Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4-1	4	0				In the Chapter 4: Observations: Cryosphere I generally miss more detailed information regarding 1) the role of terrestrial snow cover (p.e. Vavrus, S 2007. The role of terrestrial snow cover in the climate system. Climate Dynamics, 29), and 2) the understanding/estimation of the snow liquid water content/dielectric properties of dry and wet snow. Monitoring the snow water equivalent (SWE) is critical to effective management of water resources in many parts of the world that depend on the mountain snowpack for water storage (Bradford, JH, Harper, JT and J Brown 2009. Complex dielectric permittivity measurements from ground penetrating data to estimate snow liquid water content in the pendicular region. Water Resources Research, 45/W08403; Denoth, A 1980. The pendular-funicular liquid transition in snow. Journal of Glaciology, 25/91; Frolov, AD and YY Macheret 1999. On dielectric properties of dry and wet snow. Hydrological Processes, 13. [Luzi Bernhard, Switzerland]	Noted, but we disagree that these issues should be highlighted, in preference to those discussed in the chapter. Some of that mentioned is the scope of WGII.
4-2	4	0				This chapter summarizes the latest information about cryospheric observations of the current state and possible trends therein. The authors have done quite a good job, as the review is extensive and complete. [Richard Bintanja, Netherlands]	Noted with thanks - no action
4-3	4	0				In my opinion, there are some imbalances in the chapter. It is heavily weighted towards sea ice and land ice (glaciers + ice sheets). These sections comprise nearly 70% of the chapter. While these are obviously heavily studied and significant elements of the cryosphere, the result is that regional information, a global synthesis, and discussion of key processes is provided for these elements. In contrast, the terrestrial snow section includes no clear regional information, and entire key variables like snowfall and snow water equivalent are almost completely ignored. My intent in pointing this out is not to reduce the scope of the sea ice and land ice sections, rather I'd like to see a similar level of depth in the other sections - most notably snow. [Chris Derksen, Canada]	Issues regarding snowfall in the atmosphere belong in chapter 2, not Ch4. added sentence to introduction pointing this out. add regional info & discussion of SWE in snow section. The discussion on snow has been enhanced as far as space restrictions allow.
4-4	4	0				General comment - many geographic references in the text are not provided in any figure. This is a limitation for the non-expert reader. One example is the discussion of sea ice export through Nares Strait (Section 4.2.2.4.3). For those who don't know where Nares Strait is, it is not marked on any figure. [Chris Derksen, Canada]	Accepted - that this should be the goal, placenames added to existing figures.
4-5	4	0				More homogenization between the glacier and ice sheet chapter is needed: For example, mass losses are given for different time periods (Table 4.4 and Table 4.5). For comparison if possible the reported periods should be the same. Second the glacier chapter generally reports mass changes as annual rates, whereas the ice sheet chapter often reports total mass changes over various periods. Rates are more useful for comparison with other periods and with the glacier results. Also terminology is not consistent, e.g. there is SLE in the glacier chapter and 'sea level rise' in the mass change units in the ice sheet chapter [Regine Hock, US]	Taken into account: improvement will be made where published data allow, but this is not always possible (especially homogenizing time periods).
4-6	4	0				In general the glacier chapter is weak, both in structure, content and language. It needs substantial re-writing. There also seems to be a bias towards certain regions (e.g. European Alps) which does not seem justified. The chapter fails to make the point that in recent years the main advancement compared to AR4 is the large number of regional-scale mass-balance results. Traditional singe-glacier measurements should be neglected but they seem to get too much attention compared with the new regional results. [Regine Hock, US]	Accepted: this section has been substantially revised
4-7	4	0				The ice sheet chapter would benefit from adding the 'typical' box plot including all recent mass loss estimates for both ice sheets. This would visualize (and strenghen) the point regarding acceleration of mass loss and also visualize the spread. [Regine Hock, US]	Rejected - we disagree over the value of the "box plot" which is often said to be confusing for the lay-reader
4-8	4	0				Please address the possible impacts of the decline of Arctic sea ice cover on the atmospheric circulation, weather pattern and future climate change in this chapter or other relevant chapters. For examples : - Xiangdong Zhang, Asgeir Sorteberg, Jing Zhang, Ru¨diger Gerdes, and Josefino C. Comiso, 2008 : Recent radical shifts of atmospheric circulations and rapid changes in Arctic climate system, Geophysical Research Letters, 35, L22701, doi:10.1029/2008GL035607 - Jennifer A. Francis, Weihan Chan, Daniel J. Leathers, James R. Miller,and Dana E. Veron, 2009 : Winter Northern Hemisphere weather patterns remember summer Arctic sea-ice extent, 36, L07503, Geophysical Research Letters doi:10.1029/2009GL037274 - Ian Simmonds and Kevin Keay, 2009 : Extraordinary September Arctic sea ice reductions and their relationships with storm behavior over 1979–2008, Geophysical Research Letters , 36, L19715,	REJECTED: outside scope of this chapter and more appropriate to chapters in WG2 and those on feedbacks in WGI.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						doi:10.1029/2009GL039810 - Vladimir Petoukhov and Vladimir A. Semenov, 2010 : A link between reduced Barents-Kara sea ice and cold winter extremes over northern continents, J. of Geophysical Research, 115, D21111, doi:10.1029/2009JD013568 - Meiji Honda, Jun Inoue, and Shozo Yamane, 2009 : Influence of low Arctic sea-ice minima on anomalously cold Eurasian winters, Geophysical Research Letters, 36, L08707, doi:10.1029/2008GL037079 [Tsz-cheung Lee, Hong Kong]	
4-9	4	0				There appears to be a lack of balance in the lead authorship of the chapter. It is surprising that there are no Canadian lead authors given that a significant portion of the northern hemsiphere cryoshpere is found within Canada [Sharon Smith, Canada]	The composition of lead authors was agreed at a higher level, and the chapter team has no influence here. We do however have a significant number of Canadian CAs.
4-10	4	0				As far as I can see, this is a good review of what we think we know about the state of ice masses on Earth, [Robert Thomas, USA]	Noted with thanks - no action
4-11	4	0				but I confess that I have not kept abreast of all the latest publications. Indeed, I found reviews of these in this [Robert Thomas, USA]	No action required
4-12	4	0				Chapter to be particularly useful to my own education! [Robert Thomas, USA]	No action required
4-13	4	0				The „heaviest" reading was the section on glaciers, which I found a bit heavy on detail that could probably have been summarized in a shorter, more palatable form. It is not clear whether authors of Chapter 13 have thoroughly read this Chapter. Their predictions of the rate of ice contributions to future SLR cover a range, part of which has already been exceeded since 2000. Section 4.4.3 etc describes possible causes for ice-sheet changes quite well, but what appears to be missing from the Chapter is any discussion of our ability to model these changes. I think such a discussion and assessment could help guide authors of Chapter 13, or at least alert readers to some of its weaker foundations. [Robert Thomas, USA]	The glacier section is much revised and now an easier read. The literature currently contains rather little modelling of recent ice sheet changes, but we expect publications to be available for second order draft
4-14	4	0				it is a good compilation with an interesting overview of the present state of the cryosphere worldwide. Congratulation! It is sometimes frustrating not to have more details and some statements should include touches of moderation. As a sientist working on mountain glaciers worldwide, I will focuse on the glacier section mainly (section 4.3) [Patrick Wagnon, France]	No action required
4-15	4	0				Synthesize sections sometimes introduce new references/ results which must be avoided. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - text revised to avoid introducing new results on synthesis sections
4-16	4	0				Please avoid mixing the assessment of observations with attribution to the causes (e.g., 4.4.3) [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - The attribution of recent cryo changes to environmental forcing is an important focus of the chapter, but text revised to make the assessment of observations, and the attribution of its likely cause distinct.
4-17	4	0				When talking about changes, such as 'acceleration' of losses (p. 23) no mention of significance testing is given. In other instances terms such as 'certainly increasing' etc are used, with no clear basis. Please ensure to quantify any observed trends and use the AR5 uncertainty language. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted
4-18	4	0				Section 4.4: In the light of the Wu et al. paper melt losses may be considerably less than other studies listed here (which receive a higher reliability rating). This may be an emerging debate which needs to be captured in your assessment. Therefore, some specific discussion of the Wu paper is warranted. Without further discussion in the chapter, there is insufficient explanation as to why lower ratings in Appendix 4.A tables 1 and 3 correspond to the lower end loss estimates. [Thomas Stocker/ WGI TSU, Switzerland]	Text has been revised in the light of the several published and submitted papers since FOD was written.
4-19	4	0				Table 4.5: For transparency to avoid misinterpretation, unweighted values should also be reported in Table 4.5. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4-20	4	0				Careful coordination required with Chapters 3, 5, and 13 to ensure consistency regarding the Cryosphere contribution to Sea Level Rise. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted
4-21	4	0				Section 4.4.4: Care needed in this section to avoid straying into projections. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - the divide between the content of Ch4 and Ch13 has been defined more precisely.
4-22	4	0				Section 4.2.2: The chapter assessment must demonstrate that the full range of literature has been assessed. We recommend a table is added providing results from published studies to support the general statement (p. 7, l. 54-55) "trends in extent and area derived from different procedures are generally consistent". [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - text revised.
4-23	4	0				Table 4.1: We were confused by the caption terms "Sensitivity to climate" and "Potential impacts" - Suggest a more appropriate title of "Cryosphere components and their characteristics". [Thomas Stocker/ WGI TSU, Switzerland]	Accepted
4-24	4	0				Table 4.3: Caption needs to make clear what is meant by "including" / "excluding". [Thomas Stocker/ WGI TSU, Switzerland]	Accepted
4-25	4	0				Section 4.7 - Synthesis: This section strays out of the scope of the chapter and should be removed. Identification of research gaps/research difficulties are not part of this scope and may be considered self-serving. Where such factors directly influence the reported observations and their uncertainties, these should be discussed at the point in the chapter where the assessment is given. [Thomas Stocker/ WGI TSU, Switzerland]	This text has been revised
4-26	4	0				Throughout Chapter 4, ice sheets are often referred to as ‘polar ice sheets’. According to the applied terminology (page 6, line 17), there is no need to include the word ‘polar’. [Jacob Clement Yde, Norway]	Accepted
4-27	4	0				General comment: Based on my background and expertise, I mainly focus my review on the glacier section in Chapter 4.3 [Michael Zemp, Switzerland]	No action required
4-28	4	0				General comment: Overall, the present state of knowledge (and advances since AR4) on glacier distribution and observed changes are summarized in concise and correct manner. I have two major issues (related to the correct citation of datasets and to the new global glacier map) and several minor comments that hopefully are useful to further improve the ZOD. [Michael Zemp, Switzerland]	No action required
4-29	4	0				General issue (already mentionned in ZOD review): The present structure of the chapter treats the different cryospheric components in separate sub-chapters. As a consequence, cross-cutting issues might be neglected. Examples (that might need to be covered) are the interaction of snow with all other cryospheric components, or the contribution of glaciers and ice caps, snow, and permafrost to the local hazard situation, the regional hydrology, (and global sea level rise). Additional comment for FOD: Box 4.1 is a good approach to cover one of these issues (Interaction of Snow with the Cryosphere). Here, another aspect might be worth to be mentionned: Snow on glaciers was treated differently in recent modelling studies on run-off contributions (cf. Kaser et al. 2010 vs. Weber et al. 2010 vs. Huss 2011) which results in different relative glacier contributions to run-off and, hence, to (partly) different conclusions by the authors. References: Kaser, G., Großhauser, M., & Marzeion, B. (2010). Contribution potential of glaciers to water availability in different climate regimes. Proceedings of the National Academy of Sciences of the United States of America, 2010, 1-5. doi:10.1073/pnas.1008162107 Huss, M. (2011). Present and future contribution of glacier storage change to runoff from macroscale drainage basins in Europe. Water Resources Research, 47(7), 1-14. doi:10.1029/2010WR010299 Weber, M., Braun, L. N., Mauser, W., & Prasch, M. (2010). Contribution of rain, snow- and icemelt in the	General Issue - ignoring cross-cutting issues is possible, but interaction of snow is covered in Box4.1 and the impacts on local hazards are the business of WGII

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Upper Danube discharge today and in the future. Geografia Fisica e Dinamica Quaternaria, 33, 221-230. [Michael Zemp, Switzerland]	
4-30	4	0				Major issue: One of the basic requirements for the reproducibility of science is correct citation of both the literature and the datasets used. In my view, the present report does a good job with respect to correct citation of the scientific literature but (partly) fails in correct referencing of underlying datasets (and related versions). In the following, I list the corresponding statements in Chapter 4.3 and make suggestions for correct referencing. Note that correct data referencing requires a good knowledge of the available datasets (incl. lineage and versioning) and of the related literature. My comments, hence, focus on Chapter 4.3 but it might be appropriate to have the same issue checked in the other chapters. [Michael Zemp, Switzerland]	Rejected: in the majority of cases, the data sets and sources are cited by the scientifc literature, and it is the scientific papers that are peer-reviewed, while the data sources are often not peer-review and so not eligible for citation.
4-31	4	0				Other issues: [Michael Zemp, Switzerland]	No action required
4-32	4	1	1	1		Observations: Cryosphere [Medani Bhandari, Nepal]	No action required
4-33	4	1		64		Considering that ice cover on freshwaters has a tremendous socioeconomic impact this chapter contains surprisingly litte information on this topic (only three very small paragraphs on page 31 and nothing in the summary nor in the synthesis). Most critically for global biogeochemical cycling is the transition from ice covered systems to permanently open water systems. Presently 3.7 % of the ice covered lakes in the world are at risk to become permanently ice free (Weyhenmeyer et al. 2011, Global Change Biology). Such information would be useful to add. In addition, it could be useful to mention that not only mean ice duration changes in a warmer climate but also the variance, i.e. year-to-year variability in ice cover dynamics increases (also described and explained in Weyhenmeyer et al. 2011) [Gesa Weyhenmeyer, Sweden]	Rejected - we believe that the significance of lake ice is given proper weight in the chapter. Much of the local impact etc, is better covered in WGII, and the carbon cycle material is more appropriate to other WGI chapters.
4-34	4	1				The order of cryospheric topics is quite strange. Now it is: sea ice, glaciers, ice sheets, seasonal snow, frozen ground. This seems quite random to me, at least I don't see one. I believe it would be much more clear to have some sort of ordering, for instance in relevant timescale. Then the order would become: seasonal snow, sea ice, frozen ground, glaciers, ice sheets. [Richard Bintanja, Netherlands]	Rejected - the ordering is not random, it reflects the magnitude of change and signifcance of change of the particular cryo component on the global scale. Many other ordering could have been used, but we had to choose one!
4-35	4	1				With a few minor exceptions, the various cryospheric components are treated separately from each other. This is unfortunate, as we know that there are many interactions between the various components (sometimes via non-cryospheric climate components). For instance, snow can cover all other components and affect melt, or sublimation; melting of glaciers and ice caps can influence sea ice formation; there's an interconnection between the seasonality of snow cover and sea ice. It's all mentioned somewhere, I guess, but I would strongly suggest a more systemetic approach, processwise, to describe the various links/synchronicities between the cryosperic elements. [Richard Bintanja, Netherlands]	This suggestion has been considered and rejected. One interaction is highlighted in Box 4.1, and to focus on the processes of interaction in a more detailed way would likely end up with text that resembled a text-book. We focus on observations of change in the cryosphere, other chapters focus on processes of interaction
4-36	4	1				There's little information about the feedbacks related to the cryosphere. Sure, it's an observational chapter, but some background information about how changes/trends in the various components would affect climate sensitivity would be welcome, if only to establish/reinforce a closer connection with later chapters where climate sensitivity and its contributing components are discussed. [Richard Bintanja, Netherlands]	See 4-35
4-37	4	1				Perhaps a list of definitions and acronyms would be handy. For instance, 'draft' (the submerged part of sea ice) is a term that I didn't know. [Richard Bintanja, Netherlands]	Accepted - the volume will contain a glossary. But "draft" is not a technical word.
4-38	4	1				Interesting and well put-together chapter. I have made only minor suggestions. [David Parker, United Kingdom of Great Britain & Northern Ireland]	No action required
4-39	4	2	47			6% of what? Not 2080. What meaure of interannual variability is this? Maximum minus minimum years? Some standard deviation? [antony payne, uk]	Refers to page 21 - Accepted, numbers revised to be consistent.
4-40	4	3	1	53	46	The SWIPA (Snow, water ice and permafrost of the Arctic, see http://amap.no/swipa/) assessment (Arctic Council), to come out in the first half of 2012, summarizes recent scientific work about the Arctic cryosphere. If the SWIPA report comes out early enough to be considered, selected findings from this assessment could be	Accepted - specific references to SWIPA chapters have been made

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						considered to be mentioned, or referred to where space is not allowing a higher degree of detail. [Sebastian Gerland, Norway]	
4-41	4	3	1	53	46	The SWIPA (Snow, water ice and permafrost of the Arctic, see http://amap.no/swipa/) assessment (Arctic Council), to come out in the first half of 2012, summarizes recent scientific work about the Arctic cryosphere. If the SWIPA report comes out early enough to be considered, selected findings from this assessment could be considered to be mentioned, or referred to where space is not allowing a higher degree of detail. [Sebastian Gerland, Norway]	see 4-40
4-42	4	3	3	3	12	This paragraph states very well the importance of the cryosphere as a fundamental environmental control as well as a natural integrator of climate variability and a comprehensive climate change indicator. [Richard Heim, U.S.A.]	No action required
4-43	4	3	4	3	4	"the Earth's climate system" [J. Graham Cogley, Canada]	Accepted
4-44	4	3	7	3	8	"cryosphere is a natural integrator of climate variability" What does this mean? What about the ocean? [Alan Robock, USA]	Rejected - the statement seems self explanatory
4-45	4	3	7			"temperature-sensitivity of all components" What about at temperatures far below freezing? [Alan Robock, USA]	Rejected - there is temperature sensitivity in all components in the cryo, maybe not over all temperature ranges, but sensitivity nonetheless.
4-46	4	3	9	4	14	Some more text regarding improved technology (mentioned at p. 3, l. 9) is suggested in this summary. For example, new satellite platforms and sensors, as well as new autonomous operating platforms and setups (e.g. on ice installations, ice teathered platforms, gliders) could be mentioned here, also if mentioned in specific sections later again. [Sebastian Gerland, Norway]	Rejected - lists of technology improvements would not be useful at this point in the introduction
4-47	4	3	9	4	14	Some more text regarding improved technology (mentioned at p. 3, l. 9) is suggested in this summary. For example, new satellite platforms and sensors, as well as new autonomous operating platforms and setups (e.g. on ice installations, ice teathered platforms, gliders) could be mentioned here, also if mentioned in specific sections later again. [Sebastian Gerland, Norway]	see 4-46
4-48	4	3	16	3	27	When giving trends per decade for sea ice extent evolution (Arctic and Antarctic), it would be useful to give associated uncertainty, already here. [Jerome WEISS, France]	Accepted
4-49	4	3	20	3	20	"the average thickness of Arctic ice" [J. Graham Cogley, Canada]	Accepted - average inserted
4-50	4	3	29	3	39	The summary is very weak and does not give an unbiased overview of the subject (too much focus on European Alps). The selection of results seems very random and the paragraph is not well formulated. [Regine Hock, US]	Taken into account: Executive Summary text has changed with developing respective sections in main text
4-51	4	3	29		39	The paragraph need supporting references and some quantitative analysis support [Muhammad Amjad, Pakistan]	Rejected: there are no references in the Executive Summary
4-52	4	3	29			change "mountain" to "mountains" [Alan Robock, USA]	Editorial
4-53	4	3	29			"highly visible" How? Naked eye? Satellite? [Alan Robock, USA]	Taken into account: Executive Summary text has changed with developing respective sections in main text
4-54	4	3	33	3	33	In "Cold high latitude regions,", the comma should be removed: "Cold high latitude regions". [Richard Heim, U.S.A.]	Editorial
4-55	4	3	33			This is not clear. Do we mean variance compared to one another or variance compared to pre1960s values? If the former, why is this of interest (makes an assessment from limited sample harder)? Are the rates high or low? [antony payne, uk]	Accepted: Executive Summary text has changed with developing respective sections in main text
4-56	4	3	33			In cold high latitude regions ...' we now talking a comparsion of rates rather than their variance. Jumping about a bit. [antony payne, uk]	Accepted: text will be modified accordingly

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4-57	4	3	35			quantify 'recently' [antony payne, uk]	Accepted: text will be modified accordingly
4-58	4	3	36	3	37	The literature does not support that the contribution from northern Central Asia is largest. Why using a different spatial unit than the 19 ones discussed in the main chapter? [Regine Hock, US]	Taken into account, will changed according to section text improvement.
4-59	4	3	36	3	37	Here clarifiction have to be done, that contribution to sea level rise is related to mass loss of marine-terminating glaciers or runoff from glacierized river basins having direct ouflow to the ocean. It is important because prevailing part of continental glaciers in Central Asia are located in the river basins not drained to the ocean. See Table 2 in the next sheet entitled Supplement from Reviewer. Other references to the Supplement from Reviewer are also enclosed there. [Vladimir Konovalov, Russian Federation]	Taken into account, will changed according to section text improvement.
4-60	4	3	36	3	37	'glaciers in Antarctica' is confusing. Strictly, this includes all faster-flowing ice in Antarctica, and I don't think authors mean this [Robert Thomas, USA]	Taken into account, will changed according to section text improvement.
4-61	4	3	36			Why is SLR from Alaska etc greatest if central europe highlighted as having stongest mass changes? Are we mixing mass balance and mass balance change per unit area? [antony payne, uk]	Taken into account, will changed according to section text improvement.
4-62	4	3	37	3	39	The sentences contradict each other. Line 35 says that mass losses have increased recently for various regions that contribute most, however, line 38 says that the mass loss has decreased recently. This needs to be explained and made clearer. It is not really clear which time periods are referred to. [Regine Hock, US]	Taken into account, will changed according to section text improvement.
4-63	4	3	37			what is sub-antarctica - subpolar? [antony payne, uk]	Noted
4-64	4	3	38	3	38	Insert "(sea-level equivalent)" after "SLE" [J. Graham Cogley, Canada]	Accepted
4-65	4	3	38	3	39	Not clear what means "present" glacier loss rate when you compared it with 2001-2005 period. [Vladimir Konovalov, Russian Federation]	Taken into account: text will be modified
4-66	4	3	38	3	39	"glacier mass loss rates [...] slightly lower than for the five-year period 2001-2005." Does this statement still hold when considering the recently available mass-balance data for 2009/10? That year resulted in the third most negative 'global' average (at least for the 37 reference glaciers)... [Michael Zemp, Switzerland]	Taken into account: most recent data will be included and text will be modified if required
4-67	4	3	38			write out SLE (sea level equivalent) [Luzi Bernhard, Switzerland]	Accepted
4-68	4	3	38			clarify "present". 2006-2010? [Etienne BERTHIER, France]	Taken into account: text will be modified
4-69	4	3	38			quantify present [antony payne, uk]	Taken into account: text will be modified
4-70	4	3	38			"SLE" What is this? Need to define it. [Alan Robock, USA]	Accepted: text will be modified
4-71	4	3	41	3	53	Parts of the Antarctic lost ice, other parts gained it. See O'Donnell, R., et al., 2011. Improved methods for PCA-based reconstructions: case study using the Steig et al. (2009) Antarctic temperature reconstruction. Journal of Climate, 24, 2099-2115. [VINCENT GRAY, NEW ZEALAND]	Rejected - these patterns of change are discussed in the following sections in great detail
4-72	4	3	42			typo techniques [antony payne, uk]	Editorial
4-73	4	3	44	3	44	parts of the ice sheet have always lost mass (ablation area). I guess the point here is that the ice sheet as a whole is losing mass. [Regine Hock, US]	Disagree - even in ablation areas mass lost by melt is generally replaced by in-flow of ice.
4-74	4	3	46			is 'certainly' covered by the calibrated uncertainty language - should this be 'very likley' or 'virtually certain' [antony payne, uk]	Accepted - revised to use uncertainty language
4-75	4	3	48	3	50	lines 48-50: Clarify that the 6-7 mm and 4 mm sea-level equivalents refer to the entire period, not annual. [Robert Thomas, USA]	Rejected - "average" makes this clear.
4-76	4	3	48			similarly need to check use of strong and moderate in terms of the level agreement over mass balnce of two ice sheets [antony payne, uk]	Accepted

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4-77	4	3	48			in this context GRACE has no meaning and only adds confusion; better either to use only years or a phrase like 'era in which mass change is directly observable from satellite ...' [antony payne, uk]	Accepted
4-78	4	3	49	3	49	units: the mass change in Gt is reported as rates while the conversion into sea-level equivalent is reported as total over the reported period. This is confusing. Rates are preferable. [Regine Hock, US]	Accepted
4-79	4	3	49	3	50	I assume that the 6-7 mm of SLE for Greenland is total contribution over the time period: since it comes directly after a measure of Gt yr-1, it would be easy to read that as 6-7 mm SLE yr-1: I suggest adding "for the whole time period" to make it clear. [Marcus Sarofim, USA]	Accepted
4-80	4	3	49	3	51	Mentioning the source of data for mass loss in Greenland and Antarctic will be very useful. [Vladimir Konovalov, Russian Federation]	Rejected - space restrictions do not allow such detail in the Ex Summ.
4-81	4	3	50	3	51	"In the 2002-2011 period of the GRACE satellite mission" [J. Graham Cogley, Canada]	Text revised - Remove reference to "GRACE" at this point
4-82	4	3	50			explaine GRACE [Luzi Bernhard, Switzerland]	See 4-81
4-83	4	3	50			Define "GRACE" [Alan Robock, USA]	See 4-81
4-84	4	3	51			GRACE results: Note that a fraction of the GRACE signal over Greenland and Antarctica might actually come from glaciers and ice caps surrounding the ice sheets. [Michael Zemp, Switzerland]	Reject - this is discussed explicity in following sections
4-85	4	3	52			again need to check use of 'certainly' [antony payne, uk]	Accepted
4-86	4	3	55	4	4	Very good summary of snow cover change results. [Richard Heim, U.S.A.]	No action required
4-87	4	3	57			There is no evidence provided later in the snow section as basis for the statement in the Executive Summary that "…the largest declines (8%) occur in spring and are strongly correlated with atmospheric temperature and precipitation." The relationship between April SCE and air temperature is provided in Figure 4.20, but no evidence is provided of any relationship between spring snow cover (SCE or SWE) and precipitation. [Chris Derksen, Canada]	Accepted and statement for the ES has been rewritten
4-88	4	4	1	4	1	"from one region to another" suggest to change as "in different regions" [Yongjian Ding, China]	Editorial
4-89	4	4	3			More snow at high mountains is not explicitly shown in this chapter. Please reference! [Christoph Marty, Switzerland]	Accepted and statement for the ES has been rewritten
4-90	4	4	6	4	6	remove s from components. But I hope the entire document will be edited by IPCC for such errors, of which there are many, because I shall focus hereafter on content. [Robert Thomas, USA]	Editorial
4-91	4	4	6	4	7	Explain the "average temperature of the permafrost". Is it the ground surface temperature (not very interesting)? The temperature at the depth where annual amplitude is zero? [J. Graham Cogley, Canada]	Noted - the statement is revised but cannot be too detailed in the ES
4-92	4	4	6	4	14	The evidence does not really point to significant degradation of permafrost in all regions. Evidence does show warming of permafrost in all regions. However, the statement that increases of up to 3°C have been observed since the late 1970s does not really indicate the significant spatial variability and it is important to indicate this. A better statement could be presented which does indicate the larger changes are in cold permafrost (and low ice) and smaller changes in warm permafrost (ice-rich). [Sharon Smith, Canada]	Noted - the statement is revised and extended, but still needs to be a "summary" statement
4-93	4	4	8	4	8	Is there any quantitative estimate of the loss of permafrost areal extent? [Marcus Sarofim, USA]	No there is not - no change made
4-94	4	4	8	4	9	Consider changing sentence to:"In the large areas where permafrost and permafrost-free terrain coexist within short distance, thin permafrost is disappearing and the overall areal extent of permafrost is declining." REASON: The term "permafrot boundary" and the causality indicated are misleading. REFERENCE regarding boundary DOI: 10.5194/tcd-5-1547-2011 (Gruber, S. Derivation and analysis of a high-resolution estimate of global permafrost zonation. The Cryosphere Discuss., 5, 1547-1582, 2011 www.the-cryosphere-discuss.net/5/1547/2011/) [Stephan Gruber, Switzerland]	Noted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4-95	4	4	13	4	13	"present" suggest to change as "2011" [Yongjian Ding, China]	Accepted
4-96	4	4	15	4	15	"rapid melting" rapid is a relative term, should provide idea of what rate is rapid. Also indicate melting at the ice ocean interface (as opposed to the rest of the ice sheet). [Ian Joughin, USA]	Comment probably refers to page 26 line 15 - Text revised
4-97	4	4	33	4	33	"is only ~10–20%, and" would be appropriate to reference Joughin et al, Science 2008 here for this number. And perhaps again in the sentence that ends just below on line 39. [Ian Joughin, USA]	Comment probably refers to page 26 - Accepted
4-98	4	4	39	4	39	"continue to collapse" change to "have continued to collapse" [Ian Joughin, USA]	Editorial
4-99	4	4				Is there nothing that can be said in the Executive Summary about the Himalayan glaciers? This issue an important issue. [Henning Rodhe, Sweden]	Text has been considered and will take account of the most up-to-date literature
4-100	4	5	2	5	2	The sea level equivalent given for glaciers excludes the peripheral glaciers surrounding the ice sheets. This value should be 0.6 (Radic and Hock, 2010), as in Table 4.3 (pg 4-16) [W. Tad Pfeffer, United States of America]	Accepted
4-101	4	5	3	4	4	Delete "a substantial fraction of". For permafrost, it is not clear, how much substatial would be here. To be precise, the definition of permafrost is tied to 0°C to avoid the problems with defining such limits or with detecting ice. [Stephan Gruber, Switzerland]	Accepted - the language was loose.
4-102	4	5	3	5	4	"comprise" is the wrong verb (in two places). It should be "include" [J. Graham Cogley, Canada]	Reject - dictionary definition: to contain, to include, to consist of.
4-103	4	5	3	5	12	It might also be noted that permafrost also lasts several years (millenia and longer) [Sharon Smith, Canada]	Reject - we are only trying to give a flavour of the span of longevity and do not need to include all components in this statement
4-104	4	5	5	5	5	Delete 'in mountainous regions, ice caps' (according to the applied terminology; page 6, line 15-16). [Jacob Clement Yde, Norway]	Reject - since we re-define the term on page 6, here on page 5 we use the more standard definition
4-105	4	5	6	5	6	EACH COMPONENT [Roger Barry, USA]	Editorial
4-106	4	5	6			Change 'components' to 'component'. [Chris Derksen, Canada]	Editorial
4-107	4	5	10	5	11	Nevertheless, all components of the cryosphere are inherently sensitive to changes in surface temperature, precipitation and hence to a changing climate [Luzi Bernhard, Switzerland]	Accepted - text modified
4-108	4	5	11	5	11	"changes in surface temeprature and solid precipitation" [J. Graham Cogley, Canada]	Accepted - text modified
4-109	4	5	15	5	29	In figure 4.1 the snow extend line is hard to see [Sharon Smith, Canada]	Accepted - figure redrafted
4-110	4	5	20	5	21	Presumably Figure 4.1 is a placeholder, but it does not show any yellow glaciers. Say "peripheral to the ice sheet", not within the ice sheet". [J. Graham Cogley, Canada]	Accepted
4-111	4	5	31	5	31	Permafrost is also a long-lived component of the cryosphere and is also viewed as a natural thermometer [Sharon Smith, Canada]	Rejected - only one example is necessary here, and we give the most obvious
4-112	4	5	33	5	33	cryopshere' should be 'cryosphere'. [Jacob Clement Yde, Norway]	Editorial
4-113	4	5	35	5	36	Ice-sheet changes are not "highly visible", at least not without amounts of measurements to make them visible. [Stephan Gruber, Switzerland]	Accepted - text revised
4-114	4	5	35			(e.g. precipitation) and also site specific factors like topography etc. [Nadine Salzmann, Swizerland]	No action required
4-115	4	5	40	5	49	I like this paragraph providing reference to other sections of the report dealing with secondary impacts of cryospheric changes. Suggestion: I think it would be worthwile to have (at least) these sections reviewed by (lead)-authors of	We don't understand this comment - we are Chapter 4!

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Chapter 4 in order to ensure maximum consistency within IPCC AR5. [Michael Zemp, Switzerland]	
4-116	4	5	41	5	41	I think that changes in each component of the cryosphere have a significant impact also on the economic system. For example the altitudinal boundary, above which snow conditions are sufficiently reliable for winter sport in the Alps, is predicted to rise from 1200 m to 1500 m a.s.l. within the next 50 years (Elsasser and Messerli, 2001). References: Elsasser, H., and Messerli, P., 2001: The vulnerability of the snow industry in the Swiss Alps. Mountain Research and Development, 21: 335–339. [Michele Freppaz, Italy]	Rejected - this is not the remit for this chapter, it's a WGII issue
4-117	4	5	44	5	44	"has altered, and may alter in future, ocean" [J. Graham Cogley, Canada]	Accepted - text altered
4-118	4	5	46			Also there's no relation between the observed trends and arctic amplification, at least it's not discussed/mentioned here. (Page 4-5, line 46). Most trends have something to do with increased Arctic warming, so why not mention it in a cause/effect way? [Richard Bintanja, Netherlands]	Rejected - the amplification is caused by ice-albedo feedback which is related to the trend. Text was however modified
4-119	4	5	47	5	49	Whether damage to infrastructure occurs due to changing permafrost conditions will depend on the properties of the ground (ice content) and how the infrastructure is designed. There will not be impacts everywhere and it is suggested that "may" be used instead of "will". [Sharon Smith, Canada]	Rejected - there will be some damage to in structures due to permafrost change - we do not quantify them, that is the job of WGII, but some damage is bound to occur. Text modified slightly to make this clear
4-120	4	5	48	5	48	Delete "across". Or have some words been lost? [J. Graham Cogley, Canada]	Editorial - Done
4-121	4	5	48	5	48	Change to: "will damage infrastructure in the arctic and in mountain areas". REASON: Most people are exposed to permafrost in mountain ranges and, there is considerable infrastructure. REFERENCE DOI: 10.5194/tcd-5-1547-2011 (Gruber, S. Derivation and analysis of a high-resolution estimate of global permafrost zonation. The Cryosphere Discuss., 5, 1547-1582, 2011 www.the-cryosphere-discuss.net/5/1547/2011/) [Stephan Gruber, Switzerland]	Rejected - we feel the statement about the Arctic is the primary one, and the mountain glacier impacts are less important.
4-122	4	5	48	5	48	Is across a superfluous word in this sentence? [Marcus Sarofim, USA]	Editorial
4-123	4	5	48			Remove 'across' [Chris Derksen, Canada]	Accepted
4-124	4	5	52			Figure 1 - nice. Needs a scale and missing ocean from legend. Would it be possible to add glaciated areas to highlight where the glaciers and ice caps are? May be not given scale issues. [antony payne, uk]	Accepted - figure given a scale and ocean
4-125	4	5	52			Figure 2 misplaced. Is not realy discussed in the text. Should this be in the glaciers etc section? [antony payne, uk]	Accepted - figure deleted
4-126	4	5	56	5	57	"Satellite technologies, coupled with regional climate modelling, now permit" [J. Graham Cogley, Canada]	Reject - satellite estimates can do this on their own.
4-127	4	5	57			"...now permit rather precise estimates…" Vague. Change to "Satellite technologies now permit improved precision in estimates…" [Chris Derksen, Canada]	Accepted text - revised to make a more precise statement.
4-128	4	5		6		In general, terms used should be better introduced, e.g. difference betwen mass-volume or frozen ground - permafrost etc. [Nadine Salzmann, Swizerland]	Reject - these terms are listed in the glossary, so should not require definition here.
4-129	4	5				4.1 Introduction: The components of the cryosphere mentioned in the introduction should be more coherent with focus on the weight of each component given in the following sections. Also permafrost as such is not mentioned in the introduction, but in Fig.4.1 only permafrost and not frozen ground is a topic. [Nadine Salzmann, Swizerland]	Reject - in the introduction we do not think components need to be weighted equally -we are highlighting areas of interest. Figure caption to be revised
4-130	4	6	1	6	1	When reporting that "sea ice thickness can now be measured using satellite altimetry", better to give the uncertainty. [Zhaomin Wang, UK]	Reject - this is just a statement of what's new and more detail is given in sections
4-131	4	6	1	6	8	It would be good to mention the considerable efforts made during IPY to characterize current cryospheric conditions in the polar regions and to also assess how these are changing. Reference could also be made to other major assessments such as the AMAP's Snow Water Ice and Permafrost in the Arctic [Sharon Smith, Canada]	Text revised to make references to SWIPA, but not in introduction

