

Comment from Peer Reviewers					Authors' Response					
Reviewer ID - Comment #	Chapter #	Page #	Line #	Comment Text	Acknowledged. No further response or revisions are required.	Revisions have been incorporated as suggested.	Agree, but see "Notes on Response."	Disagree; see "Notes on Response."	Beyond scope of report/chapter; see "Notes on Response."	Notes on Response
				Reviewer #2						
2-1	3			In my opinion, this chapter needs a lot of work. First of all, the chapter reads like four or five separate parts, each summarizing a given author's latest research, with very little cross-referencing between parts.			X			We will work on cross-referencing the parts of the chapter, and we will work to integrate the chapter as revisions proceed (and as the schedule allows).
2-2	3			Second of all, it is not at all clear if this report is supposed to address only climate changes happening within the national boundaries of the US or is to refer to climate changes farther afield (I don't see how the topic of abrupt climate change could be addressed without reference to the whole Earth). The chapter as presently configured is way too focused on the US with passing reference to North Africa and no reference at all to abrupt climate change elsewhere around the planet. Drought in the Southeastern US is presently having a terrible economic impact and could get worse. Drought in the Nordeste of Brazil is long-lasting and devastating. Drought in the Amazon such as in 2005 is potentially catastrophic. Drought in many nations of Africa has probably caused millions of deaths in the 20th century. Failures of the monsoon in India and China have caused major crop				X		The CCSP SAP series is meant to focus on the United States, and by extension, North America. The primary audience is US policy makers. Therefore, a global perspective on hydrological variability and abrupt change is not warranted.

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				failures repeatedly in the 20th century and before. Yet none of these are discussed!						
2-3	3			<p><u>The chapter should be reordered and restructured. I would start chronologically by chronicling the record of abrupt hydroclimate change with reference to the Holocene.</u> Although the Holocene climate used to be thought of as stable, we know much better now that there have been big and rapid changes of hydroclimate particularly in the tropics. The causes of these are not understood at present, but it seems plausible that similar events could happen again. <u>In my opinion, this section should at least cover the most iconic paleoclimate records from South America (e.g. Cariaco Basin), and the monsoon region spanning from North Africa to India to China. In the latter, the key records to reference are the speleothem records (Fleitmann, Burns, Wang, Yuan, Edwards et al) that clearly document very large amplitude, abrupt, and sustained changes of monsoon precipitation in a region that today counts about half of Earth's human inhabitants.</u> The speleothem records are key, yet have not even been referenced.</p>			X	X		<p>We do not agree with reordering and restructuring the paper as suggested. The current ordering "cuts to the chase" right away with respect to the importance of hydrological variability and abrupt change on time scales relevant to human activities and societal concerns.</p> <p>We will summarize some additional Holocene length hydrological records (e.g. Hulu Cave).</p>

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2-4	3			I would drop (or greatly revise) the section on the North African humid period--in my opinion there is not really any evidence for abrupt change in the records shown in Figure 13. Basically the north and central African lakes generally rise and fall in response to the gradually increasing and decreasing summer insolation, hence precipitation (ala Kutzbach, 1981). I believe that there are abrupt climate changes in this region, but they are not well registered by the cited records.				X		We disagree and feel that the nature of the African climate changes is relevant for understanding the hydrologic responses to large changes in the large-scale controls of climate (or boundary conditions). We will try to make that more evident in the text.
2-5	3			I would drop (or greatly revise) the section on the early Holocene of North America-- there is little evidence presented for abrupt climate change with the exception of those related to the final retreat of the North American ice sheet in the early Holocene, hardly relevant to the topic of future abrupt change. Again, I believe that there are abrupt climate changes in this region, but they are not well registered by the cited records.				X		The same answer applies as above in 2-4.
2-6	3			The tree ring portion of the write-up is very good as it documents the range of natural droughts in North America. It makes the important points that natural variability includes evidence for droughts that would be catastrophic today and that we do not have a very good understanding of the origin of such droughts. These points do not rely on uncertain model results. This section does not need much work.	X					

