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

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.

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.

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.

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.

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.

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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.

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.

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.

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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).

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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.