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4-132	4	6	2	6	3	This statement implies that while only 42% of the glaciers worldwide were considered for AR4, there is now a nearly complete inventory for AR5. On page 16 line 16, however, it states that the inventory now covers 48% of estimated global glacier area. Are these statements incongruous or am I mis-reading something? [Chris Derksen, Canada]	Accepted - text revised in the glaciers section
4-133	4	6	2	6	3	See comment (17) below. [Michael Zemp, Switzerland]	Don't understand the comment
4-134	4	6	3	6	4	"Remote-sensing measurements of glacier volume change at regional scales are" [J. Graham Cogley, Canada]	Editorial - done
4-135	4	6	4			This line should probably read as follows "[...] measurements of regional glacier volume change are [...]" [Michael Zemp, Switzerland]	Editorial - done
4-136	4	6	6	6	6	"rapid loss, compared to slow recovery": this important point needs to be amplified because many readers will not understand the asymmetry that this phrase only alludes to. [J. Graham Cogley, Canada]	Rejected - we agree it would be nice to amplify further, but space means that this clear statement will have to suffice
4-137	4	6	9			…observed variability and change. [Nadine Salzmann, Swizerland]	Editorial - done
4-138	4	6	11	6	12	Delete this sentence about space constraints (thus saving some space). Or give a couple of examples of the components that are not discussed. [J. Graham Cogley, Canada]	Accepted - text revised
4-139	4	6	16	6	16	"was previously termed". [J. Graham Cogley, Canada]	Editorial -accepted
4-140	4	6	17	6	17	ice sheet' should be 'ice sheets'. [Jacob Clement Yde, Norway]	Editorial -accepted
4-141	4	6	18	6	18	Delete "roughly" [J. Graham Cogley, Canada]	Editorial -accepted - replaced with "approximately"
4-142	4	6	18	6	18	I think 1 Gt = 1100 km^3 of ice (1 km^3 of water = 1000x1000x1000 m^3 = 10^9 m^3 = 10^12 kg of water) [Zhaomin Wang, UK]	Accepted - schoolboy mistake - ouch!
4-143	4	6	18	6	19	Is this relative sea level change (account for isostatic adjustments)? [Sharon Smith, Canada]	Rejected - this is global average sea-level rise
4-144	4	6	18			change "to cubic" to "to one cubic" [Olaf Eisen, Germany]	Editorial
4-145	4	6	19	6	19	Please add a reference for the statement. [Gesa Weyhenmeyer, Sweden]	Accepted - reference added AR4
4-146	4	6	22	6	22	9-14% of global land surface have permafrost. This includes the Sourthern Hemisphere and takes into account the large uncertainty of this estimate. REFERENCE DOI: 10.5194/tcd-5-1547-2011 (Gruber, S. Derivation and analysis of a high-resolution estimate of global permafrost zonation. The Cryosphere Discuss., 5, 1547-1582, 2011 www.the-cryosphere-discuss.net/5/1547/2011/) [Stephan Gruber, Switzerland]	Accepted
4-147	4	6	22	6	22	Sub-sea permafrost can be estimated to about 2.8 Mio km2 based on: REFERENCE 1: Shakhova, N., Semiletov, I., Salyuk, A., Yusupov, V., Kosmach, D., and Gustafsson, O.: Extensive Methane Venting to the Atmosphere from Sediments of the East Siberian Arctic Shelf, Science, 327, 1246–1250, http://dx.doi.org/10.1126/science.1182221doi:10.1126/science.1182221, 2010. REFERENCE 2: Osterkamp, T. E.: Sub-sea permafrost, in: Encyclopedia of Ocean Sciences, edited by: Steele, J. H., Thorpe, S. A., and Turekian, K. K., 2902–2912, 2001. [Stephan Gruber, Switzerland]	Accepted - this source is a useful one
4-148	4	6	22	6	22	Table 1 - It is probably not correct to assume that thawing of permafrost and the water that will result from melting ice will all end up in the ocean. [Sharon Smith, Canada]	Accepted - it makes little sense to include this.
4-149	4	6	22	6	23	Table 4.1 Permafrost is written with italic. Need explanation for italic. [Hiroyuki Enomoto, Japan]	Accepted - italics removed
4-150	4	6	22			Table 4.1. For glaciers there is a new number 0.60 +-0.07 (and also includes ALL glaciers also those around Antarctic/Greenland) and therefore is a more accurate 'total'. (Radic and Hock, 2010, GJR). [Regine Hock, US]	Accepted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4-151	4	6	26	4	26	Dyuergerov' should be 'Dyorgerov'. [Jacob Clement Yde, Norway]	Rejected - both spellings are incorrect - correct one will be used
4-152	4	6	33	6	33	"replacing ice currently" [J. Graham Cogley, Canada]	Editorial
4-153	4	6				What is ??? In table 4.1 [Muhammad Amjad, Pakistan]	Accepted - no value available - delteted
4-154	4	6				Table 4.1: The references this tabel is based on, are quite old (most of them available for AR4). [Nadine Salzmann, Swizerland]	Accepted - updated where new sources are available.
4-155	4	6				Table 4.1: Row 5 regarding 'permafrost' should not be in italic. [Jacob Clement Yde, Norway]	Accepted
4-156	4	6				Table 4.1, glacier estimates: The rough glacier values given here do not exactyl consist with the values given in Dyurgerov and Meier (2005). Their global estimates for total area and SLE excluding (incl.) glaciers surrounding the ice sheets in Greenland and Antarctica are 540,000 km2 (785,000 km2) and 0.34 m (0.65 m), respectively. Furthermore, there is a spelling mistake (change 'Dyuergerov' into 'Dyurgerov') and the correct citation would be "Dyurgerov and Meier (2005), mainly based on WGMS (1989)". In my view, this table should provide global estimates of area(, volume,) and SLE for all glaciers (i.e., including the ones surrounding the two ice sheets in Greenland and Antarctica). [Michael Zemp, Switzerland]	Accepted - replaced with updated values
4-157	4	7	1	14	57	Considerations of Baltic Sea ice are missing. See e.g., 1) Jaagus J (2006) Trends in sea ice conditions in the Baltic Sea near the Estonian coast during the period 1949/1950–2003/2004 and their relationships to large-scale atmospheric circulation. Boreal Environ Res, 11:169–183. http://www.borenv.net/BER/pdfs/ber11/ber11-169.pdf. 2) Jevrejeva S, Drabkin VV, Kostjukov J, Lebedev AA, Leppäranta M, Mironov YU, Schmelzer N, Sztobryn M (2004) Baltic Sea ice seasons in the twentieth century. Clim Res 25:217–227 [Kirsti Jylhä, Finland]	Noted. - Baltic sea ice is not quantified in the passive microwave record because of large uncertainties due to land contamination and effect of antenna side lobes.
4-158	4	7	16	7	16	Divergent "and shear" pack-ice motion creates areas of open water… Indeed, during most of the year, shear dominates the deformation of sea ice, especially in the Arctic; and shear faulting creates leads, i.e. open water. [Jerome WEISS, France]	Accepted. Text modified
4-159	4	7	19			Change 'collide' to 'collides' [Chris Derksen, Canada]	Accepted
4-160	4	7	28	7	29	Along with mentioning snow ice, I would recommend to mention also superimposed ice, which is also affecting sea ice and its snow cover (for example see Onstott 1992, Geoph. Monogr. 68, Kawamura et al. 1997, JGR Oceans 102, Nicolaus et al. 2003, Physics and Chemistry og the Earth 28). With increasing temperature and more mild spells the occurrence and role of superimposed ice could also increase. [Sebastian Gerland, Norway]	Rejected. Projection of possible change and less important than other processes documented here.
4-161	4	7	28	7	29	Along with mentioning snow ice, I would recommend to mention also superimposed ice, which is also affecting sea ice and its snow cover (for example see Onstott 1992, Geoph. Monogr. 68, Kawamura et al. 1997, JGR Oceans 102, Nicolaus et al. 2003, Physics and Chemistry og the Earth 28). With increasing temperature and more mild spells the occurrence and role of superimposed ice could also increase. [Sebastian Gerland, Norway]	same as above.
4-162	4	7	38	7	38	"a dominant (but not exclusive) role" [J. Graham Cogley, Canada]	Accepted.
4-163	4	7	44	12	17	Section 4.2.2 There are other important regional studies that could be included in this discussion. For example Tivy et al (2011) used archival records from the Canadian Ice Service to examine sea ice changes since the 1960s. Other key results from Canadian IPY projects that could also be included are Howell (2009), Howell et al. (2010), Hocheim and Barber (2010). References:Tivy, A, Howell S, Alt B, McCourt S, Chagnon R, Crocker G, Carrieres T, and Yackel J (2011a) Trends and variability in summer sea ice cover in the Canadian Arctic based on the Canadian Ice Service Digital Archive, 1960–2008 and 1968–2008. 2011. Journal of Geophysical Research 116 (C03007). doi: 10.1029/2009JC005855 Tivy A, Howell S, Alt B, Yackel JJ, Carrieres T (2011b) Origins and levels of seasonal forecast skill for sea ice	Rejected. Included in overall analysis. Limited space for regional trends.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						in Hudson Bay using Canonical Correlation Analysis. J Clim 24. doi: 10.1175/2010JCLI3527.1 Hochheim, KP, Barber DG (2010) Atmospheric forcing of sea ice in Hudson Bay during the fall period, 1980–2005. J Geophys Res 115 (C05009). doi: 10.1029/2009JC005334 Howell S, Duguay CR, Markus T (2009a) Sea ice conditions and melt season duration variability within the Canadian Arctic Archipelago: 1979–2008. Geophys Res Lett, 36 (L10502). doi: 10.1029/2009GL037681 Howell S, Tivy A, Agnew T, Markus T, Derksen C (2010) Extreme low sea ice years in the Canadian Arctic Archipelago: 1998 versus 2007. J Geophys Res 115 (C10053). doi: 10.1029/2010JC006155 [Sharon Smith, Canada]	
4-164	4	7	44	12	17	Sea ice history reconstructions for the Arctic by Kinnard et al (2011, Nature) might also be relevant to the discussion [Sharon Smith, Canada]	Rejected. Not within scope of chapter. Paleo in Ch. 5.
4-165	4	7	44	15	8	A number of ice-ocean general circulation models in which data are assimilated have been forced by atmospheric reanalysis data to reconstruct the evolution of the sea ice thickness and volume over the last decades. Results from those studies should be assessed in this section. [Thierry Fichefet, Belgium]	Rejected. This chapter provides an assessment of observations and not models.
4-166	4	7	46	9	32	There is no discussion of observational uncertainties in this section. There are no uncertainties on any of the figures and no discussion on uncertainties in sea ice concentration retrievals in the text. It would be informative for the reader to include a discussion of this to allow them to assess the level of certainty in the changes shown. If "error bars" are not possible, then structural uncertainty might be assessed by plotting different estimates of some of the quantities depicted, although this would not provide a full picture (see discussion in Chapter 2). [Nick Rayner, United Kingdom of Great Britain & Northern Ireland]	Accepted. Will add uncertainties in Fig captions.
4-167	4	7	49	7	49	"temporal resolution of a few days": from P8 L24 and later places it seems clear that this should be "of one day" [J. Graham Cogley, Canada]	Accepted. Few days or better.
4-168	4	7	52	7	54	Mention the amount of ice that is missed by these definitions (ice-covered area times concentration, where concentration is less than 15%). At L52, delete "integral" [J. Graham Cogley, Canada]	Rejected. Standard definition for detection of trends.
4-169	4	8	4	8	4	Delete “relatively extensive” [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted.
4-170	4	8	4			"The relatively extensive Arctic ice cover at the end of summer…" Unclear. Relative to what? [Chris Derksen, Canada]	Accepted.
4-171	4	8	18	8	19	is the change in extent statistically significant? [Luzi Bernhard, Switzerland]	Accepted
4-172	4	8	22			period 2007 - 2011: I don't agree with the integration of the 5-year period! 1) the significance is problematic, because the years 2007/2008 are present in two periods (1999 - 2008 & 2007 - 2011); 2) overlapping year 2007: it was a record year. Either leave the last period or only a three-year period 2009 - 2011 [Luzi Bernhard, Switzerland]	Accepted - Will use 2009-11 data
4-173	4	8	26			"The 2011 extent in the spring and summer was comparable to the 2007 record low." Subjective. How close was it in absolute terms? [Chris Derksen, Canada]	Accepted
4-174	4	8	30	8	30	"between the monthly values and" [J. Graham Cogley, Canada]	Accepted
4-175	4	8	30	8	31	Given that "record" in the previous paragraph was used in its meaning of "most extreme", "record averages" in this sentence was temporarily confusing (I assume that "record" in this latter case was used in the meaning of "all the data"). I'd suggest rephrasing. [Marcus Sarofim, USA]	Accepted. Typo.
4-176	4	8	31	8	31	Explain the difference between 'ice extent' and 'ice area' to the broad readership of this report. [Jacob Clement Yde, Norway]	Defined in 4.2.2.
4-177	4	8	40	8	41	"This large spatial variability is associated with the complexity of the atmospheric circulation system as influenced by the Arctic Oscillation." A reference is needed for this statement. [Chris Derksen, Canada]	Accepted - (added Thompson and Wallace reference)
4-178	4	8	41	8	41	Insert a reference after 'Arctic Oscillation' to support this statement. [Jacob Clement Yde, Norway]	Accepted. see above

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4-179	4	8	48	4	48	lack one dot after "Comiso,2010)" [Yongjian Ding, China]	Accepted
4-180	4	8	50	8	50	ADD REFERENCE TO MAHONET ET AL 2008 JGR 113 C11005 ONEURASIAN SEA ICE TRENDS 1933-2006 [Roger Barry, USA]	Noted - we will review, consistency and quality of obs. This is based on ice charts and the HADISST data set, so perhaps should be considered as part of the new section on pre-satellite record -
4-181	4	9	13	9	13	DATA FORCANADIAN ARCTIC ARCHIPELAGO FOR 1968-2008 AREGIVEN BY TIVY ET AL 2011 JGR 116 C03007 SEE BARRY & GAN 2011 THE GLOBAL CRYOSPHERE PP. 265-275 [Roger Barry, USA]	Rejected. Regional data covered by satellite data record.
4-182	4	9	14	9	27	The reader may be not the sea ice expart. More detailed difinittion for perennial and multi-year ice and accurate interplitation on the monitoring these ices may be required. [Hiroyuki Enomoto, Japan]	Rejected. Methodology described in manusript.
4-183	4	9	34	10	43	The role and thickness of snow for sea ice could be more prominently discussed, including the still existing lack of detailed continuous large scale snow thickness information. Some of the roles of snow are mentioned, but it could be somehow highlighted that a lot of processes depend heavily on the precipitation in form of snow (e.g. freezing rates, formation of melt ponds). The connection of onset of ice formation and snow fall can also affect ice growth and influence sea ice scenarios (e.g. Notz 2009, PNAS 106). I would suggest to include an own paragraph/subchapter on snow on sea ice. [Sebastian Gerland, Norway]	Accepted, better discussion of snow on sea ice has been included in snow box
4-184	4	9	34	10	43	The role and thickness of snow for sea ice could be more prominently discussed, including the still existing lack of detailed continuous large scale snow thickness information. Some of the roles of snow are mentioned, but it could be somehow highlighted that a lot of processes depend heavily on the precipitation in form of snow (e.g. freezing rates, formation of melt ponds). The connection of onset of ice formation and snow fall can also affect ice growth and influence sea ice scenarios (e.g. Notz 2009, PNAS 106). I would suggest to include an own paragraph/subchapter on snow on sea ice. [Sebastian Gerland, Norway]	same as above.
4-185	4	9	34	10	44	While an own sub chapter is devoted to Antarctic fast ice (4.2.3.6, p. 14), I found no specific content addressing work about Arctic fast ice, its thickness and its changes. Fast ice trends are especially of interest since they are mainly dependent on thermodynamic conditions (and not dynamic conditions as for drifting ice). Work about Arctic fast ice and monitoring setups was published for example in articles by Polyakov et al. (2002, GRL 29) for the Siberian coast, Gerland et al. (2008, GRL 35) for the Barents Sea, and Druckenmiller et al. 2009, CRST 56) for Barrow/Alaska. [Sebastian Gerland, Norway]	Accepted. A section on Arctic Land-Fast Ice has been added
4-186	4	9	34	10	44	While an own sub chapter is devoted to Antarctic fast ice (4.2.3.6, p. 14), I found no specific content addressing work about Arctic fast ice, its thickness and its changes. Fast ice trends are especially of interest since they are mainly dependent on thermodynamic conditions (and not dynamic conditions as for drifting ice). Work about Arctic fast ice and monitoring setups was published for example in articles by Polyakov et al. (2002, GRL 29) for the Siberian coast, Gerland et al. (2008, GRL 35) for the Barents Sea, and Druckenmiller et al. 2009, CRST 56) for Barrow/Alaska. [Sebastian Gerland, Norway]	same as above
4-187	4	9	43	9	45	Change text to “Rothrock et al. (1999) found that ice draft in the mid-1990s was less than that measured between 1958 and 1976 in each of six Arctic Ocean sectors. The change was least (–0.9 m) in the Beaufort and Chukchi Seas and greatest (–1.7 m) in the Eurasian Basin…” [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted
4-188	4	10	1	10	6	The first sentence in this paragraph could be understood as if all challenges around satellite altimetry for sea ice thickness determination are solved. However, I would suggest to mention limitation in amount and lifetime of sensors, limitation in terms of suitable season within a year for altimetry, and limitations in accuracy. Among the challenges (l. 5-6) I would recommend to add the need of knowledge about densities and physical properties of snow and ice. [Sebastian Gerland, Norway]	Accepted - Text has been modified
4-189	4	10	1	10	6	The first sentence in this paragraph could be understood as if all challenges around satellite altimetry for sea ice thickness determination are solved. However, I would suggest to mention limitation in amount and lifetime of sensors, limitation in terms of suitable season within a year for altimetry, and limitations in accuracy. Among the challenges (l. 5-6) I would recommend to add the need of knowledge about densities and physical properties of snow and ice. [Sebastian Gerland, Norway]	same as above

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4-190	4	10	9	10	9	The ERS estimates of ice thickness (Laxon, 2003) do show a downward trend in ice thickness but the high variability and short time-series indicate that the trend cannot be considered as significant. [Seymour Laxon, UK]	Accepted. Text has been revised accordingly
4-191	4	10	17	10	17	I presume you mean Figure 4.5 not 4.2, and Kwok et al, 2009 for the citation here and in line 24. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted
4-192	4	10	21			It needs to be clearly stated that the lidar altimeter measurements from ICESat (very little penetration into the overlying snow cover) are different from the radar altimeter measurements (freeboard). [Chris Derksen, Canada]	Rejected. - details are given in the section on measurement technique.
4-193	4	10	23	10	23	Remove the subjective word 'Remarkable' [Seymour Laxon, UK]	Accepted
4-194	4	10	26	10	26	Insert "(multi-year)" after "MY" [J. Graham Cogley, Canada]	Accepted
4-195	4	10	26	10	27	Given a 42% decline in Multi-Year ice area and nearly 20% decline in thickness (Figure 4.5) one might expect the volume decline to have exceeded 50%? [David Parker, United Kingdom of Great Britain & Northern Ireland]	Rejected. This can be explained by the fact that the thinner MY ice at the margins of the the MY ice cover melts before the interior.
4-196	4	10	26			write out MY (multi year) [Luzi Bernhard, Switzerland]	ice at the margins of the the MY ice cover melts before the interior.
4-197	4	10	28	10	32	Insert '(FY)' after first-year in line 28 and delete '(FY)' in line 32. [Jacob Clement Yde, Norway]	Accepted
4-198	4	10	29	10	29	Is 14,000Km3 Multi-Year volume? [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted- text revised
4-199	4	10	35	10	43	This section should be updated with results published in Haas, 2010:VOL. 37, L09501, doi:10.1029/2010GL042652, 2010. [Seymour Laxon, UK]	Accepted
4-200	4	10	43			Change to '…as seen in satellite observations.' [Chris Derksen, Canada]	Accepted
4-201	4	10	51	10	52	On time scales of days to weeks, winds are responsible for most of the variance in sea ice motion. Only wind? No sea current? [Luzi Bernhard, Switzerland]	Rejected .
4-202	4	11	1	11	1	add "per decade" after "+8.5%" [Yongjian Ding, China]	Accepted
4-203	4	11	8	11	8	Concerning the weakening of the Arctic sea ice cover: A recent work (Gimbert et al., Recent mechanical weakening of the Arctic sea ice cover as revealed from larger inertial oscillations, submitted) analyzed in a systematic way the amplitude of inertial oscillations of IABP ice drifters, from 1979 to 2008. This analysis shows that ice inertial oscillations became significatively stronger over the period, whereas the associated forcing (Coriolis) remained by definition unchanged. Using a simple coupled upper ocean/sea ice dynamical model, this can be interpreted as a strong decrease of the average internal friction within sea ice in recent years (after 2001) compared to previous years, i.e. as a strong mechanical weakening. [Jerome WEISS, France]	Rejected - Model based analysis of potential impact of a thinned ice cover.
4-204	4	11	14	11	15	Recently, from NCEP surface pressure data across Fram Strait and a positive correlation between geostrophic winds and ice speed, Smedsrud et al. (The Cryosphere, 5, 821, 2011) argued that the sea ice area export increased by 4.9+/- 2.8 % per decade from 1957 to 2010. [Jerome WEISS, France]	Rejected. Not based on direct observations
4-205	4	11	14	11	16	New estimates of the Fram Strait area export are now available based on high resolution SAR images since 2003 (Smedsrud et al 2011). These show that the area flux was much higher in recent years, 888 * 10^3 km^2 for 2004-2010. Estimates of ice export for 1957 - 1979 show values lower than the Kwok (2009) values. New citation: L. H. Smedsrud, A. Sirevaag, K. Kloster, A. Sorteberg and S. Sandven, Recent wind driven high sea ice area export in the Fram Strait contributes to Arctic sea ice decline, The Cryosphere, Volume 5, pages 821-829, www.the-cryosphere.net/5/821/2011/ doi:10.5194/tc-5-821-2011 [Lars Henrik Smedsrud, Norway]	Rejected. Not based on direct observations
4-206	4	11	17	11	18	"Comparison … shows" [J. Graham Cogley, Canada]	Accepted

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4-207	4	11	22	11	22	Nearly a third (32% in Kwok and Cunningham's abstract). [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted
4-208	4	11	22	11	27	The 1957-2010 5% per decade trend in ice export (Smedsrud et al 2011) has been a likely contributor to the overall loss of Arctic sea ice. In particular is there a strong relationship between area export and sea ice thickness (Björk 1997), so much of the observed changes in thickness (Rothrock et al 2008) can be explained this way. The overall trend in export is driven by increased geostrophic winds in the area. Relationship between area export and thickness is fully described here: Björk, G.: The relation between ice deformation, oceanic heat flux, and the ice thickness distribution in the Arctic Ocean, J. Geophys. Res., 102, 18689–18698, 1997. [Lars Henrik Smedsrud, Norway]	Rejected; Smedrud et al (2011) not based on direct observations; mulit-decadal extrapolation using empirical relationship between gradient in sea level pressure and area export from 5-years of observations and reanalysis.
4-209	4	11	29	11	55	Recent important work has been published about the role of atmospheric and oceanic forcings for the development of sea ice (Perovich et al. 2011, Annals Glaciol. 52), and about the the role of Arctic sea ice as a contributor to the global albedo (Hudson 2011, JGR Oceans 116). This work goes somehow across different IPCC chapters, so I am not sure where it would be the best place to refer to it; it might be considered to be mentioned in 4.2.2.5. [Sebastian Gerland, Norway]	Accepted. Sentence and reference to Hudson added. Perovich et al is more regional than global and looks at changes in partitioning of solar energy into ice and ocean in the Arctic. It has not been referenced
4-210	4	11	29	11	55	Recent important work has been published about the role of atmospheric and oceanic forcings for the development of sea ice (Perovich et al. 2011, Annals Glaciol. 52), and about the the role of Arctic sea ice as a contributor to the global albedo (Hudson 2011, JGR Oceans 116). This work goes somehow across different IPCC chapters, so I am not sure where it would be the best place to refer to it; it might be considered to be mentioned in 4.2.2.5. [Sebastian Gerland, Norway]	Accepted - same as 4-209
4-211	4	11	31	11	31	"sea-ice edge advance" [J. Graham Cogley, Canada]	Noted. Style defer to TSU.
4-212	4	11	38	11	43	There are inconsistent end dates in the reported sea ice changes. Figures 4.3, 4.4. and 4.7 present Arctic sea ice information updated to 2010 or 2011. So why discuss changes only to 2007 in this paragraph? [Chris Derksen, Canada]	Accepted. The results quoted are for the dates indicated in the published paper. We will ask the authors to update their results.
4-213	4	11	38	11	55	In situ sea ice work during the last IPY gave additional information and insight about the phases from freezing to melting conditions in the Arctic Basin. Nicolaus et al. 2010 (JGR Oceans 115) investigated the melt onset in situ and intercompared findings with satellite derived data, showing that there can be discrepancies. Potentially more research is necessary in this direction. [Sebastian Gerland, Norway]	Noted.
4-214	4	11	38	11	55	In situ sea ice work during the last IPY gave additional information and insight about the phases from freezing to melting conditions in the Arctic Basin. Nicolaus et al. 2010 (JGR Oceans 115) investigated the melt onset in situ and intercompared findings with satellite derived data, showing that there can be discrepancies. Potentially more research is necessary in this direction. [Sebastian Gerland, Norway]	same as above
4-215	4	11	40	11	40	± ~0.25 rather than ± 0.20 days yr-1 to match the ± 7 days? [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted.
4-216	4	11	45			It would be telling to have a figure showing the lengthening of the ice free season (Page 4-11, lines 45-55). [Richard Bintanja, Netherlands]	Noted. Not a published report.
4-217	4	11	48	11	51	"Because melting surfaces (wet snow, melt ponds, and open water) have low albedo, they absorb more solar energy. Thus longer melt seasons mean greater energy absorption in total, and enhance the ice-albedo feedback." But in fact this is the *opposite* of what is usually understood by "ice-albedo feedback". [J. Graham Cogley, Canada]	Accepted. Text revised.
4-218	4	12	1	12	1	Suggest to swap Oden with Polynyas in the title, because the Odden was discussed first. [Zhaomin Wang, UK]	Accepted
4-219	4	12	8			"…has occurred rarely since 2000." Qualitative. [Chris Derksen, Canada]	Rejected
4-220	4	12	12	12	12	"halocline" should appear in the Glossary [J. Graham Cogley, Canada]	Noted - Halocline is now defined in the Glossary
4-221	4	12	16	12	16	"Polynya in northern Baffin Bay" [J. Graham Cogley, Canada]	Rejected. Commonly used name

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4-222	4	12	36			ditto comment no 5 [Luzi Bernhard, Switzerland]	Accepted
4-223	4	12	39	12	39	Replace 'extent' with 'ice concentrations'. [Jacob Clement Yde, Norway]	Accepted
4-224	4	12	41	12	42	For me, the Antarctic Circumpolar Wave and the trend in sea ice concentration are not linked and there is no recent paper on the ACW that justifies this sentence that is mentioned in the section. [Hugues Goosse, Belgium]	Accepted.
4-225	4	12	44	12	44	Positive trends not visible on Figure 4.6b (winter) or 4.6c (spring) in the Weddell Sea. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted for eastern weddell sea
4-226	4	12	48	12	48	Should “cover” be “extent”?. Trend in FAQ4.2 Figure 1 is +1.3%/decade but in Section 4.2.4 (Synthesis) is +1.1%/decade. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted - we will strive to get consistency by using the same data record in the various analysis.
4-227	4	12	50	12	52	Explain in a parenthesis why -0.77+/-0.39 is not a significant trend (because "significant" means "significant at the 95% level") [J. Graham Cogley, Canada]	Rejected "Significant" is not part of the IPCC uncertainty language
4-228	4	12	54	12	54	Change “inter-annual” to decadal. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted
4-229	4	12	56	12	56	Here state Antarctic Circumpolar Wave (ACW), however the long term behavior of ACW has not enough evidence. I t may happninng however not relevant to explain driving function. [Hiroyuki Enomoto, Japan]	Noted. ACW affects regional extent and depending on revisit time, the trend would be affected.
4-230	4	12	56	12	56	The link with the Southern Annular Mode and the Antarctic Circumpolar Wave is not clear to me and references should be given to justify this sentence or the revised one for SOD. [Hugues Goosse, Belgium]	Rejected
4-231	4	12	56	12	56	Insert reference after 'Wave' to support this statement. [Jacob Clement Yde, Norway]	Rejected
4-232	4	12	57	12	57	LIU AND CURRY 2010 SUGGEST THAT POSITIVE TRENDS ARE LINKED TO INCREASE IN SNOWFALL AND FRESH WATER PNAS 107 P. 1487 [Roger Barry, USA]	rejected - attribution. Covered In ch 12
4-233	4	13	3	13	3	Change to “…1999 to 2008 in gold, and 2007-2011 in black)” [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted
4-234	4	13	11	13	15	The work by Tivy et al (2011) as mentioned in comment 9 utilized a similar approach for the Arctic and should perhaps be mentioned in section 4.2.2 [Sharon Smith, Canada]	Rejected. Does not add to content
4-235	4	13	15	13	15	delete "!" [Yongjian Ding, China]	Accepted
4-236	4	13	15	13	16	"They found that maximum sea ice volume was reached later than maximum extent." [J. Graham Cogley, Canada]	Accepted
4-237	4	13	18	13	18	Mahoney et al., 2007 relates to Alaska not the Antarctic. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted
4-238	4	13	36	13	42	This comparison of sea ice changes in the Antarctic vs Arctic is better suited for the synthesis in Section 4.2.4. [Chris Derksen, Canada]	Rejected - this para discusses regional differences in timing of ice advance and retreat. It is not a discussion of overall differences between Arctic and Antarctic sea ice
4-239	4	13	36	13	42	SAM needs to be invoked to explain the asymmetric seasonal response of sea ice. For the sea ice change discussed in lines 29-34, they are apparently linked to the upward trend in SAM, because there was an associated deepening of Amundsen Low, which caused increased waem advection around Antarctic Peninsula and Bellingshausen Sea and increased cold advection in the adjacent western Ross Sea. [Zhaomin Wang, UK]	Rejected - no reference is given to support this "apparent link"
4-240	4	13	47	13	47	I am told that most members of the public understand "enhanced" to mean "better". Perhaps subsitute "more". [J. Graham Cogley, Canada]	Changed to increased.
4-241	4	13	49	13	49	A recent reference should be given if a link with SAM or the Antarctic Circumpolar Wave has been	Noted

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						demonstrated or this sentence should be modified. [Hugues Goosse, Belgium]	
4-242	4	13	51	13	51	It is wrong to say "since the late 1970s the SAM has been negative or slightly positive". It should be replaced by "since the late 1970s the SAM has been neutral or positive". [Zhaomin Wang, UK]	Accepted - SAM index depends on how normalized
4-243	4	13	55	13	55	The decrease of 8 km3 per year is for the Ronne Polynya only: the Eastern Weddell Polynya trend partly cancels it when considering the whole Weddell Sea. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Noted
4-244	4	13				The term 'Polynyas' should be defined. [Nadine Salzmann, Swizerland]	Noted - Polynya is defined as an "anomalous regions of open water or low ice concentration" when first introduced in section 4.2.2.6
4-245	4	14	10	14	19	Land fast ice anomaly and coastal ice conditions are reported by several Ice breaker voyages, more references may be added. [Hiroyuki Enomoto, Japan]	Rejected. There are other papers that discuss fast ice characteristics and seasonality. But no others that we are aware of discuss change over large areas.
4-246	4	14	16	14	20	This paragraph can be updated to include the new Randolph Glacier Inventory (http://glims.org:8080/RGI/randolph.html) [W. Tad Pfeffer, United States of America]	Accepted
4-247	4	14	21	14	57	Reconstructed changes in Arctic sea ice over the past 1,450 years Christophe Kinnard: A paper by Kinnard, Nature 479,509–512(24 November 2011)doi:10.1038/nature10581pointed out extreamly warm phase is occuring in this cencury. [Hiroyuki Enomoto, Japan]	Not within scope. But have included in new subsection on pre-satellite record.
4-248	4	14	24	14	24	"characteristics, and especially the mean thickness". Thickness changes deserve to be singled out, I suggest. [J. Graham Cogley, Canada]	Accepted -
4-249	4	14	28	14	29	use non-breaking hyphen. It is very unclear that this is -4% [Alan Robock, USA]	Accepted
4-250	4	14	31	14	31	"supply" should be "supplies" [W. Tad Pfeffer, United States of America]	Editorial (should be page 15, line 27)
4-251	4	14	33	14	33	"climate conditions and relief" should be "climate conditions and topographic relief" [W. Tad Pfeffer, United States of America]	Editorial (should be page 15, line 30)
4-252	4	14	34	14	34	at end of line, change to "The topographic relief also modifies…" [W. Tad Pfeffer, United States of America]	Editorial (should be page 15, line 32)
4-253	4	14	45	14	45	Table 4.2: Emphasize that these are *global* characteristics [W. Tad Pfeffer, United States of America]	Noted: the table will be modified extensively (should be page 15, line 43)
4-254	4	14	51	14	51	"status of change in" [J. Graham Cogley, Canada]	Editorial
4-255	4	15	1	15	1	Last line of table 4.2: "Large Regions" for Accuracy diesn't really make sense. [W. Tad Pfeffer, United States of America]	Noted: the table will be modified extensively (should be page 16)
4-256	4	15	8	15	10	Uncertainty arising from incomplete inventory could also produce underestimates; e.g. in Alaska, records from Gulkana and Wolverine glaciers are very likely underrepresentations of conditions in the St. Elias. [W. Tad Pfeffer, United States of America]	Noted and taken into account in modification
4-257	4	15	10	15	10	The paucity of measurements on calving glaciers also means that potential for dynamic response in GICs via marine-terminating glaciers is underestimated. [W. Tad Pfeffer, United States of America]	Taken into account: text modified
4-258	4	15	10	15	16	It would be useful, right at the start of this section, to explain to readers what they mean by ‘glaciers’. Do they include glaciers in Greenland and Antarctica, and if so which ones; where do they stop? This requires clear explanation, and should be consistent, not only within this Chapter, but also in Chaper 13. [Robert Thomas, USA]	Taken into account: text modified
4-259	4	15	10	19	14	As a general comment, almost all the publications about the intertropical Andes for mass balance, volume changes, surface changes are missing (e.g. Francou et al. JGR 2003, 2004; Soruco et al., AOG and GRL	Noted: only regional studies are included in the assessement

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						2008; Rabatel et al., JOG 2006, Quat Res, 2009; Jomelli et al., Palaeo3, 2010; Vuille et al, ESR, 2010; etc....). [Antoine Rabatel, FRANCE]	
4-260	4	15	10			Section 4.3 Glaciers: this section has been greatly improved since the ZOD, especially in terms of observation and data. My impression is that the authors made efforts to be short and concise which is definitely desirable. I noted that data and results (e.g. numbers of glacier shrinkage, mass loss etc) are almost exclusively provided in figures and tables. I wonder whether it may be possible to provide some of the most important observation results also in the text. I furthermore noted that some of the text is dedicated to basics of glaciology (e.g. response to climate). For a broader audience this is likely appropriate. Some of these explanations may nevertheless moved into the Glossary. [Christian Huggel, Switzerland]	Taken into account. Text was improved. Technical terms are only explained if not falling into Glossary rules.
4-261	4	15	12	15	13	The reference to the incomplete inventory in AR4 probably needs some more explanation for readers not familiar with that particular aspect. (inventory of what exactly, what was the status then, etc). [Christian Huggel, Switzerland]	Taken into account: since the complete inventory is available for the SOD, the respectivve text portions have changed.
4-262	4	15	12	15	16	How is the plan to integrate the remaining percent in the report? Actualized in SOD? [Luzi Bernhard, Switzerland]	Taken into account: the inventory is completed now and credit is given in the text
4-263	4	15	12	15	16	Since glaciers technically also include the ice sheets it would be good to briefly define here which ice masses are meant (all land ice other than the Greenland and Antarctic ice sheet but including peripheral glaciers). [Regine Hock, US]	Accepted: definition provided.
4-264	4	15	12	15	16	Statement that content of current inventories equals to 42% or 48% of worldwide glaciers looks not substantiated, because when the whole coverage (number/area of glaciers) is unknown how to estimate the part of the rest (known) coverage. [Vladimir Konovalov, Russian Federation]	Taken into account: inventary is completed now and credit is given in the text
4-265	4	15	12	15	16	Major issue on Detailed World Glacier Inventory vs. Preliminary Global Estimates: This statement, as well as the corresponding one on page 4-6 (remark 16, above), implies that an enormous progress has been made in completing the (detailed) world glacier inventory (AR4: 42% vs. AR5: "nearly complete"). However, this statement ignores the inherent difference between a 'detailed inventory' and a 'preliminary estimate' - in other words, it compares apples and oranges. Some background information: There are two detailed glacier inventories (which are regularly updated): - The World Glacier Inventory (WGMS and NSIDC 1989, updated 2012) including geographical coordinates of a label point, a time stamp and corresponding tabular information about classification, area, length, orientation and altitude range of (meanwhile) more than 130,000 glaciers; mainly based on aerial photographs and maps. - The GLIMS database (GLIMS 2011) which can be viewed as the logical extension of the (detailed) WGI mainly based on optical satellite instruments and storing (time stamped) digital glacier outlines and the full complement of WGI-parameters for roughly 100,000 glaciers. Since these detailed inventories are not (yet) complete, there have been several efforts to provide preliminary estimates of the global glacier coverage. Note that these preliminary estimates come without exact time stamp and without glacier-specific attributes. A first one was included in WGMS (1989) based on the detailed WGI available at that time and estimates for missing regions based on maps and early satellite images. Note that these were based on (analogue) glacier outlines but its digital storage was not feasible at that time. This first estimate were later refined with information from other sources by Dyurgerov and Meier (2005). Other studies used the detailed WGI for regional upscaling approaches (e.g., Meier and Bahr 1996, Radic and Hock 2010) or for a global percentage map of glacier coverage at a spatial resolution of 1°x1° (Cogley 2003, updated 2007). A first global map of (rough) digital boundaries of glacierized regions was derived from ESRI's Digital Chart of the World (Danko 1992) and other sources by Raup et al. (2000). The new 'inventory' by Arendt et al. (2012) consists of digital glacier outlines from various sources (GLIMS, DCW, WGI, and other regional inventories) without time stamps and without glacier-specific attributes. Hence, it is to be seen as the latest version of a preliminary estimate of the global glacier coverage. Clearly, the global (almost) complete availability of digital glacier outlines (at various quality levels) is a major step forward (as	Taken into account: clarification is provided

