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; but see "Notes on Response."	Notes on Response
				Reviewer #8						
8-1	2	2	7	'even <' seems misplaced			X			Formatting, will be take care in final version
8-2	2	2	17	'air temperatures' too imprecise. If you want to be specific, name locations and seasons. An implied statement about global mean temperatures cannot be supported from the literature.		X				Changed to Arctic summer temperatures
8-3	2	5	6	'air temperatures' too imprecise. If you want to be specific, name locations and seasons. An implied statement about global mean temperatures cannot be supported from the literature.		X				Changed to Arctic summer temperatures
8-4	2	5	7	4-10m? it is 4-6m later on in the chapter and in the referenced literature.		X				Revised: 4-6 m

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8-5	2	5	28	distinguish between climate models which generally don't have responsive ice sheets, and ice sheet models.		X				➔ Climate models
8-6	2	20	3	'probable' is not in the IPCC uncertainty lexicon. Is it more likely than not, likely or very likely? Check for other occurrences.			X			We used: likely
8-7	2	20	21	'a' modelled response. not 'the' modelled response.			X			
8-8	2	24	4	'1850-2000 average RATE'			X			
				Reviewer #14						

Comment from Peer Reviewers					Authors' Response					
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14-1	2			This is a well-written overview of measured change and the potential for rapid change from ice sheets and small glaciers. In particular, uncertainties are clearly expressed, explained and discussed throughout the document. It is reasonably comprehensive although my comments below focus on one significant omission.	X					
14-2	2			The recommendations are well-thought out (subject to omissions noted below) and well-supported by the data and discussion. In particular the need for a step-change in ice sheet modelling effort is well identified and I support it strongly as one of the main conclusions. Observations have provided some 'surprises' in recent years (e.g. ice shelf buttressing, movement of water under ice sheets, ocean-ice interactions) and the modelling community are scrambling to catch up. Moreover, it is likely that there will be further observations requiring enhanced model development and this community needs to be supported in its efforts to keep up with the observations.	X					
14-3	2			The report addresses all of the questions posed in the prospectus, namely: • What is the paleoclimate evidence regarding rates of rapid ice sheet melting? *(BUT see notes below on importance of the palaeo record)* • What are the recent rates and trends in ice sheet mass balance?	X					

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				• What is the impact on sea level if the recently observed rapid rates of melting continue? • What is needed to model the mechanical processes that accelerate ice loss?						
14-4	2			I think the fundamental conclusion (also noted in Executive Summary Chapter), that the IPCC AR4 sea level estimates are likely too low, is justified by the arguments presented in the report.	X					
14-5	2			*The importance of the palaeo record* I think the report does not sufficiently reflect the importance of good palaeo-records of ice sheet history and oceanographic change. It does a good job of discussing palaeo-sea level records and their implications but largely neglects the ice sheets and oceans themselves. This is important for at least five reasons: - Records of ice sheet history are important to understand the long-term dynamics, or 'trajectory' of the ice sheets. For example, geological observations by Stone et al (2003) and Conway et al (1999) have shown that the Ross Sea sector of the West Antarctic Ice Sheet (WAIS) has been thinning and retreating progressively for > 10,000 years, and at an average rate comparable to the satellite altimetry-derived present-day rate (Davis et al., 2005). The implication is that the behaviour of the WAIS in this region continues to be dominated by its long-		X				Discussion of records of ice-sheet history added.

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				term response to the end of the ice age.						
14-6				- Robust initialisation of ice sheet models used to predict future change is not possible without an understanding of the longer-term dynamics		X				Discussion added.
14-7				- Constraining the sources of "meltwater fluxes" requires detailed records of ice sheet history. These can place bounds on the volume of ice lost from particular regions during the critical periods of sea level rise, and therefore help constrain possible mechanisms and causes of the rapid ice loss.		X				Discussion added.
14-8				- Correction of satellite altimetry and GRACE data requires robust models of glacial-isostatic adjustment (GIA). In turn these GIA models require well-constrained ice loading histories to drive their evolution through time to the present-day. These models remain one of the largest sources of error on altimetry and gravity data, and the SAP correctly notes that this can be addressed through geodetic (GPS) measurements(p. 16, line 10), but neglects the importance of improved geological records of ice sheet history as a second way to achieve this.		X				

