically overestimate the impacts of specific flood scenarios. At a minimum, the applicability of IMPLAN to disaster conditions should be discussed and justified, along with addressing the above issues. The discussion on pages 62 and 63 addresses some, but not all of these issues.	
36. From the bottom of page 60 through page 62, "results" are discussed, but NO numerical results are shown here or in Appendix G. Where are the numerical "results"? Appendix G has IMPLAN multipliers, only, and NO results for the impacts of delta flooding.	Document is methodological. Results are beyond the scope of the TM.
Spatial Resolution of Economic Modeling: Spatial resolution of the economic losses and impacts is desirable to give an overview of where losses and impacts occur (Delta, Suisun, urban areas).	Methodology allows for calculations at the level of analysis zones. Some losses are zone-specific, others are not. The summation of costs depends on the scenario posited. The goal of this document is to describe methodologies, not to provide results.
Deaths and Injuries: Many potential flood events have a likelihood of a few (or in some cases many) deaths and injuries. It is unclear to me whether estimates of casualties belong in the economics section (monetized statistical values of deaths and injuries - as done in FEMA benefit-cost	This topic was not included in the scope of work for this section.

Comments	Responses
analysis) or elsewhere in the risk management section. In any case, reductions in casualties may be an important driving force for some levee improvements and the benefits of such really need to be counted in both economic and political terms someplace in the DRMS report.	

Comments on Wind-Wave Hazard Technical Memorandum and Responses of Consulting Team

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Response To Comments: Wind Wave Technical Memorandum

Comments	Responses
Reviewer: Laurine White, U.S. Army Corps of Engineers	
1. List of Acronyms and Abbreviations – Why are both CEM and DEM listed as acronyms for the Coastal Engineering Manual?	DEM was listed by mistake and was removed.
2. The wind roses for Appendix B are for the period 1 Jan 1997 – 31 Dec 2005, yet the frequency distribution tables in Appendices A and B say Year 1957 to 1965. This is confusing. If the period listed on the wind roses was the one used, this period begins in the middle of a severe storm (31Dec96 – 01Jan97) and finishes just at the end of a severe storm (30-31 Dec05). Couldn't the period of analysis have been extended to cover any severe winds in the days prior to 01 January 1997 or possible high winds for several days after 31 December 2005, when stages may have been higher in the Delta?	The data in Appendix A and Appendix B are for stations from different sources. Appendix A is for NWS stations and Appendix B is for DWR and CIMIS stations. The periods of record do not necessarily overlap. Given the length of the record, adding a few days should not change the statistics of the wind data.
3. Page 11, 2nd paragraph of Section 3.2.2.3, Spacial Wind Speed Distribution, Wind Speed Distributions, reference is made to Figure 6a-f, but Figure 6 includes only a-c. Page 12, 3rd paragraph, reference is made twice to Figure 10a-f, but Figure 10 includes only a-e, for five NWS stations.	The text was corrected.
4. The report does an excellent good job in describing the procedures for collection and analysis of wind data, and applying wind speeds for Delta areas away from the wind gage locations. I used some of the methodology described here for wind data analysis for a wave runup study I am currently involved with.	Noted.

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Comments	Responses
Application of the ACES (CEDAS) program did not produce the same results as the Twitchell Island example in the report. I could not develop a deepwater wave of 2.1 feet with a Ts of 2.6 seconds, but perhaps I was not using the right combination of Wind Observation Type and Wind Fetch Options, and could only try different observed wind durations. The problem I have is that when I enter the following data into the Wave Runup Module, wave transmission on impermeable structure: • Smooth slope runup and transmission, • Wave Height = 2.1 feet • Wave Period = 2.6 seconds • Cotangent Theta (Slope) = 1.5 The computed wave runup is 4.82 feet, not the 6.9 feet in the example. I do not know what caused this difference in wave runup results.	We cannot figure it out either.

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Comments	Responses
Schlunegger, U.S. Army Corps of Engineers	
1. Section 4.1, page 15. I was unable to find the Lookup Tables (Tables 2 thru 6) for the wind wave parameters in the text or in the back with the figures. I am hopeful they will be included in the final version of the report. I would like to see this information as it helps give the equations more meaning.	These tables were inadvertently omitted from the first version of this TM that was posted. These tables are included in the Draft 2 version of this TM (June 2007).
2. Section 5.4, page 19. Based on the summary, is the spatial wind hazard model and wind wave analysis going to be re-calibrated or adjusted to account for the model under predicting?	The wind save analysis will be revisited in Phase 2, if necessary.