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						compared to the DCW product or Cogley's GGHYDRO) and certainly of great use for global modelling studies (e.g. related to sea level rise). However, this dataset is far from being the completion of the detailed glacier inventories. Suggestion: Replace the present paragraph with a short summary of the background (with correct references to the corresponding datasets) and highlight the (real) benefit of the new digital global glacier map. References: Arendt et al. 2012 => see references on page 4-40, ff. Cogley, J.G., 2003, GGHYDRO - Global Hydrographic Data, Release 2.3.1, Trent Technical Note 2003-1, Department of Geography, Trent University, Peterborough, Ontario, Canada. Revised January 2007. Danko, D. M. (1992). The digital chart of the world project. Photogrammetric engineering and remote sensing, 58(8), 1125-1128. Dyurgerov and Meier 2005 => see references on page 4-40, ff. GLIMS (2011): GLIMS glacier database. Armstrong, R., B. Raup, S.J.S. Khalsa, R. Barry, J. Kargel, C. Helm, and H. Kieffer. 2011 (eds.), National Snow and Ice Data Center, Boulder, Colorado USA: Digital media. Meier, M. F., and Bahr, D. B., 1996: Counting glaciers: use of scaling methods to estimate the number and size distribution of the glaciers of the world. In Colbeck, S. C. (ed.), Glaciers, ice sheets, and volcanoes: a tribute to Mark F. Meier. U.S. Army Corps of Engineers, Cold Regions Research & Engineering Laboratory (CRREL) Special Report 96-27, 89–94. Radic and Hock 2010 => see references on page 4-40, ff. Raup, Bruce H., Hugh H. Kieffer, Trent M. Hare, and Jeffrey S. Kargel (2000). "Generation of Data Acquisition Requests for the ASTER Satellite Instrument for Monitoring a Globally Distributed Target: Glaciers." IEEE Transactions On Geoscience and Remote Sensing 38:1105--1112. WGMS (1989): World glacier inventory - Status 1988. Haeberli, W., Bösch, H., Scherler, K., Østrem, G. and Wallén, C. C. (eds.), IAHS (ICSI) / UNEP / UNESCO, World Glacier Monitoring Service, Zurich, Switzerland: 458 pp. WGMS & NSIDC (1989, updated 2012): World Glacier Inventory based on WGMS (1989): World glacier inventory - Status 1988. Haeberli, W., Bösch, H., Scherler, K., Østrem, G. and Wallén, C.C. (eds.), IAHS (ICSI) / UNEP / UNESCO. World Glacier Monitoring Service, Zurich, Switzerland, and National Snow and Ice Data Center, Boulder CO, U.S.A. Digital Media. [Michael Zemp, Switzerland]	
4-266	4	15	12		15	It is written that the inventory has been considerbly enhanced. The change from 42% to 48%, however, is not huge. It is written that the inventory will be completed by 2011. Will the complete inventory finally be included in the AR5? Would be important. [Nadine Salzmann, Swizerland]	Noted: inventory has been completed and now basis of assessment
4-267	4	15	14	15	31	Again, this paragraph can be revised in light of the new Randolph Glacier Inventory [W. Tad Pfeffer, United States of America]	Noted: inventory has been completed and now basis of assessment
4-268	4	15	15			48% does not seem like much of an improvement over the previous 42% [antony payne, uk]	Noted: inventory has been completed and now basis of assessment
4-269	4	15	15			Arendt and Al looks like very grey literature. No publication information other than title. [antony payne, uk]	Noted: reference is now meeting the standards
4-270	4	15	20	15	25	Sentences in this section too long, difficult to read. Split sentences, keep content. [Olaf Eisen, Germany]	Accepted: text has been restructured and refined.
4-271	4	15	22			Give a reference supporting this statement ("...back to the 16th and 17th century are among the longest..."). [Michael Zemp, Switzerland]	Accepted: reference has been included
4-272	4	15	23	15	23	While it is true that glaciers were shrinking since 1850, this statement seems misleading. Glaciers were in an extremely advanced state in 1850. Also, almost all glaciers were in a transient state far from equilibrium. In that sense it is misleading to use the extreme 1850 extent as a reference for state for glaciers. [Martin Lüthi, Switzerland]	Taken into account: text has been revsed accordingly
4-273	4	15	23	15	23	"since about 1850". The Little Ice Age maximum is in several places dated from the mid-17th to the mid 19th	Taken into account: text has been revsed accordingly

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						centuries depending on the region and depending on the glaciers, so a range of dating should be prefered to this single dating. [Antoine Rabatel, FRANCE]	
4-274	4	15	23	15	23	“glaciers shrank considerably since about 1850”. I agree with this statement but the statement should be attenuated, because as written here, it lets us think that the shinkage has been continuous since 1850, which is far from reality. Actually, it is well known that glaciers have experienced various periods of shinkage and growth during the 20th century. See the examples of Alpine glaciers [e.g. Vincent, 2002] which were in steady state or even advancing during the periods 1900-40 or 1952-85. I would complete the sentence as follow : “in overall, glaciers shrank considerably since about 1850, but still with some periods during the 20th century where some of them were in steady state or slightly advancing”. Ref : Vincent, C. 2002. Influence of climate change over the 20th century on four French glacier mass balances. J. Geophys. Res., 107(D19), 4375. (10.1029/2001JD000832.) [Patrick Wagnon, France]	Taken into account: text has been revsed accordingly
4-275	4	15	23			Give a reference supporting this statement ("...globally, glaciers shrank considerably since about 1850..."). [Michael Zemp, Switzerland]	Taken into account: text has been revsed accordingly
4-276	4	15	24	15	24	"only available for a few glaciers worldwide" - give a specific example; only 37 glaciers world-wide with records 40 years long or more. This is given at 4-17-49, but maybe should be moved up here at its first appearance. [W. Tad Pfeffer, United States of America]	Taken into account: text has been revsed accordingly
4-277	4	15	26	15	28	Missing reference: Pederson, GT, Fagre, DB, Gray, ST and LJ. Graumlich 2004. Decadal-scale climate drivers for glacial dynamics in Glacier National Park, Montana, USA. Geophys. Res. Letters, Vol 31, L12203 [Luzi Bernhard, Switzerland]	Rejected: the study concentrates on Detection and Attribution.
4-278	4	15	26	15	28	The impact of glacier change on regional water supply isn’t discussed in Chapter 3 (or Chapter 2) at present. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Editorial: should be WG2 Ch3
4-279	4	15	30	15	30	ALLOW [Roger Barry, USA]	Taken into account: text has been modified
4-280	4	15	30	15	30	remove the "s" to "allows" [Antoine Rabatel, FRANCE]	Taken into account: text has been modified
4-281	4	15	30			Might be as well to mention that snow turns to firn turns to ice which then starts to flow. [antony payne, uk]	Taken into account: text has been modified
4-282	4	15	31			The term 'warmer elevation' should be replaced by e.g. lower elevations and thus higher air temperatures [Nadine Salzmann, Swizerland]	Taken into account: text has been modified
4-283	4	15	33			…due to direct solid precipitation and snow re-distribution by wind and avalanches [Nadine Salzmann, Swizerland]	Taken into account: text has been modified
4-284	4	15	34	15	34	reword the sentence into: Ablation is due to melt and runoff, and, … [Christian Huggel, Switzerland]	Taken into account: text has been modified
4-285	4	15	34	15	34	"Ablation is mostly due to runoff of melt-water", the formulation has to be changed, for example : "Ablation results mostly from melting" [Antoine Rabatel, FRANCE]	Taken into account: text has been modified
4-286	4	15	35	15	34	remove the "s" to "contributes", because the subject of the verb are "sublimation" and "wind-blown" [Antoine Rabatel, FRANCE]	Taken into account: text has been modified
4-287	4	15	35	15	35	CONTRIBUTE [Roger Barry, USA]	Taken into account: text has been modified
4-288	4	15	35	15	35	contributes without s, as sublimation as well as loss of wind-blown snow both contribute [Patrick Wagnon, France]	Taken into account: text has been modified
4-289	4	15	35			contribute [antony payne, uk]	Taken into account: text has been modified
4-290	4	15	37	15	37	That ablation is mostly due to melt water runoff is the Alpine perspective. On many very high glaciers close to the tropics, evaporation is the main process of mass removal. [Martin Lüthi, Switzerland]	Taken into account: text has been modified
4-291	4	15	40			would be as well to say what the estimated number of glacier in the world is [antony payne, uk]	Taken into account: numbers are now given from the

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							complete inventory
4-292	4	15	40			glaciers is being used as a umbrella term for glaciers and ice caps but no mention of ice caps here. Might be worth saying that as glaciers grow/flow they may submerge topography at which point they become ice caps. what is the number of ice caps? Figure 2 would have a better home here. [antony payne, uk]	Rejected: it was agreed within WG1, 2nd LA meeting, to not make use of the misleading term ice caps
4-293	4	15	43	15	43	Citing individual contributions for these standard methods seems inappropriater. The methods are widely applied, and have sometimes been in use for more than a century. It is also striking, that the first four references in Table 4.7 refer to work that is closely linked to the institution where one of the lead author is employed. This is untenable for an assessment like the IPCC report. My recommendation: leave away all references in Table 4.7. [Martin Lüthi, Switzerland]	Accepted: References were removed
4-294	4	15	43	15	43	Table 4.7, “Length Reconstruction” is incomplete: Length reconstructions are sometimes going back to the year 600, or even mucch earlier (Holzhauser for Aletsch, Gorner) [Martin Lüthi, Switzerland]	Taken into account: the Table was modified
4-295	4	15	43	15	43	Profiling has been done before by topographic relevations. For example the nivellement of Greenland by the EGIG traverses (1953), and older expeditions like deQuervain (1912). [Martin Lüthi, Switzerland]	Rejected: We only refer here to glaciers (and laser/radar altimeters)
4-296	4	15	43	15	43	In Table 4,2 for the line "AREA" and the sub-line "Remote sensing", change 1984 (in the colomn "Earliest data") by 1955, (see Soruco et al, GRL, 2009) [Antoine Rabatel, FRANCE]	Taken into account: the Table has been modified
4-297	4	15	43	16	1	Table 4.2: This Table is incomplete and misleading. Totally lacking is any reference to thickness and its measurement, nor to mass balance, both surface and total; nor indeed to ice motion. Presumably the first 2 entries on line 16 refer to surface elevation and its change with time. This needs clarifying. The 3rd entry (Gravimetry) presumably refers to mass and its changes??. If so, it should be clearly stated [Robert Thomas, USA]	Taken into account: the Table was modified.
4-298	4	15	43			The table needs revisions (or may be simply omitted). The given ranges are too rigid. It should be made clear that these are rough numbers. Some of them are simply wrong. For example mass balance measurements have been made on far more glaciers than 50. Also reconstructions of glacier margins go back to the ice age. What is meant by 10-10,000 glaciers for 'remote sensing, area'. Why this range. This has certainly been done for far more glaciers than 10? Same applies to 'maps'. DEM differencing has also certainly be done for more than 100 glaciers from space. Giving the number of glaciers here does not seem a very useful quantity. [Regine Hock, US]	Taken into account: the Table has been modified.
4-299	4	15	43			Table 4.2. The reference list is highly biased towards the institute of one of the lead authors. There is dozens/hundreds of studies in each of the categories and it appears very random to just choose one. [Regine Hock, US]	Accepted: References were removed
4-300	4	15	43			Table 4.7, “Field” is incomplete: Length surveys are going back to 1880 (Rhone) or 1881 (Aletsch). Accuracy is probably of the order of 10 Meters, since it is usually impossible to clearly define the end of a glacier. [Martin Lüthi, Switzerland]	Taken into account: the Table has been modified.
4-301	4	15	43			Table 4.7, “Area Maps” is incomplete: The oldest maps are going back to 1846 (Aletsch) [Martin Lüthi, Switzerland]	Taken into account: the Table has been modified.
4-302	4	15	43			Table 4.2, line “Mass Field” : the date of the earliest data are from 1947 or 1908? Why two dates? And actually, I thought that the earliest continuous series of mass balance was from Clariden (Switzerland) [Müller and Kappenberger, 1991] Ref : Müller, H., and G. Kappenberger (1991), Claridenfirn, Messungen 1914–1984, Zürcher Geogr. Schrift., 40, 79S [Patrick Wagnon, France]	Taken into account: the Table has been modified.
4-303	4	15	45	15	45	Give the km^3 water to SLE conversion (362 km^3/mm SLE). I get asked this all the time, and many readers won't know how this works. [W. Tad Pfeffer, United States of America]	Accepted: conversion factor is given explicitly in the introduction as it applies to all such calculations (ice sheets and glaciers)

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4-304	4	15		20		General comment to section 4.3. Particularly in view of the great debate on glaciers in AR4, I suggest that the glacier part in AR5 needs special attention and should be wirtten with greates caution and in a much more coherent and structured way than it is in the current FOD. The whole section is in my view not easy readable and lacks clarity and a clear structure. I believe with an improved structure and clear main messages about current knows and unknows, the section could be improved a lot. [Nadine Salzmann, Swizerland]	Taken into account: the Chapter structure was revised for more clarity and better readability
4-305	4	15				Section 4.3. The Glacier section lacks a sub-section addressing the causes and processes involved in glacier response to climate change (similar to the section "Causes of Changes in Ice Sheets") [Etienne BERTHIER, France]	Taken into account: tide water processes are included by modification of text in section 4.4.4
4-306	4	15				Section 4.3. Some important research papers are not cited. I understand the difficulty to provide a comprehensive overview of a rapidly growing literature but I suggest below some references that led to major improvements of our understanding of glacier response to climate changes and that could be cited (and they would feed the "process" sub-section mentioned above). My list is certainly not exhaustive. [Etienne BERTHIER, France]	Noted
4-307	4	15				Ohmura, A. 2006. Changes in mountain glaciers and ice caps during the 20th century. Annals of Glaciology, 43, 361-368. or the updated paper cited below: [Etienne BERTHIER, France]	Noted
4-308	4	15				Ohmura, A. 2011. Observed Mass Balance of Mountain Glaciers and Greenland Ice Sheet in the 20th Century and the Present Trends. Surveys in Geophysics, 32(4-5), 537-554. [Etienne BERTHIER, France]	Taken into account: the reference was assessed
4-309	4	15				Huss, M., M. Funk and A. Ohmura. 2009. Strong Alpine glacier melt in the 1940s due to enhanced solar radiation. Geophys Res Lett, 36(L23501), 1-5. [Etienne BERTHIER, France]	Taken into account: the reference was assessed
4-310	4	15				Oerlemans, J., R.H. Giesen and M.R. Van den Broeke. 2009. Retreating alpine glaciers: increased melt rates due to accumulation of dust (Vadret da Morteratsch, Switzerland). Journal of Glaciology, 55(192), 729-736. [Etienne BERTHIER, France]	Taken into account: the reference was assessed
4-311	4	15				Glasser, N.F., S. Harrison, K.N. Jansson, K. Anderson and A. Cowley. 2011. Global sea-level contribution from the Patagonian Icefields since the Little Ice Age maximum. Nature Geoscience, 4(5), 303-307. [Etienne BERTHIER, France]	Taken into account: the reference was assessed
4-312	4	15				Leclercq, P.W. and J. Oerlemans. 2012, in press. Global and hemispheric temperature reconstruction from glacier length fluctuations. Climate Dynamics. [Etienne BERTHIER, France]	Taken into account: the reference was assessed
4-313	4	15				Vincent, C., E. Le Meur, D. Six, M. Funk, M. Hoelzle and S. Preunkert. 2007. Very high-elevation Mont Blanc glaciated areas not affected by the 20th Century climate change. Journal of Geophysical research, 112(D9), D09120. [Etienne BERTHIER, France]	Taken into account: the reference was assessed
4-314	4	15				Vincent, C., E. Le Meur, D. Six, P. Possenti, E. Lefebvre and M. Funk. 2007. Climate warming revealed by englacial temperatures at Col du Dome (4250 m, Mont Blanc area). Geophysical Research Letters, 34(16). [Etienne BERTHIER, France]	Taken into account: the reference was assessed
4-315	4	15				Table 4.2. "Tradition and modern survey" is unclear to me [Etienne BERTHIER, France]	Taken into account: the Table has been revised
4-316	4	15				Table 4.2. The references used to illustrate the different methods of glacier surveying are biased toward self-citation. I think that for each method it would make more sense to cite the first paper that used the technique. Some propositions below [Etienne BERTHIER, France]	Taken into account: the Table was revised
4-317	4	15				Table 4.2. Laser altimetry profiling. Echelmeyer, K.A. and others. 1996. Airborne surface profiling of glaciers: A case-study in Alaska. Journal of Glaciology, 42(142), 538-547. [Etienne BERTHIER, France]	Noted
4-318	4	15				Table 4.2. satellite DEM differencing. Berthier, E., Y. Arnaud, D. Baratoux, C. Vincent and F. Remy. 2004. Recent rapid thinning of the "Mer de Glace" glacier derived from satellite optical images. Geophysical Research Letters, 31(17), L17401. [Etienne BERTHIER, France]	Noted
4-319	4	15				Table 4.2. Gravimetry (GRACE). Tamisiea, M.E., E.W. Leuliette, J.L. Davis and J.X. Mitrovica. 2005. Constraining hydrological and cryospheric mass flux in southeastern Alaska using space-based gravity	Noted

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						measurements. Geophysical Research Letters, 32(20). [Etienne BERTHIER, France]	
4-320	4	15				The structure of the chapter can be improved to make it more readable. There is a chapter 'Observed changed in length, area and mass giving some numbers but then chapters 4.3.4 and 4.3.5 are also about observed changes of these variables. If the logic is meant to be that 4.3.3 covers local / single glacier changes, 4.3.3 regional and 4.3.4 global changes than the headers should make that clearer. Also this logic is not followed consistently either. In 4.3.3 there is results about regions. It seems to make more sense to report on length, area and mass changes as a chapter each and report on the different scales in each of these subchapters. If not, at least the subchapters need to be 'cleaned up', i.e. the headers should make clear the logic and the content reflect the headers. [Regine Hock, US]	Accepted: The entire Ch. 4.3 has been restructured
4-321	4	15				Table 4.2: Row Area RS / Column Earliest data: 1984: is this an example or supposed to be globally valid? In many regions remote sensing data earlier than 1984 are available and were used for glacier mapping (e.g. Landsat MSS, cf e.g. Satellite image atlas of the world). For GRACE: can 'large regions' be specified? [Christian Huggel, Switzerland]	Taken into account: the Table has been modified
4-322	4	15				Table 4.2, last line, penultimate column: define "we" [Alan Robock, USA]	Taken into account: the Table has been modified
4-323	4	15				In Table 4.2, it is not clear to me what we is. [Zhaomin Wang, UK]	Taken into account: the Table has been modified
4-324	4	15				Table 4.2: I am not sure whether this table provides enough significant information, not already mentioned in the body text, to justify its presence. Much of the table content is debatable. For instance, I guess glacier changes in length have been reconstructed back to at least the Younger Dryas; remote sensing of glacier lengths may have an accuracy much less than 100 m; some traditional field mass balance studies are been conducted at glaciers larger than 10 km2. Also, the reasoning behind the references seems inexplicable. For instance, why is the study by Paul et al. (2004) referenced and not a compilation of typical studies? I suggest this table is deleted and any relevant information added to the body text. At least, I recomment that the reference column is deleted. [Jacob Clement Yde, Norway]	Taken into account: the Table has been modified
4-325	4	15				Table 4.2 - what is the exact meaning of "number of glaciers": e.g., total number of glaciers with available observations (for glaciological mass balance: about 300) , number of glaciers with current observations (for glaciological mass balance: about 100)? - what s the exact meaning of "Tradition and modern survey": glaciological vs. geodetic mass balance surveys? point mass balance vs. glacier mass balance? [Michael Zemp, Switzerland]	Taken into account: the Table has been modified
4-326	4	16	6	16	7	It is not correct that "the largest contributions to sea level rise came from ... the Asian high mountains" (see comment 1). Besides, this statement was done at the "large uncertainty and incompleteness in glaciers data" (see next three sentences on the page 16). [Vladimir Konovalov, Russian Federation]	Taken into account: text modified to make clearer that here a summary of AR4 findings is given
4-327	4	16	7	16	7	P4-16, line 7 : Is it accepted and sure that the largest contribution to sea level rise comes from Asian high mountains? A reference should be added here. And given the lack of data in this region of the world, I am not convinced to include here Asian glaciers, together with Alaska and Arctic. Some on-going studies are revealing that Asian glaciers (western Himalaya) are not contributing [Gardelle et al, Submitted] Gardelle, J, E. Berthier, and Y. Arnaud, Karakoram glaciers slightly gained mass in the early 21st century, Nat. Geosc. Submitted. [Patrick Wagnon, France]	Taken into accout: text is modified for clarification, new literature will be enclosed
4-328	4	16	9	16	10	I guess you mean: 'where the glaciers most frequently monitored tend to be small. [Regine Hock, US]	Accepted: Text has been adjusted.
4-329	4	16	10			Smaller glaciers are also of great interest for IPCC purposes as they respond directly to climate fluctuations. Large glaciers also depends of fluids mechanics that introduce noice in the climatic interpretation [Juan Ignacio López Moreno, Spain]	Noted
4-330	4	16	11	16	11	Cogley et al. 2009c, cited here, is the same as Cogley et al. 2009a in the References. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account: The text has been rewritten.

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4-331	4	16	15	16	15	Cogley et al (2009b) and Radic and Hock (2010). Did these authors realized the inventory or did they "just" use the data of inventories realised by the community? This point should be specified. [Antoine Rabatel, FRANCE]	Taken into account: clarification is provided
4-332	4	16	15	16	23	The statements in this paragraph are not completely clear. The key issues here need to be clearly stated: (1) Current WGI (Cogley, 2009) contains 48% of estimated global glacier-covered area. (2) Using statistical methods for extrapolating WGI and deriving volume from volume-area scaling Radic&Hock (2010) produced a new estimate of global glacier area and volume, including glaciers surrounding the Greenland and Antarctic ice sheets (Table 4.3) (3) For AR5, WGI is updated and complete (covering 100% of global glacier-covered area) and the new estimates of regional and global volumes will follow soon. [Valentina Radic, Canada]	Taken into accout: paragraph has been modified according to the new glacier inventory
4-333	4	16	15			Maybe clarify here what is an inventory (e.g., difference between an inventory and a simple glacier mask) [Etienne BERTHIER, France]	Taken into accout: see comment on comment 4-265
4-334	4	16	15			Suggestion for correct data citation: "[...] Cogley (2009b) and Radic and Hock (2010) extended the World Glacier Inventory (WGMS and NSIDC 1989, updated 2009) to approximately [...]" Reference: WGMS and NSIDC (1989, updated 2009): World Glacier Inventory based on WGMS (1989): World glacier inventory - Status 1988. Haeberli, W., Bösch, H., Scherler, K., Østrem, G. and Wallén, C.C. (eds.), IAHS (ICSI) / UNEP / UNESCO. World Glacier Monitoring Service, Zurich, Switzerland, and National Snow and Ice Data Center, Boulder CO, U.S.A. Digital Media. Note that a new update of this dataset becomes available in the coming weeks. This new version includes the extensions by Cogley (2009b), Radic and Hock (2010), and other sources and now consits of detailed information from more than 130,000 glaciers covering over 470,000 sqkm: WGMS & NSIDC (1989, updated 2012): World Glacier Inventory based on WGMS (1989): World glacier inventory - Status 1988. Haeberli, W., Bösch, H., Scherler, K., Østrem, G. and Wallén, C.C. (eds.), IAHS (ICSI) / UNEP / UNESCO. World Glacier Monitoring Service, Zurich, Switzerland, and National Snow and Ice Data Center, Boulder CO, U.S.A. Digital Media. [Michael Zemp, Switzerland]	Taken into account: clarification will be provided
4-335	4	16	17	16	17	What is meant by 'Notably absent from these'? Absent from what ? [Regine Hock, US]	Taken into account: clarification will be provided
4-336	4	16	17	16	17	P4-16, line 17, and estimates of total glacier volume in Table 4.3 . How are calculated the glacier volumes worldwide? I assume that the Bahr Equation is used to assess glacier volume from area [Bahr et al, 1997]. This volume-area relationship has the advantage to exist, but it is somehow questionable (not in agreement with some observations). I would therefore suggest to cite this reference in the text, and add some limitations regarding the accuracy of the method and the corresponding results. The error range added is Table 4.3 is rather optimistic, and should be discussed thoroughly. Bahr, D. B., Meier, M. F. & Peckham, S. D. The physical basis of glacier volume_area scaling. J. Geophys. Res. 102, 20355_20362 (1997). [Patrick Wagnon, France]	Taken into account: clarification will be provided by adding a new reference
4-337	4	16	19	16	19	Remove "the" in the formulation "Greenland the ice sheet" [Antoine Rabatel, FRANCE]	Editorial
4-338	4	16	19			In sentence "…around the Greenland the ice sheet…..", delete second "the" [Thomas Voigt, Germany]	Editorial
4-339	4	16	21	16	22	The mentioned "new global inventory" is not available for scientific community. So, it is impossible to estimate its quality. For example, area of Antarctic in Fig. 8 (Ch. 4) and in Global Glacier Changes: facts and figures http://www.grid.unep.ch/glaciers/ is very different. [Vladimir Konovalov, Russian Federation]	Noted
4-340	4	16	21			this introduction on p15 says the we are using Arendt and Al inventory which is at 48% but this text suggests the Cogley/Radic and Hock inventory is at 48% but is not being use here. All very confusing! [antony payne, uk]	Taken into account: Numbers will be revised with the new global inventory

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4-341	4	16	23			what are the asteriks here and on line 30 and in Table 4.3 meant to signify? [antony payne, uk]	Taken into account: Numbers will be revised with the new global inventory
4-342	4	16	25	16	25	worlwide' should be 'worldwide'. [Jacob Clement Yde, Norway]	Editorial
4-343	4	16	25	16	26	This is absolutely true and strongly supports the criticism of reviewer on global estimations of glaciers regime and especially their role in the change of ocean level, made in Chapter 4. [Vladimir Konovalov, Russian Federation]	Noted
4-344	4	16	25			Change 'volumes' to 'volume' [Chris Derksen, Canada]	Editorial
4-345	4	16	25			volume [antony payne, uk]	Editorial
4-346	4	16	28	16	28	An independent derivation of a very similar volume/length scaling is given by Lüthi, 2009. There, it is also shown how the scaling relation depends upon mean slope and mass balance gradient (i.e. continentality of climate), and thus how the multiplicative parameter in the scaling relation can be corrected.. [Martin Lüthi, Switzerland]	Noted
4-347	4	16	30	16	30	Figure 4.8 is about area, not volume: do you mean one or more of Figures 4.10, 4.11 and 4.12? [David Parker, United Kingdom of Great Britain & Northern Ireland]	Editorial
4-348	4	16	30	16	30	Both approaches are not shown in Figure 4.8, as the sentence suggests. [Jacob Clement Yde, Norway]	Editorial
4-349	4	16	30			no such values in the table [antony payne, uk]	Editorial
4-350	4	16	33	16	33	Figure 4.8, green numbers in italics are hard to read. Improve the Figure [Antoine Rabatel, FRANCE]	Editorial
4-351	4	16	40	16	40	Arendt and Al, 2011? [David Parker, United Kingdom of Great Britain & Northern Ireland]	Editorial
4-352	4	16	43	16	43	IN TABLE LINE 2 WHAT DO EXCLUDING/INCLUDING REFER TO? [Roger Barry, USA]	Editorial
4-353	4	16	43	16	43	It is not immediately obvious what “excluding” and “including” refers to. [Martin Lüthi, Switzerland]	Editorial
4-354	4	16	43	16	43	In table 4.3. What do "excluding" and "including" mean ? [Antoine Rabatel, FRANCE]	Editorial
4-355	4	16	43	16	43	Table 4.3: What do ”including” and ”excluding” mean? Also, it looks like the new inventory will have less glaciers than the old?? [Robert Thomas, USA]	Editorial
4-356	4	16	43			Table 4.3. Where is the number 0.72 from? In AR4 Table 4.1 the SLE of the largest estimate (excluding Antarctic and Greenland glaciers) is 0.37. Is the number correct? Can't find it in AR4. [Regine Hock, US]	Editorial
4-357	4	16				Table 4.3: Meaning of asterisk not clear. [Chris Derksen, Canada]	Editorial
4-358	4	16				Table 4.3: If one just looks at the table it is unclear what ex/including means. Should be explained in the caption. [Olaf Eisen, Germany]	Editorial
4-359	4	16				Table 4.3: Excluding / including: glaciers surrounding the Greenland and Antarctic ice sheets I presume. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Editorial
4-360	4	16				Table 4.3: In row 1, '[m]' should be '[m we]' [Jacob Clement Yde, Norway]	Editorial
4-361	4	16				Table 4.3: Explain in the table caption what is meant by 'excluding' and 'including'. [Jacob Clement Yde, Norway]	Editorial
4-362	4	17	3			Section 4.3.3: Since AR4 there has been significant progress in reconstructing glacier length fluctuations by using aerial photographs and satellite imagery on regional scale. Therefore, instead of only showing length changes for a few selected glaciers based on WGMS (Figure 4.9), more robust regional signals will very likely be obtained from compiling and reporting results from recent glacier fluctuation studies (similar to the	Taken into account: more regions were added; a selection of glaciers per region was considered appropriate; selection criteria are provided

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						approach used for area and mass changes (Figures 4.10 and 4.11)). Figure 4.9 does not have the same high quality as Figures 4.10 and 4.11. [Jacob Clement Yde, Norway]	
4-363	4	17	5	17	5	Change 'Length variations of glaciers' to 'Glacier length changes' and make it bold. [Jacob Clement Yde, Norway]	Editorial
4-364	4	17	5	17	6	Length changes are also obtained from satellite imagery [Regine Hock, US]	Editorial
4-365	4	17	5	17	16	I would like a longer discussion of "glacier dynamics" that is not related to climate change. For instance, dynamical processes within glaciers are affecting glacier length regardless of climate variability and climate change, such as surging. For surging glaciers, a slow retreat of the terminus is expected through most of its life, interrupted with abrupt advances. Hence, a slow and long-lasting retreat is not always necessary a proof of a warmer climate. [Borgar Aamaas, Norway]	Taken into account: non climaticallly driven lenght variations are now mentioned in the text
4-366	4	17	5			Print "Length variations" in bold letters; to be consistent with "Glacier area change", "Glacier mass change" [Thomas Voigt, Germany]	Editorial
4-367	4	17	6	17	6	Length reconstructions are sometimes going back to the year 600, or earlier (Holzhauser for Aletsch, Gorner) [Martin Lüthi, Switzerland]	Taken into account: Text has been modified
4-368	4	17	6	17	6	variousyears > various years [W. Tad Pfeffer, United States of America]	Editorial
4-369	4	17	6	17	6	Insert ', satellite imagery' after 'photographs'. [Jacob Clement Yde, Norway]	Editorial
4-370	4	17	6	17	7	Delete the sentence starting with 'Being ...', as this has already been mentioned two times earlier in the chapter. [Jacob Clement Yde, Norway]	Taken into account: the respective text portion was modified
4-371	4	17	7	17	7	add "(" before "Zemp et al.," [Yongjian Ding, China]	Editorial
4-372	4	17	7	17	7	It is inappropriate to just cite WGMS, or its new director, when referring to glacier length changes. WGMS is has not contributed significantly to the measurements of these data, but merely distributes them. Why not cite the reviewed literature (e.g. Nussbaumer, Holzhauser, Masiokas, Zumbühl). [Martin Lüthi, Switzerland]	Taken into account: the text has been revised
4-373	4	17	7	17	9	The sentence needs reformulation. It is not clear. [Regine Hock, US]	Taken into account: the respective text portion was modified
4-374	4	17	7	17	9	The sentence 'Whereas glacier mass changes are an immediate and direct response to the annual atmospheric conditions, length variations are modified by glacier dynamics, so they are smoothed and delayed' is only valid for glaciers with 'normal' flow dynamics and no debris-cover. Therefore, it is appropriate to insert a following sentence on why it is not suitable to reconstruct climate change from tidewater (Post et al., 2011: A complex relationship between calving glaciers and climate. EOS Transactions, 92, 305-306), debris-covered and surge-type glaciers (Yde and Paasche, 2010. Reconstructing climate change: Not all glaciers suitable. EOS Transactions, 91, 189-190). Especially, glacier surging has to be mentioned as surge-type glaciers occur in 12 of 19 regions. Glacier surging does not only cause rapid advances, but also higher recession rates during their quiescent phase than normal flow glaciers (Yde and Knudsen, 2007: 20th-century glacier fluctuations on Disko Island, Greenland. Annals of Glaciology, 46, 209-214). Also, it will be relevant with a sentence on differences in recession rates between land and marine/lake-terminating glaciers (Gordon et al., 2008: Recent glacier changes and climate trends on South Georgia. Global and Planetary Change, 60, 72-84; Mernild et al., in review: Multi-decadal marine and land-terminating glacier recession in the Ammassalik region, Southeast Greenland. Cryosphere). [Jacob Clement Yde, Norway]	Taken into account: We only refer here to normal glaciers. The Text has been modified accordingly
4-375	4	17	7			Zemp et al., 2011 is a "PAGES news". Was it peer-reviewed and thus acceptable for IPCC? [Etienne BERTHIER, France]	Noted: the rules allow for compilation papers and we only use results from peer reviewed papers cited therein
4-376	4	17	8	17	8	A better example would be to write what an ELA change of 10 m has as consequence. This is of course strongly dependent upon inclination and mass balance gradient. For an ice sheet, in principle, this change	Noted: the comment indicates the complexity of the suggested issue which is thus not used in this section

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						could lead from stability to instability (Jouvet and Blatter, 2011). [Martin Lüthi, Switzerland]	
4-377	4	17	9	17	9	What is the meaning of “modified” and “magnified”? Temperature and length are incommensurable, so there is no “magnification”. The concept of climate sensitivity would make sense here (e.g. Klok and Oerlemans, 2002).. [Martin Lüthi, Switzerland]	Taken into account: the respective text portion was modified
4-378	4	17	9	17	9	"100 m or more" relative to what thickness? Express this as some characteristic fraction. [W. Tad Pfeffer, United States of America]	Taken into account: the respective text portion was modified
4-379	4	17	9	17	11	Delete the second half of the sentence from ' - but can ...'. The text could be misleading. There are several more common causes for glacier length changes of 100+ m than temperature changes of only 0.1C; e.g., glacier surging, tidewater glacier recession, detachment of entire debris-covered front behind the upper-most shear band, detachment of glacier front during frontal recession of steep topography (as shown for a valley glacier in FAQ 4.1, Figure 1). Also, what is the time-frame of the 0.1C change? [Jacob Clement Yde, Norway]	Taken into account: We only refer here to normal glaciers rather than to the exceptions. The Text was modified accordingly.
4-380	4	17	12	17	23	clarify that these changes refer to land-terminating glaciers [W. Tad Pfeffer, United States of America]	Accepted
4-381	4	17	13	17	13	FAQ 4.2' should be 'FAQ 4.1'. [Jacob Clement Yde, Norway]	Editorial
4-382	4	17	13			Substitute "FAQ 4.2" by "FAQ 4.1" [Thomas Voigt, Germany]	Editorial
4-383	4	17	14			i am not certain that the word robust can be used when the sample size is so small (~20 of 100,000+ glaciers). May be there is more evidence that would allow robust to be used but not figure 4.9 alone. Some stats would be good here - for what % of sampled glaciers (what sample size) is this statement true. [antony payne, uk]	Taken into account: the paragraph was modified
4-384	4	17	14			delete 'a large homogenouse signal (Fig. 4.9) given' because on line 22 you write that the changes are not globally synchronous [Nadine Salzmann, Swizerland]	Taken into account: the paragraph was modified
4-385	4	17	16	17	16	In this context term "stagnation" is not correct. It is better to say stability or stabilization. [Vladimir Konovalov, Russian Federation]	Taken into account: the paragraph was modified accordingly
4-386	4	17	16	17	16	Insert 'locally or regionally' before 'interrupted'. [Jacob Clement Yde, Norway]	Editorial
4-387	4	17	17	17	17	In Italy between 1975 and 1991, the cold period of the Eighties caused a positive mass balance and glacier advance [Michele Freppaz, Italy]	Noted
4-388	4	17	17	17	17	The caption for fig.9 should be changed as Cumulative glacier length changes as measured in the field. Data from WGMS (2008). Reason: very limited number of glaciers is not representative for their population. [Vladimir Konovalov, Russian Federation]	Taken into account: Text was modified
4-389	4	17	17	17	19	As it stands, this sentence seems passive and superfluous.Either delete the sentence or rephrase it so that it describes the results of Leclercq et al. (2011). [Jacob Clement Yde, Norway]	Taken into account: Text was modified accordingly
4-390	4	17	17			figure 4.9. wouldn't this be better as a table. What are the sample sizes? [antony payne, uk]	Noted: Showing these data in a table wouldn't make the point
4-391	4	17	19	17	19	... and to model their change in volume (Lüthi et al., 2010) [Martin Lüthi, Switzerland]	Noted: Due to a change of the text the comment no longer applies
4-392	4	17	19	17	19	Hoelzle et al. (2003) cover more than the European Alps. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Noted: Due to a change of the text the comment no longer applies
4-393	4	17	19			The sentence "In Figure … 2008)" could be move at the beginning of the paragraph whose current structure is not linear [Etienne BERTHIER, France]	Editorial
4-394	4	17	20	17	20	“Cumulative length variations” is cumbersome; replace by the simpler: “length variations”. [Martin Lüthi, Switzerland]	Rejected: The shown length changes are cumulative
4-395	4	17	22			Be more precise about the regional atmospheric conditions leading to advance and cite the appropriate reference (maybe Chinn, T., S. Winkler, M.J. Salinger and N. Haakensen. 2005. Recent glacier advances in	Rejected: This Chapter only reports observations