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2-7	3			I would drop the detailed section (box) on the Colorado River flow, tourism, regulation, etc. These points seem rather parochial compared to potentially much more catastrophic results of hemi-continental, decadal drought--large-scale economic disruption, crop failure, famine, etc.				X		We argue that the Colorado River illustrates the more local impact hydrologic change for the US that brings the story closer to home given the geographic emphasis of this chapter. It also wouldn't fit naturally with the main chapter's sections.
2-8	3			The first part of the chapter contains a lot of interesting information and speculation but it has problems also. Presumably this part of the chapter was supposed to cover abrupt hydroclimate change of the future. That is a hard topic because as <u>the AR4 models show--for most terrestrial regions, especially in the tropics, there is complete disagreement in model predictions about even the sign of future precipitation change.</u> Seager writes about two regions where there does seem to be some model agreement (although that is no guarantee that they are right), namely the US southwest and the Mediterranean. On the other hand, continental Africa and South America, for example, have almost no regions where AR4 models achieve any consensus. <u>I therefore, disagree with the conclusion that "wet places will get wetter</u>				X		We disagree and believe that the AR4 results are surprisingly consistent for the geographic region of interest here, North America (see Figs. S11.1, S11.14 and S11.16 in Christiansen et al. 2007). But we will add qualifying statements about subcontinental scale variability. So we will just stick to North America with regards to AR4 model comparisons.

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				<p><u>and dry places drier". Take the Amazon as an important example. Li et al. 2006, has summarized the AR4 model results to show that they range from major drying to major wetting.</u> And she shows that this is largely a result of the great spread of these models in their simulation of EN vs LN-like SST patterns in the Pacific. In other words, I do not have nearly as much faith in the model results as that expressed in this part of the chapter. And I am not heartened by the confusing and sometimes contradictory discussion of the nature of the precipitation trend in the observational data (compare statements on P. 3/line 37, p. 11/line 23, p. 12/lines 13 to 28). This part also has a lot of redundancy. It relies too heavily on one author's work, including at least three papers that are unpublished, thus cannot be evaluated by any reader. In my opinion this part also suffers because it is completely focused on western US drought while not mentioning any other regions--perhaps that was the intention of the report, but I hope not.</p>				X		<p>See comment 2.2 on the mission of this CCSP SAP report. The Amazon is not in the geographic region of interest here. However, we will try to be more specific in what we mean about wet places getting wetter and dry places getting drier.</p>
2-9	3			<p>It is also curious to me that this chapter does not once reference the following chapter on abrupt changes of the MOC/THC. Given the many model simulations (e.g. Vellinga and Wood, zhang and Delworth, Chiang et al., Broccoli et al., Otto-Bliesner et al.) that simulate major changes in tropical hydroclimate accompanying decreased MOC</p>			X			<p>Changes in tropical hydroclimate due to changes in MOC/THC is outside the geographic scope of this chapter. See comment 2.2. We will, however, cross-reference to Section 6 of the MOC/THC Chapter 4 in the SAP 3.4 report.</p>

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				(regardless of cause and effect), as well as the paleoclimate data aligning with these simulations, I think this is a serious oversight.						
2-10	3			Specific questions: (1) Are the scope and intent of the synthesis and assessment product clearly described in the report? Not at all. Are all aspects of this charge fully addressed? I don't know what the charge is. Do the authors go beyond this charge or their expertise? Again, impossible to know.				X		Acknowledged. No further comments can be made. We believe that the scope and intent of the chapter is fine.
2-11	3			(2) Are the conclusions and recommendations adequately supported by evidence, analysis, and argument? I would say that the key findings are not all well supported. Finding 1 has not been discussed. Finding 2 (floods) was not discussed. Finding 4 is undecipherable. Parts of Finding 5 are stated too strongly in my opinion and ignore other forcings for other regions (tropical Atlantic SST for the Nordeste, Sahel, and Amazon; Asian snow cover possibly for the monsoon; antecedent land-surface and vegetation conditions for the monsoon, etc.). Finding 6 is good, but how about similar findings for elsewhere in the world? The last finding is not in agreement with observations over monsoon subtropical China and the subtropical South America Convergence			X	X		<p>Finding 1: Supported in the Introduction. We will sharpen the statement and emphasize NOAA's \$B natural disaster list as showing that drought is the most expensive natural hazard on a year-in year-out basis.</p> <p>Finding 2: True. We will add something about the 1993 flood in an added flood section</p> <p>Finding 4: The wording will be improved. Repeat bottom of pg. 4 (38-42).</p> <p>Finding 5: Take out "and around the world". Reword.</p>