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14-9				- Marine sediment records potentially provide a means to investigate oceanographic change in the period before the (very short) observational record. So, for example, there should be considerable effort directed towards improving existing coverage, and developing further proxies or tracers for the 'warmer' waters that reach the continental shelf and interact with ice sheet margins.		X				Comment added.
14-10	2			I recommend that records of ice sheet history and marine records of change are incorporated more fully into the discussion throughout (including key findings and recommendations), and that there is a recommendation on p.3 "to improve coverage of longer-term (centennial to millennial) records of ice sheet and ocean history, from geological observations".		X				Added
14-11	2	3		Recommendations (p.3) – the need for better oceanographic measurements and monitoring is discussed within the document (e.g. p. 36) but is not explicitly identified in the Recommendations (lines 22-24 on p.3 discuss the sub-ice shelf component but this does not explicitly recognise the need for improved oceanographic monitoring elsewhere; along the shelf break, for example).			X			Added in the recommendation: Support field, theoretical and computational investigations of processes beneath and along ice shelves …
14-12	2	4	11	The cause, *source ice sheet(s)* and mechanism of the…..		X				Added

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14-13	2	4	17	change '> -200' to '<-200'			X			Revised to: > -160
14-14	2	5	25	remove 'early' – potentially subjective		X				
14-15	2	7	1	Antarctic ice shelf area….		X				
14-16	2	7	22-24	doesn't follow from preceding argument, and is only finally explained on p. 39. Without further explanation of how this conclusion is reached this seems an unhelpful way to finish the Summary Section.		X				Revised
14-17	2	7	30	definition of RSL is incorrect. RSL is the *net result* of local (glacio-isostatic or hydro-isostatic) and global (eustatic) changes.		X				Revised

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14-18	2	9	1	strong positive relationship (not inverse). Alernatively this could be expressed in the same way as in the Exec Summary where it notes the strong inverse relationship between CO2 conc and global ice volume.		X				Revised
14-19	2	9	24	the 19 ky-MWP and MWP-1A are probably the most important intervals of rapid SLR in the geological record, but is there a reason why discussion of MWP-1B has been left out of this discussion ?		X				Now discussed in text
14-20	2	10	2	this link between hemispheric warming and meltwater fluxes needs to be referenced and/or shown graphically.		X				References provided
14-21	2	15	21	change 'basal' to bedrock' or 'crustal'.			X			
14-22	2	15	21	This is inferred from GIA models…..			X			Added: glacio-isostatic

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14-23	2	18	22	change 2.6 to 2.5			X			
14-24	2	24	21	(and paragraphs following) The Younger Dryas temperature changes cited vary from mean annual (Severinghaus) to summer only (Denton). Does this affect the way the estimated meltwater contribution on p. 25 (line 4) is calculated ?		X				Answer to question is "No", but text has been clarified.
14-25	2	25	13	'crudely' - need to explain briefly how this was done			X			A brief explanation has been provided.
14-26	2	26	14	change 'probably' to 'does' (see above)			X			
14-27	2	26	18	change 'has to put' to 'places'			X			

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14-28	2	27	10	replace Cook et al with Morris and Vaughan (2003).			X			
14-29	2	32	8	insert Payne et al (2004) into references			X			
14-30	2	39	21	figures for SLE of ice sheet volume not consistent with summary (p. 1)			X			Revised: 7 m would come from West Antarctica
14-31	2	42	Fig 2	no representation or illustration of WAIS configuration with bedrock below sea level. Potential surface melting of ice shelf (i.e. Antarctic Peninsula) also not noted				X		This is only a schematic graph to show ice sheets and ice shelves
14-32	2	43	Fig 3	Fig 3 - poor picture of an ice shelf. There are better photographs or satellite images of Antarctic ice shelves.			X			Will include a more typical ice shelf picture if we find one that shows all the features discussed in the legend