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						Norway and New Zealand: a comparison of their glaciological and meteorological causes. Geografiska Annaler, 87A(1), 141–157.) [Etienne BERTHIER, France]	
4-396	4	17	23	17	23	Insert reference after 'Scandinavia and New Zealand' to support this statement. [Jacob Clement Yde, Norway]	Editorial
4-397	4	17	24	17	24	Insert reference after 'Karakoram' to support this statement. [Jacob Clement Yde, Norway]	Editorial
4-398	4	17	27	17	43	Figure 4.9. X-axis and Y-axis as well as caption within the graphs are almost impossible to read. To be improved. Also, and it's an important point, for the graph "16 Low Latitude", Glacier Tyndal has to be removed as it does not belong to low latitude but to the southern patagonian ice field. [Antoine Rabatel, FRANCE]	Taken into account: The Figure was modified. We refer here to Tyndalglacier in Africa.
4-399	4	17	27			Fig 4-9 : axis, legends are barely visible and should appear bigger to be readable. The mass balance series from Zongo Glacier, measured at monthly scale in the ablation zone with the glaciological method since 1991, and extended back to 1956 thanks to geodetic methods is one of the best documented in this tropical area and should appear in the Low Latitude graph [Soruco et al, 2009]. Chacaltaya, which entirely desappeared in 2008, should not appear here. As far as I know Tyndall glacier is not a low-latitude glacier Soruco, A., C. Vincent, B. Francou, P. Ribstein, T. Berger, J. E. Sicart, P. Wagnon, Y. Arnaud, V. Favier, and Y. Lejeune (2009), Mass balance of Zongo Glacier, Bolivia, between 1956 and 2006, using glaciological, hydrological and geodetic methods, Ann.Glaciol., 50, 1-7. [Patrick Wagnon, France]	Taken into account: The Figure was modified. This figure is about length changes rather than mass balance measurements. There is another Tyndalglacier in Africa
4-400	4	17	29	17	38	I found this paragraph confusing. [W. Tad Pfeffer, United States of America]	Taken into account: The text has been revised for more clarity and better readability
4-401	4	17	30	17	39	See "Rabatel et al., The Cryosphere, 2011", presenting glacier surface change in the subtropical Andes of Chile (29°S). These data can be incorporated in this paragraph and in the Figure 4.10. See also "Rabatel et al., Quaternary Reseach, 2009", presenting data of glacier surface change since the Little Ice Age maximum for the Bolivian Cordillera Real [Antoine Rabatel, FRANCE]	Noted: We here consider only mean values over entire mountain ranges rather than individual glaciers
4-402	4	17	30	19	14	Somewhere in this section should be mentioned clearly what are the main challenges in assessing global mass balance (extrapolation of in situ measurements from a small sample of glaciers, modeling) and how much this has improved since IPCC AR4. Also, how large are the uncertainties due to different methodologies. Also, it would be good to provide a Table with regional estimates of mass balance changes (SLE) for several periods giving a range of all available estimates and an error bounds. The Figure 4.11 is a great to have but has too many overlapping curves and colors and it is hard to distinguish the actual rates of changes and how they relate among the regions. [Valentina Radic, Canada]	A) Taken into account: respective text portionhas been provided; B) Taken into account: tA Table with regional and global numbers (for different periods) has been added
4-403	4	17	31	17	31	climatic change'. This is too vague. [Regine Hock, US]	Taken into account: clarification has been provided
4-404	4	17	32	17	34	geometric changes'. Do you mean the area changes ? The example is not clear? What is meat? [Regine Hock, US]	Taken into account: clarification has been provided
4-405	4	17	36	17	36	Why is the figure referred to here. There is a chapter about regional changes later. --> inconsistent chapter structure [Regine Hock, US]	Taken into account: The text will be revised for better clarity
4-406	4	17	36	17	38	Mean area loss is a meaningless quantity. If area losses are considered, rather plot the relative area change for each individual glacier. [Martin Lüthi, Switzerland]	Rejected: Mean area loss rates are frequently compared in the scientific literature
4-407	4	17	37	17	37	recently': which time period? [Regine Hock, US]	Editorial
4-408	4	17	41	17	41	Figure 4.10 is hard to decode - there are a lot of colors that aren't very distinct from one another. Maybe annotate individual curves? (might require breaking up into two panels to magnify the section be 0 and -1% [W. Tad Pfeffer, United States of America]	Taken into account: The Figure was modified
4-409	4	17	41	17	41	Figure 4.10. It is very hard to distinguish between the colors! In such a synthesis figure, data from the Bolivian Cordillera Real (Rabatel et al., Quaternary Research, 2009) and from the subtropical Andes of Chili (Rabatel et al., The Cryosphere, 2011) should be added. [Antoine Rabatel, FRANCE]	Taken into account: The Figure has been modified. Only mean values over a regional scale are considered

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4-410	4	17	41	17	42	The data used for Fig. 4.10 have to be adjusted to the common time interval. Otherwise the consideration and comparison of these data is not statistically acceptable. [Vladimir Konovalov, Russian Federation]	Rejected: This kind of alteration with the reported observation is not possible
4-411	4	17	41			Fig 4.10 : colours are too difficult to distinguish, and the figure should be improved to make it more readable. The area loss rates are calculated using a compilation of results from references that are listed for every region. Actually, there is no detail regarding the way of calculating the area loss rate. It would be interesting to add some lines for explanation. For region 15 (South Central Asia), the work of Kulkarni et al [2007] is cited. Actually, the results obtained in this study are questionable, because probably the debris-covered part of the glaciers, hard to delineate properly on satellite images, has been wrongly delineated. My doubts regarding the reliability of the results come from the fact that the authors give a snout retreat of Chhota Shigri glacier of 800 m between 1988 and 2003, although direct measurements performed in the field in 1988 [Dobhal et al, 1995] and in 2010 [Azam et al, in press] give a total retreat much reduced (not more than 100 m), proving that the delineation of the lowest part of the glacier is obviously wrong in Kulkarni et al [2007]. Azam, F. M., P. Wagnon, A. Ramanathan, C. Vincent, P. Sharma, Y. Arnaud, A. Linda, J. G. Pottakkal, P. Chevallier, V. B. Singh, E. Berthier, From balance to imbalance: a shift in the dynamical behaviour of Chhota Shigri Glacier (Western Himalaya, India), J. Glaciol., In Press Dobhal, D.B., S. Kumar and A.K. Mundepi. 1995. Morphology and glacier dynamics studies in monsoon-arid transition zone: An example from Chhota Shigri glacier, Himachal-Himalaya, India. Current Science, 68(9), 936-944 [Patrick Wagnon, France]	Fig. improvement: Taken into account: The Figure was modified Comment on used references: Noted and Kulkarni et al. (2007) was removed
4-412	4	17	46	18	6	This discussion of how glacier mass changes are determined seems exceptionally superficial. Spatial extrapolation is mentioned but not described; power law scaling methods are not even mentioned. And what about airborne altimetry (ala' Echelmeyer and Larsen), flux measurements (e.g. Gardner et al, NAture 2011), or GRACE (hard to use for GICs but still done, e.g. Jacob et al 2012)? [W. Tad Pfeffer, United States of America]	Taken into account: text was re-written and improved accordingly
4-413	4	17	53	18	12	The paragraph is outdated. Instead of the traditional methods much more focus should be given on the 'explosion' of geodetic methods/results that have marked the years since AR4. [Regine Hock, US]	Accepted: focus on each method is now given in a balanced way
4-414	4	17	53	18	12	In the basic (background) glaciological informations should be distinguished: (a) spatial unrepresentativiness, due to disadvantages in sampling methods and technique, limited, unevenly distributed, and not synchronous samples, (b) temporal uncertainty, due to using field data, not adjusted to the common time slice, (c) obsoleteness, due to known very limited number of time slices (1-2 as a rule) during of glaciers monitoring period and long interval between them. Confirmations for the point (a) are given in the Supplement from reviewer (Figs 1-3, Table 1). Fig. 4 ibid illustrates point (b). [Vladimir Konovalov, Russian Federation]	Noted: the extension in methodological details is beyond the scope of the report
4-415	4	17	53	18	12	An alternative method to quantify annual mass balance is combining the geodetic method and the annual survey of the snowline altitude as an indicator of the ELA using remote sensing images, see Rabatel et al., JOG, 2005 and 2008 [Antoine Rabatel, FRANCE]	Noted: the method is used in individual glacier or subregional studies only which cannot be assessed in this report
4-416	4	17	53			need to be very careful here. Confusing surface mass balance with mass changes. I do not think stake measurements give an estimate of glacier mass changes directly; this requires density. [antony payne, uk]	Taken into account: text was modified
4-417	4	17	54			Remove the term "net" (cf. Cogley et al. 2011). [Michael Zemp, Switzerland]	Accepted: term has been removed
4-418	4	17	55	17	56	Suggestion for correct data citation: "[...] uninterrupted time series spanning more than 30 [not 40!] years are available from 37 glaciers worldwide (WGMS 2011), [...]" Reference: WGMS (2011): Glacier Mass Balance Bulletin No. 11 (2008-2009). Zemp, M., Nussbaumer, S.U., Gärtner-Roer, I., Hoelzle, M., Paul, F. and Haeberli, W. (eds.), ICSU (WDS) / IUGG (IACS) / UNEP / UNESCO / WMO, World Glacier Monitoring Service, Zurich, Switzerland: 102 pp. [Michael Zemp, Switzerland]	Accepted: reference changed
4-419	4	17	57	17	58	it should be mentioned that care needs to be taken for any ice below ice leven in case of advance/retreat. [Regine Hock, US]	Noted: the comment is not entirely understood but changes should have improved the text

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4-420	4	17	57			now introduce another technique (repeat survey). Would be good to have a statement at the start of this paragraph ... 'the mass balance of a glacier can be measure in X ways: blah blah' and then go into detail. [antony payne, uk]	Accepted: text was modified accordingly
4-421	4	17				The attribution of glacier advance in the Karakoram only to dynamical instabilities is arguable (no reference is cited to support the statement). Some authors have shown that those advances may not only be explained by a surging behaviour but also partly explained by the good state of health of those glaciers (e.g., Quincey et al., 2009 ; Scherler et al., 2011). Quincey, D.J., L. Copland, C. Mayer, M. Bishop, A. Luckman and M. Belo. 2009. Ice velocity and climate variations for Baltoro Glacier, Pakistan. Journal of Glaciology, 55(194), 1061-1071 ; Scherler, D., B. Bookhagen and M.R. Strecker. 2011. Spatially variable response of Himalayan glaciers to climate change affected by debris cover. Nature Geoscience, 4(3), 156-159. [Etienne BERTHIER, France]	Taken into account: text has been changed
4-422	4	18	3	18	6	The first sentence is an important note. Could the second sentence ('close to zero') be further explained? [Christian Huggel, Switzerland]	Noted: the text section has been rewritten and the comment does not apply anymore.
4-423	4	18	3	18	12	It should be mentioned that there are modeling efforts to overcome simple spatial extrapolation: these models link mass balance sensitivity to climate variables that are available globally (observation, climate reanalysis). These studies can also provide quantitative assessment of uncertainties (e.g. Hock et al, 2009). It should be clearly stated that the scientist in this field can do better then simple assuming the mean from a small sample of glaciers to be the best estimate for the global mean and that the uncertainties from this assumption are not assessable. [Valentina Radic, Canada]	Taken into: text section has been rewritten and "calibrated climate modeling" approaches are given more visibility
4-424	4	18	4	18	8	Regional averages of average net balance is not meaningful. Only total volume change would be. Arithmetic averages of local, or locally averaged, quantities are misleading and should be avoided. Rather, data for individual glaciers should be shown. [Martin Lüthi, Switzerland]	Noted: the text section has been rewritten. Data for individual glaciers cannot be shown in this report
4-425	4	18	5	18	9	If the regional mass balance average is close to zero, why is it then supposed to be better taking the global average instead? Please clarify this statement. [Michael Zemp, Switzerland]	Noted: the text section has been rewritten
4-426	4	18	7			Is it demonstrated (reference?) that the global mean is the best value to use when no mass balance data are available for a region? Why is it better than mass balance of nearby regions? Or mass balance measured in a similar climate regime? [Etienne BERTHIER, France]	Taken into account: text is modified for clarification
4-427	4	18	8			regionalization by interpolation of observations OR modelling is an important topic and deserves its own paragraph and perhaps a little more detail. [antony payne, uk]	Taken into account: the text section has been rewritten for clarification
4-428	4	18	10			The extention of the directly observed mass balance dataset with modelled mass balances (from temperature and other meteo data) might be interesting and appropriate for improving the understanding of the glacier contribution to run-off and sea level rise. However, caution is to be exercised when it comes to its us as climate proxy since the modelled mass balances are not independent from climate data anymore. [Michael Zemp, Switzerland]	Noted: the text section has been rewritten. The use of glaciers as climate proxies in not in the focus of Ch4
4-429	4	18	15	18	15	Fig. 4.11. For example, #14. Five lines on this picture have different trend, amplitude and values during 1960-2010 . What is criteria of their quality? Where are results of independent hydrological confirmation of mass balance calculation? [Vladimir Konovalov, Russian Federation]	Noted: By adding a new paper (Gardner et al., submitted) the request is largely met. Hydrological models are on basin scale not on mountain region scale and do thus not fit into the compilation. A respective note is added to the text.
4-430	4	18	15	18	15	Figure 4.11. Hard to read, very small. Do the low latitudes data include the work realised by Soruco et al, GRL, 2008, apparently not, because this paper is not quoted. These data should be included. [Antoine Rabatel, FRANCE]	Noted: the Soruco paper provides volume change series ofindividual glaciers, in Fig. 4.11 we only present regional values. Graphical layout was improved.
4-431	4	18	15	18	25	Panel 19 of Figure 4.14 does not appear to have a Marzeion et al. in prep line despite being in the panel 19 legend [Matt King, UK]	Noted: the figure was rearranged and modified
4-432	4	18	15			Fig 4.11 and p 4-18, lines 1-5 : same as the others, legend too small, colours hard to distinguish... barely readable. I agree that any spatial extrapolation from single glacier-measurements will bring large uncertainties	Noted: Lay out has been improved; Uncertainty: it is noted that some extra measurements would improve

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						for regional glacier-mass changes, difficult to quantify. But I guess that using satellite imagery will help to spatially extrapolate local observations. On the other hand, using models of mass balance that rely on temperature and other meteorological variables (such as precipitation which is very site specific, and imposible to constrain accurately at regional scale) at regional scale or even at global scale will give even much more uncertainty. Consequently, I would suggest not to mix both methods and to present only results from the first method, and not from the second, on Fig 4.11. For panel 15 (central Asia South), results are not in agreement with recent observations on Chhota Shigri glacier where mass balance during the last decade of the 20th century were null to positive, before changing to negative values since 2000 [Azam et al., In press]. For panel 16 (Low latitudes), same comment, no agreement with the results of Soruco et al [2009] showing mass gain for 1963-75 and then a continuous decrease after on Zongo glacier Azam, F. M., P. Wagnon, A. Ramanathan, C. Vincent, P. Sharma, Y. Arnaud, A. Linda, J. G. Pottakkal, P. Chevallier, V. B. Singh, E. Berthier, From balance to imbalance: a shift in the dynamical behaviour of Chhota Shigri Glacier (Western Himalaya, India), J. Glaciol., In Press Soruco, A., C. Vincent, B. Francou, P. Ribstein, T. Berger, J. E. Sicart, P. Wagnon, Y. Arnaud, V. Favier, and Y. Lejeune (2009), Mass balance of Zongo Glacier, Bolivia, between 1956 and 2006, using glaciological, hydrological and geodetic methods, Ann.Glaciol., 50, 1-7. [Patrick Wagnon, France]	the quality of extrapolations if available; in turn, model approaches have clear uncertainty bars; Disagreement with measured values: individual glaciers often deviate from mean regional behaviour and individual glacier behaviour treatment is out of scope of the chapter.
4-433	4	18	18			Figure 4.11 i thought we were using Gt? The figure has too much information and is very hard to interpret. Would it be better in an Appendix and a simplified version used here. May be report total change over some period and report this as bars (different for each estimate with uncertainty) with region on the x axis? What does the line 'incomplete regional ... are up-scaled' actually mean how was this done? [antony payne, uk]	Taken into account: figure and labelling has been improved. Providing a simpler version was discussed but seems not feasible
4-434	4	18	19			The sections 'global synthesis' and 'regional synthesis' are not clear, what you really want to say here, and how the two sections distinguish from each other. [Nadine Salzmann, Swizerland]	Taken into account: the cahpter 4.3 structure has been changed
4-435	4	18	27	18	27	Regional synthesis. I'm currently work on a paper making a synthesis of all the data (surface change, volume, mass balance) collected and reconstruted by our team GREAT-ICE (IRD-LGGE) and our local partners in Bolivia, Peru, Ecuador and Colombia. I hope the paper will be submitted within the next months and that the data persented could help to complete this paragraph for the second draft. [Antoine Rabatel, FRANCE]	Noted: submitted paper has been included
4-436	4	18	29	19	22	Section 4.3.4 In this regional synthesis reference could be made to Fisher et al (2011) which placed more recent changes in melt rates within the context of the longer record for the Canadian Arctic (Queen Elizabeth Islands). Barrand and Sharp (2010) also looked at recent changes in Yukon glaciers. Sharp et al (2011) may also be relevant. References: Fisher D, Zheng J, Burgess D, Zdanowicz C, Kinnard C, Sharp M, Bourgeois J (2011) Recent melt rates of Canadian arctic ice caps are the highest in four millennia, Global and Planetary Change. doi:10.1016/j.gloplacha.2011.06.005 Barrand NE, Sharp MJ (2010) Sustained rapid shrinkage of Yukon glaciers since the 1957–1958 International Geophysical Year. Geophysical Research Letters 37: L07501. doi: 10.1029/2009GL042030 Sharp M, Burgess DO, Cogley JG, Ecclestone M, Labine C, Wolken G (2011) Extreme melt on Canada's Arctic ice caps in the 21st century. Geophysical Research Letters 38 (L11501). doi:10.1029/2011GL047381 [Sharon Smith, Canada]	Noted: paper have been included
4-437	4	18	29			No references (except Benn and Lehmkuhl which relates to a specific process) to support any of these assertions. If the term 'robust' is to be used then much greater efforts must be made to support it. [antony payne, uk]	Taken into account: figure 4.11 and respective text have been updated
4-438	4	18	30	18	30	I assume robust evidence has some formal meaning in IPCC-speak. I seem to recall similar phrases in AR4. These terms and their meanings should be clearly explained at the start of every chapter. [Robert Thomas, USA]	Taken into account: wording has been adapted to IPCC rules
4-439	4	18	34	18	34	Individual glaciers are also advanced due to glacier surging in most other regions. For the Karakoram, it is unclear whether the sentence refers to surging or climate-driven advances. [Jacob Clement Yde, Norway]	Taken into account: text has been revised and provides clarity on this
4-440	4	18	35	18	38	Suggestions for rewording: - "[...] with the largest (flat) glaciers[...]" - "[...] medium-sized (steeper) mountain glaciers[...]" - "[...] smaller glaciers showing high variability but a clear retreat trend[...]"	Taken into account: text has been revised

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						- "[...]Modifiying factors on many glacier tongues are a debris-covered surface, ..., or lake formation which can lead to high retreat rates due to calving processes [...]" [Michael Zemp, Switzerland]	
4-441	4	18	36	18	36	Delete 'mountain'. [Jacob Clement Yde, Norway]	Editorial
4-442	4	18	36	18	37	This different glacier terminus behavior is largely due to geometry (i.e. slope and length), as has been shown many times (e.g. Lüthi, 2009; Lüthi and Bauder, 2010). [Martin Lüthi, Switzerland]	Taken into account: text has been revised
4-443	4	18	37			Influence of debris cover is briefly discussed here. Why focusing only on this factor? What about the influence of dust, soot and other feedbacks such as albedo changes, hypsometric feedbacks? [Etienne BERTHIER, France]	Taken into account: text has been revised
4-444	4	18	38	18	38	Not all debris-cover glacier surface strongly reduces melt compared to clean ice, that is mainly depend on the debris layer thickness(Han et la,2006). Haidong, H., Yongjing, D., Shiyin, L., 2006. A simple model to estimate ice ablation under a thick debris layer. Journal of Glaciology, 52: 528-536 [Yongjian Ding, China]	Taken into account: text has been revised
4-445	4	18	39	18	40	Area losses of 2% yr-1 in the Alps, 3% 40 yr-1 in Norway are not evident on Figure 4.10. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account: The Figure and text was modified accordingly
4-446	4	18	39	18	40	Mention the time period for these area losses. [Jacob Clement Yde, Norway]	Taken into account: The text was revised.
4-447	4	18	39	18	42	I appreciate that the authors provide numbers here (see previous comment). However, some references should be given to accound for the traceable evidence. I know these numbers are from multiple papers/studies but some review/compilation references should be provided. [Christian Huggel, Switzerland]	Taken into account: The text will be revised for better clarity
4-448	4	18	40	18	40	Mean area loss is a meaningless quantity, and likely dominated by the more frequent small glaciers. [Martin Lüthi, Switzerland]	Rejected: We compare area weighted rather than arithmetic mean values
4-449	4	18	40	18	40	instead of quoting Peru only at the end of the brackets, put tropical Andes, because a large area loss is also observed in Bolivia, Ecuador, and Colombia [Patrick Wagnon, France]	Taken into account: The text has been revised for better clarity
4-450	4	18	42	18	42	"Fig 4.3.5) and the climatic…" should be "Fig 4.3.5) while the climatic…" [W. Tad Pfeffer, United States of America]	Editorial
4-451	4	18	42	18	42	"regional similarities for the last five decades"… in contrast to what other period? [W. Tad Pfeffer, United States of America]	Taken into account: text has been changed
4-452	4	18	44	17	44	(… , and modeled plus calving)'. This does not make sense. What do you mean? What types? The term geodetic only refers to how the mass loss is obtained. It is not a 'type' of mass loss but a 'type' of calculating it. [Regine Hock, US]	Taken into account: text has been changed
4-453	4	18	44	18	45	I doubt that the distinction between total and climatic mass loss is clear to many readers. If these terms need to be used, can they be further explained? References?. Furthermore, the wording of the sentence should be improved. [Christian Huggel, Switzerland]	Taken into account: text has been changed
4-454	4	18	44			The distinction between geodetic mass loss and climatic mass loss is quite unclear, I would at least use differerent phrases for those processes (Page 4-18, lines 44 and further). [Richard Bintanja, Netherlands]	Taken into account: text has been changed
4-455	4	18	44			dangerously mixing observation and modelling here. Wouldn't a better way of cutting this up be on two dimensions: total mass change versus mass change related to surface mass balance only; and modelled versus observed? Implying that calving mass loss is not related to climate is wrong. GRACE is an observation ot total mass change; most models supply only SMB-related change; and most stake etc observations are of SMB-related change. [antony payne, uk]	Taken into account: text has been changed
4-456	4	18	44			What relevance do the first few sentences of this paragraph have for the remainder? Which references are based on which types of data? Again a lack of references - is this a description of the Hock et al results only? [antony payne, uk]	Taken into account: text has been changed
4-457	4	18	48	18	48	"resent" should be "recent" [W. Tad Pfeffer, United States of America]	Editorial

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4-458	4	18	50	18	50	Is there a justificaiton for grouping of Sub-Antarctic and Antarctic glaciers given their quite different climatological settings? This grouping also masks the very few glaciers with mass balance/volume change in Antarctica, especially those not feeding former ice shelves [Matt King, UK]	Noted: the consideration mentioned holds for basically all regions but regions had to be grouped. In addition, there is too little mass and too littleinformation about, to make a separate sub antarctic region.
4-459	4	18	52			It might be worthwile to mention here that lower mass loss rates in cold high latitude regions might be due to the reduced climate sensitivity of polythermal and cold glaciers as part of the incoming energy is consumed to warm the ice to the melting point. [Michael Zemp, Switzerland]	Noted: Not all cold regions show lower mass loss rates.
4-460	4	18	53	18	53	RECENT [Roger Barry, USA]	Editorial
4-461	4	18	53	18	53	resent mispelled : recent [Patrick Wagnon, France]	Editorial
4-462	4	18	53	18	53	resent' should be 'recent'. [Jacob Clement Yde, Norway]	Editorial
4-463	4	18	53			change resent to recent [Olaf Eisen, Germany]	Editorial
4-464	4	18	53			recent' instead of 'resent' [Nadine Salzmann, Swizerland]	Editorial
4-465	4	18	55	18	55	It is wrong to lump Alaska together as one climatic region. The Pacific coast is extremely maritime, but not for example the Brooks Range, or interior parts of the Alaska Range and the eastern Chugach. [Martin Lüthi, Switzerland]	Rejected: the same argument would hold for each region
4-466	4	18	56	18	56	"indicative for climate signal" should be "indicative of a climate signal" [W. Tad Pfeffer, United States of America]	Editorial
4-467	4	18	56	19	9	Which are Figures 4.3.1 and 4.3.4? [W. Tad Pfeffer, United States of America]	Editorial
4-468	4	18	57	18	57	"region shows its" should be "region shows their" [W. Tad Pfeffer, United States of America]	Editorial
4-469	4	18				Section 4.3.4: Again, as in the comment above, it would be useful to have the range of mass balance estimates over a certain period shown in a table for each of 19 regions. Also, for each region there can be additional column showing (low, medium, large) the number of studies from which the rate is derived (observations or/and modeling). This compilation would be much more transparent than the Figure 4.11 and a text with general statements. [Valentina Radic, Canada]	Taken into account: Table 4.4 has been added
4-470	4	19	1	19	1	What is a “strong linear trend of increasing loss”? Is the trend linear, or the trend change? [Martin Lüthi, Switzerland]	Taken into account: text has been changed
4-471	4	19	1	19	1	"contribution potential" should be "potential contribution" [W. Tad Pfeffer, United States of America]	Taken into account: text has been changed
4-472	4	19	1			I do not think the mass changes in Central Europe follow exactly a "linear trend". Papers by Vincent et al., 2004 and more recently Huss et al., 2010 (and maybe others?) have shown that they are strong multi-decadal oscillations of the mass loss. Vincent, C., G. Kappenberger, F. Valla, A. Bauder, M. Funk and E. Le Meur. 2004. Ice ablation as evidence of climate change in the Alps over the 20th century. Journal of Geophysical Research-Atmospheres, 109(D10). ; Huss, M., R. Hock, A. Bauder and M. Funk. 2010. 100-year mass changes in the Swiss Alps linked to the Atlantic Multidecadal Oscillation. Geophysical Research Letters, 37. [Etienne BERTHIER, France]	Taken into account: the newly assembled figure 4.11 and the respectiv text portions revise the picture
4-473	4	19	4	19	4	FOR THE CLIMATE SIGNAL [Roger Barry, USA]	Editorial
4-474	4	19	4	19	7	A big mistake is made by this procedure. First, averages of different mean glacier mass balances are constructed, and then these averages are multiplied by the total area. The only meaningful approach would be to calculate volume changes for all observed glaciers, and then extrapolate those to the total sample of glaciers. The description of this paragraph is easy to attack as unsound science. [Martin Lüthi, Switzerland]	Rejected: the comment is incorrect. It is well known that an extrapolation along the vertical balance profile of each individual glacier would be more precise. Unfortunately, this approach is hindered by the extensive lack of data. The applied method is not as precise but it is not wrong at all.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4-475	4	19	4			I do not understand what is going on here. Are the specific mass balances of Figure 11 being multipled by a glaciated area for each region? What is the error bar on this preceedure (assumes that all glaciers are affected equally)? How have the multiple studies for each region been reconciled to obain a single figure for each region? [antony payne, uk]	Taken into account: figure 4.11 has been updated and modified and the respective text has been changed
4-476	4	19	8	19	8	awkward English, delete 'recent climate effect [Regine Hock, US]	Taken into account: text has been modified
4-477	4	19	8	19	10	What are the numbers in brackets [Regine Hock, US]	Taken into account: text has been modified to clarify the meaning of the numbers (region numbers)
4-478	4	19	10	19	10	add "surface" before "velocity" [Yongjian Ding, China]	Taken into account: text has been changed
4-479	4	19	10	19	14	While present/future accumulation areas are evidence of glacier imbalance I think this is not necessarily true for velocity changes, and less clear for length-volume changes. [Christian Huggel, Switzerland]	Taken into account: text has been changed
4-480	4	19	10			The paper by Span and Kuhn could also be cited here: Span, N. and M. Kuhn. 2003. Simulating annual glacier flow with a linear reservoir model. Journal of Geophysical Research-Atmospheres, 108(D10). [Etienne BERTHIER, France]	Rejected: the paper does not contribute to the statement
4-481	4	19	11	19	11	Spelling: Lüthi, not Luethi [Martin Lüthi, Switzerland]	Accepted: may become an EndNote problem but will be fixed for the final report
4-482	4	19	12			Note that an update of the study by Bahr et al. (2009) is currently prepared by Mernild and colleauges and might be available by end of July 2012. [Michael Zemp, Switzerland]	Noted: no respective paper was identified
4-483	4	19	17	19	17	Fig.4.12. Panels (a) and (b) lower graphs. I have the same questions as in the comment 12. [Vladimir Konovalov, Russian Federation]	Noted
4-484	4	19	21	19	21	Kaser, 2006 should be Kaser et al. 2006 which is duplicated in the References. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Editorial
4-485	4	19	24	19	26	Braithwaite (2009, see reference below) suggests a bias in the AR4 estimates of global changes in glacier mass balance because the available mass-balance measurements are biased towards wetter conditions. This paper has likely been missed because of the obscure title and low profile status of the journal, but the issue needs to be addressed in this section: Braithwaite, RJ, 2009: After six decades of monitoring glacier mass balance we still need data but it should be richer data. Annals of Glaciology 50(50), 191-197. [Sarah Raper, United Kingdom of Great Britain & Northern Ireland]	Noted: the reference to AR4 has been kept short, mainly because the new complete glacier area inventory provides a major step forward which makes it difficult to relate new results to AR4
4-486	4	19	24			* is not explained as footnote [Muhammad Amjad, Pakistan]	Editorial
4-487	4	19	24			What does asterisk mean, here and elsewhere in chapter? [Alan Robock, USA]	Editorial
4-488	4	19	26	19	46	This paragraph is rather difficult to follow and fully understand. Could the wording be improved? [Christian Huggel, Switzerland]	Accepted: the section has been renamed, restructured and rewritten after assimilating newly submitted material, based on the newly completed inventory
4-489	4	19	26	19	51	Global synthesis looks not reliable, see comments 11-13 and 1. Assumptions used in models and upscaling of local data to global level are not substantiated enough and independently tested. In this part of Chapter 4 it would be better to consider Fig. 8 more detail, after confirmation quality of the recently obtained results and their dissemination to the community. [Vladimir Konovalov, Russian Federation]	Rejected: the methods used and the respective uncertainties are each presented in peer reviewed literature
4-490	4	19	26			Discussion for Figure 4.12 is far better and links to literature that provided these esimates far more tightly. [antony payne, uk]	Noted
4-491	4	19	27	19	27	"proportion of calving glaciers" - do you mean "proportion of loss from calving glaciers"? [W. Tad Pfeffer, United States of America]	Rejected: it is the number of calving glaciers in the data set compared to the total number of calving glaciers. Text has been revised making also this point clearer

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4-492	4	19	28			I'm sorry to say that this statement ("Cogley (2009c) compiled 4,146 annually directly measured mass budgets from 344 glaciers") is an unbelievable expression of ignorance and disrespect towards the hundreds of observers sharing their data and towards the monitoring services compiling and disseminating this scientific results (free of charge) for more than half a century! The mass balance dataset used by Cogley (2009c) consists of more than 90% of data from the WGMS (at that time: WGMS (2009, and earlier issues)). Suggestion for correct data citation: "Cogley (2009c) compiled, mainly based on WGMS (2009, and earlier issues), 4,146 annually directly measured mass budgets from 344 glaciers" Reference: WGMS (2009): Glacier Mass Balance Bulletin No. 10 (2006-2007). Haeberli, W., Gärtner-Roer, I., Hoelzle, M., Paul, F. and Zemp, M. (eds.), ICSU (WDS) / IUGG (IACS) / UNEP / UNESCO / WMO, World Glacier Monitoring Service, Zurich, Switzerland: 96 pp. Note that an updated version of the latter dataset is published, and mass balance data for 2009/10 are digitally available: WGMS (2011): Glacier Mass Balance Bulletin No. 11 (2008-2009). Zemp, M., Nussbaumer, S.U., Gärtner-Roer, I., Hoelzle, M., Paul, F. and Haeberli, W. (eds.), ICSU (WDS) / IUGG (IACS) / UNEP / UNESCO / WMO, World Glacier Monitoring Service, Zurich, Switzerland: 102 pp. [Michael Zemp, Switzerland]	Accepted: reference added
4-493	4	19	29			Similar as above but here the proportion of additional data compiled by Cogley (and his students) from the literature is larger. Suggestion for correct data citation: "[..separate this sentence from the one above..]Based on WGMS (2008, and earlier issues) and many other sources, Cogley (2009c) compiled 16,383 annual values from 754 volume changes measurements from an additional 327 glaciers, [...]" Reference: WGMS (2008, and earlier issues): Fluctuations of Glaciers 2000-2005 (Vol. IX). Haeberli, W., Zemp, M., Kääb, A., Paul, F. and Hoelzle, M. (eds.), ICSU (FAGS) / IUGG (IACS) / UNEP / UNESCO / WMO, World Glacier Monitoring Service, Zurich, Switzerland: 266 pp. Note that the WGMS database meanwhile contains >850 geodetic volume changes from >430 glaciers. [Michael Zemp, Switzerland]	Accepted: reference added
4-494	4	19	30	19	30	Instead of “pentades” directly say “5-year periods”. [Martin Lüthi, Switzerland]	Rejected: the term 'pentad' is adopted from the original literature and it is considered to be a widely understood term
4-495	4	19	30	19	31	"excluding those around the ice sheets" - clearer to say "excluding the peripheral glaciers surrounding the ice sheets" [W. Tad Pfeffer, United States of America]	Accepted: text modified
4-496	4	19	30			the distinctions between SMB-related change and total change (18-44) might be better introduced here where they are more relevant. [antony payne, uk]	Taken into account: text has been restructured and revised
4-497	4	19	31	19	31	"upscaled to a total" - clearer to say "upscaled to a total including the peripheral glaciers" [W. Tad Pfeffer, United States of America]	Taken into account: text has been restructured and revised
4-498	4	19	35	19	35	scaling factor should be given here. It is crucial to know how many % were added considering that the percentage is pretty large. [Regine Hock, US]	Taken into account: text has been restructured and revised
4-499	4	19	36	19	40	WHILE …, INVENTORY IS NOT A SENTENCE [Roger Barry, USA]	Editorial
4-500	4	19	37	19	41	This part was unclear to me (will be understandable only by those that read the papers recently) [Etienne BERTHIER, France]	Taken into account: text has been restructured and revised
4-501	4	19	38	19	38	"Hirabayashi et al. (2010) upscale daily from example glaciers" does not make sense [Matt King, UK]	Taken into account: text has been restructured and

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							revised
4-502	4	19	38	19	38	Delete "extended from". [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account: text has been modified
4-503	4	19	39	19	39	Change “for each individual glacier as available from the present inventory.” to “by training on 15 individual alpine glaciers available in the present inventory, and then applying the relationships to other glaciers ”. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account: text has been restructured and revised
4-504	4	19	43	19	44	What is meant by 'bold in Table 4.4'? [Jacob Clement Yde, Norway]	Taken into account: section has been restructured and revised
4-505	4	19	50	19	51	Consider revising this sentence. Does 'SLE derived from terminus variations lag considerably behind others, particularly since mass losses increased around 1985' incidate that glaciers were in balance with the present climate before 1985? [Jacob Clement Yde, Norway]	Taken into account: text has been restructured and revised
4-506	4	19	50			SLE is nowhere explained [Christoph Marty, Switzerland]	Taken into account: SLE is now explained at the beginning of the glacier section
4-507	4	19	51			Good strong statement to end with. It might be worth adding something about the fact that snowlines are now often above the top of glaciers, which suggests that they must be in decline. [antony payne, uk]	Taken inti account: the inbalance of present time glaciers with climate is addressed
4-508	4	19	53	20	1	Table 4.4: The error estimates on the last 3 entries in this Table appear to be FAR too small. Considering the reality of how these estimates are made, the errors have to be a quite large fraction of the total signal. [Robert Thomas, USA]	Taken into account: a 90% confidence level has now been applied
4-509	4	19	54	20		Table 4.4: There is nothing in bold letters (in contrast to the mention at p. 19, line 44-45) [Thomas Voigt, Germany]	Editorial
4-510	4	19	56	19	56	Is this upscaling procedure published? This is a slippery slope, and the procedure maybe easily attackable. [Martin Lüthi, Switzerland]	Noted: it is discussed and published in the cited papers
4-511	4	19				Table 4.4: The Copley 2009 error ranges in Figure 4.12a, upper panel, appear smaller than would be expected from the ± 0.26 mm SLE yr-1 given here as this would accumulate to more than 20mm over 90 years. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account: Table 4.4, now 4.5 has been recalculated
4-512	4	19				Figure 4.12: The error bounds should be explained in the figure caption. [Valentina Radic, Canada]	Taken into account: Table 4.4, now 4.5 has been revised including the caption
4-513	4	19				Table 4.4. It would be good to add more estimates into this table, such as Hock et al (2009) and a recent one with estimates from GRACE Jacob et al 2012. Also, it would be good that the periods are the same as in a table for SLE from ice sheets (page 25, Table 4.5) since the direct comparison would be much easier. [Valentina Radic, Canada]	Noted: the entire global estimate analysis and discussion have been redone on the basis of the new inventory and all items are revised accordingly
4-514	4	19				Table 4.4. Estimates for glacier contribution to global sea level rise: I suggest to update values in this table (by Cogley 2009c based on Kaser et al. 2006) with available mass balance data for 2009/10 (as available from the WGMS) and to compare the results with other studies such as by Meier (1984), Zuo and Oerlemans (1997), Meier et al. (2007), Oerlemans et al. (2007), Leclercq et al. (2011). References: Meier, M. F. (1984). The contribution of small glaciers to sea level rise. Science, 226, 1418–1421. Zuo, Z., & Oerlemans, J. (1997). Contribution of glacier melt to sea-level rise since AD 1865: a regionally differentiated calculation. Climate Dynamics, 13(12), 835-845. Meier, M. F., Dyurgerov, M. B., Rick, U. K., O’neel, S., Pfeffer, W. T., Anderson, R. S., Anderson, S. P., et al. (2007). Glaciers dominate eustatic sea-level rise in the 21st century. Science (New York, N.Y.), 317(5841), 1064-7. doi:10.1126/science.1143906 Oerlemans, J., Dyurgerov, M., & van de Wal, R. S. W. (2007). Reconstructing the glacier contribution to sea-level rise back to 1850. The Cryosphere, 1, 59-65. doi:10.5194/tcd-1-77-2007	Noted: the entire global estimate analysis and discussion have been redone on the basis of the new inventory and all items are revised accordingly