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				<p>Zone region among others. Recommendation 1 is almost undecipherable.</p> <p>Recommendation 3 needs a qualifier about the development of predictive models with high levels of forecast skill: "to the extent that this is even possible." Does anyone honestly believe that such a forecast will ever be possible?</p>						<p>Finding 6 is good. See response to 2-2.</p> <p>There is no discussion in the body of the chapter about floods. We are adding a flood section.</p>
				Reviewer #4						
4-1	3			<p>This chapter is a summary of the state of knowledge about drought variability and its causes during historical and Holocene times, primarily in North America. The report is up-to-date, including many recently published or in press studies. It is reasonably comprehensive and balanced in its treatment of North American drought, although somewhat biased toward the research of the chapter's authors (about a third of the references are by chapter authors). The integration of observational data and modeling studies in evaluating potential drivers of drought is one of the chapter's strengths. In addition, the observation that Medieval droughts were unusual because of their long duration, not necessarily their magnitude, is a finding of particular</p>	X					

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				significance.						
4-2	3			<p>The report is considerably narrower in its focus than the title (Hydrological Variability) implies – <u>water quality is mentioned multiple times</u> – in fact, the first line of the Introduction says “clean fresh” water is essential – but there is no discussion of salinization resulting from drought or other issues related to water-quality variation driven by climate and natural processes. <u>There also is no discussion of the literature on floods</u>, which are an equally important natural hazard. Thus, given that the scope of this report is primarily on drought, it would seem preferable to have a title that reflects the content. <u>The report is also not global in perspective – the majority of the text centers on North America.</u> There is a section on the African pluvial, but no justification for this choice and the exclusion of other global areas where past, present, and future hydroclimate variation are of critical importance, such as Asia and the Amazon. So again, the title should reflect the content. In addition, some statement should be made at the beginning to suggest that North America is being used as a model system, because it is understood in more detail than many other globally</p>			X			We will add a brief statement about water quality issues to the chapter. For examples, salinity changes on the order of those observed for Moon Lake would have clear societal impacts.
							X			See Response to Comment 2-11.
							X			See Response to Comment 2-2.

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				significant regions (or whatever the appropriate logic is).						
4-3	3			There are a number of places, as described below, where I suggest that the wording be changed to be more specific so that the data are accurately and carefully described. In addition, throughout the report the language used to describe model predictions of future climate conveys a greater certainty than I think exists (for example, the phrase "is expected" is used repeatedly in the Introduction to report model results). I would urge more caution and a more explicit statement of model uncertainties.			X			"is expected" will be replaced with "is likely to" (based on SPM1: "Terms to assess likelihood")
4-4	3			In addition to simple technical editing, the document also needs a concerted effort at harmonization of language – right now the transitions from one section and author to another are glaringly abrupt in several places.	X					
4-5	3	1	20	Is it really accurate to say that floods are more localized in time and space? What about the 1993 flooding, which resulted from widespread high precipitation throughout much of north-central North America? The paleoclimatic literature on flooding and high precipitation intervals is certainly more limited than that on drought, but I don't think floods should be dismissed as unimportant, as			X			We are putting something about the 1993 flood in north-central North America in the flood section being added. Add in something about floods in Section 6 on "Other Aspects ..."

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				this sentence implies. I'd suggest sticking to the positive "Droughts occur on sub-continental to continental scales....." etc						
4-6	3	2	13	I would recommend being somewhat more cautious about predictions of future climate, because of the considerable uncertainty in the model predictions. The term "is expected" (the drying is expected) is used here and multiple times in the Introduction, as well as elsewhere in the body of the text. I think that language conveys more certainty than exists.			X			Agree. See response to 4-3.
4-7	3	3	38 + 44	Same comment as 4-6 above.			X			Agree. See response to 4-3.
4-8	3	4	33	the activation of sand dunes and lowering of lake levels also occurred in the last 1000 years, so this sentence does not adequately convey the more extensive drying that characterized the mid-Holocene.				X		Wording will be changed in pgs. 4 and 27 to indicate wide-spread persistent dune activity during the mid-Holocene and the relative magnitudes of the aeolian activity in the last millennium and the mid-Holocene (see Fig. 3.15f)
4-9	3	6	46	What does "it is unclear if they represent anything more than a local expression of external forcing" mean? This sentence seems to be dismissing the importance of Atlantic SSTs. The reasoning needs to be articulated more clearly. Similarly, isn't it appropriate to				X		Co-author Richard Seager will address this issue.