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				Reviewer #23						
23-1	2			This chapter generally meets the criteria of correctness, relevance, attention to uncertainties, lack of bias, balance, documentation, and impartiality. It is heavily weighted toward the ice sheets of Antarctica and Greenland, where much current attention is focused, even though the "small glaciers" are likely to continue to be more important to society in this century. Note that the Executive Summary defines abrupt changes in time scales of one or more decades. Changes of importance to society on this time scale are happening due to the sea-level rise caused by small glaciers, whether the ice sheets will cause similarly rapid changes in this century is an open question.	X					
23-2	2			It suffers a bit from being written by a committee, with, for instance, multiple definitions and terms for "mass balance." Also, the terminology varies, for instance, is the Jakobshavn a glacier, an isbrae, a tongue, or an ice shelf? (its proper Greenlandic name is not mentioned). It is easy to tell which sections were written by the paleo , the ice sheet, and the small-glacier communities; perhaps an aggressive editor could bring the two together. One step might be to print out the			X			Changed to:" Jakobshavn Isbræ , also known as Jakobshavn Glacier and Sermeq Kujalleq (in Greenlandic)", and edited the text to call the glacier Jakobshavn Isbræ throughout the text.

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				subheads and then check to see that the content of each paragraph applies only to that subhead.						
23-3	2	1	4	I'd change "nearly" to "about" to reflect most recent results (Meier et al 2007).		X				
23-4	2	1	28	add "as well as other tidewater glaciers and ice cap outlets."		X				
23-5	2	4	22	I would strongly support "• develop new models using scaling or other modern stochastic methods to obtain a more reasonable estimate of the areas and other characteristics of the multitude of unmeasured small glaciers, for modeling of their variations and sea-level contributions." [will GLIMS ever be sufficient?]					X	This recommendation has not been included; GLIMS is currently updating their assessments and a new initiative has been launched to cover a larger number of glaciers worldwide. .
23-6	2	5	23-25	It is interesting that the future contributions of small glaciers is discounted here, but in the paragraphs above no such limitation is given for the ice sheets.		X				Text has been clarified.

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23-7	2	6	4-8	These statements need qualification. Meltwater runoff is not measured. Increased surface melting area does not lead directly to increased runoff because of internal accumulation. Precipitation has decreased recently.		X		X		Text has been clarified on the meltwater. Precipitation actually has been increased (i.e., Greenland ice sheet)
23-8	2	6	22-30	The first part of this paragraph suggests that basal melting from warmer ocean water is the main cause of ice shelf breakup, but the last part of the paragraph (p.8) correctly points out the importance of air temperature on this breakup. Obviously, we're talking about time scales here.				X		The first paragraph (ocean / ice shelf interaction) does not refer to ice shelf break-up.
23-9	2	11	1	a reference to the book on mass balance techniques by Bamber and Payne would have obviated pages of discussion.	X					
23-10	2	11	2	add, "of snow in shallow cores or pit walls or the"		X				Text has been revised
23-11	2	11	9	after "precipitation" add "internal accumulation." This a major term for ice sheets and polar glaciers that is often ignored, invalidating the results.		X				

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23-12	2	11	21-22	Say "balance." "Net snow accumulation" is ill-defined; what is meant is "snow accumulation at the surface plus internal accumulation less surface or subsurface melting, together with losses due to ice discharge, ….."		X				Text will be clarified.
23-13	2	11	23	after "altimetry" add leveling or photogrammetry"		X				
23-14	2	11	26	say. "seasonal or annual mass balance…"		X				
23-15	2	12	1	Balance		X				
23-16	2	12	10	Again, internal accumulation is ignored!				X		Not appropriate to mention it here.