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						Leclercq, P. W., Oerlemans, J., & Cogley, J. G. (2011). Estimating the Glacier Contribution to Sea-Level Rise for the Period 1800–2005. Surveys in Geophysics, 32, 519-535. doi:10.1007/s10712-011-9121-7 [Michael Zemp, Switzerland]	
4-515	4	20	3	20	42	Box 4.1 does a very good job summarizing the interaction of snow with the cryosphere. [Richard Heim, U.S.A.]	Noted
4-516	4	20	9	20	9	add and explanation: “through its insulating effect”. [Martin Lüthi, Switzerland]	Editorial
4-517	4	20	9	20	11	The persistence of the snow cover is strongly dependent from the wind action. Snow redistribution by wind action is critical to microclimate in the alpine, affecting for example soil temperature (Williams et al., 2009) and fluxes of trace gases such as CO2, N2O, and CH4 (Filippa et al., 2009). Changes in wind speed could affect the historical patterns of snow distribution. In particular several regional studies looking at the United States, Australia, China and parts of Europe have shown decreasing wind speeds just above the planet's surface. Climate change, afforestation and urban development had been suggested as possible causes (Vautard et al., 2010). References: Williams, M.W., Helmig, D., Blanken, P., 2009. White on green: under-snow microbial processes and trace gas fluxes through snow, Niwot Ridge, Colorado Front Range. Biogeochemistry 95 (1), 1-12. Filippa, G., Freppaz, M., Liptzin, D., Seok, B., Chowanski, K., Hall, B., Helmig, D., Williams, M.W., 2009. Winter and summer nitrous oxide and nitrogen oxides fluxes from a seasonally snow-covered subalpine meadow at Niwot Ridge, Colorado. Biogeochemistry 95, 131-149. Vautard, R., Cattiaux, J., Yiou, P., Thépaut, J.N., Ciais, P., 2010. Northern Hemisphere atmospheric stilling partly attributed to an increase in surface roughness. Nature Geoscience 3, 756-761. [Michele Freppaz, Italy]	Noted
4-518	4	20	17	20	17	so, snow is not part of the cryosphere? [Martin Lüthi, Switzerland]	Taken into account: The text was revised for better clarity
4-519	4	20	22	20	22	"a fresh snow cover" [J. Graham Cogley, Canada]	Editorial
4-520	4	20	23	20	23	"bare" is in the wrong font [J. Graham Cogley, Canada]	Editorial
4-521	4	20	23	20	23	The word "bare" differs by font from the rest of the text. [Andrey Shmakin, Russia]	Editorial
4-522	4	20	23	20	23	The word "bare" differs by font from the rest of the text. [Andrey Shmakin, Russia]	Editorial
4-523	4	20	23	20	24	"…- but here snow cover protects the ground from cooling or warming depending on the season." [Christoph Marty, Switzerland]	Editorial
4-524	4	20	24	20	26	Move the last sentence of this paragraph to the end of the former paragraph. [Christoph Marty, Switzerland]	Editorial
4-525	4	20	28	20	29	Delete "of even a thin". For thin snow cover, the albedo-induced cooling can greatly outweight insulation. Depends on vegetation and amount of radiation... [Stephan Gruber, Switzerland]	Editorial
4-526	4	20	28	20	33	Autumn-snow effect? Cooling of frozen ground, in shadowed regions, by clear sky: heat deficit by longwave emission is higher then the isolation of snow (if the snow depth is small) (Keller, F and M. Tamas 2003. Enhanced ground cooling in periods with thin snow cover in the Swiss National Park. Proceedings of VIII. International Conference on Permafrost, Zürich. 531-536) & Keller, F 1994. Interaktionen zwischen Schnee und Permafrost. Mitteilungen der Versuchsanstalt für Wasserbau, Hydrologie und Glaziologie der ETH Zürich 127: 145pp) [Luzi Bernhard, Switzerland]	Noted: This short overview does not consider all physical aspects.
4-527	4	20	28	20	33	The insulation properties of the snow cover are also strongly influenced by its density. The higher the density of the snow cover, the lower is the air content, with a significant reduction of its insulation properties (Rixen et al., 2008). Snow density is likely to increase in a warmer climate, as higher temperatures may cause wetter snow and increase rain-on-snow events (e.g. Rasmus et al., 2004). Interestingly, a warmer climate will not necessarily result in warmer soils: a thinner and denser snow cover will reduce the insulation of the soil. Consequently, alpine ecosystems might face the counterintuitive situation that soils could become colder in winter in a warmer climate (Edwards et al., 2007; Freppaz et al., 2008). References: Rixen C., Freppaz M., Stoeckli V., Huovinen C., Huovinen K., Wipf S (2008) Altered snow density and chemistry change soil nitrogen mineralization and plant growth. Arctic Antarctic and Alpine Research vol. 40 n.3: 568-575. Rasmus, S., Ra¨	Noted

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						isa¨nen, J., and Lehning, M., 2004: Estimating snow conditions in Finland in the late 21st century using the SNOWPACK model with regional climate scenario data as input. Annals of Glaciology, 38: 238–244. Edwards, A. C., Scalenghe, R., and Freppaz, M., 2007: Changes in the seasonal snow cover of alpine regions and its effects on soil processes: a review. Quaternary International, 162–163:172–181. 9. Freppaz M, Marchelli M, Celi L, Zanini E (2008) Snow removal and its influence on temperature and N dynamics in alpine soils (Vallée d'Aoste - NW Italy). Journal of Plant Nutrition and Soil Science 171: 672-680. [Michele Freppaz, Italy]	
4-528	4	20	28			"For frozen ground, this second characteristic…." [Christoph Marty, Switzerland]	Editorial
4-529	4	20	32	20	32	"and protect permafrost" (not "or") [J. Graham Cogley, Canada]	Editorial
4-530	4	20	35	20	35	"are", not "is" [J. Graham Cogley, Canada]	Editorial
4-531	4	20	36	20	40	The measurement of the temperature at the snow-ice interface during winter on the Indren Glacier (Monte Rosa Massif-Italy) showed how a thick snow cover maintained the ice temperature close to -5 °C, indipendenlty from air temperature (Maggioni et al., 2009) References: Maggioni M., Freppaz M., Piccini P., Williams M.W., Zanini E. (2009). Snowpack evolution on the Indren glacier (NW Alps, Italy) under different meteorological conditions. Arctic Antarctic and Alpine Research vol.41 n.3: 323-329. [Michele Freppaz, Italy]	Noted
4-532	4	20	39	20	39	add "in some regions" before "but". Because in center Asia, there are many mountain glaciers accumulation manily come from snowfall in summer. Fujita (2008) pointed out that higher climatic sensitivities occur for the glaciers located in a summer-precipitation climate than for those located within a winter-precipitation climate, and The heaviest precipitation occurs during summer in both Yarkant River Basin and Beida River Basin in China, where the mean summer precipitation comprises 81.6 % and 81.0% of the annual precipitation in the two basins, respectively (Zhang et al., 2011). Fujita K. 2008. Effect of precipitation seasonality on climatic sensitivity of glacier mass balance. Earth and Planetary Science Letters 276: 14-19 Shiqiang Zhang ,Xin Gao, Baisheng Ye, Xiaowen Zhang, Stefan Hagemann. A modified monthly degree-day model for evaluating glacier runoff changes in China. Part II: application Hydrological Processes,2011, Online First., doi:10.1002/hyp.8291 [Yongjian Ding, China]	Editorial
4-533	4	20	45			Section 4.4: I noticed some repetition of material between the subsections of this section, and in that respect it should be checked for economical use of space [J. Graham Cogley, Canada]	Accepted - revised text has reduced repetition
4-534	4	20	45			The whole section 4.4 would profit from a more continuous story line. Details of observed behaviour of several outlet glaciers are spread out through the section, with conclusions preceding the observations (e.g. Jakobshavn Isbrae).. [Martin Lüthi, Switzerland]	Accepted - revised text has improve the flow
4-535	4	20	49	20	49	For Antartcica this sentence may go beyond evidence. Apart from likely warming driven melt/collapse in the Antarctic Peninsula (not what is meant by "vast polar ice sheets", has the Antarctic mass loss be linked to a warming climate? For instance, Pine Island, Totten mass loss cannot be yet connected to anything other than natural perturbations in warm ocean waters [Matt King, UK]	Noted, but the existing sentence does not make any causal attribution, it says the ice sheets are retreating as climate warms
4-536	4	20	49	20	49	Why “our climate”. Better “the polar climate”. [Martin Lüthi, Switzerland]	Accepted - change made
4-537	4	20	49			I am not certain that the small changes in mass that we currently observe entitle us to use a work like 'shrinking' which to my mind implies something more than fractional rates of loss [antony payne, uk]	Accepted - word "shrinking" removed
4-538	4	20	51	20	51	add "melt of " before "some glaciers" [Yongjian Ding, China]	Rejected - not grammatic
4-539	4	20	51			"Greenland and Antarctica, some glaciers are accelerating" You need to explain what glaciers are on Greenland and Antarctica. Are they defined as parts of the ice sheets, or independent, separate small ice sheets detached from the ice sheets. [Alan Robock, USA]	Accepted - replaced "glaciers" with term in Glossary
4-540	4	20	52			"As a result of these processes" Does this imply the glaciers are part of the ice sheets? [Alan Robock, USA]	See 4-539
4-541	4	20	53	4	53	‘very likely’ is AR4 IPCC-speak I think. Earlier in this chapter, it was clearly stated that these ice sheets are definitely shrinking, so why the diffidence? The next sentence is also quite squeemish. Moreover, it fails to	Accepted

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						explain that the increased melting thins and weakens buttressing ice shelves, and it is this that triggers glacier acceleration. [Robert Thomas, USA]	
4-542	4	20	53	20	53	The speculative statement “are very likely increasing” could be replaced by “have increased considerably during the last decade”. [Martin Lüthi, Switzerland]	Noted
4-543	4	20				Page 4-20. Title of box 4.1 is very strange; now it is as if snow is not part of the cryosphere. [Richard Bintanja, Netherlands]	Taken into account: The text will be revised for better clarity
4-544	4	20				Box 4.1. Not certain that I understand the purpose of this box. The title stresses interactions with the cryosphere but the majority of the text is about why snow is important in the general climate system. Needs to think about aim of the box and add a line near the start that makes this explicit. Change the title to 'Why snow is important'? [antony payne, uk]	Taken into account: The text will be revised for better clarity
4-545	4	20				Box 4.1: This box should appear after the permafrost sections [Nadine Salzmann, Swizerland]	Editorial
4-546	4	20				Box 4.1: The titel is confusing, snow is part of the crysphere! It should be ' Interaction of snow with the other components of the cryosphere' [Nadine Salzmann, Swizerland]	Taken into account: The text will be revised for better clarity
4-547	4	20				Box 4.1: In general, I am not sure about the value of this box. I suggest you re-think if this box is worth or if this information should better be part of the 'normal' text. Also, there is not one single reference in the whole box text, which is not adequate for an assessment report. [Nadine Salzmann, Swizerland]	Noted: The intention of the box is to cover cross-cutting cryosphere issues
4-548	4	21	1	21	1	Warm water not necessarily leads to an acceleration, but merely melts ice. [Martin Lüthi, Switzerland]	Accepted. Minor revisions made to sentence.
4-549	4	21	1	21	2	This should be more specific: “Warm ocean water changes terminus geometry through melting, which influences the dynamics of the marine-terminating parts of the ice sheet”. [Martin Lüthi, Switzerland]	As for previous comment.
4-550	4	21	2	21	2	Insert reference after 'ocean' to support this statement. [Jacob Clement Yde, Norway]	Accepted. Added references to Motyka et al., 2011 and Pritchard et al., 2012
4-551	4	21	4	21	4	Figure 4.14b shows increasing snow accumulation in the Indian Ocean sector of East Antactica. There is a recent paper indicateg significant increase of snow accumulation in this area for a long time apan and recent increase. Fujita et al, The Cryosphere, 5, 1057-1081, 2011, Spatial and temporal variability of snow accumulation rate on the East Antarctic ice divide between Dome Fuji and EPICA DML [Hiroyuki Enomoto, Japan]	Reject. Regional reference so not added.
4-552	4	21	4			The whole chapter 4.4.2 would benefit from some restructuring. 4.4.2.1. is about techniques but also includes results (numbers) while numbers are also given later when the balances of the ice sheets are given. It is more logical to have the techniques explained here without results and leave the results for the subchapters later. [Regine Hock, US]	Noted - we have checked the section to ensure there are no results included with the methods.
4-553	4	21	7	21	7	I would put "made since AR4" directly after "improvements" [J. Graham Cogley, Canada]	Accepted - text changed
4-554	4	21	12	21	12	"centimeter" should be "centimeters" [W. Tad Pfeffer, United States of America]	Accepted ?? Have changed cm to centimeters at lines 17 and 25, and other places where cm is used as a noun - not a unit. Is this was the reviwer means? Thereis no "centimeter" at lines 12 or 21.
4-555	4	21	13	21	14	Lines13-14: Information retrieval also limited by wide orbit separation. [Robert Thomas, USA]	Accepted
4-556	4	21	17	21	17	Not only from GRACE, so the first sentence should be “Greenland Ice Sheet mass losses determined with different methods. (a-c) GRACE...” [Martin Lüthi, Switzerland]	Noted
4-557	4	21	18	21	20	None of the cite references provide mass balance estimates for the period through 2011. [Ian Joughin, USA]	Rejected. Timeseries updated using published techniques.
4-558	4	21	18	21	20	Rignot et al doesn't give a regional breakdown, so these appear to be unpublished numbers. [Ian Joughin,	Accepted. Submission of regional values for

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						USA]	Antarctica expected before July deadline.
4-559	4	21	18	21	20	Why are older Velicogna numbers used (a 2009 paper) used for 2002-2011 results, when she has published more recent results (through 2010). [Ian Joughin, USA]	Taken into account. Updated citation added.
4-560	4	21	18	21	20	For c in the caption and the accompanying figures, there seems to be some cherry-picking going on here to show agreement. Rignot et al 2011b provides both mass flux and GRACE estimates for Antarctica that differ by about 100 Gtons/yr, yet older papers are cited that give far better agreement. Three different papers are used to for GRACE regional estimates, none of which cover the period from 2002-2011. The sum of the estimates differs substantially from total Antarctica GRACE estimage in 2011b (a bit over 100 eyeballing the plot in that figure). At lease show the range rather than forcing the agreement. [Ian Joughin, USA]	Accepted. Figure updated to include newly published values and range.
4-561	4	21	18	21	20	The captioned figures state 2002-2011, but the Rignot reference only provides results through 2010. Likewise the Ivins et al paper is only 2003-2009. [Ian Joughin, USA]	As previous comment 557.
4-562	4	21	18	21	20	Where do the numbers in red dots come from. Rignot et al 2011b is cited, but unfortunately no regional breakdown is given in this paper for the individual regions, so this seem to represent unpublished numbers. If such a break down is permitted, then a similar breakdown of most recent and consistent GRACE results from the Rignot et al paper should be presented, since this represents the best apples to apples comparison. [Ian Joughin, USA]	As previous comment 558.
4-563	4	21	19	21	19	Delete “from GRACE” because Rignot et al. 20011b is from mass budget. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Reject. Reference covers both GRACE and mass budget.
4-564	4	21	21	21	21	"centimeter" should be "centimeters" [W. Tad Pfeffer, United States of America]	Accepted ?? Have changed cm to centimeters at lines 17 and 25, and other places where cm is used as a noun - not a unit. Is this was the reviwer means? Thereis no "centimeter" at lines 12 or 21.
4-565	4	21	25	21	33	Panels a-c show mass change over the ice shelves from GRACE. Given these are irrelevant to ice sheet mass balance they should be masked. Velicogna numbers need checking as per their definition of "ice sheet" since GIA model errors over the ice shelves may be substantial [Matt King, UK]	Accepted. Ice shelves masked out.
4-566	4	21	36	21	36	The surface mass balance is not necessarily an input. It is a balance between accumulation and surface ablation and can be negative. The method rather relies on determining the climatic component and ice discharge into the ocean separately [Regine Hock, US]	Accepted changed to "net surface balance"
4-567	4	21	43	21	43	Uncertainies are certainly much larger than 'several percent'. It is difficult to determine how much until the ice thickness are known. Best to delete the last 3 words. [Regine Hock, US]	Accepted. Phrase deleted.
4-568	4	21	43	21	43	Atmospheric climate models provide the boundary conditions, not the estimates. [W. Tad Pfeffer, United States of America]	Rejected. The word "Regional" is critical to understand here.
4-569	4	21	43			quantify several percent [antony payne, uk]	As previous comment 567.
4-570	4	21	45	21	47	these numbers could be updated to the most recent surface mass balance compilation by Lenaerts et al., 2012 (GRL, in press). The period under study is longer (1979-2010) and model horizontal resolution is finer (27 km). You can contact me for the exact numbers and if you want to update Figure 4.14d. [Jan Lenaerts, The Netherlands]	Accepted. Estimates updated, paper cited.
4-571	4	21	45	21	52	The sources of the cited uncertainties are unclear. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. Updated, reference added.
4-572	4	21	46	21	47	How is a number for a very specific period 1989-2009 derived from papers published in 2006. [Ian Joughin, USA]	Accepted. Time period corrected.
4-573	4	21	46	21	47	Better “Surface mass balance in Antarctica (Arthern et al., 2006; Monaghan et al., 2006) averaged 2,080 Gt yr-1 in 1989–2009” as these two papers describe the techniques but predate the results for 1989-2009. [David Parker, United Kingdom of Great Britain & Northern Ireland]	As previous comment.

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4-574	4	21	46	21	48	Curious to quote these studies but show van den Broeke's results in Fig 4.14d. Cross referencing this figure does not seem relevant to the details of the sentence. [Matt King, UK]	Accepted. Reference to van den Broeke added line 45.
4-575	4	21	47	20	48	300 is ~15% of 2080 (not 6%) and 90 is indeed ~5% as stated. [Robert Thomas, USA]	Agreed. Corrected.
4-576	4	21	48	21	48	"an average uncertainty of 5% or 90 Gt yr-1" This suggests the SMB for Antarctica is known to 5% when spread of the various estimates is far greater. Some of estimates in the cited Moghnihan et al have formal uncertainties at this level, but the differ by amounts way outside there formal uncertainties. So to say the SMB for Antarctica is X to 5% is misleading. [Ian Joughin, USA]	Accepted. Numbers updated.
4-577	4	21	48	21	48	Surface runoff is negligible in Antarctica, but there is quite a bit of mass loss from under-ice processes. The same is true for Greenland, and usually completely ignored. [Martin Lüthi, Switzerland]	Accepted. There are no observations, but sentence added on process.
4-578	4	21	49	21	50	In their Table 2, Hanna et al (2008) report surface mass balances over Greenland almost down to 100 Gt in some years. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. This is a running 10-year mean. Text accepted.
4-579	4	21	50	21	50	"with an average uncertainty of 40 Gt yr-1 (7%) " I don't think this is a credible number for uncertainty. The best model to my knowledge is RACMO, for which the total SMB uncertainty for Greenland is about 17% (M Van den Broeke, personal comm). [Ian Joughin, USA]	Accepted. Number has been updated with most recent literature.
4-580	4	21	50	21	50	Surface runoff is only one component, basal water runoff is estimated to about 5%-10% of surface runoff, and cannot be neglected. [Martin Lüthi, Switzerland]	Noted
4-581	4	21	50	21	50	'7%' should be '7 to 13%' Moreover, what do these error estimates refer to: error in estimate for an individual year or for an average over some period; and if so, what period? [Robert Thomas, USA]	Accepted. Text corrected.
4-582	4	21	50	21	51	Citation of the vandenBroeke papers is mandatory for Greenland surface mass balance. [Martin Lüthi, Switzerland]	Accepted. Citation added.
4-583	4	21	50			The 200 km3/yr difference observed by Hanna et al., 2011 (JOURNAL OF GEOPHYSICAL RESEARCH, VOL. 116, D24121, doi:10.1029/2011JD016387, 2011) need to be discussed here. Quoting their abstract: "We show very good agreement of our SMB series variations with existing, independently derived SMB series (RACMO2) variations for the past few decades of overlap but also a significant disparity of up to 200 km3/yr in absolute SMB values due to poorly constrained modeled accumulation reflecting a lack of adequate validation data in southeast Greenland." [Etienne BERTHIER, France]	Accepted. Revision undertaken.
4-584	4	21	52	21	52	"about 100 Gt … about 50 Gt" [J. Graham Cogley, Canada]	Accepted. Text corrected.
4-585	4	21	52	21	52	"are about 101 Gt yr-1 in Antarctica and 51 Gt yr-1 in Greenland.:" I am not sure how such numbers can be presented given that various spread of credible estimates differs by far more than this. [Ian Joughin, USA]	As previous comment.
4-586	4	21	52	21	52	implies smaller % errors in ice discharge estimates, which appears unrealistic, recalling earlier admission of quite large errors in ice thickness. Moreover, the surface mass balance used in the estimate must refer to an average over some period, and this will increase total errors, because of both interannual variability and poor knowledge of what period to choose. [Robert Thomas, USA]	Accepted. Text revised.
4-587	4	21	55	21	55	S is a strange symbol for surface elevation, but because does not appear later the simplest thing would be to delete "dS/dt" altogether [J. Graham Cogley, Canada]	Accepted. Removed.
4-588	4	21	55			"dS/dt" "S" needs to be defined and needs to be in italics, like all other variables. [Alan Robock, USA]	As previous comment.
4-589	4	21	56	21	56	"from tides" -> "for tides" [Matt King, UK]	Accepted - text changed
4-590	4	21	56			suggests density change is unimportant for ice shelves which may not be the case [antony payne, uk]	Accepted - text changed
4-591	4	22	1			The repeat altimetry section should mention the measurements of coastal elevation changes from sequential DEM analysis (in areas where repeat altimetry fail to provide a comprehensive sampling of an often complex pattern of elevation changes). Howat et al., 2008 is a reference for Greenland and Shuman et al., 2011 for the Antarctic Peninsula. Howat, I.M., B.E. Smith, I. Joughin and T.A. Scambos. 2008. Rates of southeast	Accept. Short sentence added - but these studies are regional in scope and do not allow whole ice sheet assessment.

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						Greenland ice volume loss from combined ICESat and ASTER observations. Geophysical Research Letters, 35(17). ; Shuman, C.A., E. Berthier and T.A. Scambos. 2011. 2001-2009 elevation and mass losses in the Larsen A and B embayments, Antarctic Peninsula. Journal of Glaciology, 57(204), 737–754. [Etienne BERTHIER, France]	
4-592	4	22	4	22	4	"of early SRALT sensors was 20 km" it would be worth mentioning that mass flux esimates where thickness is derived are also senstive to this foot print, especially since they rely on absolute elevation rather than relative change as in dh/dt. [Ian Joughin, USA]	Noted.
4-593	4	22	4	22	4	"allowed for" unclear. "estimated and and corrected for" would be more accurate. [Ian Joughin, USA]	Accepted - text changed
4-594	4	22	5			also worth adding uncertainty about the depth within the firn that scattering occurs at and whether this depth changes at the same rate as the surface. [antony payne, uk]	Noted.
4-595	4	22	13	22	13	Insert "and" before "accuracy" [J. Graham Cogley, Canada]	Accepted - text changed
4-596	4	22	15			No error estimates for SRALT and laser in the way that they have been made for mass budget [antony payne, uk]	Accept. Errors added where available in literature.
4-597	4	22	27	22	27	"due to" [J. Graham Cogley, Canada]	Accepted - text changed
4-598	4	22	27	22	27	add 'to' after 'due' [Zhaomin Wang, UK]	Accepted - text changed
4-599	4	22	31	22	31	This "uncertainty" is from model differencing in some of these cases and hence are not rigorous. [Matt King, UK]	Noted.
4-600	4	22	32			change "allowed for" to "addressed" [Alan Robock, USA]	Accepted - text changed
4-601	4	22	34	22	35	Is 10% "small"? Maybe clarify this. [W. Tad Pfeffer, United States of America]	Accepted. "small" removed.
4-602	4	22	35	22	36	These words have been chosen carefully, but it would be prudent nevertheless to insert "(that is, acceleration)" after "rate" in L36 [J. Graham Cogley, Canada]	Rejected. Extra words make the sentence clumsy.
4-603	4	22	38	22	40	This sentence is about elastic response confirming mass loss. Thus it inappropriate to cite Thomas et al 2011 here because that paper has nothing to with the crustal response (it confirms increasing loss but through other means so is irrelevant here). [Ian Joughin, USA]	Accepted. Missing reference added.
4-604	4	22	38	22	40	The citation to Thomas et al 2011 appears to be the wrong paper (wrong Thomas). Correct citation would be Thomas, I.D., M.A. King, M.J. Bentley, P.L. Whitehouse, N.T. Penna, S.D.P. Williams, R.E.M. Riva, D.A. Lavallee, P.J. Clarke, E.C. King, R.C.A. Hindmarsh and H. Koivula 2011. Widespread low rates of Antarctic glacial isostatic adjustment revealed by GPS observations. Geophysical Research Letters, 38: L22302 doi:10.1029/2011GL049277. [Matt King, UK]	Same as previous comment.
4-605	4	22	45	22	45	also cite vanden Broeke (2009). [Martin Lüthi, Switzerland]	This appears to refer to p21, not p22. van den Broeke reference has been added at p21, line 45.
4-606	4	22	48	22	49	This sentence is about GRACE agreement, so remove no GRACE references (Pritchard and Thomas and any others). They support Greenland loss overall, but are not about GRACE agreement. [Ian Joughin, USA]	Accepted. There are two papers from Pritchard, that intended does address GRACE, published in J. Glaciol.
4-607	4	22	50	22	51	Though studies that have looked at the RA coasal sampling suggest is not that important (e.g.. Howat et al, GRL, 2008). [Ian Joughin, USA]	Accepted. Phrase deleted.
4-608	4	22	54			figure 4.15 very nice. It would be worth adding a line to the caption that explains the basis on which studies were rated high/medium/low reliability. No need to name names but some indication of how this was done is needed - was it based on technique alone? might be worth adding some sample curves from the various	Noted. Weighted averages have been removed because of perceived bias. The description of how Fig 4.15 was derived has been moved from the

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						papers as well, such as best fit to this line and worst fit. [antony payne, uk]	"appendix" to the main text
4-609	4	22	57	22	57	also cite vanden Broeke and Bamber (2011). [Martin Lüthi, Switzerland]	Rejected - citations at this location are only for quoted numbers.
4-610	4	23	13	23	13	also cite vanden Broeke and Bamber (2011). [Martin Lüthi, Switzerland]	As previous comment.
4-611	4	23	14	23	15	It would be easier for readers to judge the increase of the contribution if the periods quoted were 1992-2001 and 2002-2009, as in Table 4.5. The same comment holds for P24 L22. [J. Graham Cogley, Canada]	Accepted. Time periods quoted have been changed to be consistent within this Chapter and with those used in Chapter 13.
4-612	4	23	14			A good reason to use absolute values and not annual rates here? [Etienne BERTHIER, France]	Accepted - annual rates have been added.
4-613	4	23	14			are these summary figures based on the same analysis as produced figure 4.15? [antony payne, uk]	Noted. Yes.
4-614	4	23	19	23	19	The reference to Fig 4.13d does not make sense here, as no partitioning is shown. [Martin Lüthi, Switzerland]	Accepted. Citation to figure 4.13d has been removed.
4-615	4	23	19	23	19	The reference to Figure 4.13d is not obvious. Consider deleting '(Figure 4.13d)'. [Jacob Clement Yde, Norway]	As previous comment.
4-616	4	23	26	23	27	"Five of the highest runoff years over the past 49 years occurred since 2001". Vague. The five highest? Or five out of some number? [Chris Derksen, Canada]	Accepted. Text changed and reference updated to Hanna et al, 2011.
4-617	4	23	31			Ref to Howat et al., 2011 probably relevant also (?) Howat, I., Y. Ahn, I. Joughin, M. Van den Broeke, J. Lenaerts and B. Smith. 2011. Mass balance of Greenland's three largest outlet glaciers, 2000-2010. Geophysical Research Letters, 38, L12501. [Etienne BERTHIER, France]	Agreed. Howat et al., 2011 replaces earlier Howat paper.
4-618	4	23	33	23	33	avoid acronums: SMB [Regine Hock, US]	Accepted - text changed
4-619	4	23	36	23	36	Insert 'outlet' after 'many'. [Jacob Clement Yde, Norway]	Accepted - text changed
4-620	4	23	38	23	38	concentrated'. You mean 'largest'? [Regine Hock, US]	Accepted - text changed
4-621	4	23	38	23	46	Note that a fraction of the GRACE signal over Greenland might actually come from glaciers and ice caps surrounding the ice sheets. A recently finished detailed (!) inventory by Rastner et al. (2012) identifies 12'000 glacier with a total area of about 129'000 km² (i.e., 7% of the total ice-covered area on Greenland) that are not connected or clearly seperable from the ice sheet. Note that the resulting glacier cover is much larger than the earlier preliminary estimtes (!) based on Weidick and Morris (1998). This needs to be taking into account in order to avoid a double-counting of the SLR contribution from Greenland. References: Rastner, P., Bolch, T., Mölg, N., Le Bris, R. and Paul, F. (2012): The first glacier inventory for entire Greenland. EGU abstract. Weidick, A., and Morris, E., 1998: Local glaciers surrounding continental ice sheets. Chapter 12. In Haeberli, W., Hoelzle, M., and Suter, S. (eds.), Into the second century of world glacier monitoring — prospects and strategies. A contribution to the IHP and the GEMS. Prepared by the World Glacier Monitoring Service. UNESCO Publishing, 197–207. [Michael Zemp, Switzerland]	Accepted. Addition of column in Appendix table stating whether or not local glaciers are included. In order to ensure that double counting does not occur the numbers passed forward to Chapter 13 will include within estimates for Greenland and Antarctica the local glaciers. We note that Chapter 13 have asked for a number for Greenland local glaciers. We will supply this estimate, but it is then the responsibility of Chapter 13 to ensure they do not double count. In tables and figures we will correct those estimates that do not include local glaciers to do so with a number from the glaciers group.
4-622	4	23	43	23	43	Change "2003-5" to "2001-6" to represent the period studied by Joughin et al. 2010b. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted - text changed
4-623	4	23	46	23	46	Here, one should make the explicit statement, that higher flow velocity means faster mass transport to the ocean (i.e. increased calving flux). [Martin Lüthi, Switzerland]	Accepted - text changed
4-624	4	23	46	23	46	Insert reference after '1996' to support this statement. [Jacob Clement Yde, Norway]	Accepted. Reference added.
4-625	4	23	48			Section 4.4.2.3: It is essential that this subsection cite and discuss Zwally, H.J., and M.B. Giovinetto, 2011, Overview and assessment of Antarctic ice-sheet mass balance estimates: 1992-2009, Surveys of Geophysics, 32(4-5), 351-376 [J. Graham Cogley, Canada]	Accepted. A discussion of Z&G 2011 has been added

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4-626	4	23	52	23	52	use of the phrase "the GPS-GRACE combined approach" suggests the Wu approach is the same as Ivins. They are very different. [Matt King, UK]	Accepted. Phrase changed.
4-627	4	23	57			not certain 'wet' can be used if the precipiation is snowfall [antony payne, uk]	Accepted - text changed
4-628	4	24	1	24	1	also Lenaerts et al., 2012 (GRL, In press) show that no significant SMB trend exists on the AIS over the period 1979-2010. In addition, Kuipers Munneke et al., 2012 (GRL) show that no surface melt trend is found on the AIS. [Jan Lenaerts, The Netherlands]	Agreed. Citation added.
4-629	4	24	8	24	8	Dong-Chen et al., 2009 to E et al. 2009" [Zeng-Zhen HU, USA]	Accepted. Name corrected
4-630	4	24	9	24	9	Referring to the table that this caption represents, the -82.9 number near as I can tell is subtantially lower number than suggested by the regression line in that paper (the central value, which should correspond to the average is more like -120 to -130. If the numbers in the table are a revision to Retal2011, then they should be so noted. [Ian Joughin, USA]	Noted. A 60 Gt/yr error was introduced in the plot of the figure in Rignot et al., GRL 2011, however with no impact on the overall results. Chapter 4 is using the correct monthly data. Note will be added in the Table.
4-631	4	24	13	24	14	"based on an assessment of its reliability: High reliability = weighting of 1.0, Medium = 0.5, Low = 0.2. The number of estimates used in this composite varies with time, with only 2 per year in the 1990s and up to 12 per year after 2002." This reliability weighting seems highly subjective. For example, flux from Retal11 is deemed highly reliable but has an error of 91Gtons/year, the highest formal error of all. As it is, this error probably is an underestimate as I have heard much higher estimates of the uncertainty in SMB model from the model developers. Moreover, as good as that model is there are still credible estimates of SMB based on data that deviate substantially (by an amount that his more than the uncertainty of the estimate). Zwally et al are rated as low for lack of data in the Peninsula, but Rignot et al 2008 (and presumably 2011) only sample 50% of the area and 40% of the mass balance yet are rated high. Again its not clear why Velicogna 2009 is used in place of the more recent Retal2011 GRACE results, which by the way differ from the flux estimates by >100 GT. Despite the claims of good agreement, flux produces an imbalance nearly 2x larger than the corresponding GRACE estimates in that paper. Furthermore there are large areas (15%) of Antarctica not surveyed by flux estimates. My point here is not to say one method is better than the other or to attack any particular method; they all have merit. But I can't see that this subjective weighting, especially weighting some methods by 5x others makes any sense nor is fair (especially when an equally credible group of scientists could be assembled and reach a completely different conclusion). Weight everything equally. [Ian Joughin, USA]	Accepted. (i) Weighted averages have been removed because of perceived bias. The description of how Fig 4.15 was derived has been moved from the "appendix" to the main text (ii) The values will also be made consistent as to whether or not they include the local glaciers. See comment 621 response. (iii) paragraph added on values prior to 1992. (iv) Time periods made consistent with chapter 13.
4-632	4	24	13	24	19	Referring to the table that supports this caption, given the large uncertainties, the numbers in the Greeland and Antarctica tables should be rounded to the nearest Gigaton [Ian Joughin, USA]	Accepted - text changed
4-633	4	24	24	24	24	"Significantly the rate of ice loss is almost certainly increasing with time" [J. Graham Cogley, Canada]	Accepted - text changed
4-634	4	24	24	24	24	"Significantly, ice loss is almost certainly increasing with time…" A more accurate statement is that Signicantly, ice loss has increased with time over the last 2 decades. [Ian Joughin, USA]	Same as previous comment.
4-635	4	24	24	24	30	Note that a fraction of the GRACE signal over Antarctica (mainly over the Antarctic Peninsuly) might actually come from glaciers and ice caps surrounding the ice sheets. The reason and potential consequences are the same as for Greenland (see comment above) only that for Antarctica there is not yet a detailed glacier inventory available. [Michael Zemp, Switzerland]	Same as comment 621. This is something we still plan to discuss
4-636	4	24	26			change "constant." to "constant over GRACE's lifetime". [Olaf Eisen, Germany]	Accepted - text changed
4-637	4	24	28	24	29	"Comparison … indicates" [J. Graham Cogley, Canada]	Accepted - text changed
4-638	4	24	29	24	29	"...of GRACE and the mass budget methods indicate an increase in ice loss of 14 ± 2 Gt yr-1 every year for 30 1992–2010 versus 21 ± 2 Gt yr-1 for Greenland during the same time period (Rignot et al., 2011b)." Given that the above sentence starts to attribute change to ice dynamics, it would be good to add In Greenland about 40% of this change is due to ice dynamics and 60% due to changes in SMB (cite Retal11). Without this the paragraph conveys the idea it is all ice dynamics. This extra sentence could perhaps be added in the next section, "partioning ice loss". [Ian Joughin, USA]	Rejected. The partitioning of the mass loss in Greenland is already quoted in 4.4.2.2

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4-639	4	24	29			unclear, whether this means that the loss rate is changing by this number or whether the cumulative loss increases by this number each year. ("indicate an increase in ice loss of 14 ± 2 Gt yr-1") [Olaf Eisen, Germany]	Reject. Clear in text.
4-640	4	24	33	24	33	"In the near-absence of surface runoff and lack of long-term change" [J. Graham Cogley, Canada]	Accepted - text changed
4-641	4	24	33	24	33	"In the absence of surface runoff and long-term change in total snowfall, Antarctic long-term changes in grounded ice mass are almost entirely explained by increased glacier speed." Taking the breakdown in the trends given by Retal11, about 60% in Antarctica is from increased ice discharge and about 40% from SMB. It would be worth noting this given the 14Gt/yr number is given just above. [Ian Joughin, USA]	Accepted. Text modified. ER
4-642	4	24	34	24	34	"explained by faster delivery of ice to the grounding line". My experience is that referring increased mass loss to faster speed puzzles many non-specialists. [J. Graham Cogley, Canada]	Accepted. Text modified. Citation added.
4-643	4	24	34	24	34	Here, one should make the explicit statement, that higher flow velocity means faster mass transport to the ocean (i.e. increased calving flux). [Martin Lüthi, Switzerland]	Same as previous comment.
4-644	4	24	34			changes in glacier speed do not explain mass loss in themselves. This needs to associated with an increase outflow across the grounding line. Thinning can be explained by incraesed velocity but this mass has to go somewhere. [antony payne, uk]	Same as previous comment.
4-645	4	24	39	24	39	“continue to collapse” is sloppy. Better: where several ice shelves have broken up during the last two decades”. [Martin Lüthi, Switzerland]	Accepted - text changed
4-646	4	24	49	25	5	new paper by Padman, Fricker in JGR oceans adds temporal context to the changes in the Ant Pen back to 1970s. In constrast King et al 2009 JGR Earth surface show Amery Ice Shelf stable elevation over 40 years despite significant inter-decadal fluctuations [Matt King, UK]	Accepted. Citation added.
4-647	4	24	52	24	52	"flow of the grounded ice" [J. Graham Cogley, Canada]	Accepted - text changed
4-648	4	24	53	24	54	Citation of a publication "in submission". [Olaf Eisen, Germany]	Noted. PDF was available.
4-649	4	24	55	24	55	Tedesco et al. 2011 isn’t about ice shelves. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. Citation removed.
4-650	4	24	56	24	57	Citation of a publication "in submission". [Olaf Eisen, Germany]	Noted. This is allowed and a PDF was made available.
4-651	4	25	12	25	12	"equivalent to 1.08 mm yr-1" error bars please. [Ian Joughin, USA]	Accepted. Error limits have been added.
4-652	4	25	15	25	15	How about some discussion of ice sheet and glacier *and* ice cap losses considered together, possibly with a table or figure? [W. Tad Pfeffer, United States of America]	Accepted. Synthesis table to be added to document.
4-653	4	25	19	25	20	Table 4.5: It would be useful to explain how the uncertainty estimates were derived. [Robert Thomas, USA]	Accepted. Explanation that was in Appendix in FOD has been moved to main text.
4-654	4	25	19			why not show the full details in this table so that give figures for each of 12 estimates. [antony payne, uk]	Rejected - full details in Appendix.
4-655	4	25	27	25	29	Given that above the text notes a trend for the Peninsula, perhaps it would be good to say except for the Peninsula. [Ian Joughin, USA]	Agreed. Text added.
4-656	4	25	28			not certain why you need the but here - isn't the first clause explaining the second? [antony payne, uk]	Accepted - text changed
4-657	4	25	31	25	39	Kobashi et al 2011: High variability of Greenland surface temperature over the past 4000 years estimated from trapped air in an ice core, Geophys. Res. Lett., 38, L21501, doi:10.1029/2011GL049444 showed the rise of Greenland temperature. However. They concluded as the case of temperature rise that the current decadal mean temperature in Greenland has not exceeded the envelope of natural variability over the past 4000 years, a period that seems to include part of the Holocene Thermal Maximum. Notwithstanding this conclusion, climate models project that if anthropogenic greenhouse gas emissions continue, the Greenland temperature would exceed the natural variability of the past 4000 years sometime before the year 2100. [Hiroyuki Enomoto, Japan]	Rejected - not relevant to chapter.