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				at least mention the body of work that argues for the influence of the AMO on continental drought, even if the authors conclude that it's influence is secondary to that of the Pacific?						
4-10	3	8	13	Is it wise to use popular literature (Egan 2006) here?				X		Books are appropriate.
4-11	3	10	12	find an alternative and more appropriate word than "clean"			X			Replace "rarely clean" with "difficult to interpret"
4-12	3	13	24-30	The first few sentence in this paragraph refer to prehistory – isn't this section on historic drought impacts? If these sentences are retained, given the Yancheva reference to the Tang Dynasty, shouldn't Hodell et al. 1995 be used to reference the Classic Maya decline?			X			Hodell reference will be added. Remove "followed by rebellion" because we really don't know.
4-13	3	13	41	This is among the places where water quality impacts are mentioned as significant, but there is no discussion of the issue. Perhaps one of the recommendations should be that the issue deserves more attention.			X			Sentence not necessary. It has been removed.

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4-14	3	14	34	It is unclear if "this drought" refers to the 19 th c or the recent drought.			X			Add "21 th century" to "this drought" and reword the sentence to reflect the importance of increased demand to the impact of the 21 st century drought.
4-15	3	16	31	I think this sentence should be more specific. The 16 th century mega-drought exceeded 20 th century drought in intensity in some regions, but not all. Spatial variability is an important issue.			X			Add "in many regions" to the end of the sentence.
4-16	3	17	23	The term "severe" is used here – I think the terminology to describe drought needs to be more carefully defined here and in general. The 4 Medieval droughts in Figure 3.8 are widespread, but not necessarily large in magnitude (which is what I think of when the term severe is used). Individual site data tell us that, at some locations, the drought was large in magnitude relative to the 20 th c, but at other locations it was not. The point is made later that Medieval droughts were prolonged and widespread but not necessarily more severe, but the report needs to be clear in all instances. It is also important to make clear in this paragraph that the authors are talking about areal severity, not drought magnitude, because the 16 th century "megadrought" is a relatively minor feature on Fig. 3.8. Fig. 3.9 conveys a different impression, where the magnitude of 16 th century drought is approximately equal to			X			Replace "severe" with "wide-spread". This covers the areal extent issue that contributes to the overall impact of "severe" droughts. The last two sentences have also been changed for clarity.

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				some of the dry intervals of Medieval times.						
4-17	3	Section 3.3		It is relevant to mention the research in the Nebraska Sand Hills that suggests that atmospheric circulation may have been very different in Medieval times – the dune sands provide direct evidence that the wind fields were different than at present (Sridhar V, Loope DB, Swinehart JB, Mason JA, Oglesby RJ & Rowe CM 2006. Large wind shift on the Great Plains during the Medieval Warm Period. Science 313: 345-347).			X			This reference will be added.
4-18	3	20	16	“has largely skipped” – I’d suggest adding “many of” or somehow tempering this statement. I think the agricultural areas of the central Great Plains would strongly disagree with the statement that the recent droughts “skipped” them.			X			“Skipped” has been removed and the sentence restructured to be clearer about what was meant.
4-19	3	20	18	It would also be appropriate to mention the eolian record of major drought here (Mason JA, Swinehart JB, Goble RJ & Loope DB 2004. Late-Holocene dune activity linked to hydrological drought, Nebraska Sand Hills, USA. Holocene 14: 209-217).			X			This reference will be mentioned.

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4-20	3	20	26	It is also worth pointing out here that the 20 th century is not only "benign" in terms of the magnitude of the drought, but also was unusually wet, as evidenced in Fig. 3.11			X			The "benign" sentence will be modified.
4-21	3	21	10-12	Do we really know that 21 st century climate will change more than that observed in proxy records of the last millennia!			X			This whole paragraph is being rewritten for clarity and expression, but the general observation can be justified by noting the magnitude of changes in the large-scale controls of climate among the periods discussed here.
4-22	3	22	7	I don't see much evidence for "abrupt" changes in Figure. 3.13				X		We disagree. The level of "abruptness in some of the records in Fig. 3.13 are pretty obvious. Panels 3.13d-f will be deleted (not discussed).
4-23	3	Sec 4.2		This section needs some justification. It's not clear to me why a document that up to this point has focused entirely on North America and on drought has such an extensive section on the Holocene pluvial in Africa – this seems inappropriate given the balance of the document. And why Africa? Why not discuss Holocene droughts in Asia or South America or Australia? I think this section should be eliminated. Alternatively, the chapter should be expanded to include a more global perspective that is parallel in structure to that for North America and includes a			X			Reviewer raises valid point. We will justify it at the end of pg. 21. The transition from humid to arid conditions in Africa is part of the variations in the "global monsoon". Also see Comment 2.2 and 2.4.