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23-17	2	12	14	ground-based surveys, photogrammetry, or from satellites…"		X				
23-18	2	12	19	"is rarely measured using hydrologic or oceanic observations. Traditionally, it is inferred from stake…"		X				Text has been clarified.
23-19	2	12	27-28	These numbers seem extremely optimistic to me, but perhaps the problem is 'per cent of what?'. Also, errors for glaciers and ice caps have been analyzed and reported very well.		X				They are not really. 5% for Greenland accumulation is 20-25 Gt/a, consistent with the 35 Gt/a for total balance. We added an error estimate for Antarctica of 7%.
23-20	2	13	2	These errors seem more reasonable. We estimated an error of +/- 95 Gt/a for glaciers and ice caps as of 2006 (see Meier et al, 2007).	X					
23-21	2	13	4	increasing?				X		Yes, according to the latest satellite observation, the Greenland ice sheet is increasing in height above 2000 m elevation. See Johannessen, O., K. Khvorostovsky, M. Miles, and L. Bobylev. 2005. Recent ice-sheet growth in the interior of Greenland. Science, 310, 1013-1016.

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23-22	2	13	4	This is probably quibbling, but most of the authors I read use h or H for ice thickness and S for area.	X					
23-23	2	13	14	The pioneering work in this field was by Echelmeyer and colleagues at UA-F, and should be credited.			X			We will cite Arendt et al. (2002) here (already in Reference list) to acknowledge the University of Alaska pioneers.
23-24	2	15	15-20	internal accumulation increases density			X			Text has been clarified will be clarified.
23-25	2	16	15	Pages 13-16 already discuss primarily Antarctic and Greenland mass balances; pages 17-20 just amplify these results. This could be rearranged or edited into a more coherent story. Perhaps the problem is that pages 11-16 pay lip service to small glaciers in 'techniques' and 'errors' but this doesn't seem necessary considering the fuller exposition beginning on page 21.						This will be addressed as part of the written-by-a-committee problem (see R23 Comment 2).
23-26	2	21	10	The WGMS data set is not as complete as those published by Dyurgerov in cooperation with the National Snow and Ice Data Center, nor have the WGMS data been quality-checked. Dyurgerov is widely used (together with Cogley's) , and formed the main data set for the Kaser et al paper and the last three IPCC reports..See			X			Text has been clarified.

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				(http://instaar.colorado.edu/other/occ_papers.html) and (http://instaar.colorado.edu/other/occ_papers.html)						
23-27	2	21	22-23	I strongly disagree with this statement! The results in Meier et al (2007) have been appropriately corrected for area distributions and thus minimize the effect of the small European glacier contribution, and they show a marked acceleration since 2001 that is reasonably consistent with the curve C05a. This acceleration is not due to a local anomaly in 2003. The last pentad in Kaser et al (2006) does suffer from incomplete recent data. It certainly is true, as mentioned in lines 24-25, that spatial bias is a problem and that this may be apparent in the difference between the various curves for the most recent pentad in Kaser et al.			X			This apparent disagreement will be resolved by explaining more clearly that C05a is a crude arithmetic average. It is discordant with all of the other curves in Kaser et al. (2006) (including R23's).
23-28	2	22	1-2	This statement is true only if the total balance is considered (the surface mass balance of a calving glacier in equilibrium must be positive to balance the calving loss). Also, tidewater glaciers may be unstable with a long time-scale oscillation between slow growth and brief, rapid disintegration (e.g., Meier and Post 1987).			X			The asymmetry in the behaviour of tidewater glaciers (slow buildup, rapid collapse) will be expanded upon here.
23-29	2	22	8-13	This comparison is not valid because different areas were used. Larson et al did not cover the big iceberg producers such as Columbia Glacier. Also, the Arendt et al results for the Gulf of Alaska glaciers were nicely confirmed by space				X		These comments are under consideration, but note that Larsen et al. and Arendt et al. are not being compared; they are two separate "illustrations".