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4-658	4	25	32	25	32	Please incert "Recent Greenland temperature reconstruction indicates current decadal temperature (2001-2011) is one of there warmest period during the last 1000 years with 1140s and 1930s, but it is more norm in earlier period of the past 4000 years (Kobashi et al., 2010; 2011)." Kobashi, T., J. P. Severinghaus, J. M. Barnola, K. Kawamura, T. Carter, and T. Nakaegawa (2010), Persistent multi-decadal Greenland temperature fluctuation through the last millennium, Climatic Change, 100, 733-756. [Takuro Kobashi, Japan]	Same as previous comment.
4-659	4	25	32	25	32	Change “the warmest since 1978” to “an exceptionally warm year” as Tedesco et al. (2011) don’t mention 1978 but do cite various station statistics, e.g. Nuuk had its warmest year in the record which began in 1873. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Noted
4-660	4	25	33	25	34	Other studies (e.g., Kushner, P. J., Held, I. M. & Delworth, T. L. Southern Hemisphere Atmospheric Circulation Response to Global Warming. J. Climate 14, 2238–2249 (2001)) showed that global warming can also cause the increase in the SAM index, though some modelling studies showed that Antarcic stratospheric ozone depletion alone can cause an increase in the SAM index in summer. This is still under debate, because current generation atmospheric models still need to be improved. [Zhaomin Wang, UK]	Reject. Comment out of scope for Chapter.
4-661	4	25	35			not certain what relevance winter temperatures in WAIS have on its mass budget. Ditto summer temperature in EAIS. There is no link here to melt given although you are trying to explain change in the mass budget of the ice sheets. [antony payne, uk]	Accepted - text clarified to explain relevance of this text.
4-662	4	25	36	25	39	Please note that the reduction of sea ice extent is also a response to atmospheric circulation change, so here using sea ice retreat is not explaining the real reason. And for the winter temperature change in West Antarctica, the SAM also needs to be invoked for the time scale considered in the report. (tropical forcing effects are mainly on interannual time scales.) [Zhaomin Wang, UK]	Rejected - out of scope of this section.
4-663	4	25	38	25	39	Should "caused by" be "attributable to"? And what does "responding to" mean? I.e. how can one set of temperatures "respond" to another set? [J. Graham Cogley, Canada]	Accepting - text reworded.
4-664	4	25				Table 4.5: How were the error bars on the trends estimated? [David Parker, United Kingdom of Great Britain & Northern Ireland]	Same as comment 653.
4-665	4	26	8	26	8	Leave away “wind driven”, as there are other driving forces. [Martin Lüthi, Switzerland]	Accepted - words "wind-driven" removed.
4-666	4	26	8			mixing greenland and antarctic togther does not help here. There is no evidence that warm waters from lower latitudes are responsible for WAIS changes. This is likely true of Greenland but deep CDW is implicated for WAIS, which is only indiectly fed by the lower latitides. The first two lines give a misleading impression. The winds diving the water mass to the poles may not be the same ones that are responsible for fluctuations in heat delivery to the ice sheets - may be for Greenland but certainly not for Antarctica. [antony payne, uk]	Accepted. Sentence modified, citation added.
4-667	4	26	9	26	9	Jacobs et al. 1992 relates to Antarctic ice shelves, not to the North Atlantic Oscillation. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. Reference deleted.
4-668	4	26	14	26	14	also cite Holland (2008) [Martin Lüthi, Switzerland]	Accepted. Reference added.
4-669	4	26	15	26	15	"warm waters in contact with the ice" [J. Graham Cogley, Canada]	Accepted - text changed
4-670	4	26	27	26	27	specify: lakes form in the a narrow zone at the margin of the ice sheet. [Martin Lüthi, Switzerland]	Accepted. Text modified.
4-671	4	26	27			AT THIS POINT I STARTED TO RECOGNISE MATERIAL THAT I PROVIDED TO THE AUTHORSHIP TEAM. I HAVE THEREFORE STOPPED THE REVIEW BECAUSE OF THIS CONFLICT OF INTEREST. [antony payne, uk]	Accept. Tony Payne added as Contributing author.
4-672	4	26	27			THIS AFFECTS SECTIONS 4.4.3 AND 4.4.4. BEFORE STARTING THE REVIEW, I CHECKED WHETHER I WAS LISTED AS A CONTRIBUTING AUTHOR (I WAS NOT) SO THAT I ASSUMED THAT THE MATERIAL WHICH I HAD PROVIDED HAD NOT BEEN USED. [antony payne, uk]	Same as previous comment.

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4-673	4	26	27			I HOPE THAT THE REVIEWS OF SECTIONS 4.1, 4.3 AND REMAINDER 4.4 IS OF USE. [antony payne, uk]	Yes. Thank you. Accepted.
4-674	4	26	28	26	28	Instead of “lubricate” a meaningful description of the process is required: “basal water pressure increases, and thus basal friction is reduced”. [Martin Lüthi, Switzerland]	Accepted. Text modified.
4-675	4	26	28	26	41	Not only melt induces acceleration, but also heavy rainfall events, which are more frequent in the ice sheet already now, and might increase in the future. So precipitation and melt should be mentioned together. [Martin Lüthi, Switzerland]	Rejected. There are no observational papers to support precipitation driven flow increases in Greenland.
4-676	4	26	31	26	31	also cite Hoffman and Catania, J Geophys Res, in press [Martin Lüthi, Switzerland]	Accepted. Citation added.
4-677	4	26	33	26	33	Change "fall" to "less" [J. Graham Cogley, Canada]	Accepted - text changed
4-678	4	26	33	26	33	what is the meaning of “annual increase in speed”. Should this be: speed increase with respect to the annual mean? [Martin Lüthi, Switzerland]	Accepted. Text changed.
4-679	4	26	35	26	36	Instead of “melting continues to increase” rather say: “under continuous supply of surface water” (including both melt and rain). [Martin Lüthi, Switzerland]	Accepted. Text modified.
4-680	4	26	36	26	36	“speedup becomes less” should be “basal water pressure, and thus basal motion, is reduced”. [Martin Lüthi, Switzerland]	Accepted. Text modified.
4-681	4	26	39	26	39	Calving glacier speedup is mainly an effect of terminus geometry, and thus processes changing this geometry (i.e. all the studies by Vieli and Nick). [Martin Lüthi, Switzerland]	Reject. Comment not relevant to this sentence.
4-682	4	26	39	26	39	It should also be mentioned, that at the K-transect a reduction of flow speed was observed during 15 years, while melt intensity increased considerably (vande Wal 2008, Nature). [Martin Lüthi, Switzerland]	Reject. This is said in the previous sentence and the paper is cited.
4-683	4	26	39	26	41	If Parizek and Alley (2004) are to be cited as such, the time scale of this result needs to be included to avoid misunderstanding. [Joel Harper, United States]	Accepted. Sentence has been modified in response to a previous comment to remove future.
4-684	4	26	41	26	41	The Parizek study relies on an unphysical sliding relation, which disagrees with all observations discussed in this section. It should not be mentioned here, or only stating that it is based on wrong assumptions (sliding depends on melt water supply, instead of water pressure, as has been known since 150 years). In a certain sense, this study is a worst-case scenario. [Martin Lüthi, Switzerland]	As previous comment.
4-685	4	26	43	26	45	These lines are in Section 4.4.3.2.1 titled “Basal Lubrication”. These sentences do not seem appropriate here since they do not concern basal water processes. [Joel Harper, United States]	Accepted. New subsection added.
4-686	4	26	44	26	44	Near-basal ice is warmed through dissipation and geothermal heat flux (see modeling studies by Budd, Greve, Funk etc). The Phillips study explains that the whole ice body could be warmed by supply of heat through vertical pathways, and thus explains the unusual temperature profile measured in the Pakitsoq area by Thomsen. This, however, might only be a local effect around moulins, as our own temperature measurements in the area imply. [Martin Lüthi, Switzerland]	Noted. The process is somewhat speculative, and is introduced as such.
4-687	4	26		27		In pages 4-26 and 4-27 the interaction between glacial melt and expanding sea ice is overlooked. [Richard Bintanja, Netherlands]	Rejected. We know of no papers that would support this relationship.
4-688	4	27	1	27	1	The term 'ice-ocean interaction is unfortunate. Also calving is part of and affected by ice-ocean interaction. The term includes more than marine melting. What is meant here is obviously to distinguish between 2 types of frontal ablation: submarine melting and iceberg calving and the headers should reflect that. [Regine Hock, US]	Agreed. Discussion has been undertaken with Chapter 13 to standardize terminology.
4-689	4	27	3	27	3	Change "Numerical models suggest that ice melting" to "By definition, the flux of meltwater" [J. Graham Cogley, Canada]	Accepted. Sentence modified
4-690	4	27	6	27	7	The subject of greater ice melting increasing the buoyancy of the melt-water plume is in Holland et al. 2008b not 2008a. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. Citation date corrected

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4-691	4	27	7	27	7	Change "conclusion" to "expectation" [J. Graham Cogley, Canada]	Accepted - text changed
4-692	4	27	9	27	9	Compared to AR4 it has become far more evident that submarine melting can be very large. This should be stressed here including the latest references on that topic (e.g. Motyka et al., [Regine Hock, US]	Agreed. Motyka et al., 2011 added to references.
4-693	4	27	9	27	12	I found this puzzling. Should "nearly horizontal" be "nearly vertical"? And are the marine-terminating margins aground or afloat? [J. Graham Cogley, Canada]	Rejected. No correction needed to "nearly horizontal". Tidewater glaciers are either afloat or grounded.
4-694	4	27	12	27	12	Motyka et al (JGR, 2011) find melt rates of 200-300 m/a [Martin Lüthi, Switzerland]	Accepted. Citation added but actual measurements are not to be listed in the sentence
4-695	4	27	12	27	13	"robust" and "likely" do not go together, and would be redundant if they did. I would delete "liekly". [J. Graham Cogley, Canada]	Accepted.Changed to "Multiple lines of evidence"
4-696	4	27	14	27	14	The subject of subtropical ocean waters is in Holland et al. 2008a not 2008b. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. Citation data corrected
4-697	4	27	16	27	16	The process is more complicated. As ice stream velocity is purely determined by geometry the statement should be: Ice melting changed terminus geometry and loss of ice shelves, thus changing terminus geometry. This in turn lead to temporary acceleration of many outlet glaciers due to loss of buttressing. (Some outlets slowed down when their geometry had sufficiently changed, i.e. Helheim Glacier). [Martin Lüthi, Switzerland]	Accept. Text modified.
4-698	4	27	24	27	24	"Amundsen" (also at P40 L16) [J. Graham Cogley, Canada]	Rejected. Amundson is correct spelling of this author.
4-699	4	27	32	27	42	There are instances "we" in this paragraph that are inconsistent with the rest of the document, which does not use "we". [Ian Joughin, USA]	Accepted - text changed
4-700	4	27	33	27	33	the word 'feeling' should be avoided. Better 'consensus' [Regine Hock, US]	Accepted. Text changed.
4-701	4	27	34	27	34	"thought to be" [J. Graham Cogley, Canada]	Accepted - text changed
4-702	4	27	34	27	34	add 'to' after 'thought' [Zhaomin Wang, UK]	Accepted - text changed
4-703	4	27	41	27	42	"prevent thinning from reversing" is a contorted way of stating the problem. What I think the clause is trying to say is "because at its new lower (and therefore warmer) surface elevation the ice sheet would be able to grow thicker only slowly in a cooler climate". That is, the point to be emphasized is the slowness of re-growth. [J. Graham Cogley, Canada]	Accepted. Text changed.
4-704	4	27	46	28	36	"(Joughin et al., 2010b" this should be changed to "Joughin et al, 2008, Science " [Ian Joughin, USA]	Accepted. Error was at p28 (not 27) and has been corrected.
4-705	4	27	48	27	48	rather say “consequent speedup and thinning”. Speedup is a consequence of geometry change. [Martin Lüthi, Switzerland]	Accepted - text changed
4-706	4	27	48	27	48	also cite “Motyka et al, (2011) [Martin Lüthi, Switzerland]	Accpeted. Text changed.
4-707	4	27	49	27	49	why this simplified (and wrong) statement “increased surface melting which increases ice flow”. As discussed above, this process chain is much more complex. Rather refer to “increased basal motion”. [Martin Lüthi, Switzerland]	Accepted. Text changed.
4-708	4	27	50	27	50	also mention thinning (and thus weakening) of ice shelves (e.g. Motyka et al., 2011) [Martin Lüthi, Switzerland]	Accepted. Text changed.
4-709	4	27	54	27	54	Griggs and Bamber, 2011a: This is not a suitable reference, as it referes to a programme, but not to a publication: [Olaf Eisen, Germany]	Accepted: reference revised
4-710	4	27	54	27	55	Pritchard et al., 2011: I do not consider an AGU fall meeting contribution, even if listed in the EOS Transactions, a suitable contribution to the list of references: [Olaf Eisen, Germany]	Accepted: reference revised
4-711	4	28	1	28	6	This is essentially the 3rd place where the ocean is mentioned mostly in the context of increased heat to the	Accepted. We will ensure no repitition of text, but ice-

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						margins. Given this is established above, this section should focus on how the heat actually affects flow. Some editing to consolidate ocean text would be good. [Ian Joughin, USA]	ocean interactions are a new and important part of AR5 and will thus appear in a number of locations.
4-712	4	28	3	28	3	ocean currents are not just controlled by wind. If this stays, also mention changing sea ice cover, among other processes. [Martin Lüthi, Switzerland]	Accepted. Reference to winds removed.
4-713	4	28	6	28	6	Preferable a neutral statement “direct and rapid impact on calving flux”. (ice loss implies that this process is only going in one direction, which is not true). [Martin Lüthi, Switzerland]	Accepted. Text changed.
4-714	4	28	11	28	11	"of equivalent sea-level rise", with a similar change at P28 L26 [J. Graham Cogley, Canada]	Accepted - text changed
4-715	4	28	22	28	22	Joughin 2010a should probably replace Joughin 2008 here as it is more appropriate (altimetery and velocity observations as well as model show PIG changes propagating 100s of km inland rapidly). [Ian Joughin, USA]	Accepted -reference changed
4-716	4	28	23	28	24	reference needed [Regine Hock, US]	Accepted. Reference added.
4-717	4	28	28	28	28	"the region changing most rapidly at present" ("presently" means "soon"). [J. Graham Cogley, Canada]	Accepted. Text changed for clarity (although we disagree with the reviwers North American (?) version of English).
4-718	4	28	34	28	34	Jakobshavns' should be 'Jakobshavn'. [Jacob Clement Yde, Norway]	Accepted - text changed
4-719	4	28	34			"recent rapid retreat of Jakobshavns" Is this "runoff?" [Alan Robock, USA]	Reject. No it is not runoff.
4-720	4	28	36	28	36	"…factors". It would be appropriate to say here "such as weakening of sea ice melange (JOURNAL OF GEOPHYSICAL RESEARCH, VOL. 113, F04006, doi:10.1029/2008JF001023, 2008 and the Jason Amundsen paper cited above). [Ian Joughin, USA]	Accepted. Text modified.
4-721	4	28	40	28	40	The slowdown had partially geometric reasons, e.g. at Helheim (Nick, Vieli). [Martin Lüthi, Switzerland]	Reject. We are not including the causes of the slowdown here.
4-722	4	28	43	28	43	Insert a reference to 'Mernild et al., in review: Multi-decadal marine and land-terminating glacier recession in the Ammassalik region, Southeast Greenland. Cryosphere' after 'slowly' to support this statement. [Jacob Clement Yde, Norway]	Reject. Paper suggested is regional but sentence is Greenland-wide.
4-723	4	28	48	28	53	Are there examples of other irreversible changes other than Larsen B? Domack et al. suggests that many of the shelves are at the climatic limit of viability... I might start with the Larsen B example, and then draw implications for the rest of the Peninsula... for example, the following, if the authors think it is still grounded in the literature: "On the Antarctic Peninsula, the 2002 Larsen B collapse is an example of an irreversible change. Larsen B had been a stable component for the past 11 kyr, but gradual thinning over thousands of years combined with recent decades of warmths led to the unprecedented breakup (Domack et al., 2005). Even if calving were to cease entirely, regrowth would take centuries. Air temperatures rising across the Peninsula (cite), and warm CDW is becoming widespread on the western shelf (cite). Therefore, other shelves on the Peninsula at their climatic limit may be vulnerable to similar irreversible collapses, though some smaller shelves have seen both decay and regrowth in the past several thousand years (Domack et al. 2005)." [Marcus Sarofim, USA]	Accepted. Larsen B is the only collapse with evidence of long timescales since its last collapse. However, there is a paper that refers to Larsen A which is added to the text.
4-724	4	28	49	28	49	"times the global average rate" [J. Graham Cogley, Canada]	Accepted - text changed
4-725	4	28	52			Page 4-28, line 52. 'Irreversible' is not really an appropriate phrase, I guess they mean 'unlikely to return to the present state within some overseeable time period' (about 100 years or so). Irreversible means that is never comes back, which can't be predicted. [Richard Bintanja, Netherlands]	Rejected. Irreversible is defined in the text and in a different way than this reviewer uses it.
4-726	4	28	53	28	53	"…would take centuries" this should be supported by a citation. [Ian Joughin, USA]	Accepted. Sentence added on flow speed of ice shelf.
4-727	4	29	1	29	1	Insert 'Isbrae' after 'Jakobshavn'. [Jacob Clement Yde, Norway]	Accepted - text changed
4-728	4	29	2	29	2	A good example here would be Helheim which did readvance after the 1930s warming (Joughin et al 2008 - in	Accepted. Added Helheim Glacier to discussion.

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						the current ref list and Andresen et al, Nature geoscience, recently online, Rapid response of Helheim Glacier in Greenland to climate variability over the past century). [Ian Joughin, USA]	
4-729	4	29	2	29	2	Young et al. 2011 should be a different reference from the Antarctic paper by Young, D.A. et al; Young, N.E. et al. (2011): Response of Jakobshavn Isbræ, Greenland, to Holocene climate change Geology, 39, 131-134. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. There are 2 different Young et al., 2011 papers. Correct one has now been referenced here.
4-730	4	29	7	29	7	"…will be required" change to "is required" [Ian Joughin, USA]	Accepted - text changed
4-731	4	29	9	29	9	Picky point but "will be oberved" would be better to say "will occur" (they will happen whether or not we are around to observe them). [Ian Joughin, USA]	Accepted - text changed
4-732	4	29	11	31	36	Combination of seasonal snow and freshwater ice cover is not natural - separate sections [Richard Essery, UK]	Rejected - there is not enough new research on the freshwater ice cover to make it a separate section.
4-733	4	29	11			Section 4.5: Observations and model simulations suggest an increasing trend in Arctic snowfall (Min et al., 2008; Zhang et al., 2007). Increases in snowfall coupled with the observed spring warming signal have obvious implications for the water cycle (Rawlins et al., 2009). Warming temperatures have also had an impact on precipitation phase (Screen and Simmonds, 2011). Given these developments, why are snowfall trends not clearly considered within what is quite a brief snow section? Min, S-K., X. Zhang, and F. Zwiers. 2008. Human-induced Arctic moistening. Science. 320: 518-520. Zhang, X., F. Zwiers, G. Hegerl, H. Lambert, N. Gillett, S. Solomon, P. Stott and T. Nozawa. 2007. Detection of human influence on twentieth-century precipitation trends. Nature. doi:10.1038/nature06025. Screen, J., and I. Simmonds. 2011. Declining summer snowfall in the Arctic: causes, impacts and feedbacks. Climate Dynamics. DOI 10.1007/s00382-011-1105-2. Rawlins, M., M. Steele, M. Holland, J. Adam, J. Cherry, J. Francis, P. Groisman, L. Hinzman, T. Huntington, D. Kane, J. Kimball, R. Kwok, R. Lammers, C. Lee, D. Lettenmaier, K. McDonald, E. Podest, J. Pundsack, B. Rudels, M. Serezze, A. Shiklomanov, Ø. Skagseth, T. Troy, C. Vorosmarty, M. Wensnahan, E. Wood, R. Woodgate, D. Yang, K. Zhang,, and T. Zhang. 2010. Analysis of the Arctic system for freshwater cycle intensification: observations and expectations. Journal of Climate. 23: 5715-5737. [Chris Derksen, Canada]	Rejected - snowfall is the domain of Chapter 2. Sentence added to introduction to clarify.
4-734	4	29	11			Another variable not presented clearly in Section 4.5 is snow water equivalent (SWE). SWE was not reported in AR4 because the limitations in characterizing SWE include a very sparse conventional observing network, and high uncertainty in satellite retrieval algorithms based on passive microwave measurements. Since AR4, however, significant progress has been made on SWE retrievals, including new approaches that combine microwave measurements, conventional observations, and snow emission modeling in an assimilation framework (i.e. Takala et al. 2011). This results in significantly lower uncertainty than previous algorithms, and led to the development of validated hemispheric SWE time series (Takala et al., 2011; Derksen et al., 2011). Derksen, C., and R. Brown. 2011. Terrestrial Snow (Arctic). In State of the Climate in 2010. Bulletin of the American Meteorological Society. 92: S154-S155. Takala, M., K. Luojus, J. Pulliainen, C. Derksen, J. Lemmetyinen, J-P Kärnä, and J. Koskinen. 2011. Estimating northern hemisphere snow water equivalent for climate research through assimilation of space-borne radiometer data and ground-based measurements. Remote Sensing of Environment. doi:10.1016/j.rse.2011.08.014. [Chris Derksen, Canada]	Accepted -
4-735	4	29	15	29	15	Delete "one of" [Richard Essery, UK]	Accepted
4-736	4	29	15			Some reference to SNOTEL NETWORK and snow courses data could be of interest [Juan Ignacio López Moreno, Spain]	Rejected. These are but two of many approaches - text notes "a variety of instruments and techniques" - and if these two were mentioned, then the omission of other techniques would be problematic.
4-737	4	29	23			"Northern Hemisphere (NH)." Previously, "northern hemisphere" was used without capital letters. Be consistent. [Alan Robock, USA]	Accepted - changed to upper case in Box 4.1 for consisteny with rest of chapter

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4-738	4	29	24	29	25	NOAA is not the only organization with geostationary satellites that can view snow, and they do not only see the NH [Richard Essery, UK]	Accepted - text removed and/or clarified
4-739	4	29	24	29	25	The AVHRR instruments producing imagery used in the snow mapping discussed by Robinson et al. (1993) fly on polar orbiting platforms, not geostationary [Richard Essery, UK]	Accepted - text removed and/or clarified
4-740	4	29	24	29	25	Geostationary satellites see both hemispheres equally. Robinson et al. (1993) used a mix of geostationary and polar-orbiters. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted - text removed and/or clarified
4-741	4	29	25	29	26	It should be noted that satellite estimations of snow depth and SWE still have numerous problems in their quality; depending on snow wetness, vertical structure, presence of above-snow vegetation, errors in determining these parameters can be very high (Romanov P. and D. Tarpley (2007) Enhanced algorithm for estimating snow depth from geostationary satellites, Remote Sensing of Environment, 108, 97-110; Clifford D. (2010) Global estimates of snow water equivalent from passive microwave instruments: history, challenges and future developments. International Journal of Remote Sensing, Volume 31, Issue 14, 2010 Special Issue: Satellite-Based Observations of Hydrological Processes. DOI: 10.1080/01431161.2010.483482, pages 3707-3726.) [Andrey Shmakin, Russia]	Taken into account - text revised to note problems
4-742	4	29	25	29	26	It should be noted that satellite estimations of snow depth and SWE still have numerous problems in their quality; depending on snow wetness, vertical structure, presence of above-snow vegetation, errors in determining these parameters can be very high (Romanov P. and D. Tarpley (2007) Enhanced algorithm for estimating snow depth from geostationary satellites, Remote Sensing of Environment, 108, 97-110; Clifford D. (2010) Global estimates of snow water equivalent from passive microwave instruments: history, challenges and future developments. International Journal of Remote Sensing, Volume 31, Issue 14, 2010 Special Issue: Satellite-Based Observations of Hydrological Processes. DOI: 10.1080/01431161.2010.483482, pages 3707-3726.) [Andrey Shmakin, Russia]	duplicate of comment 4-742
4-743	4	29	33	29	46	Section 4.5.2: It should be explicitly stated that the addition of uncertainty estimates to the NH SCE time series (Figure 4.19), obtained through the statistical analysis of multiple datasets, represents an improvement over our understanding of the time series in AR4, which only included fixed, ad hoc uncertainty estimates. [Chris Derksen, Canada]	Accepted
4-744	4	29	35	29	35	Figure 4.19 could also be interpreted as showing little trend over the last 20 years following a shift in the 1980s, rather than an increasing rate of decrease [Richard Essery, UK]	Accepted. Text changed
4-745	4	29	39			"..since winter 1972/1973.." [Christoph Marty, Switzerland]	Accepted
4-746	4	29	42	29	42	Change “shorter snowmelt season” to “shorter snow-cover season”. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Noted
4-747	4	29	43	29	46	The text say shortning of melting period but number is positive (-0.5 +1.0): this shows enlonging of period. [Hiroyuki Enomoto, Japan]	Accepted
4-748	4	29	46			Substitute +1 day/ yr by -1 day/ yr; because it is mentioned a "shortening" of the melt season [Thomas Voigt, Germany]	Accepted
4-749	4	29	53	29	53	It might be useful to explain why the rates of SCE in the fall and spring are different… [Marcus Sarofim, USA]	Noted
4-750	4	29	57	29	57	What does "reductions in the surface energy balance" mean? [Richard Essery, UK]	Taken into account - text revised
4-751	4	29				Section 4.5. You may be interested in Marty, C. and R. Meister (2012): Long-term snow and weather observations at Weissfluhjoch and its relation to other high-altitude observatories in the Alps. Theoretical and Applied Climatology. doi: 10.1007/s00704-012-0584-3. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account in revision
4-752	4	30	11	30	16	I would remove any reference to southern hemisphere snow cover. The Foster et al study focused on South America, and their paper showed that the passive microwave SWE retrievals had almost no skill. Retrieving snow extent from microwave data in high relief areas with ephemeral snow cover has a high uncertainty that is	Rejected. In our assessment, the snow extent data are more reliable than the SWE data, and we focus on the snow extent data. Most of the variability in extent

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						well documented. [Chris Derksen, Canada]	arises from snow on the plains of Argentina, where the flat terrain and lack of forest cover are conducive to satellite retrievals unlike the mountains where most of the signal in SWE originates. This is a unique climate record and should be mentioned.
4-753	4	30	20	30	27	Section 4.5.3: Figure 4.21 is too complicated and virtually unreadable. This does not effectively synthesize trends from the various in situ datasets. A clearer perspective on trends could be illustrated from the NOAA snow extent Climate Data Record, either in the form of gridded trends in the snow cover duration, or a Hoevmuller type diagram. [Chris Derksen, Canada]	Accepted - figure revised for clarity, and TSU will be urged to ensure that the final appearance resembles the original high-resolution file
4-754	4	30	20			The work: Impacts of climate change on snow, ice, and permafrost in Europe: Observed trends, future projections, and socio-economic relevance ETC/ACC Technical Paper 2010/13: http://acm.eionet.europa.eu/reports/ETCACC_TP_2010_13_Cryosphere_CC_Impacts could be taken into consideration [Juan Ignacio López Moreno, Spain]	Accepted. We are grateful to the reviewer for bringing this report, and the papers and reports it cites, to our attention. We have used it to fill some important gaps.
4-755	4	30	21	30	21	Delete “from 14 countries” as this isn’t explicit in Brown and Mote (2009). In their Table 2 they list 14 or 15 models depending on whether both versions of the CSIRO model are counted. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted; however, the number 14 came directly from the authors (one of whom wrote this section). 12 of those can be deduced on page 2125.
4-756	4	30	27	30	32	A full review of the relevant literature is not provided. Why are snowfall trends presented here for China, but not other regions? (for example, Mekis, E., and L. Vincent. 2011. An overview of the second generation adjusted daily precipitation dataset for trend analysis in Canada. Atmosphere-Ocean. 49: 163 – 177.) Why are ‘visual observations of snow cover duration on a mountain in Scotland’ highlighted? This is not of appropriate scope for an IPCC assessment. [Chris Derksen, Canada]	Last comment accepted; first 2 parts rejected. IPCC is an assessment, not a review. And snowfall is covered in Chapter 2.
4-757	4	30	30	30	32	I suggest deleting the sentence about Scotland because it relates to a single site in complex terrain. Furthermore 1979 was an exceptionally snowy winter-spring in the UK and will have influenced the 1979-2003 trend unduly. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted.
4-758	4	30	40	30	40	There is no asterisk apparent in Figure 4.21 [Richard Essery, UK]	Noted. The asterisks appeared in the column labeled 'N' and are now explained in the text.
4-759	4	30	47	30	47	There are also suggestions that changes in vegetation could be changing the albedo of snow-covered landscapes. e.g. Sturm et al. (2005), J Geophys Res 110 G01004, Loranty et al. (2011), Environ. Res. Lett. 6(2) [Richard Essery, UK]	accepted - text now mentions vegetation
4-760	4	30	49	30	57	If the objective of this chapter is to use cryospheric observations as an indicator of a changing climate then it is not clear why there is a discussion of the changing albedo in response to human activities (increased combustion etc.). The change in albedo is not an indicator of climate change but is the result of something else. While it is understood that there are feedbacks to the climate system due to changing albedo, is the subject of climate forcing not better discussed elsewhere (another chapter). [Sharon Smith, Canada]	Noted. This section on black carbon was called for in the scoping document for chapter 4. Both chapters 7 and 8 cover issues related to surface albedo, but the
4-761	4	30	49	31	2	The reduciton in snow and sea ice albedo in the Northern Hemisphere due to black carbon was calculated as 1% and that globally was calculated as 0.4% in Jacobson, M.Z., The climate response of fossil-fuel and biofuel soot, accounting for soot’s feedback to snow and sea ice albedo and emissivity, J. Geophys. Res., 109, D21201, doi:10.1029/2004JD004945, 2004. The corresponding BC in snow was ~5.5 ng/g globally. The temperature effect of such BC globally was +0.06 K (+0.03 to +0.11 K). [Mark Z. Jacobson, U.S.A.]	Noted. Here we assess progress in observations since 2006, and the comment points to a modeling-based paper in 2004.
4-762	4	30	52	30	52	After “warming” add: The dust in snow, which is significant in large arid and semiarid regions, can also reduce the albedo (Huang et al., 2011). Reference: Huang J., Q. Fu, W. Zhang, X. Wang, R. Zhang, H. Ye, and S. Warren, 2011: Dust and Black Carbon in Seasonal Snow Across Northern China, Bulletin of the American Meteorological Society, doi: 10.1175/2010BAMS3064.1. [Jianping Huang, China]	Taken into account. The sentence specifically talks about human influence on albedo; sentence added pointing out the role of dust.
4-763	4	30	54	30	56	You could potentially refer to other estimates of the radiative forcing of black carbon in snow and ice, and not just Flanner et al. (2007). [Borgar Aamaas, Norway]	Noted. This is an assessment, not a review, so a single representative paper is adequate.
4-764	4	30	56	30	56	Change “black carbon” to “black carbon – snow-cover interactions” [David Parker, United Kingdom of Great	Accepted

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						Britain & Northern Ireland]	
4-765	4	30	56	30	56	Make it clear that the 0.05 W/m2 refers to forcing by black carbon _due to deposition on snow and ice_ (and that the number is global, to my understanding): also, the details about the Arctic wildfires in 1998 and 2001 are not necessary for this sentence. [Marcus Sarofim, USA]	Taken into account: see response to 4-765
4-766	4	31	1	31	1	Change “indicate” to “suggest” because Doherty et al. 2010 make caveats about methodological changes. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted
4-767	4	31	1	31	2	"albedo changes have not been responsible for reductions in Arctic ice and snow": I don’t think that this sentence is very clear. At minimum, it should state "recent reductions", but even so, just because it is cleaner now than 20 years ago doesn't mean that the darkening of snow is not contributing to the melting, it just isn't contributing as much as if it had stayed the same: maybe rephrase it to state that "the recent increased rate of reduction of Arctic ice and snow is due to factors other than an reduction in albedo due to snow impurities"? [Marcus Sarofim, USA]	Accepted
4-768	4	31	1		2	"…and hence albedo changes have not been responsible for reductions in Arctic ice and snow." Strong message, further references would be useful. [Thomas Voigt, Germany]	taken into account: see response to 4-768
4-769	4	31	6	31	6	"number of observations" or "duration and extent"? The former phrase makes me think that we've stopped looking at freshwater lakes. [Marcus Sarofim, USA]	Accepted
4-770	4	31	9			"y" The SI unit for year is "a" and needs to be used throughout. Farther on, you also use "yr" [Alan Robock, USA]	Accepted
4-771	4	31	18			at lower latitudes than at higher latitudes This needs to be explained. Is it because the higher latitude ones are below freezing the entire year? [Alan Robock, USA]	text will be clarified
4-772	4	31	28	31	28	SEE BARRY & GAN 2011 THE GLOBAL CRYOPSHERE PP. 195-2002.ESPECIALLY REGRETA LAKES WANG 2010 EOS NO 5, P. 41 [Roger Barry, USA]	Unclear what this comment refers to
4-773	4	31	38			Section 4.6: Avoid using the term "permafrost boundary". REASON: The term "permafrot boundary" is misleading as it suggests a sharply delineated feature that permfrost is not – at least not at thesaceles of interest, here. REFERENCE regarding boundary DOI: 10.5194/tcd-5-1547-2011 (Gruber, S. Derivation and analysis of a high-resolution estimate of global permafrost zonation. The Cryosphere Discuss., 5, 1547-1582, 2011 www.the-cryosphere-discuss.net/5/1547/2011/) [Stephan Gruber, Switzerland]	The term will be fully defined at the first use, and then the term will be stated as "the boundary of permafrost" or zone.
4-774	4	31	42	31	43	Change to "...but where the ground remains at or below...". This is because "frozen" implies ice content that is not required in the definition of permafrost. [Stephan Gruber, Switzerland]	Acepted - change made
4-775	4	31	42	31	43	The permafrost definition by temperature is not very useful. It should rather be stated that the ground is at or below the phase change temperature (which varies with pressure, solute content, and geometry of fissures). [Martin Lüthi, Switzerland]	Rejected: This is a widely accepted definition in the global permafrost community and must be reflected here.
4-776	4	31	42	31	53	Is this information really necessary here? Presumably this is covered in WG2 [Sharon Smith, Canada]	Note the comment, but believe that this is importmant material. Howvever, we have added citations that link to Chapter 6 (Carbon) and Polar regions chapter of WGII
4-777	4	31	42	31	53	In this section motivating the research and monitoring of frozen ground, it might be worthwile to mention the relevance of permafrost changes and its potential impact on the hazard situation in densly populated mountain ranges such as the European Alps. [Michael Zemp, Switzerland]	Noted. This is largely the scope of WGII, however, the addition of two words ("…and mountain inhabitants") could make this point. This has been done.
4-778	4	31	43	31	43	Insert 'at' after 'remains'. [Jacob Clement Yde, Norway]	Accepted and changed.
4-779	4	31	43			delete "or" [Alan Robock, USA]	accepted and changed.

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4-780	4	31	45		46	…directly couple… -> this is only true without a snow cover. [Nadine Salzmann, Swizerland]	Accepted - deletion of "direclty" makes this OK.
4-781	4	31	47	31	48	Change to "When permafrost degrades, dramatic changes in geomorphology, ecosystems, and hydrological processes can occur (REFS). (A) "Ice-rich" is not understandable to laypersons and in fact changes also occur in non-ice-rich permafrost. (B) Thermokarst in lowlands (Jorgenson et al 2006) and rock fall (Gruber and haeberli 2007) are important geomorphic consequences. [Stephan Gruber, Switzerland]	Acepted - "The most drammatic changes can occur…. When ice-rich permafrost degrades")
4-782	4	31	51			CO2 and CH4 : need to use subscripts for the 2 and 4 [Alan Robock, USA]	Accepted
4-783	4	31	52	31	53	Change to "Permafrost degradation also affects the lives of people, both in northern and high-mountain areas, through dramatic changes in landscape, vegetation and impacts on infrastructure." The exclusive focus on northern areas is an observer bias because that is where most people investigate permafrost phenomea. [Stephan Gruber, Switzerland]	Accepted - same response as 4-777
4-784	4	31	52	31	53	Will be good to mention infrastructure related to industrial developments [Vladimir Romanovsky, USA]	Noted. Will add a few words but this mainly for WGII .
4-785	4	31	52			"inhabitants" and industrial installations, such as oil wells, [Alan Robock, USA]	Disagree - this is WGII territory
4-786	4	31	55	31	55	It is suggested that section 4.6.2 heading be changed to "Changes in Terrestrial Permafrost" as this is what the section covers. [Sharon Smith, Canada]	Noted. The term "terrestrial Permafrost" is not a commonly used phrase, but "Subsea Permafrost" is. So to avoid creating a new term, we should not make this change to the heading. But we've added a couple of sentences to the background paragraph to make this clear.
4-787	4	31	55			4.6.2: It would be great to have an indicator of area covered by at permafrost: this section discusses temperature increase in permafrost, movement of the boundary of continuous permafrost in km (which is getting close), and change in depth, but it would be nice to have a km^2 measure of permafrost degradation. Another useful metric, rather than the change in area of continuous permafrost, another useful metric would be the area covered by permafrost which has changed enough to disrupt infrastructure (whether or not any infrastructure is actually built on it at the moment). [Marcus Sarofim, USA]	Noted and Rejected - The data required to produce such a statement is not available in the literature. Will make the statement in the text that such data is not available.
4-788	4	31		35		In the section 'Frozen ground' I am missing the relation of thawing with the (potential) release of methane from the soil. This process is a potentially important climate feedback on the decadal to century time scale. [Richard Bintanja, Netherlands]	As noted previously, a cross-reference to Chapter 6, will highlight this issue
4-789	4	31				Section 4.5.5. New reference: Wang, J., X. Bai, H. Hu, A. Clites, M. Colton, and B. Lofgren, 2012: Temporal and Spatial Variability of Great Lakes Ice Cover, 1973–2010*. J. Climate, 25, 1318–1329. doi: http://dx.doi.org/10.1175/2011JCLI4066.1 [David Parker, United Kingdom of Great Britain & Northern Ireland]	This comment is misplaced. Phil Mote to deal with it.
4-790	4	31				Section 4.6 - Regarding the Permafrost component of this section - It is suggested that the focus be on variables that are recognized as key indicators by the WMO such as permafrost thermal state and active layer thickness. Processes such as coastal erosion can be the result of several factors (and are also natural processes) and do not necessarily have as direct a link with climate as the recognized indicator variables (additional comments to follow). The section could have benefitted greatly from authorship of those in the permafrost community that are recognized for their work in permafrost monitoring and application to indicators of climate change including the substantial contributions related to IPY (and also largely responsible for the scientific results reported in this chapter). [Sharon Smith, Canada]	Noted. This section covers not only permafrost but frozen ground in general. Changes in permafrost thermal state and active layer are important part of the work here, thermokarst and thaw lakes, coastal erosion, and seasonally frozen ground are also important part of frozen ground.
4-791	4	32	3	32	10	Do you mean air temperature is the key parameter? Note that local factors are important in determining the spatial variability in permafrost thermal state - this results in a rather scattered relationship between latitude and permafrost temperature even within the same region (as discussed further in Smith et al. 2010 & Romanovsky et al. 2010a). Note also that most of what is in this paragraph including the regional differences in permafrost temperature and relation to ocean circulation has essentially come from Romanovsky et al. (2010a) [Sharon Smith, Canada]	Agreed - change made "the temperature of the PF is the key parameter that determines its physical state.