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				discussion of areas where hydroclimatic change is critically important, such as Asia and the Amazon.						
4-24	3	26	30	“across most of the continent” simply isn’t true. What strikes me most about Figure 3.14 is the sparse distribution of sites – there’s virtually nothing in the SE, in the central and southern Plains, or in the southwest. So the figure should be described appropriately about where we have evidence of aridity and where we have no or sparse data			X			Wording is changed to “most sites”.
4-25	3	26	39	Given that you’re mentioning evidence for aridity in pollen and eolian deposits, shouldn’t you also mention lakes (salinity and lake level change) here (for example, Fritz SC, Metcalfe SE & Dean W 2001. Holocene climate patterns in the Americas inferred from paleolimnological records. In: Markgraf V (Ed) Interhemispheric Climate Linkages pp 241-263). Academic Press). It would also be worth mentioning the geomorphic evidence from the southern Plains (for example, various papers by Vance Holliday), which is not portrayed in any of the data included in fig. 3.14 or 3.15.			X			Will add Fritz et al. 2001. Other refs will be added as well. Holliday results are included in Fig. 3.13f, based on the summary in Forman et al. (2001)

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4-26	3	27	12-13	There are several recent papers from western NA that provide evidence for continental moisture fluctuations linked to high-frequency variation in the Pacific (for example, Anderson L, Abbott MB, Finney BP & Edwards ME 2005. Palaeohydrology of the Southwest Yukon Territory, Canada based on multiproxy analyses of lake sediment cores from a depth transect. The Holocene 15: 1172-1183 and Stone JR & Fritz SC 2006. Multi-decadal drought and Holocene climate instability in the Rocky Mountains. Geology 34: 409-412). These might be worth referring to here.				X		Although the PDO is a useful indicator for diagnosing western North American climate anomalies, it is likely simply a "reddened" or integrated index of ENSO/SOI (Newman et al. 2003; Schneider and Cornuelle (2005) See also 4-28 ;
4-27	3	27	31-39	The specific reference to Steel Lake sticks out because no other single site has been described elsewhere in the document. I suggest this paragraph be deleted.				X		The use of Steel and Elk Lakes are used as examples. Reorganize to go from general to specific.
4-28	3	Sec 4.3		There is some evidence, albeit limited, for Pacific SST influence on mid-continental drought throughout the mid-Holocene (Stone JR & Fritz SC 2006. Multi-decadal drought and Holocene climate instability in the Rocky Mountains. Geology 34: 409-412), as well as in the late Holocene (Holocene Gray ST, Fastie CL, Jackson ST & Betancourt JL 2004. Tree-ring based reconstruction of precipitation in the Bighorn Basin, Wyoming since 1260 AD. Journal of Climate 17: 3855-3865). It would seem appropriate to mention				X		The growing belief is that North Pacific SSTs is just a reddening of ENSO variability.

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				this somewhere in this section on the mid-Holocene and in the late-Holocene section, given all the discussion earlier in the document on Pacific forcing of continental drought.						
				Reviewer #8						
8-1	3	2	19	This would be great, but it's not a recommendation. It's just a wish.	X					
8-2	3	3-20		this chapter is unbalanced with way too much emphasis on North America compared to the rest of the world. I don't see that the charge to this CCSP was this specific, and while I understand that the lead authors may be more knowledgeable on N. American issues than some other regions, that is no excuse for ignoring them. For instance, nowhere in this chapter is a even the mention of the Sahel drought - surely this is the largest and most important drought event of the 20th Century? Frankly, I'm flabbergasted that this is not thought worthy enough of mention. There is not even a minor justification for the focus on N. America!				X		See the response to 2-2.

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8-3	3	25	14-28	This feedback analysis is very limited in scope. Specifically, it only looks at the local feedback - i.e. local precip changes compared to local vegetation changes. That is unlikely to be the dominant response, since most feedbacks will be non-local in effect (i.e. warming further north might increase precipitation further south). Therefore, a caveat is required here pointing out the local nature of this result and it's incompleteness.				X		We disagree, but wording will be changed for clarity.
8-4	3	32	12	Models suggest increases of precip on that order, but it is not (yet) an observed fact. Increases in aerosols may play a significant role in modulated the hydrological cycle independently of temperature - and may well have done during the 1970s-1980s Sahel drought which needs to be discussed!				X		The Sahel drought is not our charge (see the response to 2-2.).
8-5	3	33	29	A relevant comparison between solar-driven and GHG-driven tropical hydrological changes can be found in Shindell et al, 2006 (GRL).				X		Other references like Mann et al. (2005) and Emile-Geay et al. (2007) should be added, but not Shindell et al. (2006).
				Reviewer #18						