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				gravity (GRACE) results (Meier et al 2007). Also, the statement in lines 13-15 overstate our ignorance: we do know the rate of calving (Gt/a) of many glaciers around the Gulf of Alaska, Patagonia, Devon Island, and the Russian Arctic. Unfortunately these results have to be gleaned from original sources; they are not reported by WGMS.						
23-30	2	22	19	not an instability.			X			Agreed; clause deleted.
23-31	2	25	10-11	Meier et al estimated 240 +/- 128 mm by 2100 as an upper bound (assumes acceleration continues at present rate) but only 104 +/- 25 mm as a lower bound (acceleration ceases). Raper and Braithwaite estimate (extended) falls into this range. The big problem here is that the global area/thickness/size distribution of glaciers, and thus their changes with non-zero mass balances, is poorly known because a world glacier inventory was never finished.			X			Text will be clarified. R23 is correct in drawing attention to a lack of basic facts as a hindrance to prediction.
23-32	2	34	17	"100s to more than 10,000 meters per year, and glacier surges, tidewater glacier flow, and ice stream motion are…"			X			revised

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23-33	2	35	2	Lots of more explicit literature on this subject. See, especially Kamb et al (1994) and Meier et al (1994) (both in JGR) that show the effects of changes in water pressure and water storage at the bed on flow, from actual observation.				X		Unfortunately there is no room for more references.
23-34	2	41	4-5	I'd delete the phrase, "and is therefore …of the glacier ice." Grounding lines occur under glacier tongues afloat, and do not suddenly change from glacier ice to something else. Grounding line merits it own definition, not just as part of an ice shelf.			X			Revised.
23-35	2		Fig 3	A pretty picture but a marginal example of an ice shelf. How about a real ice shelf like the Ross?			X	X		Will try to find a better one if it shows the same features.
23-36	2	44	7-8	Why is "balance" questioned here but not in the case of energy or force balances?			X			Text has been clarified.
23-37	2		Fig 2.2	I think that this is misleading. The lay reader will connect the dots and infer a sea level rise of 40 to 60 meters by 2100!!!			X			Text has been clarified

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				Reviewer #24						
24-1	2	summary	6	I'm not familiar with any reliable estimates of global mean surface temperature from the Eemian. What is the basis for the statement that global mean surface air temperatures were warmer than today? Higher sea level alone isn't sufficient for this conclusion. The IPCC AR4 is more circumspect, concluding only that high-latitude Northern Hemisphere summer temperatures were warm enough to e.g. melt the Greenland ice sheet, which is adequate to explain the sea level observations.		X				Revised – see response to Rev. comment 8-2.
				Reviewer #29						
29-1	2			Responses to questions posed to reviewers: • Are the scope and intent of the synthesis and assessment product clearly described in the report? Are all aspects of this charge fully addressed? Do the authors go beyond this charge or their expertise? The charge is fully addressed. There are very few instances when some bias leaks through in the presentation. These are indicated in my	X					

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				detailed comments.						
29-2	2			• Are the conclusions and recommendations adequately supported by evidence, analysis, and argument? The breadth of the supporting evidence for the stated conclusions is a strength of the report.	X					
29-3	2			• Are uncertainties or incompleteness in the evidence explicitly recognized? Uncertainties are explicitly addressed. It would be helpful if some discussion was included that indicated a strategy and timeline for specific milestones in reducing uncertainty.	X					
29-4	2			• Are the data and analyses handled completely? The synthesis of the field is comprehensive. The only findings that I feel are missing are the most recent results—some published in 2007 and others to be presented next month.	X					
29-5	2			• Are the report's exposition and organization effective? Yes.	X					

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29-6	2			• Is the report fair and appropriately balanced? Is the tone impartial and devoid of special pleading? Where the report is unbiased is noted in my detailed remarks. The vast majority of the report is unbiased. Slight biases I note do not impact the overall conclusions and summary.	X					
29-7	2			• Are any of the report's findings based on value judgments or the collective opinions of the authors? If so, is this acknowledged, and are scientifically defensible reasons given for reaching those judgments? Some biased statements are defended, but they still represent equivocal expressions and are not community consensus. Thus, they should be modified to return the overall document to an unbiased report.	X					
29-8	2			• Does the executive summary concisely and accurately describe the key findings and recommendations? Is it consistent with the other sections of the report? Some key findings are stated poorly in the ES and others are missing altogether.	X					
29-9	2	1	24	WA does not hold 7 meters SLE if 110% of WA ice volume below sea level is removed from the calculation. This removal is required to fill the basin presently displaced by the ice sheet. With this adjustment, the correct SLE is between 5 and 6 meters.			X			Text has been revised