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4-792	4	32	12			Elevation is not really the factor, elevation is only an inidirect factor, the real factor is air temperature, or even better, the energy/radiation balance. [Nadine Salzmann, Swizerland]	Agreed, We will delete first sentence of this paragraph, but retain the sense of the following sentence.
4-793	4	32	13	32	13	There is permafrost even on parts of the summit of Kilimanjaro. See http://www2.gi.alaska.edu/ScienceForum/ASF19/1985.html. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted - modification made.
4-794	4	32	13	32	13	There are known permafrost occurrences much closer to the equator, e.g. mount Kilimanjaro [Vladimir Romanovsky, USA]	Accepted - see response 4-793
4-795	4	32	17	32	20	In this section, the authors could probably remove the older references (i.e. those from 1958-2007) as most of the required information is contained in the regional papers published as a special IPY issue of Permafrost and Periglacial Processes (these include Romanovsky et al. 2010b, Smith et al., 2010, Christiansen et al. 2010, Zhou et al 2010, Veiera et al 2010) [Sharon Smith, Canada]	Disagree - the reference to Brewer, 1958 is the key reference for measurements back to 1940s, and so stands. The same is true to many other important papers in this regard.
4-796	4	32	24	32	25	Romanovsky et al. citations 2010a and b should be interchanged in the caption of Figure 4.22. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted - change made
4-797	4	32	32	32	38	Note that Romanovsky et al. (2010a) and Smith et al. (2010) showed that within the discontinuous permafrost zone mean annual ground temperatures are generally above -2°C so that it is more correct to say that warm permafrost is generally found in the disc. zone rather than cold permafrost accounting for most of the permafrost in the cont. and disc. zone. In this section there also needs to be the distinction between forested and tundra environments and also soil vs bedrock as the thermal behaviour will depend on vegetation and subsurface conditions. [Sharon Smith, Canada]	Accepted - additional discussion will be added.
4-798	4	32	32	33	7	The authors should also consider referring to the recent Arctic Report Card and the State of the Climate Report for updated information. Key references include: Romanovsky, V.E., Smith, S.L., Christiansen, H.H., Shiklomanov, N.I., Drozdov, D.S., Oberman, N.G., Kholodov, A.L., and Marchenko, S.S. 2011. Permafrost [in Arctic Report Card 2011]. pp. 139-147. Lantuit, H., Christiansen, H., Zhao, L., Noetzli, J., Romanovsky, V., Shiklomanov, N., Smith, S., and Vieira, G. 2011. [Global Climate] Permafrost thermal state [in "State of the Climate in 2010"]. Bulletin of the American Meteorological Society, 92(6): 48-49. The Arctic permafrost section of this same Bulletin by Romanovsky et al. could also be consulted [Sharon Smith, Canada]	Noted and Disagree - the Arctic report contains no more information than is available from other, more obviously, peer-reviewed literature.
4-799	4	32	33	32	33	should be Figure 4.22 [Vladimir Romanovsky, USA]	Accepted
4-800	4	32	33	32	36	It is not clear why -2 C was chosen as a threshold between cold and warm permafrost [Vladimir Romanovsky, USA]	Noted. The -2C was chosen based on the article by Smith et al. (2010) and this reviewer is a co-author of the article.
4-801	4	32	33			Substitute '(Figure 4.21)' by '(Figure 4.22)' [Thomas Voigt, Germany]	Accepted
4-802	4	32	36	32	37	A short explanation of this fact is needed here [Vladimir Romanovsky, USA]	Accepted - addition made with new references
4-803	4	32	36	32	37	It is more correct to refer to rates of observed temperature change rather than rates of warming. It is suggested that the sentence be reworded "Permafrost temperature increases are generally observed to be smaller in warm permafrost, especially if ice-rich, compared to cold permafrost where latent heat effects will be less important." [Sharon Smith, Canada]	Accepted - similar to 4-802. Change made
4-804	4	32	36		37	This needs some more explanations [Nadine Salzmann, Swizerland]	Accepted - similar to 4-802. Change made
4-805	4	32	36			"–2°C or higher" where? [Alan Robock, USA]	Accepted - addition to make the depth explicits
4-806	4	32	37	32	38	Change to "Cold permafrost accounts for the majority of continuous permafrost zones, where permafrost temperatures have increased...". The "majority of discontinuous permafrost" is not substantiated and to me appears unlikely: If a significant part of the subsurface has temperatures above 0°C, why should much of the	Accepted and changes are made.

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						rest be cold? [Stephan Gruber, Switzerland]	
4-807	4	32	38	32	38	None of discontinuous permafrost shows such a large warming (see Table 4.6) [Vladimir Romanovsky, USA]	Agreed - Changes will be made to the table to separate disc and cont. A more detailed Table will be made and add more information.
4-808	4	32	38	32	38	In this discussion of temperature increases we need to be clear about the depth. There was an attempt in the papers in the IPY special issue of Permafrost and Periglacial Proc. to refer to temperatures at or near the level of zero annual amplitude so that shorter term variations were filtered out. [Sharon Smith, Canada]	Agreed - a statement about shallow measurements and annual cycles is added to the caption
4-809	4	32	38			"permafrost temperatures" where? [Alan Robock, USA]	Noted and will make it at specific depth below the surface.
4-810	4	32	39	32	39	should be Table 4.6 [Vladimir Romanovsky, USA]	accepted.
4-811	4	32	39			Substitute '(Table 4.5)' by '(Table 4.6)' [Thomas Voigt, Germany]	accepted.
4-812	4	32	41	32	45	Additional information regarding permafrost conditions in the mountain permafrost in the southern Yukon (in addition to that found in Smith et al. 2010) can be found in Lewkowicz et al. 2011. Reference: Lewkowicz, A.G., Etzelmuller, B., and Smith, S.L. 2011. Characteristics of discontinuous permafrost from ground temperature measurements and electrical resistivity tomography, southern Yukon, Canada. Permafrost and Periglacial Processes, 22(4): 320-342. [Sharon Smith, Canada]	Noted and add additional information and references.
4-813	4	32	45	32	46	Change to "In these sites, permafrost..." There is so much lateral variability in mountains that one cannot conclude from some samples to the area. [Stephan Gruber, Switzerland]	Accepted
4-814	4	32	47	32	49	In some regions there have been longer periods of cooling. This has been the case for example in the eastern Canadian Arctic (e.g. norther Que and Baffin Is) where climate and permafrost temperature records have shown cooling from mid 20th century until about the mid 1990s. This was mentioned in Smith et al. (2010) which also commented that the warming of permafrost occurred later in eastern North America compared to western North America (see also Chouinard et al 2007). This was also shown in Throop et al. (2010). Additional references: Chouinard, C., Fortier, R., and Mareschal, J.C. 2007. Recent climate variations in the subarctic inferred from three borehole temperature profiles in northern Quebec, Canada. Earth and Planetary Science Letters, 263: 355-369. Throop, J.L., Smith, S.L., and Lewkowicz, A.G. 2010. Observed recent changes in climate and permafrost temperatures at four sites in northern Canada. In GEO2010, 63rd Canadian Geotechnical Conference and the 6th Canadian Permafrost Conference. Calgary, Sept 2010. GEO2010 Calgary Organizing Committee, p. 1265-1272. [Sharon Smith, Canada]	Noted and will add additional information here.
4-815	4	32	52	32	52	Be more specific about "recent trends". Only the observation during the IPY is rather short, other times need to be described. [Stephan Gruber, Switzerland]	Noted - the period of the record, from which the trends were derived will is made clear in the Table caption.
4-816	4	32	52	32	52	Table 4.6: Add data from the PERMOS network to this table. REFERENCE: Noetzli, J. Vonder Muehll, D. 2010. Permafrost in Switzerland 2006/2007 and 2007/2008, Cryospheric Commission of the Swiss Academy of Sciences. [Stephan Gruber, Switzerland]	accepted. A more detailed table will be made.
4-817	4	32	52	32	52	suggest to use word "changes" instead of "trends" because trends (deg C/year) are not shown in this table [Vladimir Romanovsky, USA]	Accepted
4-818	4	32	52	33	1	CLARIFY THE RELATION BETWEEN TEMPERATURE CHANGE AND DEPTHS LISTED [Roger Barry, USA]	Accepted - the same as earlier comment
4-819	4	32	52			Table 4-6. Since time periods are variable, it would be better for comparison between regions, to present the change as °C per decade rather than the change over the time period. There is a fair bit of variability in record length within regions and between regions - ranges from <5 years to >40 years. [Sharon Smith, Canada]	Disagree - these may be changes rather than trends in a conventional sense.
4-820	4	32	52			Table 4-5. It would be better to separate Ellesmere Island which is the high Arctic from Mackenzie Delta and	Accepted - this table is reformatted to separate types

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						northern Alaska - These are not all in the same region so it doesn't make sense to group them. Burn and Zhang 2009 should be added to the list of references for Mackenzie Delta - northern Alaska region (or western north American Arctic). Additional reference: Burn, C.R., and Zhang, Y. 2009. Permafrost and climate change at Herschel Island (Qikiqtaruq), Yukon Territory, Canada. Journal Geophysical Research, 114(F02001): 16. [Sharon Smith, Canada]	of environement.
4-821	4	32	52			Table 4-5. Allard et al 1995 could be removed as the data are updated in Smith et al. 2010. Burn and Kokelj 2009 should also be removed as they do not discuss the Mackenzie Valley. There may be more recent Osterkamp references for interior Alaska that are relevant (possibly 2008 Int. Permafrost Conf Proc.?). It would also make sense to discuss northern Quebec separately because it is in eastern Canada and onditions are quite different here as warming has occurred later than in western North America. [Sharon Smith, Canada]	Disagree - the Allard refernce is an important one, and contains the majority of the data. B&K, 2009 has data about N. Alaska.
4-822	4	32		33		Table 4.6: How does the huge variation in depth of the measurements affect the interpretation? [Alan Robock, USA]	will take into account, add more text to explain
4-823	4	32		33		Table 4.6: Does the water content make a difference? [Alan Robock, USA]	In theory, yes, but we are talking about permafrost soils here, not the active layer.
4-824	4	33	3	33	3	"in response to" is preferable to "cause [Sharon Smith, Canada]	accepted.
4-825	4	33	3	33	5	Note that Smith et al (2010) and Romanovsky et al (2010a) also stated that these higher rates of permafrost temperature increase were in cold permafrost in the tundra regions or in bedrock or low ice soils. The lack of a surface buffer layer and the high thermal conductivity of the earth materials results in temperature changes propagating in the ground at a higher rate than in forested areas or in ice-rich soils. [Sharon Smith, Canada]	Noted, but his is consistent with the text here.
4-826	4	33	5	33	7	The sentence should be revised (see comments above) to: In forested areas with warm permafrost (especially if ice-rich), changes in permafrost temperature are relatively small due to the significant surface buffer layer and the effects of latent heat (Isaksen et al. 2011; Romanovsky et al. 2010a). [Sharon Smith, Canada]	Accepted, will modify accordingly.
4-827	4	33	9			Section 4.6.2.2. - It is unclear how a discussion of permafrost degradation can occur before consideration of changes in active layer thickness and increasing thaw depths. [Sharon Smith, Canada]	Noted, the active layer is considered as seasonally frozen ground in this version. Will consider to separate the active layer over permafrost and seasonally frozen ground in non-permafrost occupied area.
4-828	4	33	9			Section 4.6.2.2 - There are a number of recent Canadian references that could also be added, a number of which are summarized in Smith (2011): Smith, S. 2011. Trends in permafrost conditions and ecology in northern Canada, Report Canadian Biodiversity: Ecosystem Status and Trends 2010 Technical Thematic Report No. 9 Published by the Canadian Councils of Resource Ministers. http://www.biodivcanada.ca/default.asp?lang=En&n=137E1147-1 Fortier, R. and Aubé-Maurice, B. 2008. Fast permafrost degradation near Umiujaq in Nunavik (Canada) since 1957 assessed from time-lapse aerial and satellite photographs. In Proceedings of the 9th International Conference on Permafrost. Fairbanks, AK, 29 June to 3 July, 2008. Edited by Kane, D.L. and Hinkel, K.M. Institute of Northern Engineering, University of Alaska Fairbanks. Vol. 1, pp. 457-462. Vallee, S. and Payette, S. 2007. Collapse of permafrost mounds along a subarctic river over the last 100 years (northern Quebec). Geomorphology 90:162-170. Smith, S.L., Burgess, M.M., and Riseborough, D.W. 2008. Ground temperature and thaw settlement in frozen peatlands along the Norman Wells pipeline corridor, NWT Canada: 22 years of monitoring. In Ninth International Conference on Permafrost. Edited by D.L. Kane and K.M. Hinkel. Fairbanks Alaska. Institute of Northern Engineering, University of Alaska Fairbanks, Vol.2, pp. 1665-1670. [Sharon Smith, Canada]	Noted. However, IPCC Assessment Report is assessment, not a review. Not all papers will be cited as long as all major findings are covered.
4-829	4	33	13			"within permafrost" vertically or horizontally? [Alan Robock, USA]	Noted: usually refers to vertically.
4-830	4	33				Table 4.6: suggest to use the range 0 - 2.0 (instead of 0.3 - 2.0) as Permafrost Temperature Change for Interior Alaska, Mackenzie Valley, and Northern Quebec [Vladimir Romanovsky, USA]	table 4.6 will be expanded with more details. Will consider the suggestions here.
4-831	4	33				Table 4.6	Accepted and will add more information. Table 4.6 will

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						Add values for Europe, for example from Harris et a. (2009) or from references therein. Reference: Harris, C., Arenson, L.U., Christiansen, H.H., Etzelmüller, B., Frauenfelder, R., Gruber, S., Haeberli, W., Hauck, C., Hoelzle, M., Humlum, O., Isaksen, K., Kääb, A., Kern-Lütschg. M.A., Lehning, M., Matsuoka, N., Murton, J.B., Nötzli, J., Phillips, M., Ross, N., Seppälä, M., Springman, S.M. and Vonder Mühll, D. (2009): Permafrost and climate in Europe: Monitoring and modelling thermal, geomorphological and geotechnical responses. Earth Science Reviews 92: 117-171. [Michael Zemp, Switzerland]	be expanded.
4-832	4	34	1	34	1	Coastal areas are dynamic environments and coastal erosion is a natural process. Retreat of coasts can not always be attributed to permafrost thaw as there are other triggers which include increased wave energy and storminess (may be associated with reduced sea ice and more open water). It is not a robust indicator of climate warming and is a less direct indicator of climate warming than the key permafrost indicators identified by WMO. [Sharon Smith, Canada]	Agreed - sentence modified to say Permafrost degradation has acelerated erosion.
4-833	4	34	3	34	3	suggest to change "…immidiately degrades…" to "...immidiately starts to degrade…" [Vladimir Romanovsky, USA]	accepted.
4-834	4	34	5		9	"…Subsea permafrost degradation rates…." could be removed to section "4.6.3 Subsea Permafrost" [Thomas Voigt, Germany]	Accepted - all statements on subsea permafrost are moved to the section 4.6.3
4-835	4	34	7	34	9	Kokelj et al (2009) should also be consulted. Slumping around lakes may progress as the vegetation cover is disturbed by the initial slump. Removal of the vegetation mat results in surface warming and thawing of ice-rich material. This may occur even if there is no change in climate but is a change in micro climate. The initial trigger may be expansion of the talik. Kokelj, S.V., Lantz, T.C., Kanigan, J., Smith, S.L., and Coutts, R. 2009. Origin and polycyclic behaviour of tundra thaw slumps, Mackenzie Delta region, Northwest Territories, Canada. Permafrost and Periglacial Processes, 20(2): 173-184. [Sharon Smith, Canada]	Noted and will consider.
4-836	4	34	8	34	9	It has to be mentioned that these numbers were derived from the results of numerical modelig. No measured data are available for the current rate of permafrost thawing under lakes [Vladimir Romanovsky, USA]	Accepted - the model results described by Ling et al. are deleted.
4-837	4	34	11			define "rock glaciers" [Alan Robock, USA]	Accepted - this is added
4-838	4	34	17	34	17	spelling: Kääb, not Kaab [Martin Lüthi, Switzerland]	accepted.
4-839	4	34	22	34	23	Change to "Exposed ice and event statistics (Gruber and Haeberli, 2007) as well as the emerging understanding of the link between warming and destabilization (Hasler et al. 2011, 2012) support the hypothesis that this is in part due to thaw of permafrost on steep slopes." Two key processes supporting this are now understood better. REFERENCE 1: Hasler, A., Gruber, S. & Beutel, J. (in press) Kinematics of steep bedrock permafrost, Journal of Geophysical Research, doi:10.1029/2011JF001981. REFERENCE 2: Hasler, A., Gruber, S., Font, M. & Dubois, A. (2011): Advective heat transport in frozen rock clefts – conceptual model, laboratory experiments and numerical simulation, Permafrost and Periglacial Processes, 22, 378–389, doi: 10.1002/ppp.737. [Stephan Gruber, Switzerland]	accepted and will add more here.
4-840	4	34	25			Section 4.6.3 - Subsea permafrost is similar to terrestrial permafrost in the shelf areas because it was essentially formed under terrestrial condtions. This is however not the case for subsea permafrost in deeper water. Changes in subsea permafrost temperature in coastal shelf areas is not a good indicator of climate change as the warming is occurring due to innundation by the warmer sea water, i.e. surface temperature increases from <-10°C at time of permafrost formation to near 0°C as the area is flooded. The permafrost is warming to adjust to new surface temperatures not a change in climate. This does not reflect more recent climate change and increasing air temperatures. It is suggested that the subsea permafrost not be included in a chapter that is to discuss indicators of climate change. Since this paragraph largely focusses on changes in sea water temperature it is more appropriately discussed in a chapter on oceans. [Sharon Smith, Canada]	Disagree. It is not intended to use changes in subsea permafrost temperature as an indicator of climate change, rather, it is intended to discuss how permafrost degrades under seawater regardless of its cause. No statement of attribution is intended
4-841	4	34	36	34	36	Howat et al., 2011 (GRL) could be included [Jan Lenaerts, The Netherlands]	Noted.

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4-842	4	34	36	34	45	Does this discussion focus more on impacts of climate change rather than indicator of climate change - better placed in WG2? [Sharon Smith, Canada]	Noted.
4-843	4	34	39	34	39	Not Romanofskii, but Romanovsky [Hiroyuki Enomoto, Japan]	accepted.
4-844	4	34	43			"Arctic" needs to be capitalized here and in rest of chapter. [Alan Robock, USA]	accepted.
4-845	4	34	52			4.6.4.1 Changes in Active-Layer Thickness. The depth of the permafrost table (100-200 cm) is essentianl for the classification of the Cryosols and Gelisols according to the World Reference Base for Soil Resources and Soil Taxonomy soil classification systems, respectively. Therefore any changes in the active layer thickness could strongly influence the percentage area occupied by permafrost soils on a global level according to these widespread classification systems. References: IUSS Working Group WRB. 2006. World reference base for soil resources 2006. 2nd edition. World Soil Resources Reports No. 103. FAO, Rome; Soil Survey Staff. 2010. Keys to Soil Taxonomy, 11th ed. USDA-Natural Resources Conservation Service, Washington, DC. [Michele Freppaz, Italy]	noted. Will consider in new text.
4-846	4	34	55	34	56	Figure 4.23 shows an increase in active layer thickness (ALT) only for the European North. Generalized statement about the increase of ALT for the entire "discontinuous permafrost regions in high latitudes" is not supported by the included in this report data [Vladimir Romanovsky, USA]	Noted and will make changes accordingly.
4-847	4	34	55	34	56	Statement is incorrect as a strong postive trend in ALT has not been observed in all regions (see for e.g. Smith et al. 2009) [Sharon Smith, Canada]	Noted and will make changes accordingly. See 4-846.
4-848	4	35	3	35	5	Wrong use of the reference (Viereck et al., 2008). This paper discusses the dynamic of permafrost after a forest fire. The data on the active layer thickness in undisturbed conditions included in this paper clearly show that there is no trend in ALT at this site for the period between 1971 and 2006 [Vladimir Romanovsky, USA]	Noted and will carefully check with the reference.
4-849	4	35	3			Delete '(Figure 1)' or substitute it by '(Figure 4.23)' [Thomas Voigt, Germany]	accepted.
4-850	4	35	4	35	4	What is Figure 1? [Vladimir Romanovsky, USA]	noted.
4-851	4	35	5	35	5	Note that Burn and Kokelj (2009) only refer to one particular area and this is not necessarily the case everywhere in this region (see Smith et al. 2009). Note that there is a great deal of interannual in the example that is cited. [Sharon Smith, Canada]	Noted.
4-852	4	35	14			Change to "In the locations measured, ALT...". No inference can be made on entire regions. [Stephan Gruber, Switzerland]	Accepted.
4-853	4	35	15	35	15	Sharkhuu et al., 2007 report 40cm per decade, not per year; but Zhao et al. 2010 report 40 cm per year at one Mongolian site where the mean annual ground temperature was close to 0°C. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Noted and will check with references.
4-854	4	35	24	35	25	Change “Low rates or no change in ALT increase” to Low rates of increase or no change in ALT” and change “on the Qinghai–Tibetan Plateau” to “in Mongolia” to agree with Zhao et al., 2010. [David Parker, United Kingdom of Great Britain & Northern Ireland]	accepted.
4-855	4	35	27	35	27	See Smith et al (2008) regarding subsidence in peatlands. Smith, S.L., Burgess, M.M., and Riseborough, D.W. 2008. Ground temperature and thaw settlement in frozen peatlands along the Norman Wells pipeline corridor, NWT Canada: 22 years of monitoring. In Ninth International Conference on Permafrost. Edited by D.L. Kane and K.M. Hinkel. Fairbanks Alaska. Institute of Northern Engineering, University of Alaska Fairbanks, Vol.2, pp. 1665-1670. [Sharon Smith, Canada]	Noted and will check with the references provided.
4-856	4	35	29	35	31	Change “6-15cm” to “4-10cm” because the rates of 2 to 5 cm/yr cited by Overduin and Kane (2006) span a 2-year interval not a 3-year interval. Streletskiy et al. (2008) report 12-13cm subsidence but I was unable to access Mazhitova & Kaverin (2007). [David Parker, United Kingdom of Great Britain & Northern Ireland]	will check with the reference to make correct numbers.
4-857	4	35	33	35	37	Note that the importance of settlement in explaining reduced changes in ALT was pointed out by Canadians several years ago. See Smith et al. (2001) and Atkinson et al (2006) (subsidence also discussed in Smith et al	Noted. However, we cannot list all of references.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						2009). It is therefore important to distinguish between thaw penetration measured from a fixed reference and ALT. References: Atkinson, D.E., Brown, R., Alt, B., Agnew, T., Bourgeois, J., Burgess, M., Duguay, C., Henry, G., Jeffers, S., Koerner, R., Lewkowicz, A.G., McCourt, S., Melling, H., Sharp, M., Smith, S., Walker, A., Wilson, K., Wolfe, S., Woo, M.-k., and Young, K. 2006. Canadian cryospheric response to an anomalous warm summer: a synthesis of the Climate Change Action Fund Project "The state of the Arctic Cryosphere during the extreme warm summer of 1998". Atmosphere-Ocean, 44(4): 347-375. Smith, S.L., Burgess, M.M., and Nixon, F.M. 2001. Response of active-layer and permafrost temperatures to warming during 1998 in the Mackenzie Delta, Northwest Territories and at Canadian Forces Station Alert and Baker Lake, Nunavut, Report Current Research 1001-E5. Other earlier refs include: Nixon, M., Tarnocai, C., and Kutny, L. 2003. Long-term active layer monitoring: Mackenzie Valley, northwest Canada. In Proceedings of the 8th International Conference on Permafrost. Edited by M. Phillips, S.M. Springman, and L.U. Arenson. Zurich Switzerland. July 2003. A.A. Balkema, Lisse, the Netherlands pp. 821-826. Tarnocai, C., Nixon, F.M., and Kutny, L. 2004. Circumpolar-Active-Layer-Monitoring (CALM) Sites in the Mackenzie Valley, Northwestern Canada. Permafrost and Periglacial Processes, 15: 141-153. [Sharon Smith, Canada]	
4-858	4	35	33	35	37	Note that other process can result in changes of surface elevation so the observed deformation (from InSAR) may not necessarily result from only changes in permafrost/active layer conditions [Sharon Smith, Canada]	Noted - this has been conisdered.
4-859	4	35	42			Substitute '(Figure 3)' by '(Figure 4.24)' [Thomas Voigt, Germany]	accepted.
4-860	4	36	1	37	31	FAQ 4.1: This FAQ is clearly written in langauge which I think will be understandable to a general non-specialist reader, and is appropriately formatted. I have no modifications to suggest. [David Wratt, New Zealand]	Noted
4-861	4	36	5	36	11	The numbers of disappeared and disappearing glaciers stand out as weakly supported by research, due to the lack of references. Also, it is often debatable whether a small ice mass is a glacier or a snowfield. To overcome this weakness, I suggest that a sentence on disappered glaciers is added to section 4.3.1 Background (page 15, lines 20-28) with supporting references to peer-reviewed literature. [Jacob Clement Yde, Norway]	Taken into account: As citations are not allowed in a FAQ, we will add references int he main section.
4-862	4	36	6	36	7	The disappearance of more than one hundered glaciers in the Swiss Alps is not explicitly shown in this chapter. Please reference! [Christoph Marty, Switzerland]	Taken into account: References will be added in the main text.
4-863	4	36	7			FAQ 4.1: Why only until 1970? This is 40 years ago… [Nadine Salzmann, Swizerland]	Noted: The number refers to a citable study (that is now added in the main text).
4-864	4	36	13	36	14	It is dangerous to compare glaciation to 1850, since this was a Holocene extreme glacier extent. Rather speak of rapid recent glacier changes. [Martin Lüthi, Switzerland]	Taken into ccount: Text will be revised
4-865	4	36	18	36	21	The region Karakoram, and western Himalaya (North India) should not be included in this general statement. Indeed, there is clear evidence that glaciers started to shrink after 2000 in western Himalaya (so during the last decade, and not during the last two decades) [Azam et al, In press]. There is also confirmation of the so called “Karakoram anomaly”, where glaciers have been in steady state or even advancing during the last decade [e.g. Hewitt, 2005; Gardelle at al., Submitted]. Azam, F. M., P. Wagnon, A. Ramanathan, C. Vincent, P. Sharma, Y. Arnaud, A. Linda, J. G. Pottakkal, P. Chevallier, V. B. Singh, E. Berthier, From balance to imbalance: a shift in the dynamical behaviour of Chhota Shigri Glacier (Western Himalaya, India), J. Glaciol., In Press Gardelle, J, E. Berthier, and Y. Arnaud, Karakoram glaciers slightly gained mass in the early 21st century, Nat. Geosc. Submitted. Hewitt, K., The Karakoram anomaly? Glacier expansion and the ‘elevation effect’, Karakoram Himalaya, Mountain Res. Dev., 25(4), 332-340, 2005. [Patrick Wagnon, France]	Noted
4-866	4	36	27	36	28	"have fully adjusted their extent to the present climate." This is not how it works. The present climate is changing, so how can the glaciers adjust to it? [Alan Robock, USA]	Rejected: The present climate is as it is. It might change in the future.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4-867	4	36	42	36	42	The increase varies largely between glaciers/regions/climates. This should be reformulated (weakened or a range given). [Regine Hock, US]	Taken into account: Text will be modified accordingly.
4-868	4	36	42			change "degree" to "Celsius degree" or "Kelvin" You need to include the units. [Alan Robock, USA]	Editorial
4-869	4	36	55	36	55	Delete the second 'have'. [Jacob Clement Yde, Norway]	Editorial
4-870	4	36				FAQ 4.1: The answer for this FAQ needs to be much shorter and clearer. Also, there is not one single reference in the whole FAQ! [Nadine Salzmann, Swizerland]	Noted
4-871	4	36				FAQ 4.1: The closing paragraph states that the fate of glaciers will be ....dependent on their specific characteristics. While this is true, it misses the importance of the external driving factors, i.e., temperature changes, precipitation changes, which are scenario depended. Please avoid the impression that the fate of glaciers only depends on the glacier dynamics. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account: The text will be modified accordingly.
4-872	4	36				FAQ 4.1: ELA should be introduced explicitly in a separate sentence as it is a central component to this FAQ. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account: The text will be revised accordingly.
4-873	4	36				FAQ 4.1: It would be interesting to include some brief perspective from the Paleo-record - eg, have the glaciers disappeared in the past? (e.g., recent results on Kilimanjaro) [Thomas Stocker/ WGI TSU, Switzerland]	Rejected: Paleo is not covered here, limited observational evidence, text would get too long.
4-874	4	36				FAQ 4.1, Fig 1: For an FAQ, 'Cirque glacier' is not a term the reader will be familiar with. Define in text or caption, or use other terminology. To aid the visual interpretation, we suggest that the blueish shade is consistently used to indicate the ice below the ELA, with white above, i.e., visually distinguishing the ablation and accumulation areas. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account: The text and the figure will be modified accordingly.
4-875	4	36				FAQ 4.1: Are glaciers in mountain regions disappearing? I think that this is a good topic for a glacier-related FAQ (and a good opportunity to correct IPCC AR4-statements on projected glacier changes in the Himalaya region). However, the present section is much too long and comes without any references! Suggestions: - shorten to about half a page and give reference to publications supporting the statements and numbers given, such as: - line 7: Maisch et al. (2000) => see reference list of ZOD - line 9: WGMS (2008, and earlier issues): Fluctuations of Glaciers 2000-2005 (Vol. IX). Haeberli, W., Zemp, M., Kääb, A., Paul, F. and Hoelzle, M. (eds.), ICSU (FAGS) / IUGG (IACS) / UNEP / UNESCO / WMO, World Glacier Monitoring Service, Zurich, Switzerland: 266 pp. - line 23: Bahr et al. (2009) => see FOD reference list; WGMS (20011): Glacier Mass Balance Bulletin No. 11 (2008-2009). Zemp, M., Nussbaumer, S.U., Gärtner-Roer, I., Hoelzle, M., Paul, F. and Haeberli, W. (eds.), ICSU (WDS) / IUGG (IACS) / UNEP / UNESCO / WMO, World Glacier Monitoring Service, Zurich, Switzerland: 102 pp. - line 40: there is an increasing number of related modelling studies for entire mountain ranges (e.g. European Alps, Norway) - for the future of glaciers in the Himalaya see: - Cogley, J. G., Kargel, J. S., Kaser, G., & Van Der Veen, C. J. (2010). Tracking the source of glacier misinformation. Science, 327, 522. - Fujita, K., & Nuimura, T. (2011). Spatially heterogeneous wastage of Himalayan glaciers. Proceedings of the National Academy of Sciences, 108(34). doi:10.1073/pnas.1106242108 - Bolch, T., Kulkarni, A., Kääb, A., Huggel, C., Paul, F., Cogley, G., Frey, H., Kargel, J.S., Fujita, K., Scheel, M., Bajracharya, S. and Stoffel, M. (in revision): The State and Fate of Himalayan Glaciers. Science. [Michael Zemp, Switzerland]	Rejected: Citations are not allowed in a FAQ
4-876	4	37	8	37	8	Insert ', surging' after 'covered'. [Jacob Clement Yde, Norway]	Editorial

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4-877	4	37	22			It could be mentioned that some glaciers that have retreated noticeably in the last years are currently located in very favourable locations of elevation, wind blowing, feeded by avalanches and sheltered from solar radiation. In the future they can evolve much slower independently of the climatic conditions. Ref. López-Moreno, J. I., Nogués-Bravo, D., Chueca-Cía, J. and Julián-Andrés, J. (2006), Change of topographic control on the extent of cirque glaciers since the Little Ice Age, Geophysical Research Letters, 33, L24505, doi:10.1029/2006GL028204. [Juan Ignacio López Moreno, Spain]	Rejected: These special conditions are mentioned in 4-36:34-35.
4-878	4	37	28			change "above" to "north of" [Alan Robock, USA]	Editorial
4-879	4	37	34	38	51	FAQ 4.2: This FAQ is clearly written in langauge which I think will be understandable to a general non-specialist reader, and is appropriately formatted. I have no modifications to suggest. [David Wratt, New Zealand]	No change requested
4-880	4	37	44			"increased slightly (1.3 % per decade)," Please state whether this is a significant increase. Or is steady-state a better way to say it? [Alan Robock, USA]	Uncertainty language added indicating that trend is "statistically significant"
4-881	4	37	46			change "it" to "its" [Alan Robock, USA]	Accepted.
4-882	4	37				FAQ 4.2: A note of caution regarding the final sentence of the FAQ - "Strong statements" -> should be written as 'robust statements'. [Thomas Stocker/ WGI TSU, Switzerland]	Revised to use uncertainty language
4-883	4	37				FAQ 4.2, Fig 1: The plotted trends should be the main focus of this figure, but currently they are overwhelmed by other detail. Suggest these are repositioned as large plots below each map. Consider simplification of maps so that a minimum number of arrows are used to indicate the main circulation patterns, and give emphasis to the %change numbers. [Thomas Stocker/ WGI TSU, Switzerland]	Diagram revised
4-884	4	38	3	38	4	The temporal coverage of the satellite data and spatial coverage of the submarine data limits the certainlty that can be placed on a claim that the thickness of the ice and violume are decreasing. I would say that the data "suggest" rather than "show" that ice thickness and volume have decreased, or alternatively one could say that ice thickness and volume are decreasing over a large part of the Arctic. [Seymour Laxon, UK]	Accepted
4-885	4	38	10	38	10	I suggest that "below 78S" to most laypeople means "south of 78S" whereas in this sentence, it appears to mean "north of 78S". I'd suggest either using "above" instead of "below", or using "north of". [Marcus Sarofim, USA]	accepted
4-886	4	38	10			change "below" to "north of" [Alan Robock, USA]	Check - Near the coast sea ice drift is predominatlu east to west
4-887	4	38	13	38	17	This part gives to me the false feeling that snow-ice is the dominant contributor of sea ice formation while to my knowledge it is between 10 and 25% at most. [Hugues Goosse, Belgium]	Accepted. Altered wording to avoid this perception. Amount of snow-ice is not well enough known to give a value here.
4-888	4	38	28	38	30	Page 4-38, lines 28-30. These can be omitted, as this has been mentioned in the previous paragraph. [Richard Bintanja, Netherlands]	Accepted. Text revised.
4-889	4	38	32	38	32	"small increase": Small relative to what? I'd suggest "small relative to natural variability", or, perhaps, delete the word "small" entirely. [Marcus Sarofim, USA]	Altered - Recommend "small but statistically significant" NOT "smaller"
4-890	4	38	34	38	34	According to FAQ 4.2, Figure 1 there is an increase in sea ice extent in the Indian Ocean sector as well as in the Ross Sea: together these just outweigh the decrease in the Bellingshausen and Amundsen Seas. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Rejected.
4-891	4	38	57	39	3	These lines talk about the extent of Arctic summer sea ice cover. It is not clear what is declining in line 2. To clarify and make a better distinction, it might help if that part of line 2 reads, "will likely accelerate the decline of the permanent summer sea ice cover and seasonal sea ice will". [Richard Heim, U.S.A.]	Noted, but the Synthesis section has been completely revised making this comment obsolete
4-892	4	38				Section 4.7: There is no singel reference in the whole section, agian. [Nadine Salzmann, Swizerland]	Rejected - none of the FAQ texts will appear with references

