

Comment from Peer Reviewers					Authors' Response					
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				Reviewer #8						
8-1	ES	3	17	page 3, line 17. This is not a correct reading of the IPCC report (see discussion on chapter 1 and 2).		X				Wording revised to acknowledge caveats raised by IPCC on sea-level projections.
				Reviewer #23						
23-1	ES	3	7-17	p. 3, lines 7-17. No mention of glaciers and ice caps, although their abrupt changes are happening and are known to be major causes of sea-level rise! Many of these that end in the ocean are also exhibiting dynamic instabilities and accelerating flow., a further reason why the IPCC 07 projections are too low.			X			Although glaciers and ice caps may be undergoing rapid changes, they simply don't have the volumetric capacity to cause an abrupt global sea-level rise that will have any significant impact, i.e., as per our definition of abrupt.
23-2	ES	3	9-11	p.3, lines 9-11 – The emphasis on ice shelves seems a little misplaced; this is certainly correct for the Antarctic Peninsula and perhaps Pine Island glacier, but the speedups of many Greenland glaciers (such as those on the east coast) have no ice shelves.		X				Wording has been changed to more generally acknowledge multiple mechanisms that may trigger abrupt ice dynamical changes.

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23-3	ES	12	7	p. 12, lines 7 – Is special pleading for an InSAR (not defined) mission appropriate? I think that we need a complete inventory of glacier ice and its current change, which requires more than just a radar mission.		X				We have added reference to IceSAT-II and a GRACE follow-on mission.
23-4	ES	12	14	p. 12, line 14 – add, “glacier and” before “ice-sheet..”				X		Insofar as our focus is on those processes that may influence abrupt sea level, this applies only to ice sheets.
				Reviewer #24						
24-1	ES	8	3-5	<p>1. lines 3-5 (page 8): [this appears to have been taken straight from chapter 3 and so the same criticism applies, as repeated here]: The wording here is problematic. Surely an ‘adequate’ (if not conclusive) explanation for the extended duration La Nina-like conditions in the tropical Pacific during the Medieval <i>is</i> available, i.e. the Mann et al (2005) article discussed and cited in chapter 3.</p> <p>By contrast, the confidence with which it can be stated that the droughts in question were caused by La Nina conditions is overstated here: there are other mechanisms that can explain the drought conditions, including</p>				X		<p>Two points made by reviewer. Re: explanation for duration: as discussed in Chap. 3, we have added some wording to reflect the Mann result, but because GCMs show a different response than the Mann result, we leave the “adequate” caveat in.</p> <p>Re: “other mechanisms”: Chap. 3 does mention the "Perfect Ocean for Drought", i.e. the Indian Ocean as a possible player in the development of droughts over NA, especially in more recent times. It is not</p>

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				changes in the strength of the zonally-symmetric (Hadley) circulation, or changes in Indian ocean temperatures that are entirely independent of what the Pacific is doing (e.g. the Hoerling and Kumar 2003 'Perfect ocean for drought' mechanism).						at all clear how much of a role the Indian Ocean played in the development of megadroughts in the Medieval Period. The likely coupling between the Indian Ocean and the tropical Pacific argue for some influence perhaps, but we are not aware of any paleo evidence to directly support it other than to say that the global map of precipitation/drought anomalies during Medieval times argues for a near-global impact that is likely to also include the Indian Ocean as an influence. We have added some wording to reflect this.
24-2	ES	8	21-23	2. lines 21-23 (page 8): It should be mentioned here that there is an explanation available for the prolonged La Nina-like state as a response of the tropical Pacific to combined solar and radiative forcing changes (Mann et al, 2005). These latter studies are important in potentially closing the loop in the potential relationship between radiative (including anthropogenic) forcing and continental drought and it is appropriate and important to discuss such issues in the executive summary as well as the specific chapter on hydrologic change and drought (i.e., chapter 3).				X		The statement in question is only pointing out that patterns of SSTs may be responsible for Medieval megadroughts, versus the general warming of the oceans and atmosphere today. There is no discussion of mechanism here, and the Mann et al. result has not "closed the loop" insofar as GCMs have not replicated it.

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				Reviewer #29						
29-1	ES	12-13		The NRC Decadal Survey should be mentioned here. ICESat-II and a GRACE follow-on should be mentioned specifically.		X				
29-2	ES	12-13		An InSAR mission is mentioned. There seems to be an implicit assumption that this would be a US mission, as many countries already have operating SAR missions. How does this InSAR mission compare with the InSAR mission recommended in the Decadal Survey?				X		This statement does not imply US missions only nor satellite missions only; aircraft missions would be adequate to fill the gap; we left this recommendation as general as possible to be inclusive.
29-3	ES	12-13		At the end of the paragraph, there is an implicit view that satellite missions could not provide the high spatial resolution called for.				X		This recommendation suggests that InSAR mission can be obtained by dedicated aircraft mission at high resolutions.
29-4	ES	12-13		This paragraph is an example of multiple statements driven by author biases.				X		We are not at all clear as to what the reviewer expects for revision, nor do we agree with this assessment. We have left as is.

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				Reviewer #34						
34-1	ES			The Executive Summary is well-written and easy to read. It is useful in defining abrupt change and then outlining four specific areas of potential change. The four specific areas are certainly the four major areas that are frequently considered in scientific literature and other reports. Under each of these areas the Executive Summary does a good job of providing a concise description of major findings for each of the four major areas.	X					
34-2	ES			The major flaw is that there is very little inclusion of uncertainties, unless the uncertainties could produce an even more problematic result. To be more specific, there is good (and warranted) discussion of the potential of more rapid changes in ice sheet dynamics, drawing from the more rapid melt of the last 5 years and known flaws in models. However, the period of observation is short, and the potential for variability seems large, yet one gets the impression that the melt and more rapid flow observed in some locations can only go in one direction. The recommendations indicate that we need more observations and better models – a good conclusion. But, it seems strange that the		X				<p>We have addressed the comment on distinguishing between short-term variability versus unidirectional change in ice sheets.</p> <p>We have also addressed the comment on land hydrology variability re: impact of future of ENSO relative to warming.</p> <p>X Re: comment on global inventories on methane production and global perspective on wetlands, we are not sure what to respond to here, other than detailed information on these inventories is provided in Chapter 5.</p>

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				<p>notion of variability, so inherent in other aspects of the climate system, is not considered here.</p> <p>The same could be stated for other sections. If we look at land hydrology, we capture clearly the tendency for drier areas in a greenhouse world and we capture clearly the notion of historical mega-droughts. We also see that droughts (and their persistence) are related to patterns of SSTs and their persistence. Fine – all good information – yet we don't incorporate this fully into the discussion – the information suggests that drought location and drought persistence isn't just the warming, but will also reflect the degree of variability in tropical and other SSTs. What happens to El Nino in a warmer world? Does it lock, is it the same but with a different amplitude or frequency. The discussion makes this seem important, and could make droughts more persistent or less and could define the locations. Yet, all we get in the summary is a global model tendency without the added information on how the decadal to annual variability could impact the issue.</p> <p>IF there are global inventories on methane production, and a global perspective on wetlands, I missed it.</p>						

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34-3	ES			I think the bulleted recommendations on how we can improve understanding are excellent.	X					