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29-10	2	3	10	The statement implies a US SAR mission is needed. Clarify this and state why existing non-US missions are inadequate.				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.
29-11	2	3	12	The major uncertainty of the post-glacial rebound is so critical to GRACE conclusions that improving PGR analysis must be included in ANY recommendation for continuing GRACE-like investments. This weakness was repeatedly glossed over in this chapter.				X		The uncertainties of post glacial rebound was discussed in detail in chapter 3.1.3 with error bares for GRACE estimates for both Greenland and Antarctica.
29-12	2	4	7	Numbers are inconsistent. Upper bound of 10 m rise in sea level requires that most of the contribution be from other than the Greenland ice sheet since the GIS can only contribute 5 m SLE.		X				Corrected – upper bound is 6 m.
29-13	2	4	10	Need to state the SLE of Greenland ice sheet for this statement to be meaningful.		X				Added SLE of Greenland ice sheet
29-14	2	4	31	omit "already"		X				done

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29-15	2	6	2	Tidewater glaciers enters here, but as an undefined term. They are probably very important but do not figure prominently in the chapter's discussion. I think this is a mistake, although it may be a reviewer bias.		X				We have tried to address this important point by collecting material scattered through the text. But the main things to be said are that, notwithstanding a rich literature, calving as a behaviour remains unpredictable, and as a natural phenomenon remains under-measured.
29-16	2	6	10	glaciers to glacier		X				yes
29-17	2	6	20	consequences on flow rate and mass loss… to ….consequences of increased flow rates and ice mass loss.		X				revised
29-18	2	6	24	The 95% figure for Antarctic ablation is misleading in the context of the chapter which is about sea level.				X		This chapter is a summary of the ocean influence on ice sheets and the 95% statement is correct in that context.
29-19	2	6	26	not modeled… to …not well modeled.		X				Revised

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29-20	2	7	22	This final sentence is confusing given the preceding text.		X				This has been clarified.
29-21	2	12	28	this statement begins a rather damning interpretation of the SRALT results. Clearly the author is very cognizant of the technical details of SRALT data analysis, but I think the balance between the criticisms of this technique and those of either the GRACE-based or InSAR based approaches is not neutral. The space spent on detailed criticisms of the SRALT analysis risk undermining the strength of the report. The report never states that the SRALT results should not be valued, so the purpose of the deeper critique seems personal.		X				The critique of SRALT dates from shortly after the appearance of the controversial results of Zwally et al. (2005). Now, almost two years on; the discussion has been revised and shortened.
29-22	2	15	23	The basal uplift value for Antarctica is too large. There is no reason to expect such a large value across the entire continent.				X		The one mm/yr value was only used as an example to show the mass-balance error
29-23	2	16	13	By the end of the gravity satellite section I was surprised not to see any mention of the MASCON-based results of Luthke and others. This very innovative approach is far superior to the global-product based approach used by many others. Not only should it be mentioned with other GRACE results, it should lead the discussion.				X		A new paragraph was added quoting the Luthke et al (2006) paper using the MASCOM solutions.