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4-893	4	39	2	39	2	Replace "will likely" with "may'. There are also negative feedbacks for example thin ice will grow more quickly than thick ice under the same forcing conditions. The fact that the ice cover recovered somewhat from the minimum in 2007 is an indication that negative feedbacks may also play a role. [Seymour Laxon, UK]	Noted, but the Synthesis section has been completely revised making this comment obsolete
4-894	4	39	2			"ice albedo feedback effects will likely accelerate the decline" This is an attribution statement and has no business being here. It is not supported by any of the text of the chapter. Furthermore, it is wrong. Forcing causes changes - not feedbacks. Also, feedbacks work in both directions. So it is global warming that is accelerating the decline. [Alan Robock, USA]	Noted, but the Synthesis section has been completely revised making this comment obsolete
4-895	4	39	2			"ice albedo feedback effects will likely accelerate the decline" Are you sure the decline is accelerating or just that it will continue? I think you mean that the linear trend will continue, and not that it will accelarate. In any case, it is wrong, as stated in the comment above. [Alan Robock, USA]	Noted, but the Synthesis section has been completely revised making this comment obsolete
4-896	4	39	2			"ice albedo feedback effects will likely accelerate the decline" Furthermore, the ice albedo feedback is only one of the processes involved in determining the rate of sea ice change. And the albedo feedback is only important in the spring and summer when there is sunlight. The melting starts long before that. [Alan Robock, USA]	Noted, but the Synthesis section has been completely revised making this comment obsolete
4-897	4	39	3	39	3	Change 'volume' to 'mass'. [Jacob Clement Yde, Norway]	Noted, but the Synthesis section has been completely revised making this comment obsolete
4-898	4	39	3	39	3	Delete 'mountain', as this is also relevant for other types of glaciers. [Jacob Clement Yde, Norway]	Noted, but the Synthesis section has been completely revised making this comment obsolete
4-899	4	39	7	39	8	There are comlex of sensor name and satellite name. [Hiroyuki Enomoto, Japan]	Noted, but the Synthesis section has been completely revised making this comment obsolete
4-900	4	39	9	39	9	The evidence does not really point to a large fraction of of the permafrost thawing. The evidence does show warming of permafrost in all regions. [Sharon Smith, Canada]	Noted, but the Synthesis section has been completely revised making this comment obsolete
4-901	4	39	12	39	31	When addressing knowledge gaps, it could be mentioned that in the Arctic new scenarios occur, where no comparable data from earlier observations exist. The ratio of first-year ice vs. multiyear ice is changing, we see now open water at times and in regions (and latitudes) that used to be ice covered. Different ice types have different properties, and atmospheric forcings are different for different regions. [Sebastian Gerland, Norway]	Noted, but the Synthesis section has been completely revised making this comment obsolete
4-902	4	39	12	39	31	When addressing knowledge gaps, it could be mentioned that in the Arctic new scenarios occur, where no comparable data from earlier observations exist. The ratio of first-year ice vs. multiyear ice is changing, we see now open water at times and in regions (and latitudes) that used to be ice covered. Different ice types have different properties, and atmospheric forcings are different for different regions. [Sebastian Gerland, Norway]	Noted, but the Synthesis section has been completely revised making this comment obsolete
4-903	4	39	13	39	13	Excellent point and very well expressed. [Richard Heim, U.S.A.]	Noted, but the Synthesis section has been completely revised making this comment obsolete
4-904	4	39	15	39	15	better: “that are not completely understood”. [Martin Lüthi, Switzerland]	Noted, but the Synthesis section has been completely revised making this comment obsolete
4-905	4	39	15	39	19	It is good that this incomplete understanding is noted, and that the Antarctic sea ice cover and surrounding ocean temperature trends are mentioned as an example of the need for further study. [Richard Heim, U.S.A.]	Noted, but the Synthesis section has been completely revised making this comment obsolete
4-906	4	39	16			"is increasing" Please state whether this is a significant increase. Or is steady-state a better way to say it? [Alan Robock, USA]	Noted, but the Synthesis section has been completely revised making this comment obsolete
4-907	4	39	19	39	19	Delete 'mountain', as this is also relevant for other types of glaciers. [Jacob Clement Yde, Norway]	Noted, but the Synthesis section has been completely revised making this comment obsolete
4-908	4	39	19	39	31	The chapter should not end with a statement about how hard the work is. It sounds like complaining and making excuses. If this is an issue, put it in a section in the chapter, but don't make it a conclusion. [Alan	Noted, but the Synthesis section has been completely revised making this comment obsolete

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						Robock, USA]	
4-909	4	39	20	39	21	"Much of the glacier change data are available only in local and non-English publications." This is really an offensive statement. First of all, it implies that if the data are local or non-English they are somehow inferior. Second, it implies that English-speaking scientists are too lazy to extract the data with the aid of those who can speak the language. It sounds like making excuses. [Alan Robock, USA]	Noted, but the Synthesis section has been completely revised making this comment obsolete
4-910	4	39	21	39	21	The lack of English publications is presented as a drawback. This is a very anglo-saxon point of view, and fits badly into a report that claims to be the effort of an international group of scientists. Extracting data from these sources should be a minor effort, that can be easily accomplished by local scientists. [Martin Lüthi, Switzerland]	Noted, but the Synthesis section has been completely revised making this comment obsolete
4-911	4	39	23	39	26	It's not only the observation and understanding of snow thickness and density, but also to estimate the snow liquid water content/dielectric properties of dry and wet snow. [Luzi Bernhard, Switzerland]	Noted, but the Synthesis section has been completely revised making this comment obsolete
4-912	4	39	25	39	26	"Regional changes in snow cover can be difficult to interpret since regionally specific factors can dominate." I don't understand what this means. Why can't we understand the regional factors? [Alan Robock, USA]	Noted, but the Synthesis section has been completely revised making this comment obsolete
4-913	4	39	28			Change "Such issue" to "Such an issue" [Alan Robock, USA]	Noted, but the Synthesis section has been completely revised making this comment obsolete
4-914	4	39	30			Change "they" to "it" [Alan Robock, USA]	Noted, but the Synthesis section has been completely revised making this comment obsolete
4-915	4	40				The reference list needs some work. Some are duplicated for example (Kaser et al., 2006 or Cogley et al., 2009). [Etienne BERTHIER, France]	Accepted
4-916	4	41	58	41	61	Duplicated references Cogley 2009a nd 2009c [Hiroyuki Enomoto, Japan]	Accepted
4-917	4	42	46	42	46	correct the reference (I am a native Chinese and also know this Chinese name "E"). It should be "E, D.-C., Y.-D. Yang, and D.-B. Chao, 2009: The sea level change from the Antarctic ice sheet based on GRACE. Chinese J. Geophys. (in Chinese), 52 (9), 2222-2228." [Zeng-Zhen HU, USA]	Accepted
4-918	4	44	11	44	11	This reference contains the first names, not the family names, of the four Chinese authors (see also P16 L29) [J. Graham Cogley, Canada]	Accepted
4-919	4	45	5	45	9	spelling: Kääb, not Kaab [Martin Lüthi, Switzerland]	Accepted
4-920	4	45	9	45	11	Kaser et al (2006a) and (2006b) refer to the same paper [Patrick Wagnon, France]	Accepted
4-921	4	45	9	45	12	Double entry of Kaser et al. (2006) [Michael Zemp, Switzerland]	Accepted
4-922	4	46	25	46	25	spelling: Kääb, not Kaab [Martin Lüthi, Switzerland]	Accepted
4-923	4	47	4			upper right world map should be removed [Regine Hock, US]	Reject - (Refers to page 67 not 4). We prefer to retain this useful graphical reminder to the areas discussed.
4-924	4	49	62	49	62	I assume ', Earth' should be deleted from the title. [Jacob Clement Yde, Norway]	Accepted
4-925	4	51	54	51	54	Citation incomplete. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted
4-926	4	52	9	52	11	This citation is incorrect. It should be: Ettema, J. , M.R. van den Broeke, E. van Meijgaard, and W. J. van de Berg, 2010: Climate of the Greenland ice sheet using a high-resolution climate model - Part 2: Near-surface climate and energy balance. The Cryosphere, 4, 529-544. [Jan Lenaerts, The Netherlands]	Accepted
4-927	4	52	45			Citation of WGMS datasets: WGMS citation recommendations (as specified above) provide versioning and lineage of the dataset which	Reference altered, but may include multiple years in single reference.

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						assures scientific reproducibility; and has been designed in accordance with IUGG(IACS) and ICSU(WDS). Lumping these citations together into "various years" cuts the versionning off and introduces ambiguity, especially in the case of glacier fluctuation datasets which are often subject to corrected and re-analysed data series. Recommendation: individually cite (WGMS) datasets and versions as proposed in the remarks above. [Michael Zemp, Switzerland]	
4-928	4	52	63	52	63	add "D13108, doi:10.1029/2007JD009539." [Zeng-Zhen HU, USA]	Accepted
4-929	4	53	19	53	19	spelling: Zumbühl, not Zumbhul [Martin Lüthi, Switzerland]	Accepted
4-930	4	53	20	53	20	Insert missing journal name. [Jacob Clement Yde, Norway]	Accepted
4-931	4	54	1	54	1	Delete 'Polar' (according to the applied terminology; page 6, line 15-16). [Jacob Clement Yde, Norway]	Accepted - text changed
4-932	4	54	1			Mass loss is computed by averaging published mass loss, but each study is given a weight according to reliability. This is problematic because highly subjective. Different scientistis would come up with very different ratings. It seems like reliability correlates with the studies published by lead authors or co-workers. Due to the subjective nature such weighting should not be done. [Regine Hock, US]	Accepted. Weighted averages have been removed because of perceived bias. The description of how Fig 4.15 was derived has been moved from the "appendix" to the main text
4-933	4	54				Appendix 4.A, Table 1: The paper by Mernild et al. (2009. Greenland Ice Sheet surface mass-balance modelling and freshwater flux for 2007, and in a 1995-2007 perspective. Hydrological Processes, 23, 2470-2484) has been overlooked. It contains a calculation of ice loss from Greenland of -265 km3/yr for the period 1995-2007. This result should be implimented in the calculations of the cumulative sea-level rise (Figure 4.15) and the paper should be added to Appendix 4.A, Table 1. [Jacob Clement Yde, Norway]	Accepted. Reference has been added and results included in the Greenland average
4-934	4	55	3			why did you present the sources that not to be used for calculation ice loss from greenland and also you put a comment for your work. You indirectly did not confirm the article. I don't suppose this responsibilty of IPCC. You may choose another way for presenting your work. [Fatemeh Rahimzadeh, Iran, Islamic Republic of]	Noted. Many ice sheet mass balance assessments that were used in AR4 have been superseded by later studies using longer-term data. We acknowledege the earlier studies (by listing them in an "appendix"), but use only the most recent estimates in this AR5 assessment.
4-935	4	56	3	56	3	Dong-Chen et al., 2009 to E et al. 2009" [Zeng-Zhen HU, USA]	Accepted - Endnote Web finds such references very difficult to handle
4-936	4	56	6			same as comment no. 27 [Fatemeh Rahimzadeh, Iran, Islamic Republic of]	Noted. See response to 4-934.
4-937	4	59	1	59	2	Brooks Range in the northjan Alaska may not have such glacier area, Siberia has more. [Hiroyuki Enomoto, Japan]	Noted, but no evidence supplied to support this statement
4-938	4	59		59		The "Sea ice 30 Yr Ave Extent" seems to be line in the figure 4.1, and the "Snow Extent Line" seems to be plogon in the figure, however, they are different symbols in the legend panel. I Suggest that the symbol of "Sea ice 30 Yr Ave Extent" changed as line, and the "Snow Extent Line" changed as "Snow Extent" [Yongjian Ding, China]	Accept. Figure will be modified
4-939	4	59		59		Fig 1: snow-extent line does not show up well [Robert Thomas, USA]	Accept. Revised
4-940	4	59				Fig 4.1: Legend: the white color (Snow Extent Line) is bad visible [Luzi Bernhard, Switzerland]	Noted
4-941	4	59				The coloring of Fig. 4.1 is totally unclear. [Richard Bintanja, Netherlands]	Noted - figure has been revised
4-942	4	59				Figure 4.1: The northern hemisphere terrestrial snow line is illegible. [Chris Derksen, Canada]	Accept. Revised
4-943	4	59				Figure 4.1: This needs to be augmented with a third view showing at least the Asian mountain ranges. This is	Accept. Revised

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						important because many people live there and, because it provides an exemplar that cryosphere is not anexclusive polar phenomenon. [Stephan Gruber, Switzerland]	
4-944	4	59				Figure 4.1: Sharp permafrost boundaries mis-communicate ist true nature. This *IS* important because this perception prevents the understanding of scaling conflicts in e.g. continental-scale model assessments. [Stephan Gruber, Switzerland]	Accept. Revised
4-945	4	59				Fig. 4.1: Colors are very similar and hard to distinguish. Use colors with much more contrast. [Alan Robock, USA]	Accept. Revised
4-946	4	59				Figures general: include an indication of uncertainty where possible. [Thomas Stocker/ WGI TSU, Switzerland]	Accept. Revised
4-947	4	59				Fig 4.1: The figure and legend needs to use more distinctive colors. [Thomas Stocker/ WGI TSU, Switzerland]	Accept. Revised
4-948	4	59				Figure 4.1: The more than 1650 local glaciers on Disko Island and Nuussuaq Peninsula, West Greenland (similar number of glaciers as in Norway) deserve to be shown with yellow, as they are not within the Greenland Ice Sheet. [Jacob Clement Yde, Norway]	Noted. Will revise
4-949	4	59				Data citation in Figure 4.1: I'm not quite sure, which glacier dataset was used in this figure - probalby ESRI's Digital Chart of the World (Danko 1992) as available from the GLIMS website (cf. Raup et al. 2000) - but I'm sure that the given sources are not appropriate: - Weidick et al. (1992) covers West Greenland only. - Who is Zheltyhina? The correct citation of the GLIMS database is "GLIMS (2011)" However, I would suggest using here the glacier layer of the new global glacier map by Arendt et al. (2012). References: Danko, D. M. (1992). The digital chart of the world project. Photogrammetric engineering and remote sensing, 58(8), 1125-1128. GLIMS (2011): GLIMS glacier database. Armstrong, R., B. Raup, S.J.S. Khalsa, R. Barry, J. Kargel, C. Helm, and H. Kieffer. 2011 (eds.), National Snow and Ice Data Center, Boulder, Colorado USA: Digital media. Raup, Bruce H., Hugh H. Kieffer, Trent M. Hare, and Jeffrey S. Kargel (2000). "Generation of Data Acquisition Requests for the ASTER Satellite Instrument for Monitoring a Globally Distributed Target: Glaciers." IEEE Transactions On Geoscience and Remote Sensing 38:1105--1112. [Michael Zemp, Switzerland]	Noted: The glacier locations in the SOD will be based on the new Randolph Glacier Inventory
4-950	4	60	1			Left graphics: Location of Examples should also include Alaska, where most glaciers are land terminating. [Martin Lüthi, Switzerland]	Noted - figure has been completely revised
4-951	4	60	4			I guess 'Location of examples' means 'Examples of locations' ? Such glaciers are also found in other regions. [Regine Hock, US]	Noted - figure has been completely revised
4-952	4	60	4			The ranges given are to rigid. There are land-terminating glaciers that are far smaller than 10 km. All ranges are only very rough. [Regine Hock, US]	Noted - figure has been completely revised
4-953	4	60	4			Tidewater glacier and marin-terminating glaciers makes it sound is if these were different glaciers which they are not. Per definition a tidewater glacier is a marin-terminating glacier. [Regine Hock, US]	Noted - figure has been completely revised
4-954	4	60	4			not sure if 'cool temperate' is the appropriate term for a climate regime. Here probably 'maritime' is better. [Regine Hock, US]	Noted - figure has been completely revised
4-955	4	60	5	60	5	Please clarify the difference o tidewater and marine glaciers, their climatological meening. [Hiroyuki Enomoto, Japan]	Noted - figure has been completely revised
4-956	4	60				Fig 4.2: ice extent in km2, not km [Luzi Bernhard, Switzerland]	Noted - figure has been completely revised
4-957	4	60				Figure 4.2: Depending on the definition of "temperate climate (e.g. by latitude versus temperature characteristics) land-terminating glaciers in high mountainous areas do not only occur in temperate climate,	Noted - figure has been completely revised

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						but also in (cold dry) continental climates, sub-polar regions as well as sub-tropics and tropics. The current usage is ambiguous. The southern part of the Greenland ice sheet is by definition not in the polar region anymore, as it is south of the polar circle. "Exclusively" is too strong here. [Olaf Eisen, Germany]	
4-958	4	60				Figure 4.2: Land-terminating glaciers in mountain regions also frequently exist in polar environments [Stephan Gruber, Switzerland]	Noted - figure has been completely revised
4-959	4	60				Figure 4.2, captions: Delete 'polar' in line 5. [Jacob Clement Yde, Norway]	Noted - figure has been completely revised
4-960	4	60				Figure 4.2: - in my view, this figure has not much added value and is too fuzzy in terms used, for example: - the accumulation area seems not having any implication on the glacier characteristics?! - the left scheme indicates (mean or max?) ice extents and thicknesses of 10-100km and 100-1000m, respectively, for European Alps, Andes, Himalayas, and Africa. However, glaciers in these regions are generally much smaller. - the given terms for climate regimes (temperate, high mountain, cold temperate, polar) seem to be not very consise; I would rather use continental vs. maritime (for climate regimes)and temperate vs. polythermal vs. cold (for ice temperature regimes) - this figure implies that "tidewater and marine-terminating" glaciers are typically "cold temperate to polar"; I would rather say that tidewater glaciers are common in maritime climate regimes. Recommendation: remove or clarify. [Michael Zemp, Switzerland]	Noted - figure has been completely revised
4-961	4	61				Fig 4.3: ditto comment no 5 [Luzi Bernhard, Switzerland]	Accepted
4-962	4	62				year should be mentioned along x-axis [Muhammad Amjad, Pakistan]	Accepted
4-963	4	62				Fig 4.4: change the x-axe descriptions. Above perenial trend, below multi-yr trend; like the values in the graph [Luzi Bernhard, Switzerland]	Accepted
4-964	4	62				Figure 4.4 misses the time axis. [Richard Bintanja, Netherlands]	Noted
4-965	4	62				Please mark the x-axis appropriately. [Øyvind Christophersen, Norway]	Accepted - figure revised
4-966	4	62				Figure 4.4: x-axis labels missing. [Chris Derksen, Canada]	Accepted - figure revised
4-967	4	62				Figure 4.4: Are both panels (extent and area) needed? [Stephan Gruber, Switzerland]	Noted
4-968	4	62				Fig 4.4: Please specify 'sea' ice in figure title and caption and add years to the x-axis. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - figure revised
4-969	4	62				Figure 4.4: Insert years on x-axis. [Jacob Clement Yde, Norway]	Accepted - figure revised
4-970	4	63				Fig 4.5: write out the years, 2004, 2005 ... 2008 [Luzi Bernhard, Switzerland]	Noted
4-971	4	64				Fig 4.3: ditto comment no 5 [Luzi Bernhard, Switzerland]	Noted
4-972	4	64				Fig 4.6: Some confusion here with 2007, 2010, 2011 as listed in the caption. Why is 2010 singled out here - should this not simply be 2007-2011 (as used in the caption of Fig 4.3). We also question the decision to double-count 2007 in these analyses, and this decision requires explanation. [Thomas Stocker/ WGI TSU, Switzerland]	Noted
4-973	4	65	4			"of linear decadal trends" Be sure to indicate that it is annual average data in all plots on left. [Alan Robock, USA]	Noted
4-974	4	65				Figure 4.7 (e) It seems more logical to present in this figure the trend of sea ice speed obtained by (Rampal et al., JGR-C, 114, 2009), published previously, and which covers the entire period 1979-2007, as for the other	Noted

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						observables of this figure 4.7 - or, maybe even better, to present both datasets (Rampal et al. 2009 and Spreen et al. 2011). [Jerome WEISS, France]	
4-975	4	66	1	66	1	At first sight there is confusion between region numbering and glacier area. It would be helpful to indicate area (white numbers) everywhere with the units, i.e."89 km2" for Alaska (region 1). [Martin Lüthi, Switzerland]	Taken into account: The Figure wil be revised and updated.
4-976	4	66	6			Fig 4.8: Arendt and Al 2011, not Arendt et al. 2011 [Luzi Bernhard, Switzerland]	Rejected
4-977	4	66	11			Fig 4.8: Arendt and Al 2011, not Arendt et al. 2011 [Luzi Bernhard, Switzerland]	Rejected
4-978	4	66				Figure 4.8: change "(white)" to "(white numbers)" [Olaf Eisen, Germany]	Editorial
4-979	4	66				Figure 4.8: if the number '5' should refer to a total glacier area of 5000 km2 for the tropics, then this number is too high, at least for present conditions (my guess would be something on the order of 2500 km2 but I would need to check this in more detail). [Christian Huggel, Switzerland]	Taken into account: The Figure wil be revised and updated.
4-980	4	66				Fig 4.8 - 4.11: We suggest this analyses could be made more comprehensive by providing a Table listing all 19 regions, including area, length changes, annual loss rates, and mass change rates. Consider to start with one basic map introducing the regions without any quantitative information. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account: Glacier area and volume per region are shown in a table. Rejected: Length changes could not be shown in the table.
4-981	4	67	1	67	1	What is the reason of the selection of these seven areas? [Hiroyuki Enomoto, Japan]	Noted: Only regions with long time series are shown. More regions were added now.
4-982	4	67	1			Figure axes are not labeled [Martin Lüthi, Switzerland]	Editorial
4-983	4	67	4	67	4	"Cumulative length variations" is cumbersome; replace by the simpler: "length variations". [Martin Lüthi, Switzerland]	Rejected: The graphs refer to cumulative values.
4-984	4	67	5	67	5	"Data from WGMS" is inappropriate. List the individual data contributors, as is done in Figures 4.10 and 4.11. [Martin Lüthi, Switzerland]	Rejected: WGMS is all data contributors
4-985	4	67				Fig 4.9: the quality of the graphs is bad [Luzi Bernhard, Switzerland]	Editorial
4-986	4	67				Figure 4.9: Numbers in upper right graph difficult to read, use smaller font. [Olaf Eisen, Germany]	Editorial
4-987	4	67				Figure 4.9: interesting and informative figure but legends and axis numbers of sub-figures are not readable. [Christian Huggel, Switzerland]	Editorial
4-988	4	67				Fig 4.9: Why are only these 9 regions selected? Explanation must be provided in the text or caption. [Thomas Stocker/ WGI TSU, Switzerland]	Noted: Only regions with long time series are shown. More regions were added now.
4-989	4	67				Figure 4.9: This figure should be replaced by a figure showing the compiled results of glacier fluctuation studies for all 19 regions [Jacob Clement Yde, Norway]	Noted: Only regions with long time series are shown (will be extended and revised).
4-990	4	67				Figure 4.9: If these Icelandic glaciers should be included in this figure, the wide readership of the report should be informed about their surge activities to avoid misleading interpretations. [Jacob Clement Yde, Norway]	Taken into account: Surge type glaciers have been mentioned.
4-991	4	67				Figure 4.9, suggestion for correct data citation: "Data from WGMS (2008 updated, and earlier issues)." [Michael Zemp, Switzerland]	Editorial
4-992	4	67				Figure 4.9, suggestion for improved caption: "Cumulative length changes of selected glaciers as measured [...]" [Michael Zemp, Switzerland]	Editorial
4-993	4	68	1	68	1	This figure is important but the colour indication is not clear. The bars can be numbered. [Hiroyuki Enomoto, Japan]	Editorial
4-994	4	68				In the Spanish Pyrenees was accounted a decrease of the 28% of the glaciated surface during the period 1992 (495.7 ha) to 2000 (355.5 ha). Chueca J, Julian, A., Peña-Monne, JL. 2002. Comparación de la situación de los glaciares del pirineo español. Boletin Glaciológico Aragonés 3: 13-41 [Juan Ignacio López Moreno,	Noted: Is there also a peer-reviewed reference?

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						Spain]	
4-995	4	68				Figure 4.10: I have sympathy for this figure because it is transparent and fair, showing the mean loss rates over given periods of time, and directly allows to trace back individual studies. The problem is that it is not very reader-friendly. There is just too many lines, and the colors are not well distinguisable. I do not have a perfect solution, though. An option could be to blow up the y-axis part of 0 to -2% and describe the other two outliers in the fig caption. The numbers of the regions could then be added to the lines to better identify them. [Christian Huggel, Switzerland]	Taken into account: The minimum value shown is now -2% which considerably improves the readability.
4-996	4	68				Why is a type of graphics shown which has never published in the reviewed literature? In my opinion it is meaningless to calculate average area change for an area. Such a plot depends entirely on the statistical sample, and will be dominated by behavior of the numberous small glaciers. For example: Aletschgletscher area is reduced by 1%, Pizolgletscher area grows by 2%, so the average is +1%. Meaningless. Better would be to display relative area change for individual glaciers. [Martin Lüthi, Switzerland]	Rejected: This type of graphic is common in the literature, e.g. to show the mass budget estimates for the ice sheets over time. Rejected: The calculation of mean changes always refer to the total area change (over an entire mountain range) rather than to arithmetic averaging.
4-997	4	68				Somehow this looks like unpublished (not peer-reviewed) work is pushed into a high profile report. The IPCC report should assemble the state of the field, not serve as platform for new (experimental) data analysis which has not proven its usefulness. [Martin Lüthi, Switzerland]	Rejected: This graph is not related to data analysis, it just summarizes the reported observations from the peer-reviewed literature in a graph instead of a table.
4-998	4	68				Fig. 4.10: Why are there many more lines than in the legend? [Alan Robock, USA]	Noted: Several regions have more than one observation (cf. Fig. caption).
4-999	4	68				Fig. 4.10: I see no brackets mentioned in the caption. [Alan Robock, USA]	Editorial: The subregions are in brackets.
4-1000	4	68				Fig. 4.10: The colors are not distinct enough to identify the curves. There need to be better labels. [Alan Robock, USA]	Editorial: The colours will be adjusted for the final version.
4-1001	4	68				Fig 4.10: Why are only 14 out of 19 regions plotted? [Thomas Stocker/ WGI TSU, Switzerland]	Noted: Only for these regions we found results in the literature.
4-1002	4	68				Figure 4.10: Insert a reference to 'Mernild et al., in review: Multi-decadal marine and land-terminating glacier recession in the Ammassalik region, Southeast Greenland. Cryosphere'. This paper calculates a mean annual area loss rate of 1.06% per year between 1986 and 2011 for local glaciers in the region '5 Greenland'. [Jacob Clement Yde, Norway]	Noted: We here exclude marine terminating glaciers and consider only mean values over entire mountain ranges..
4-1003	4	69				Figure 4.11. Impressive figure but lack a bit of readibility (font of the legend too small). Including the estimate from Hirabayashi et al., 2010 adds a lot of noise… [Etienne BERTHIER, France]	Readability: Taken into account: readability was improved; Hirabayashi: Taken into account: new model runs were considered
4-1004	4	69				Figure 4.11: Caption should provide explanation for regions shown in upper middle right graph, which indicates the region numbering. In this graph, as above (Figure 4.9), numbers are too big. Decrease font size for clarity. [Olaf Eisen, Germany]	Taken into account: region description has been rearranged for more clarity
4-1005	4	69				Figure 4.11: I appreciate the great efforts put into this figure and information condensation but I really am afraid there is too much info in it for a reader to understand. The readability is also strongly questionable. For instance, color shading in the sub-figures is not clear. The individual lines are simply not distinguishable in this size. Is SLE in mm/yr and over which period? I'm also wondering whether it would not be more useful to indicate the mass (volume) changes in mm w.e., although this might involve some conversion from some of the studies that where mass changes are given in kg/m2/yr. [Christian Huggel, Switzerland]	Taken into account: Figure has been improved
4-1006	4	69				Figure axes are not labeled. [Martin Lüthi, Switzerland]	Taken into account: labeling was added
4-1007	4	69				It is questionable how meaningful it is to plot regional averages of average net balance. This type of graphics will be dominated by the behavior of small glaciers. For example: Aletschgletscher average net balance is -1 m/a, Pizolgletscher +1 m/a, so the average is 0 m/a, i.e. equilibrium? Such arithmetic averages are simply not meaningful. Rather, data for individual glaciers should be shown (point plots). Trends can be easily read from such data. [Martin Lüthi, Switzerland]	Rejected: this figure compiles regional studies into which small and large glaciers are included

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4-1008	4	69				"[kg m-2 yr-1]" So what are the SLE numbers on each panel? [Alan Robock, USA]	Taken into account: labeling has been added
4-1009	4	69				"[kg m-2 yr-1]" What is SLE? [Alan Robock, USA]	Taken into account: SLE is now explained at the beginning of chapter 4.3
4-1010	4	69				"[kg m-2 yr-1]" Why does the first panel have a definition rather than an SLE value? [Alan Robock, USA]	Noted: we do not understand the comment
4-1011	4	69				Figure 4.11: 'Cogely' should be 'Cogley'. [Jacob Clement Yde, Norway]	Editorial
4-1012	4	69				Figure 4.11: The paper by Mernild et al. (in review: Response of land-terminating glaciers and ice caps in the northern North Atlantic region to climate change. Journal of Climate) contains additional estimates on glacier mass change rates from Greenland, Iceland, Svalbard and Scandinavia. [Jacob Clement Yde, Norway]	Noted: the paper was rejected by the time of the SOD submission dedaline
4-1013	4	69				For correct citation of core datasets used by Cogley (2009) see remarks (7 and 8) above. [Michael Zemp, Switzerland]	Taken into account: references have been added in the text respectively
4-1014	4	69				Figure 4.11 I propose to clearly differentiate between observation methods (glaciological, geodetic, gravimetric) and modelling approaches due to the different characteristics of the results. [Michael Zemp, Switzerland]	Taken into account: the revised figure is now discussed in the revised text
4-1015	4	70	1	70	1	Left figure can indicate the right figures area by box indication. [Hiroyuki Enomoto, Japan]	Taken into account: boxes have been added
4-1016	4	70				Fig 4.12: What is meant by 'numbers of measurements'? Individual glaciers? Point measurements? Careful coordination needed to ensure consistency with the assessment provided in Chapter 13 and vice-versa. Consider to visually put more emphasis on mass changes than SLE. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account: Figure, text and related table 4.5 have been modifed, crosschecking with Ch13.
4-1017	4	71	6			"Circles" But why are there multiple circles, and what do the different ones of the same color mean? [Alan Robock, USA]	Accept. Caption edited.
4-1018	4	71				In Fig. 4.15 I miss the recent study of Graversen et al. (Climate Dynamics, 2011). [Richard Bintanja, Netherlands]	Reject. Paper is about modelling not observations.
4-1019	4	71				Fig 4.13 and Fig 4.14: Add years to all panels - currently missing for d - f which causes confusion. [Thomas Stocker/ WGI TSU, Switzerland]	Accept. Years added.
4-1020	4	71				Figure 4.13, caption, line 7: 'blue' should be 'grey'. [Jacob Clement Yde, Norway]	Accept. Corrected.
4-1021	4	71				Figure 4.13, caption, line 5-6: The first reference to Velicogna (2009) seems to be incorrect. The paper by Velicogna (2009) only contains data until 2009, not until 2011, and does not show temporal pattern figures. [Jacob Clement Yde, Norway]	Reject. Same medthodology (as previous comment).
4-1022	4	72	9			"surface mass balance" How can this be positive for regions with ice loss in panels a-c? [Alan Robock, USA]	Reject. Surface mass balance is not the same as mass balance.
4-1023	4	72				Figure 4.14, caption, line 6: The first reference to Velicogna (2009) seems to be incorrect. The paper by Velicogna (2009) only contains data until 2009, not until 2011, and does not show temporal pattern figures. [Jacob Clement Yde, Norway]	As comment 1021.
4-1024	4	73				Figure 4.15: what does "ext." in the figure legend stand for? Explain in caption. [Olaf Eisen, Germany]	Accepted. Figure caption has been clarified.
4-1025	4	73				Why is vanden Broeke and Bamber (2011) not included? [Martin Lüthi, Switzerland]	Rejected - that citation does not contain suitable data
4-1026	4	73				Fig 4.15 and Fig 4.16: For transparency and to avoid misinterpretation, these plots should also include the unweighted results. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Only unweighted averages are now shown in plots and tables Studies rejected e.g because they are superseded, or do not present error estimates are noted in the "appendix".
4-1027	4	74	5	74	5	Dong-Chen et al., 2009 to E et al. 2009" [Zeng-Zhen HU, USA]	Accepted - change to references will be made

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4-1028	4	74	7	74	7	also cite vanden Broeke and Bamber (2011). [Martin Lüthi, Switzerland]	Rejected - this paper has no new data.
4-1029	4	74				Figure 4.16: what does "ext." in the figure legend stand for? Explain in caption. [Olaf Eisen, Germany]	Accepted. Figure caption has been clarified.
4-1030	4	75	4			Error bars should be included [Regine Hock, US]	Accept. Error bars are included and legend improved.
4-1031	4	75				Fig. 4.17: First three label marks are actually 6 yr periods and not 5 yr on x-axis. [Alan Robock, USA]	Reject. Reviewer has miscounted.
4-1032	4	75				Fig. 4.17: Last label mark should be plotted closer to others, to make time linear on x-axis. [Alan Robock, USA]	Accepted. X-axis has been changed to reflect uneven time periods.
4-1033	4	75				Fig 4.17: It would be useful to add uncertainties to the plotted values. We would also suggest to delete the lines which are very misleading. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Individual points with error bars have been plotted - not curves.
4-1034	4	76	5			"marine-based parts of the ice sheet highlighted" How? I don't see any. [Alan Robock, USA]	Accept. Figure caption corrected
4-1035	4	76	5			"arrows showing access routes" Should be pointing in opposite direction. [Alan Robock, USA]	Accept. Arrows removed and replaced by areas of current changes.
4-1036	4	76				Figure 4.18: Greenland inset map lower left should be explicitly mentioned in caption as a part of map from upper left. [Olaf Eisen, Germany]	Accepted
4-1037	4	76				Fig 4.18: It seems the information contained in this figure could be usefully incorporated into Figures 4,13 and 4.14. [Thomas Stocker/ WGI TSU, Switzerland]	Reject. At the scale of the figures 4.13 and 4.14 the details cannot be seen. Also we wanted to show Greenland and Antactica at the same scale.
4-1038	4	77	4			Define "SCE" [Alan Robock, USA]	Accepted - Text revised
4-1039	4	77				Figure 4.21 is quite difficult to comprehend. I suggest to rework it to make things more clear. [Richard Bintanja, Netherlands]	Accepted - figure revised for clarity, and TSU will be urged to ensure that the final appearance resembles the original high-resolution file
4-1040	4	78	4			Define "SCE" [Alan Robock, USA]	has already been defined in the text and in the caption of Fig 4.19
4-1041	4	79	1	79	1	Meaning of the regend is not clear. -10C - 5C? [Hiroyuki Enomoto, Japan]	accepted - notation added to legend
4-1042	4	79	4			"metrics" all of them need to be defined [Alan Robock, USA]	accepted - now refers to text for definitions
4-1043	4	79				Fig 4.21: bad quality of the figure. And the cited authors are not listed in the reference chapter 40ff [Luzi Bernhard, Switzerland]	Accepted - figure revised for clarity, refs added, and TSU will be urged to ensure that the final appearance resembles the original high-resolution file
4-1044	4	79				Figure 4.21; many of the references are missing [Richard Essery, UK]	Accepted - refs added
4-1045	4	79				Fig. 4.21: The metrics "SD" or "SCD" has not been explained. [Christoph Marty, Switzerland]	accepted - now refers to text for definitions
4-1046	4	79				Fig. 4.21: The listed studies are not referenced in the corresponding chapter. [Christoph Marty, Switzerland]	Accepted - refs added
4-1047	4	79				Fig. 4.21: The study of Marty, which analyzed SCD was publsihed in 2008 and not 2011. [Christoph Marty, Switzerland]	Accepted - figure revised
4-1048	4	79				Fig. 4.21: In the Marty (2011) study 100% of the stations showed change < 0. [Christoph Marty, Switzerland]	Accepted - figure revised
4-1049	4	79				Fig. 4.21: The different labeling of the lower and upper x-axis is misleading. [Christoph Marty, Switzerland]	Accepted - figure revised
4-1050	4	79				Fig 4.21: Colour key needs to be prominently displayed. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - figure revised

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4-1051	4	80	9			"063" Remove leading 0 in this and other stations [Alan Robock, USA]	Accepted
4-1052	4	80	10			648°E cannot be right. [Alan Robock, USA]	Noted - typo addressed
4-1053	4	80				Fig 4.22: Add uncertainties. Is this the complete network or a selection of boreholes? [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - Data are from a network of serveral hundred stations
4-1054	4	81				Figure 4.23: Sharp permafrost boundaries mis-communicate ist true nature. This *IS* important because this perception prevents the understanding of scaling conflicts in e.g. continental-scale model assessments. [Stephan Gruber, Switzerland]	Noted: However, we have focussed on using "measured" data to provide an overview of changes, modeling results are helpful, but not that is not the focus here
4-1055	4	81				Fig. 4.23: Colors are so close, they are hard to distinguish, particularly Discontinuous. [Alan Robock, USA]	Accepted
4-1056	4	81				Fig 4.23 - The data presented do not appear in Shiklimanov et al (2010) - this focusses on Barrow not other regions. [Sharon Smith, Canada]	Noted a better citation has been used
4-1057	4	81				Fig 4.23: Need to include continental margins to help reader get a geographical orientation. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted
4-1058	4	82	5			"changes" Departure from what? [Alan Robock, USA]	Noted: clarification has been made
4-1059	4	82	5			"bottom:" What are the three different lines? [Alan Robock, USA]	Noted: clarification has been made
4-1060	4	82				Figure 4.24: What is different about the sites represented by blue dots? [David Parker, United Kingdom of Great Britain & Northern Ireland]	Noted: clarification has been made
4-1061	4	82				Fig 4.24: What is the distinction between red and blue marked stations? Please clarify the data basis for the lower panel. [Thomas Stocker/ WGI TSU, Switzerland]	Noted: clarification has been made
4-1062	4	83				FAQ 4.1, Figure 1: change (left) and (right) to (left rectangle) and (right rectangle), respectively. [Olaf Eisen, Germany]	Editorial
4-1063	4	84	1	84	1	Winter sea ice in the northerhemisphere has decreasing trend. It is large in the lateral area in the sub arctic seas. Thus, Sea of Okhotsk should include in this information. [Hiroyuki Enomoto, Japan]	Noted -
4-1064	4	84				FAQ 4.2, Figure 1: What is the source for these sea ice trends? The %/decade trends for the Canadian Arctic Archipelago do not match the results of Tivy et al., 2010, who reported 2.9%/decade. Tivy, A, Howell S, Alt B, McCourt S, Chagnon R, Crocker G, Carrieres T, and Yackel J (2011) Trends and variability in summer sea ice cover in the Canadian Arctic based on the Canadian Ice Service Digital Archive, 1960–2008 and 1968–2008. 2011. Journal of Geophysical Research 116 (C03007). doi: 10.1029/2009JC005855. [Chris Derksen, Canada]	The source is an updated version of data reported in Comiso and Nishio (2008). The Tivy et al result is for a particular region in Canada and for a different time period.