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18-1	3			I have now finished reading the chapter I was assigned, and it looks all right to me. I know that this is usually considered a not-so-useful comment from a reviewer, but that's my assessment. I could always complain about the fact that they did not cite any of my work (such as the stochastic modeling of hydroclimatic episodes), but that's a moot point.	X					
18-2	3			Congratulations to the authors for all their hard work.	X					
				Reviewer #24						
24-1	3			The chapter addresses each of the questions it was charged to address. The authors have done a good job describing the past history of drought and hydrologic change, the role of large-scale forcing including the modes of ocean-atmosphere variability that may be implicated, the role of natural and anthropogenic radiative forcing changes, and the potential measures that would signify that abrupt hydrologic change is underway. There are some minor issues that need to be dealt with, as described below. In some cases,	X					

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				there are important problems that need to be fixed before the chapter will be suitable for publication.						
24-2	3	1	15-17	"...are the greatest natural hazards..." is an over-statement. "...among the greatest natural hazards..." would be defensible. See comment #17 below.			X			"Arguable" will be replaced with "among".
24-3	3	1	19-21	While the statement made is technically true for any one specific flood event, the tendency for flooding and in particular, changes in the duration and frequency of floods, can certainly change on large spatial scales and long timescales. This should be reworded.				X		Wording has been changed.
24-4	3	1	35	the wording 'especially responsible' is overly strong (see comment 4 below) as well as awkward wording. 'especially conducive to' would be more appropriate.			X			Fixed already.
24-5	3	1	39-44	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 is available, i.e. the Mann et al (2005) reference cited elsewhere in this chapter. 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			X			The problematic wording will be modified.

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				drought conditions, including 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).						
24-6	3	1	46	The use of the term 'boundary conditions' here is somewhat confused and inconsistent. If anthropogenic impacts are termed a 'boundary condition' rather than a forcing, than it is unclear why changes in explosive volcanism and solar irradiance (possible explanations for long-term prehistoric changes in drought) are not also considered changes in 'boundary conditions'. More conventional terminology classifies each of these as 'forcings', while 'boundary conditions' are reserved for the slowly changing constraints on the climate system (i.e., continental configuration, earth-orbital configuration, etc).			X			"Boundary conditions" will be replaced with "large-scale forcing" or "large-scale controls" as appropriate.
24-7	3	2	19-22	What is meant by "predictive models of drought at timescales of years to decades". Aren't already-existing climate models predictive models of drought? Is it meant that the skillfulness of these models in predicting drought needs to be improved? Certainly the path to that is better climate model-based prediction of decadal-centennial timescale climate change. This is certainly an existing prior area of the climate modeling			X			Already taken care of.

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				community. Perhaps the authors simply mean here to endorse this activities as a high priority undertaking.						
24-8	3	2	28-32	See comment 24-7. It is unclear how what is being proposed is different from current climate modeling priorities.			X			Already being done.
24-9	3	2	34-42	Tree-rings have been a key source of information in the reconstruction of past drought, but they have some significant limitations (e.g. resolving centennial and longer timescale variability). For this reason, it is absolutely essential that equal priority be given to the development of alternative high-resolution proxy records of hydrological change.			X	X		The 34-37 Bullet is not restricted to tree rings. The 39-42 Bullet has been modified. "other proxies" added.
24-10	3	3	1-4	Well stated. This is indeed a key goal, and it is a high-priority activity of the international PAGES/CLIVAR intersection as well.	X					
24-11	3	3	37-38	A similar point was made earlier (though the argument was somewhat different) by Hoerling and Kumar (2003) and this earlier work should be cited [Hoerling, M. P., and A. Kumar, 2003: The perfect ocean for drought. <i>Science</i> , 299, 691-694]			X			Agree. We will cite Hoerling and Kumar.