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29-24	2	19	18	…wider than in Greenland (reducing effects of rough… to … wider and smoother than in Greenland (reducing uncertainties associated with rough…		X				revised
29-25	2	20	1	PGR must be brought into the discussion here. It a major concern of these gravity based results. Further, the next statement about the estimates differing by more than the error estimates is made without further comment, while in the context of the SRALT results, such a statement initiated a extended discussion of this fact as a major weakness of the existing analyses. This contrast is a glaring illustration of bias in this chapter. As I mentioned above, I don't think it taints the chapter's conclusions, but it risks being used to discount the entire report as biased.			X			A paragraph has been added to discuss the postglacial rebound uncertainty for ice mass balance assessment with GRACE satellite data.
29-26	2	20	20	The assignation of unattributed sea level rise to Antarctica is not justified here and shows bias. Other possibilities, such as unmeasured Arctic glaciers and deep groundwater, must be included. The report even includes Arctic ice as a possible unmeasured source on page 25. The report is inconsistent on this point.		X				This sentence has been deleted
29-27	2	21	24	…a measurement network with good spatial coverage…is a nebulous statement. I find it doesn't add value to the discussion here.		X				Text has been clarified.

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29-28	2	21	31	…probably many more cold glaciers than temperate… is a conjecture that is not defended.				X		True, but we consider it a plausible conjecture..
29-29	2	23	5	A very awkward sentence begins here concluding the paragraph.		X				Text has been clarified.
29-30	2	24	12	This paragraph confuses me. It presents a quick calculation, but some of the numbers used are conjectural enough that it doesn't make a substantive point.		X				Whole paragraph deleted.
29-31	2	27	8	There was substantive work done by Fahnestock on this point. He showed nicely that ice shelf collapse was preceded by intense summer melt.		X				Reference and text was added
29-32	2	28	3	I urge the authors to introduce tidewater glacier dynamics into this discussion. They are implied, but not explicitly mentioned. Enough is known about tidewater glacier "drastic retreat" to make them a valuable addition. The controlling processes responsible for "drastic retreat" are still uncertain, but the repeated observations and inferred evidence of the process are central to the discussion at this point.						See Chapter 3.3.2 where a more in-depth discussion of tide water glacier was added. We refer to this chapter in the text

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29-33	2	29	18	Omit "It is an empirical fact that…"		X				revised
29-34	2	29	24	"…no existing satellite or other smart technology that can provide… to …no sustained observational program to provide…		X				revised
29-35	2	29	27	…With these albeit limited observations, it has been established… to …Limited observations have established…		X				revised
29-36		30	31	Define meteoric ice.		X				Also called snow/ice
29-37	2	31	29	omit "For abrupt climate change scenarios, attention should be focused on the latter."		X				

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29-38	2	31	31	omit this sentence. It repeats statement above. The entire end of this paragraph is sloppy and repetitive.		X				
29-39	2	32	14	omit …in a collection…		X				
29-40	2	32	30	…process (outcome… to process (because the outcome…		X				
29-41	2	33	18	"almost certainly" contradicts earlier statement that ice shelf collapse was "possibly related" to atmospheric warming. Be consistent. I think the connection is inferential—strongly inferential, but still not unequivocal because other possibilities (the report mentions warming ocean waters) were not measured. The possible connection with warm ocean waters is mentioned in line 23.		X				
29-42	2	35	2	**There is an entire section on sub-ice-sheet hydrology that is absent!** This is an exciting new field that has only just emerged through a variety of published papers in 2007. Its importance is unequivocal and it will be the focus of a large amount of field work in the near term.			X			Two paragraphs of text were added to address this point

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				Direct relationships between properties of the subglacial hydrology and flow rates do not exist, but will be suggested. Getting a computational handle on this interaction will be central to credible predictions of future ice sheet behavior.						
29-43	2	35	10	Contact Ian Joughin (University of Washington) on new results on this point.				X		We reviewed the current new literature and this point is not yet settled, but is is unlikely that melt water can penetrate over 1000-1500 m through cold ice to the underside of the ice sheet.
29-44	2	35	24	Contact Ian Joughin (University of Washington) on new results on this point.				X		Latest results are not published yet in the literature and cannot be quoted
29-45	2	36	28	Christian Schoof is regarded as having the best solution to the difficulty with smooth solutions across the ice-stream/ice-shelf transition. My impression is that the premier modelers in this field feel this hurdle has now been overcome.			X			Paragraph has been added to refer to Christan Schoff's latest publication on marine ice sheet dynamics