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24-12	3	4	22-23	See comment 24-5 above. The qualifier 'perhaps' applies equally to the conclusion that the droughts in question were specifically associated with tropical Pacific SSTs.			X			"perhaps" has been removed.
24-13	3	4	24	It is incorrect that past droughts are necessarily 'dynamically distinct' from potential future drought. While the radiative forcing may be different (e.g. volcanic and solar instead of GHGs and tropospheric aerosols), the <i>dynamical mechanism</i> is potentially the same (at least this appears true for several of the models in the IPCC AR4 where the Clement et al dynamical thermostat mechanism indeed appears to dominate the response of ENSO to GHG forcing)! This needs to be reworded.			X			Wording is being changed to reflect the reviewer's concern.
24-14	3	4	41-43	[this applies also to 3rd 'key findings' bullet item on page 1]: Actually, the potential categorization as 'abrupt change' runs deeper than this. It isn't just the sluggishness of human response to drought change that is relevant. There is sufficient evidence in the paleoclimate record to show that the transitions to drought themselves have often been abrupt. To the extent that the causative factors are associated with e.g. ENSO, it is well known that the underlying non-linear dynamics potentially allows for abrupt changes in response to changes in the	X					

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				underlying forcings (in fact, this is discussed to some extent in first paragraph of page 11). In this sense, the potential for abrupt change is not fundamentally different than for changes in the meridional overturning ocean circulation, or ice sheet collapse.						
24-15	3	6	22	Does the question mark after 'factors' indicate a lack of confidence in the statement? Or is it just a stray character?	X					Stray character.
24-16	3	6	44-46	'even though it is unclear they represent anything more than a local...'. A more diplomatic statement would be 'though other have argued that the anomalies in tropical Atlantic SST may simply represent a local...'. And the reference for this would be Mann and Emanuel (2005) [Mann, M.E., Emanuel, K.A., Atlantic Hurricane Trends linked to Climate Change, <i>Eos</i> , 87, 24, p 233, 238, 241, 2006].			X			Will address.
24-17	3	7	12-13	'...no model experiments suggest...'. Perhaps that is the case, but some empirical studies nonetheless do suggest that there is a connection [Gershunov, A., Barnett, T.P., Interdecadal modulation of ENSO teleconnections, <i>Bull. Amer. Soc.</i> , 2715-2725, 1998] and this should be acknowledged here.			X			Offending sentence removed.

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24-18	3	7	41	'Greatly inflated by the 2005 hurricane season' isn't a defensible statement. It would seem imprudent to write 2005 off as an aberration, and certainly the reinsurance industry does not see it that way. The previous (2004) storm season was also a very costly one. And many would argue that the high Atlantic tropical cyclone activity of the past decade and the associated destruction is a sign of even greater impacts to come. The fact remains that tropical cyclone costs currently exceed those due to drought. It is neither appropriate nor necessary to diminish the threats posed by other natural phenomena for the sake of bolstering drought as a societal threat. Its not a competition.			X			"Greatly inflated" has been deleted.
24-19	3	11	12-15	There is a rather egregious sin of omission here. The attribution of these changes specifically to natural (volcanic and solar) forcing of ENSO over the past millennium was provided by Mann et al (2005) (discussed elsewhere in this chapter), well before the recently <i>submitted</i> paper of Seager et al that is referenced. Proper credit needs to be given here.			X			Mann et al. 2005 will be added.
24-20	3	12	8-11	I agree with the point being made, but in fact even further caveats are warranted. The mean state and amplitude of variability of ENSO cannot be entirely disconnected. In simple low-order models, for example, the two are inextricably linked, due to the asymmetric nature of El Ninos and La Ninas relative to			X			

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				the base state. There is some evidence that this is the case in the observations as well. Should one therefore trust the AR4 simulations which, on average, predict a Walker Cell weakening, if we know that they are still not getting basic attributes of ENSO right (for example, the structure of the tropical Pacific ITCZ itself)? On average, the AR4 simulations do shown a weakening walker cell, but a significant minority of simulations suggest just the opposite. And there is a near even split as to whether variability is predicted to increase or decrease over time. I think the authors could be even more circumspect here about the true response of the Walker circulation to anthropogenic climate change.						We will address the concerns brought up here.
24-21	3	12	13-15	I don't see how this statement follows at all from the preceding statements.			X			Taken care of b 24-21 response.
24-22	3	12	35-46	There is a somewhat conspicuous absence here and elsewhere in the early part of the report of any discussion of Asian Monsoon failure, as well as its relationships with ENSO. Certainly this is an important aspect of modern and potential future hydro-climatic change. There is some discussion of the role of changes in the Monsoons later on in the report in the discussion of paleoclimates, so it				X		See the response to 2-2.

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				would be helpful for some discussion to be added here in the context of modern and future climate change.						
24-23	3	19	20-23	It is unclear to me why the Seager et al (2007) approach goes 'a step farther' than Graham et al? Isn't it just the opposite? Graham et al (2007) were certainly aware of the Cobb data, but used a more comprehensive ENSO proxy dataset to provide a continuous long-term ENSO SST forcing pattern. The basic approach is identical, but the Seager et al (2007) analysis is limited by the disjoint and short nature of the Cobb fossil coral segments.			X			Changes made.
24-24	3	19-20	44-46 1-10	There is an omission of another key supporting line of evidence for the negative relationship between radiative forcing and ENSO response, namely the significant relationship established between tropical volcanic (negative radiative) forcing and El Nino back through the early 17 th century based on proxy records of explosive volcanism and ENSO as described by Adams et al (2003) [Adams, J.B., Mann, M.E., Ammann, C.M., Proxy Evidence for an El Nino-like Response to Volcanic Forcing, <i>Nature</i> , 426, 274-278, 2003]. This is an important example because the timescale of the response to volcanic forcing is short, and			X			References will be added.

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				some of the factors that may mitigate the 'tropical Pacific thermostat' response to longer timescale (solar or GHG) forcing, such as subducting extratropical water masses feeding back on the tropical Pacific thermocline, are less likely to come into play—therefore, the response to volcanic forcing represents a best case scenario for observing this effect. It is noteworthy in this context that it is primarily volcanic (and not solar) forcing which drives much of the response shown in Mann et al (2005), including the La Nina like state of earlier centuries.						
24-25	3	21	12	It is both inappropriate and imprudent to cite just Hegerl et al work here. As with any individual study, there may be specific criticisms; in the case of Hegerl et al (2007) for example, a rather significant criticism of their approach was published in <i>Nature</i> this year by Tapio Schneider. It is far more sensible to cite assessments or review papers that examine the results of multiple studies, and find that the key conclusions (e.g. the anomalous nature of recent warmth) are robust across numerous studies. In this regard, more appropriate cites would be IPCC AR4 chapter 6 (section 6.6) and the Jones and Mann (2004) review paper [Jones, P.D., Mann, M.E., <i>Climate Over Past Millennia, Reviews of Geophysics</i> , 42, RG2002, doi: 10.1029/2003RG000143, 2004].			X			References will be added.

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24-26	3	21	12-18	The statements here are simply untrue. The forcing over the 20 th century is dominated by anthropogenic impacts, and is therefore fundamentally different from the forcing that prevailed over previous centuries, which is believed to be primarily natural (e.g. solar and volcanic, with perhaps a bit of astronomical and human land use thrown in). In fact, it could be argued that this contrast even exists within the instrumental record (i.e. the early 19 th century vs. the late 20 th century). There is good reason to study changes in the deeper past. For example, the past few millennia (in fact, arguably, the past several million years!) does not contain any 'warm climate' analogs to what we might experience by 2100 under business-as-usual anthropogenic activity. But the motivation for studying deeper time changes should not rely on a straw-man characterization of the situation for the more recent past.			X			We disagree, but we will clarify the relative differences in the forcing among the intervals we discuss (early and mid Holocene, the last millennium, the first half of the 20 th century, the last half of the 20 th century, and the 21 st century).
24-27		21	23-25	The statement here is inappropriately weak to the point of being misleading. Not only is there not any evidence that the medieval period was globally warmer than today, the AR4 report (chapter 6, section 6 and also the Working Group I SPM) stated in unambiguous language that it is <i>likely</i> that recent warmth is greater than that seen for at least the past 1300 years. The wording should be revised appropriately.			X			(We think this comment pertains to text on p.31, not p. 21). Agree. We will use IPCC language.

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24-28	3	33	24-25	The statement is simply wrong (see also comment #23 above). It is very clearly demonstrated in Mann et al (2005) that it is actually the <i>volcanic</i> radiative forcing (i.e. little explosive tropical volcanism prior to AD 1600, and much greater activity thereafter), and not the much weaker solar radiative forcing, that primarily drives the observed changes in tropical Pacific climate. The authors need to revise the discussion so that it is consistent with what has actually been demonstrated in the peer-reviewed literature regarding this issue. The current wording greatly overstates the relative role of solar forcing here.			X			There is a matter of interpretation here that may differ from the reviewer's take on things. We will discuss this.
24-29	3	33	27-28	Same problem as comment 24-28 above. The actual modeling work that has been done in this area (i.e. Mann et al, 2005) indicates that it is primarily volcanic, and not solar, radiative forcing that appears responsible for the main long-term trends in tropical Pacific climate.			X			See the above response.